

C. E. Ferguson's lost reply to Joan Robinson on the theory of capital Scott Carter¹

I. INTRODUCTION

The period of 1966 through 1971 was a critical time in the theory of capital. The Cambridge Controversies were in full swing and neoclassical economists were reeling from the breakdown of their theory which on an important level was happening before their very eyes. The Cambridge Critics², referring to the Cambridge England critics of neoclassical capital theory, had scored a significant victory with the publication in 1966 of the famous symposium on reswitching in the *Quarterly Journal of Economics*. There it was demonstrated without question that the twin “perversities” or paradoxes of reswitching of techniques and reverse capital deepening were generally valid and only in simple one-commodity or malleable capital models could they be excluded from consideration. This drastically limited the generality of neoclassical theory. The situation by 1971 had gotten quite acrimonious and heated. Although the eventual “answer” forwarded in the neoclassical defense would eventually take the form of intertemporal and temporary general equilibrium models, thus side-stepping completely any homogenous or malleable notion of capital, this would not come into the foreground until 4 years later, culminating in Frank Hahn (1975) and especially Christopher Bliss (1975). Put simply, neo-Walrasian models of temporary and/or intertemporal equilibrium remained pretty much under the neoclassical radar by the end of 1971.

In spring 1970 Joan Robinson, one of the most virulent of the Cambridge Critics, commenced a debate with the American neoclassical economist C. E. Ferguson that appeared in the pages of the *Canadian Journal of Economics* in 1970 and 1971.

Ferguson is best known for having said that reliance of neoclassical theory in light of the paradoxes was a “matter of faith” to be sorted out among the econometricians.³ Here we have one of the most forceful statements both of reliance on the robustness of one-commodity or malleable capital models as well as refuge in the argument that perversities notwithstanding, in the end it all came down to an empirical question, with the latter embodying the neoclassical methodology of prediction over logical consistency and explanation (Cohen 1984). In May 1970 Robinson published in the *Canadian Journal of Economics* a blistering account of the capital controversies in her article “Capital Theory up to Date” (Robinson 1970a), still one her many often-cited works on the theory of capital. In that article she explicitly mentions the theoretical paucity of Ferguson and other notable neoclassical economists. The following month of June 1970 she published in the *Economic Journal* her review of Ferguson’s (1969) book, *The Neoclassical Theory of Production and Distribution*,⁴ an account that was equally as blistering as that of one month earlier. In 1971 Ferguson wrote a reply to Robinson (Ferguson 1971) and Robinson immediately replied (Robinson 1971), again in the pages of the *Canadian Journal*. And so the published account of the debate ended there. Robinson of course would continue her criticism of neoclassical capital theory literally up until her death in 1983, although in the last decade she was concerned more along methodological rather than theoretical lines of argumentation. For his part Ferguson died in January 1972 at the young age of 43.

Recent archival evidence at the Robert Solow Papers and the Martin Bronfenbrenner Papers, both of the Economics Papers Project of Duke University, has unearthed some

very interesting correspondence between Ferguson and Robert Solow and between Ferguson and Martin Bronfenbrenner. Also unearthed in the Martin Bronfenbrenner Papers is an unpublished response to Robinson's Reply entitled "The Cambridge Confusion: Reply to a Reply" that Ferguson had written by September of 1971, only three months before his death. That Reply is for the first time published as an Appendix to this essay. In this Reply, Ferguson articulates a forceful defense that is strictly within the confines of the neoclassical capital theory of the time, namely (i) that aggregate malleable capital models are nonetheless appropriate tools of analysis, paradoxes notwithstanding; and (ii) it all boils down to an empirical question in the end anyway. This Reply, and the help that Ferguson had in drafting his published response to Robinson by Robert Solow, provide insight into the history of the capital controversies at this crucial time, especially within the American neoclassical camp.

The remainder of this brief essay is structured as follows. In section 2 some methodological questions and issues over the Cambridge capital controversies will be developed. There it will be argued that the approach taken in Avi Cohen and G. C. Harcourt (2005) provides a very good account of the nature of these questions, especially as regards "faith" in the robustness of simple one-commodity or malleable capital models as well as the refuge neoclassical took in "empirical verifiability". Also discussed there is the useful breakdown Philip Mirowski (1989) introduces as regards the two phases in the neoclassical defense. As we shall argue, the Ferguson-Robinson exchange belongs wholly to the former phase. In section 3 the Ferguson-Robinson debate is discussed and the role Robert Solow played in influencing Ferguson, whom Harcourt once called

Solow's "most enthusiastic proponent" (Harcourt 1976, p. 51) is highlighted. In section 4 the Robinson-Ferguson debate is revisited with discussion of the recently uncovered lost "Reply" by Ferguson and the subsequent path that Robinson took that focused less on the technical questions and more on methodological issues and ramifications involved. The final section concludes.

II. SOME HISTORICAL AND METHODOLOGICAL CONTEXT

In order to understand the significance of the Reply that Ferguson gives to Robinson, the historical context of the debate at that time needs to be addressed. In considering the history of the Cambridge Capital Controversies, it is useful to consider Mirowski's (1989) two-phase typology of the neoclassical defense as broadly broken down into two distinct periods, a "disoriented and disorganized" vs. a "subtle and sophisticated" phase:

"The response of the neoclassicals to their November 1966 debacle (i.e. the 1966 *QJE* "Symposium") was...very instructive. At first, disoriented and disorganized, some retorted that critics had a point, but that the putative prevalence of perverse behavior of capital was really an empirical issue...With the passage of (historical) time, the neoclassical responses grew more subtle and sophisticated" (Mirowski 1989, pp. 343-344).⁵

The first phase is associated with the continued adherence to the homogenous malleable capital notion – hence the one-commodity model⁶ - with an emphasis on empirical verifiability. The second phase in contrast eschewed completely the notion of homogenous capital and developed neo-Walrasian models of heterogeneous capital in an inter-temporal/temporary general equilibrium framework absent of a general rate of

return on the supply prices of the various capitals. The years 1973-1975 mark the watershed between the two phases and the publication of Bliss (1975) signifies the second phase's ultimate triumph.⁷ What is clear is that in the period relevant for the present essay, 1970-1971, the neoclassical defense was in the throes of the first "disoriented and disorganized" phase. It is here that Ferguson's contributions wholly belong and it is here that his debate with Robinson was waged.

The famous 1966 Symposium on Reswitching in the *Quarterly Journal of Economics* had demonstrated unequivocally the possibility of the dual paradoxes of reswitching, defined as the return to techniques at two or more different rates of profit, and capital reversals, a generalization of the reswitching phenomenon resulting in a violation an inverse relation between capital intensity and the rate of profit - hence an inverse demand curve for capital cannot in general be assumed. By the time of this Symposium all sides accepted that Paul Samuelson's student David Levhari (1965) had been mistaken in his attempt to disprove the paradoxes with the so-called "non-reswitching theorem" and that such possibilities were in fact generally valid.⁸ Where the two Cambridges differed concerned the *implications* of these possibilities. The Cambridge, UK school held (holds) that all this uncovered serious logical problems in neoclassical theory than in effect renders it vacuous in terms of explaining actually existing capitalistic economic systems. An extreme version of this approach holds that the capital controversies show in a capital-theoretic context the weaknesses of (i) the subjective theory of value, (ii) the concept of the margin of constrained choice, and hence questions (iii) the generality and indeed

relevance of the marginal method itself. According to this critique it is the fundamental standard neoclassical *vision* of an economy that becomes problematic.⁹

To this neoclassicals scoffed. Perhaps there had been a demonstration that aggregate J.B. Clark parables do not have the generality they were once thought to have, however there's a baby in that bathwater and hence there is no need to throw away 100 years of fruitful developments and evolution in the theory and empirics emanating from the marginalist revolution. The question became one not of high theory, but practical relevance. This practical relevance took the form of the standard neoclassical methodology of prediction over explanation as regards the ultimate criterion of theory.¹⁰ This criterion of prediction assumed two expressions in the early neoclassical defense, (i) that of an attempt through model analysis to demonstrate that the conditions that allow for reswitching are even further restrictive, in effect reviving and modifying Levhari's (1965) methodology in his earlier failed attempt to disprove the paradoxes, and (ii) that in the end it remained an empirical question after all. For his part Solow at this time chose to abandon the aggregate capital concept and tried to revive Fisher's notion of the social rate of return on investment, thus allowing him to circumvent the difficult problem of capital stock valuation altogether (Solow 1963, 1967, 1970).¹¹

The neoclassical defense, 1970-71

The basic methodology of neoclassical defenses of capital theory circa 1970-71 assumed the form of somehow justifying the results of one-commodity/malleable capital models within some variant of the two-sector model. Since the conventional two-sector model

exhibited without a doubt reswitching and capital reversals, models that ruled out reswitching had to in some sense modify the convention. What these neoclassicals held to be evident from the models they built was that upon further inspection, the possibility of reswitching can be even more unlikely. Threshold values of different aspects of these two-sector model extensions were posited and paradoxes in capital theory were increasingly understood to be the production-dual of what Giffen's paradox is to the theory of demand; indeed C. E. Ferguson and Robert F. Allen (1970, p. 109) explicitly refer to "Mrs. Robinson's Paradox" (Stiglitz 1974, p. 896 also makes a great deal of the analogy with Giffen's paradox).

Murray Brown (1969) represents one such attempt. Developing a line of analysis introduced in John Hicks's (1965) *Capital and Growth*, "composition effects" are distinguished from "substitution effects" in the assessment and valuation of capital. If substitution effects dominate (up to a threshold) then the case for reswitching is further restricted and qualified. Conversely if composition effects dominate, then the possibility (*not* the necessity) of reswitching arises. Brown called this condition "capital intensity uniqueness" (CIU) and showed that a systematic relation can be posited between CIU and reswitching of techniques such that "enough" substitutability ensures against capital perversities. This methodology is definitely adopted in Ferguson's (1969) book, "[i]f there is 'enough' substitutability in the economy, either between factors of production or between commodities in demand, neoclassical theory emerges unscathed. Otherwise not." (Ferguson 1969, p. 258).¹²

Ferguson and Allen (1970) develops the interaction between factors of production and commodities in demand in the context of the reswitching story. They wed the Hicks-Brown composition-substitution threshold-effect story to the (macroeconomic) general equilibrium framework developed in the seminal work of Ronald Jones (1965).

Considered in the analysis are comparative-static changes in the macroeconomic general equilibrium model with focus on the relationship between demand prices and factor prices just prior to and just subsequent to a switchpoint. With the Hicks-Brown substitution-composition effect adjustment process assumed it is shown that even in the case when reswitching would be possible, the viable values of the profit rate that ensure reswitching are even further restricted, thus ensuring the outcome of reswitching less likely.

Similar in methodology, Kazuo Sato (1974) introduces the concept of the “technology frontier” and makes a great deal out of this (in a letter to Solow dated May 20, 1971 Sato claims ownership to this concept as an “innovation of mine”)¹³. The technology frontier is defined as the set of all available efficient techniques of production; basically it is Joan Robinson’s “book of blue prints” (whom he does acknowledge which makes it more puzzling how he thinks that he’s the “innovator” of the concept). Sato takes the “technology frontier” and constructs a model such that the *substitution parameter* along the frontier is upper-bounded by unity. From there a Hicks-Brown dominant substitution effect argument is employed and reswitching is shown to be further anomalous.¹⁴ Lowell Gallaway and Vishwa Shukla (1974) posit – erroneously as shown in separate comments by Pierangelo Garegnani (1976b) and Sato (1976) – the condition that rules out

reswitching as the case when commodity prices in the system are positive and finite for any positive value of the interest rate. Here we find a similar methodology Ferguson and Allen (1970) with the introduction of equations for commodities resulting in a “significant” modification and restriction of paradoxes in capital theory. Gallaway and Shukla (1976) ultimately concede as correct the refutations of Garegnani’s and Sato’s (separate) critiques.¹⁵ All this served to buttress the argument that in the end it was an empirical matter after all.¹⁶

III. FERGUSON’S DEBATE WITH JOAN ROBINSON AND THE INFLUENCE OF ROBERT SOLOW

Such was the state of the defense of neoclassical capital theory in 1970-71. And Joan Robinson attacked this line of defense ruthlessly. She begins by setting Ferguson and other prominent neoclassical economists in her sites in two important articles that came out in 1970. In May of that year the article “Capital Theory Up to Date” (Robinson 1970a) came out in the *Canadian Journal of Economics* and one month later her review of Ferguson (1969) came out in the *Economic Journal* (Robinson 1970b). In both she came out very critical of the neoclassical defense of capital and agreed whole-heartedly that given what she perceived as logic problems,¹⁷ reliance on that theory did require ‘faith’. The underlying theme to Robinson’s 1970 line of attack played heavily on Ferguson’s own account, thus allowing for her to use his own words *against* the neoclassical theory that was ostensibly being defended. The opening salvo of Robinson’s review in the *Economic Journal* suffices to illustrate this underlying tone:

“A clear uncompromising statement of the principles of neoclassical economics will be very useful to their opponents. (I doubt whether their supporters will welcome it as much)” (Robinson 1970a, p. 336).

As do the final sentences in her original *Canadian Journal* article:

“No doubt Professor Ferguson’s restatement of ‘capital’ theory will be used to train new generations of students to erect elegant-seeming arguments in terms they cannot define and will confirm econometricians in the search for answers to unaskable questions. Criticism can have no effect. As he himself says, it is a matter of faith” (Robinson 1970b, p. 317).

It is of interest to note that Harcourt’s (1970) review of Ferguson’s book is equally critical of the neoclassical framework but less harsh than Robinson’s; indeed Ferguson himself calls Harcourt a “gentle critic” (Ferguson and Hooks 1971, p. 354).¹⁸

Because Ferguson’s (1969) book had been singled out by Robinson in both articles, he no doubt felt compelled to respond. From the Robert Solow Papers at the Economics Papers Project of Duke University, we find that on 10 February 1971 Ferguson sent to Solow a first draft of his response to Robinson under the cover of the following letter:

“Dear Bob,

Attached is a copy of an inadequate response to Mrs. Robinson’s review article of my recent book. It will appear, sooner or later, in the Canadian Journal of Economics along with the reply by Mrs. Robinson. Asimakopulos said that I could reply to Mrs. Robinson’s reply. I may call on you for assistance in this regard

Looking forward to Miami Beach.¹⁹

Very cordially yours,

[signature “Charlie”]

C. E. Ferguson

Professor of Economics”

(RSP, Economics Papers Project, Duke University).

Solow responds to Ferguson on February 22, 1971, commencing with an important warning against entering into a discussion with Robinson. The entire letter is reproduced below:

“Dear Charlie,

You know you’re playing a sucker’s game. It’s impossible to reason with Joan. She’ll never let you alone, and you can spend hours trying to figure out what is going through her mind. Her last letter to me (I hope the British mail strike lasts forever!) made it quite obvious that she doesn’t have the concept of simultaneous equations. That makes it kind of tough for us fellows.

Anyhow, I’m glad to help out on our side. I agree with nearly all you say in this reply, but let me make some comments anyhow, that you can use if they help.

Page 2. The MP equations are merely input demand equations. By themselves they contain more unknowns than equations, hence ‘determine’ nothing. It takes market-clearing equations for inputs and a lot of production equations too to solve

the system. It's not clear to me in what sense the aggregate system is underdetermined. Not if you include a time-preference equation, or some other kind of saving or demand-for-wealth equation. But that's much like the micro-system.

Page 3. Obviously, I agree with you about the empirical utility of aggregate analogies. I just wish you had called it a working hypothesis rather than a 'faith.' But this note should make it quite unambiguous.

Page 4. So far as I know, Pasinetti's 'unobtrusive postulate'²⁰ is never postulated by thee and me. In one-commodity models the very question can't arise (I suppose you might say it is postulated by default, but I regard that again as an empirical question.) In even two-sector models, whatever happens to the value (in consumption numeraire) of the capital stock just happens. I can't remember what does happen in the Uzawa model, for instance.

Page 5. I would be inclined to qualify what you say about the equality of the interest rate and rate of return on social saving. For one thing, in more general models the maturity structure is not flat and there will be many rates of interest for different loans, though they will equal the shadow price of the right constraints. Secondly, why is not the equality of the real wage and the marginal product of labor an equally important test? Third, one should distinguish (as Joan never does) between the logical consistency of neoclassical theory and its

empirical relevance or validity. On logical consistency, Joan is simply all wet. On empirical relevance she has less to say than you or I. In fact, years of experience have taught me that Joan's deepest motive is ideological. She really thinks that we are merely apologists for capitalism, and marginalism is our tool.

By the way, the activity analysis, linear-programming version of neoclassical theory is especially good at all those short-run fixed-ratio problems (except Keynesian unemployment) but Joan always pooh-poohs that as trivial.

Don't take it too seriously!

Yours,

Robert. M. Solow"

(RSP, Economics Papers Project, Duke University).

Solow's Papers also contain the original draft sent by Ferguson and his (Solow's) marginal notes, reflecting in the main the content of the 22 February reply. This response by Solow certainly influenced Ferguson's published reply tremendously, as textual comparison of the various drafts attests. Notice especially how chagrined Solow is by Ferguson's use of the term "faith", and how "working hypothesis...for aggregate analogies" is much preferred language. Ferguson certainly made this change in his published response to Robinson, and it remains an important theme of his unpublished Reply.

On 25 February Ferguson responds to Solow, very appreciative of the suggestions and somewhat introspective regarding his debate with Robinson:

“Dear Bob,

Thanks for your thoughtful and helpful letter. I am undoubtedly a sucker, and guess I will always play a sucker’s game.

I think that I have always taken a candid appraisal of my own abilities. This has resulted in the following: (1) I am not inventive in the sense that you and Paul are; (2) I am the best expositor or writer in the profession. While (1) remains true, (2) has been proven false.

The most important of your comments refer to my statement concerning the ‘undetermined’ neoclassical system. This was intended to be a slap at Joan because of her ignorance of simultaneous equations. Both you and Martin Bronfenbrenner have written about this point. I now must be the worst expositor. I am going into sackcloth and ashes to atone for this.

It seems that I must request the CJE to accept a modified version, which they may or may not do. In any event, I will not implicate you.

Looking forward to Miami Beach,

Very cordially yours,

[signature ‘Charlie’]

C.E. Ferguson

Professor of Economics

P.S. I have just talked to Asimakopoulous...at CJE and he will take any revised version. Of this, I am thankful. CEF”

(RSP, Economics Papers Project, Duke University).

Ferguson's response here is very interesting. Obviously he feels the negative ramifications from the manner in which his book was being received, embraced by the Cambridge Critics for all the wrong reasons and ignored by the mainstream. His published response to Robinson aided by Solow's comments is written very much as a clarification of certain themes in his book. He distances himself from the strong professions of "faith" and stresses the role of the "empirical usefulness of aggregate analogies". He clarifies the idea of "the" neoclassical theory, backtracking significantly from the stance of his book, yet in the end still recognizes the merit of the neoclassical approach:

"As a neoclassical theorist, I can only reply that the relevant question is what is relevant: should we make our predictions on the basis of what Mrs Robinson has called perverse technical behaviour or on the basis of the relations that have been repeatedly observed?" (Ferguson 1971, p. 254).

Robinson had little sympathy with this defense of neoclassical theory, and her reply to Ferguson reflects this quite so. Her argument is that because of the marginalist straight-jacket, neoclassical theorists *cannot* conceptualize a modern capitalistic system outside of what she calls their "pre-Keynesian view of modern capitalism" (Robinson 1971a, p. 255). We can trace this period in Robinson's evolving analysis as representing a significant point of clarification. The reswitching debates had demonstrated that the generality of neoclassical parables was seriously compromised by paradoxes in capital theory. But instead of considering the methodological and logical ramifications for neoclassical theory in light of these paradoxes, neoclassical theorists, argued Robinson,

responded with technical questions involved in model analysis that could be manipulated so as to be most favorable to the Good Old Theory of marginal productivity.²¹

IV. FERGUSON'S UNPUBLISHED REPLY AND ROBINSON'S SUBSEQUENT PATH²²

It is at this stage that Ferguson's heretofore unpublished Reply comes into the story. In the Martin Bronfenbrenner Papers at the Duke Archive we find a letter dated 14 September 1971, reproduced below, which serves as the cover for a response to Robinson's Reply:

"Dear Martin,

...

As you previously wrote, any author would be please to get a reply such as mine published. After the reply of Saint Joan of Leningrad (not Lorraine, I am afraid), I was prompted to write the attached reply to a reply. I don't think that even the CJE will let me get away with this. If they should, it would mark a milestone in publishing history.

Yours for neoclassicism.

Very cordially yours,

[signature 'Charlie']

C.E. Ferguson"

((MBP, Economics Papers Project, Duke University).

The brief response is entitled “The Cambridge Confusion: Reply to a Reply” and consists of three sections. The first section eschews completely the logical implications of paradoxes in capital theory and reiterates that “the chief question is an empirical one” (Appendix, p. A1), to which Ferguson articulates three “approaches” posed by neoclassical theory to address it: (i) the construction of a linearized Leontief input-output model which “does provide right empirical estimates of the interrelations among the various sectors of production”; notice how this echoes the last paragraph of Solow February 22 letter to Ferguson, (ii) the specification of methods of aggregation such that “the underlying microeconomic relations entail the existence of corresponding macroeconomic relations”, and (iii) “aggregate[ion] by analogy, using conventionally defined aggregate measures” (Appendix, pp. A1-A2). Ferguson clearly favors the latter approach:

“Such a system is not rigorous, but has offered many useful working hypotheses for the econometrician”. (Appendix, p. A2).

Ferguson in this first section simply reiterates the principle issue as an empirical one and defends the invocation of the J. B. Clark “fairytale”; indeed in a note he remarks that “Even Mrs. Robinson must believe Samuelson had *something* in mind when he called it a ‘fairy tale’” (Appendix p. A4, note 7).²³

The second section makes reference to capital-labor substitution where Ferguson cites David Ricardo’s chapter On Machinery as evidence of this concept as far back as 1817: “This simple proposition has been fundamental ever since, and has been repeatedly

observed. Capital, however defined, can be substituted for labor, and vice versa. I can see no ambiguity in this” (Appendix, p. A3).

The third and final section contains Ferguson’s last words on his exchange with Robinson. The first paragraph of this section echoes the last paragraph of Solow’s 22 February letter to Ferguson, namely that Robinson “thinks that we are merely apologists for capitalism, and marginalism is our tool” (Solow to Ferguson, quoted above). For her part Robinson does in her published Reply to Ferguson’s original comment lament against the paucity of the “new defence” of the “pre-Keynesian view of modern capitalism” she sees as underlying neoclassical capital theory. She does mention how the defense of *laissez faire* implicit in this defense is “knocked about” by the realities of “the arms race, inflation, pollution, the persistence of poverty in the rich nations and growing misery in the Third World” (Robinson 1971a, p. 256). To these notions Ferguson is baffled, and argues that this is “the crux of the matter and probably the chief confusion of the Cambridge Confusion...Neoclassical theory...is neither a defense not an advocate of any particular doctrine. It is a theory that is applicable to any economy in which the rate of interest or profit is regarded as an important decision variable” (Appendix, p. A3).

Which leaves us with Ferguson’s final words intended for a larger audience (as opposed to private correspondence) written on the subject of capital theory:

“[C]learly...Mrs. Robinson now regards capital theory as *disequilibrium theory*.

Perhaps this is so; but it is difficult to unlearn that which one has learned. I ‘grew

up' on Böhm-Bawerk, Wicksell, Knight, and Mrs. Robinson's admirable The Accumulation of Capital. That is, I have always been taught that capital theory is a study of a stationary state or a "golden age" growth model. That is what capital theory is all about. If Mrs. Robinson holds a different opinion, she is welcome to it.²⁴ But I must add: *Credo quia impossibile*" (Appendix, p. A4).

In Ferguson's final reply we find the standard neoclassical recalcitrance of that era to abandon marginalist theory and method and a defense couched in the supposed empirical verifiability of the matter wrapped in an implicit "faith" in the robustness of neoclassical fairy tales and parables. It is not known for certain whether or not Ferguson had sent this or any other manuscript to the *Canadian Journal of Economics* that was eventually rejected for publication.²⁵ Certainly nothing more by Ferguson was published in that journal, nor excepting the Appendix to the present essay has his Reply ever made its way into print.

This neoclassical recalcitrance to abandon marginal theory generally speaking made explicit in the Ferguson-Robinson exchange, reiterated in Ferguson's "lost" Reply, met Joan Robinson with increasing frustration as she aged, perhaps contributing to the nihilism she felt late in her life. Towards the end Robinson had pretty much grown disillusioned by the continued resurgence of what she termed pre-Keynesian modes of analysis and thinking, hence her call for a "thorough *spring cleaning*" in economic theory:

"It seems to me that the whole complex of theories and models in the textbooks is in need of a thorough spring cleaning. We should throw out all self-contradictory

propositions, un-measurable quantities and indefinable concepts and reconstruct a logical basis for analysis with what, if anything, remains” (Robinson 1985, p. 160)²⁶

Indeed we can locate 1971 as a period in Robinson’s own theoretical development that constitutes a significant watershed and point of departure for her last decade’s work, with her exchange with Ferguson emerging as the beginning of the end of one phase in her life and work and the start of a new. In September of 1971 Robinson published her own obituary of the question on the measure of capital, titling this work “the end of the controversy” (Robinson 1971b). There she attacked the notion that somehow neoclassical theory is an adequate “predictor” of reality, and on that score cites the critique E. H. Phelps Brown (1957), Franklin Fisher (1969) and others levied against the Cobb-Douglas production function as an example of this.²⁷

But it is really the logical aspect of these malleable-capital model defenses that Robinson objected to the most, specifically regarding the presence of circular reasoning embedded in the marginal method itself:

“There is no way of presenting a quantity of capital in any realistic manner apart from the rate of profit, so that to say that profits measure, or represent or correspond to the marginal product of capital is meaningless” (Robinson 1971b, p. 601).

From the point of responding to Ferguson in 1971 forward, Robinson became increasingly unconcerned about the technical difficulties in more general neoclassical models and began to focus on the *methodological* questions that underlie the debates. In her account of Robinson’s legacy, Ingrid Rima (1991) notes that:

“Reflecting back on the decade of the 1950s at Cambridge, when the Keynesian revolution was being consolidated and expanded, [Robinson’s] agenda for the 1970s became what she saw as the ongoing challenge to reeducate those whom she held responsible for returning economics to its pre-Keynes status because they perpetuated neoclassical approaches to economic theory” (Rima 1991, pp. 3-4).

This methodological approach of the late-Robinson is a well-documented line of research in many of the various retrospectives on her life and legacy, an approach that begins we argue with an important “putting to rest” as it were of neoclassical defenses and justifications explicit her exchange with Ferguson.²⁸

V. CONCLUSION

Joan Robinson’s exchange with C.E. Ferguson represents the beginning of a significant point of departure in the approach to capital theory the British Economist adopted from the point of this debate forward with a focus more on logical tenability and less on the technical aspects of the problems. Ferguson’s responses, both the published version and especially the recently uncovered unpublished Reply, is also very telling in that they represent a succinctly stated recalcitrance to logical criticisms emanating from the Cambridge UK encampment and reinforce argumentations and justifications of neoclassical theory based on model analysis and empirical verifiability. All of this provides a snapshot into the twilight of the earlier “disoriented and disorganized” phases of neoclassical defenses in capital theory that was based on “faith” in the robustness of surrogate parables and casual empiricism.

Beginning in 1975 intertemporal and temporary general equilibrium models were advanced at the frontiers of neoclassical theorizing and mainstream macroeconomic analysis began its love affair with so-called “microfoundations” based in neo-Walrasian models and representative agents extended to the macroeconomic sphere. Yet at the same time this was progressing on the frontiers of orthodox theorizing, aggregate neoclassical growth models were being developed and extended that continued to work within the milieu of malleable models of aggregate capital. This is especially true of the rapid resurgence in the “new” growth theoretic framework and extensions of Solow’s original growth model in the 1980’s and 1990’s. And it is this latter phenomenon that allows – indeed forces - us to revisit the early neoclassical defenses in the theory of capital especially representative of the view held by Ferguson. His response and defense betray we argue a representative *de facto* view in modern usages of aggregate capital and production theory. By bringing C.E. Ferguson’s heretofore unpublished Reply to Joan Robinson to light, it is hoped that the current generation of economists who have remained ignorant of these questions can begin to appreciate the complexity of the issues involved and the unresolved nature of the main questions that they raise.

Appendix A²⁹

The Cambridge Confusion: Reply to a Reply

C.E. Ferguson
Texas A&M University

I now agree entirely with Solow: the participants in this debate really never communicate.

I also agree with Mrs. Robinson in the sense that her reply may be exactly described by the words she used to describe my comment: “It is nothing but a repetition of several confusions that are only too sadly familiar in this tedious and unnecessary debate.”¹ The relevant question now is what confusions are relevant?²

It is natural for Mrs. Robinson to dislike being confronted with the “Cambridge Confusions”. But she made them so painfully clear in her note that I feel it is incumbent upon me to reemphasize them here.

1. “The main confusion is between a Walrasian supply-and-demand system and ‘production of commodities by means of commodities’.”³ I do not think this to be the principal confusion, but it is certainly one of them. The Walrasian system certainly embraces supply-and-demand equilibria, in each market and in the aggregate economy (Walras’ Law). But it is also a system of production of commodities by means of commodities and labor. This is clear in Walras, and it is made abundantly clear in the works of Kuenne and others.⁴

The chief question is an empirical one: what can be made of this Walrasian system of production of commodities by means of commodities? On a very different level one may concentrate exclusively on the production side of the model, linearize, and construct a Leontief input-output model. When implemented, the Leontief model does provide right

empirical estimates of the interrelations among the various sectors of production; but it gives little if any insight into behavior of economic aggregates.

A second approach is to define special aggregates and special techniques of aggregation so that the underlying microeconomic relations entail the existence of corresponding macroeconomic relations. The rigor of general equilibrium theory is preserved; but this method, even if practicable, is not practical.⁵ Therefore, most neoclassical economists adopt a third approach. They aggregate by analogy, using conventionally defined aggregate measures, conventionally defined index numbers, etc. Such a system is not rigorous, but it has offered many useful working hypotheses for the econometricians.

A second empirical question raised by Mrs. Robinson comes from her following statement: "For Walras, there is an arbitrary given stock of specific capital goods which is somehow already in existence. In the other system, only *the techniques of production are specified in advance*; the stock of capital goods is brought into being by profit-seeking investments."⁶ There is no question about the validity of the statement; the relevant question is what is relevant. Do we observe a real world in which there is an existing set of capital goods, or do we observe a world in which there is a pseudo-production function and in which entrepreneurs are sitting around waiting to be told a set of prices and wages in order to select a specific set of capital goods?

I suggest that in the real world we observe the former, although in the theoretical world we may certainly conceive of the latter. Indeed neoclassical theorists construct models much like Mrs. Robinson's, assuming well-defined production functions and profit maximization (or at least cost minimization). When pushed to the extreme, this

becomes the J. B. Clark fairy tale⁷ or the leets-labor-leets world in Mrs. Robinson's terminology. This is done not because neoclassical theorists believe it is a true and factual representation of the real world. Rather, it is intended to provide working hypotheses for econometricians in the hope that they can explain the existing structure of capital goods and its relation to the wage rates and rates of return (or is it the rate of interest?) that have prevailed over an observable period of time.

2. In regard to the production function or, as Mrs. Robinson would have it, the pseudo-production function, I am completely baffled. At one point Mrs. Robinson says that in her system "...only the techniques of production are specified in advance."⁸ At another point she says that "The coexistence at a moment of time of a number of techniques that would be eligible at different rates of profit is conceived only as a logical device to disentangle the ambiguity of 'substitution between labour and capital'."⁹ This only indicates confusion and the fact that Mrs. Robinson is not so much a Ricardian-Marxist as she would have people believe.

Ricardo, in his chapter "On Machinery", strongly emphasized the substitution of capital for labor when the real wage rate rises. This simple proposition has been fundamental ever since, and it has been repeatedly observed. Capital, however defined, can be substituted for labor, or vice versa. I can see no ambiguity in this. Of course, there could be an ambiguity if the capital is defined in value terms. But neoclassical theory, in its basic microeconomic formulation, deals with the substitutability between machinery and labor (and among machinery of different types). There is no valuation problem. If a profit seeking entrepreneur changes techniques so that more machinery and fewer men are used, we say there has been a substitution of capital for labor.

On this score I can only add that Solow proved far more than "...that a switch-point is a switch-point";¹⁰ however, I am pleased to note that I am welcome to my own beliefs.

3. Now we come to the crux of the matter and probably to the chief confusion of the Cambridge Confusion. This involves Mrs. Robinson's last paragraph¹¹ in which she talks about arms races, poverty amidst riches, and more generally, of neoclassical theory being a latter day defense of *laissez faire* capitalism. Neoclassical theory, to which Mrs. Robinson has been an outstanding contributor,¹² is neither a defense nor an advocate of any particular doctrine. It is a theory that is applicable to any economy in which the rate of interest or of profit is regarded as an important decision variable.

More to the point, this paragraph clearly indicates that Mrs. Robinson now regards capital theory as *disequilibrium theory*. Perhaps this is so; but it is difficult to unlearn that which one has learned. I "grew up" on Böhm-Bawerk, Wicksell, Knight, and Mrs. Robinson's admirable *The Accumulation of Capital*.¹³ That is, I have always been taught that capital theory is a study of a stationary state or a "golden age" growth model. That is what capital theory is all about. If Mrs. Robinson holds a different opinion, she is welcome to it. But I must add: *Credo quia impossibile*.

Endnotes to “Cambridge Confusion”

¹ Joan Robinson, “Capital Theory up to Date: A Reply,” *Canadian Journal of Economics*, IV (1971), p. 254.

² In my article [“Capital Theory up to Date,” *Canadian Journal of Economics*, IV (1971), pp. 250-4], I use the clause “the relevant question is what is relevant.” By this I meant should predictions be based upon repeatedly observed results or upon theoretically valid conjectures that have never been observed. Mrs. Robinson makes a great tease about this (*op. cit.*, p. 256), a subject I shall take up later in this note.

³ Robinson, *op. cit.*, p. 254.

⁴ Robert E. Kuenne, *The Theory of General Economic Equilibrium* (Princeton: Princeton University Press, 1963). For an extremely elementary exposition, see my *Microeconomic Theory* (Homewood Ill.: Richard D. Irwin, Inc., 3rd ed., 1972), Chapter 15.

⁵ See Henri Theil, *Linear Aggregation of Economic Relations* (Amsterdam: North Holland Publishing Co., 1954).

⁶ Robinson, *op. cit.*, p. 254. My italics.

⁷ Even Mrs. Robinson must believe that Samuelson had *something* in mind when he called it a “fairy tale”.

⁸ *Op. cit.*, p. 254.

⁹ *Ibid.*, p. 255.

¹⁰ *Ibid.*, p. 255.

¹¹ *Ibid.*, p. 255-6.

¹² For example, see Joan Robinson, *The Economics of Imperfect Competition* (London: MacMillan and Co., Ltd., 1933).

¹³ (London: Macmillan and Co., Ltd., 1956).

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Endnotes

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² The terms “Cambridge Critics” and “Cambridge Criticism, referring to the Cambridge, UK, contingent was coined by Ferguson in his 1969 opus (Ferguson 1969, p. xv and p. 251).

³ On the subject of Ferguson’s “faith” see Carter (2010); on the question of “faith” in capital theory generally speaking see Cohen and Harcourt (2005).

⁴ Ferguson’s (1969) book represents his opus. It was reviewed on four occasions, interestingly two by admitted Cambridge Critics (Robinson 1970b; Harcourt 1970). It is an interesting fact the *Southern Economic Journal* did not review his book since that is the journal of the Southern Economics Association the presidency of which belonged to Ferguson at the time of his death.

⁵ Mirowski continues:

“The orthodox history of the CCC [Cambridge Capital Controversies] now goes something like this...: Granted, some neoclassical economists had grown sloppy in their employment of the capital concept, and the Cambridge U.K. critics did catch them out. Nevertheless, this in no way impugned the real logic of neoclassical economic theory, embodied in the model of Walrasian general

equilibrium. This model has no need of the concept of capital...because it explicitly accounts for every commodity and productive input separately. Further, there is no requirement for an equalized rate of profit in this model; in any event, one would not expect it in intertemporal general equilibrium. The accomplishment of the Cambridge, U.K., critics was ultimately small potatoes, because each and every one of their results was already known by sophisticated neoclassicals prior to the CCC...In conclusion, how did everyone manage to get so worked up over so trivial and incident?" (Mirowski, 1989, pp. 343-344)

⁶ The term "one-commodity model" is here defined to include a range of homogenous malleable capital models including but not limited to the Ricardian corn model, Marx's two-sector equal organic composition model, Samuelson's surrogate production function, Swan's Meccano sets model, Robinson's leets model, Solow's aggregate capital model, etc. See Cohen (1989) for a discussion of the relevance of the one-commodity model in different types of economic theory; see also Cohen and Harcourt (2005, pp. *xli-xlv*) for a more recent discussion especially as regards some methodological implications.

⁷ The date of when the two phases in the neoclassical defense is subject to interpretation. This is especially evident in the recent (and very thorough) account of the Cambridge controversies in Lazzarini (2010).

⁸ Robinson (1970a) notes that "Professor Samuelson very handsomely admitted he had been mistaken" (p. 311).

⁹ Following Cohen and Harcourt (2005) we can identify four important elements of the neoclassical vision: (i) ubiquity of scarcity, (ii) individual choice and non-satiation, (iii)

substitutability (hence indifference) in production and consumption, and, (iv) the diminishing margin both of utility (value) and productivity (physical production).

¹⁰ On methodological issues in the Cambridge Controversies see Dow (1980, 1982), Salanti (1982), Cohen (1984, 1985, 1989), Bernstein (1985) and Cohen and Harcourt (2005).

¹¹ See also the Pasinetti's (1969) critique of Solow's model and Solow's subsequent response (Solow 1970). Harcourt (1972, 1976) also discusses the social rate of return debates. See Hagemann (1997) for a more recent retrospective.

¹² At the time the book was published Brown's essay on CIU had yet to make it in print; Ferguson (1969) references an earlier version (Brown 1967). Notice in the quote that Ferguson extends the question of substitutability to the commodity market; this will be the main theme of the macroeconomic general equilibrium framework in his later collaboration with Allen (Ferguson and Allen 1970). Robert F. Allen was Ferguson's dissertation student at Michigan State University who successfully defended his thesis on this question in 1969.

¹³ An interesting correspondence initiated by Sato to Robert Solow on his (Sato's) approach took place from 20 May, 1971 to 22 November 1971:

Sato to Solow, 20 May, 1971:

“The innovation of mine is to introduce the technology frontier as representative of the state of the arts...what I have found is...with the minimum assumption about the technology frontier, it can be conclusively (and for that matter, very simply) shown that the complete reversal of the neoclassical postulate is impossible...”

Solow to Sato, 28 May, 1971:

“I’m afraid that I just can’t figure out from your letter exactly what you have proved about the reswitching business. Obviously, you must have found a different formulation from the conventional one, because there is little doubt about what happens in the two-sector case in the conventional formulation. That would make sense to me, because I have long thought we were perhaps looking at the reswitching problem in the wrong space.” (RSP, Economics Papers Project, Duke University).

The Solow Papers have one more exchange between Sato and Solow dated 5 November and 22 November, 1971, respectively. In the end Solow encourages Sato to “get this in shape for publication”, the final version of which is Sato (1974).

¹⁴ Kazuo Sato and Murray Brown were both at SUNY Buffalo during this time.

¹⁵ It is well-known that the blow-by-blow account of the Cambridge Controversies is Harcourt (1972); for an extension of this type of approach to the above latter-day developments in aggregate-capital defenses, see Harcourt (1976). For an opposite view see Blaug (1974). More recent historical retrospectives appear in Cohen and Harcourt (2003a, b), (2005) and Bliss (2005).

¹⁶ See Carter (2010) for a more thorough account of the “empirical” defense of neoclassical capital theory in the “disoriented and disorganized” phase.

¹⁷ “[T]here is no point in discussing which is the most ‘likely to be found in reality.’...the argument is concerned with a point of logic, to which the number of instances has no relevance one way or the other” (Robinson and Naqvi 1967, p. 591; also quoted in Cohen 1984, p. 623).

¹⁸ A theme Harcourt brought out in his review that stayed with Ferguson was the critique of the definite article “the” in Ferguson’s title:

“The ‘The’ in the title may be pretentious, nevertheless the book itself is modest and worthwhile, admirably fulfilling the author’s intention of giving a systematic exposition and analysis of ‘the’ theory” (Harcourt, 1970, p. 809)

To this Ferguson would backtrack in subsequent work:

- “To avoid possible confusion, let me state what I (possibly alone) believe neoclassical theory to be” (Ferguson 1971, pg. 251);
- “In this essay the views expressed are mine alone and should not necessarily be imputed to any other neoclassical theorist” (Ferguson 1972, p. 164, note 9);
- “The description of neoclassical theory that follows is my view only and should not necessarily be ascribed to any other neoclassical theorist” (Ferguson and Nell 1972, p. 438);
- “Before proceeding further it will be useful to pause for *my* definition of terms. ‘My’ is italicized because these views should not be necessarily attributed to anyone else” (Ferguson 1973, p. 2)

¹⁹ This was the site of the Southern Economics Association Annual Conference held in November 1971. In his capacity as President of the SEA Ferguson had in a letter dated November 17, 1970 invited Solow to give the Invited Lecture, to which Solow accepted.

²⁰ This is reference to Pasinetti’s (1969) critique of the purported robustness of the one-commodity neoclassical model:

‘[T]he belief has become widespread that...an economic system in which commodities are produced by labour and capital goods behaves like the particular case of an infinite-techniques one-commodity world...The origin of this belief can be traced to an unobtrusive proposition which, for some time now, has been adopted as a postulate, i.e. as a proposition that is so evident as not to need any discussion or justification’ (Pasinetti, 1969, p. 520).

²¹ Martin Bronfenbrenner has often been credited with coining the term “Good Old Theory” to refer to the marginal productivity theory of distribution. Certainly he did use that term, especially prominently in his very well written 1971 tome on income distribution theory (which is where he is often credited as coining that phrase; see Bronfenbrenner 1971, p. 407). However both he and Robert Solow must share in this honor. In separate papers given at the 1964 Palermo conference on Income Distribution (proceedings published as Marchal and Ducros 1968), the term “Good Old Theory” appears in Solow’s paper (Solow 1968, p. 452) as well as Bronfenbrenner’s (Bronfenbrenner 1968, p. 495).

²² For a recent retrospective on evolution in the thinking of Joan Robinson on the theory of capital, the reader is referred to chapter 7 and 8 of Harcourt and Kerr (2009).

²³ “[Invoking]...the J.B. Clark fairytale...is done not because neoclassical theorists believe it a true and factual representation of the real world. Rather, it is intended to provide working hypotheses for econometricians in the hope that they can explain the existing structure of capital goods and its relation to the rates of return...that have prevailed over an observable period of time” (Appendix, p. A2).

²⁴ That Robinson may have changed her view, in this case regarding the *disequilibrium* character of capital theory, reflects a critical self-reflection she engaged in throughout her life and in her works. In a different context, Harcourt refers to such self-reflection as “not criticism but praise”. He continues:

“Her favourite story about Keynes is that when someone remonstrated with him for being inconsistent, he responded: ‘When someone persuades me that I am wrong, I change my mind. What do you do?’” (Harcourt, 1986, p. 99, note 3).

²⁵ Ferguson’s own unpublished archival material and/or correspondence cannot be found and is presumed to be lost.

²⁶ “Spring Cleaning” is published in Feiwel (1985) under the title “The Theory of Normal Prices and Reconstruction of Economic Theory”. Feiwel’s introductory chapter on “Joan Robinson’s Challenge” (pp. 69-80) is very instructive indeed, as is his Introduction to Feiwel (1989). On the question of Robinson’s nihilism see Harcourt (1995), Harcourt and Kerr (2009), Feiwel (1989), and especially Cohen (1993).

²⁷ The basic point here is that the “good fit” of the Cobb-Douglas cannot be taken as corroborating marginal productivity theory but rather is an (ex post) measure of technological/evolutionary change and/or an income identity; thus Robinson seriously questions the claims made that neoclassical results are somehow borne out in the real world. It is well-known that Robinson championed around this time then Columbia graduate student Anwar Shaikh’s (1974) HUMBUG production function article (see Turner 1989, p. 196). See Carter (2009) for an exposition of the recent debates around the Cobb-Douglas as well as original correspondence between Robert Solow and Herbert Simon in 1971 on this subject.

²⁸ Robinson certainly took this position in the December 1971 Ely Lecture of the American Economics Association she gave in New Orleans, published in 1972 as “The Second Crisis in Economic Theory” (Robinson 1972). She reinforced this in 1974 with the publication of “History versus Equilibrium”, a scathing account of the static and timeless character of modern equilibrium analysis.²⁸ In 1975 she engaged in what would be her last exchange with Robert Solow and Paul Samuelson with the publication of her “The Unimportance of Reswitching” and the ensuing discussion (Robinson 1975a, b; Solow 1975; Samuelson 1975).

²⁹ MPP, Economics Papers Project, Duke University.