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# **Keynesian Uncertainty: The Great Divide between Joan Robinson and Paul Samuelson in their Correspondence and Public Exchanges**

Harvey Gram with the collaboration of G.C. Harcourt

## **Abstract**

Joan Robinson and Paul Samuelson found little to agree upon in a correspondence which began in 1946, shortly after the death of Keynes, and ended a year prior to Robinson's death in 1983. One way to read the correspondence is to keep in mind that Keynesian uncertainty was central to Robinson's understanding of how capitalist economies function. Samuelson, never impressed by Keynes's handling of uncertainty, understood capital theory—if not capitalism—in terms of dynamic programming, with its perfect foresight entailments. This is evident throughout his letters to Robinson, although rarely acknowledged in a straightforward way, particularly during the period from 1971 until 1975 when their disagreements came to a head. On several occasions, Robinson despaired of making any progress in getting Samuelson to acknowledge the importance of her questions. Unfailingly polite to her, he granted only in a letter to Solow that, “She is on to a real problem...”

## **Keywords**

Consumption loans; perfect foresight; capital theory; constant returns to scale

## **Contributors**

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role of capital theoretic problems in two-sector growth models, general equilibrium theory, and the pure theory of international trade. Book reviews, dictionary entries, and several articles on Joan Robinson have also been published. He is an Associate Editor of the *Cambridge Journal of Economics*.

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# **Keynesian Uncertainty: The Great Divide between Joan Robinson and Paul Samuelson in their Correspondence and Public Exchanges**

Harvey Gram with the collaboration of G.C. Harcourt<sup>1</sup>

## **1. Introduction**

The Joan Robinson/Paul Samuelson correspondence spans a period of thirty-six years.<sup>2</sup> An initial five letters are mutually flattering. Robinson begins, complimenting Samuelson (22 October 1946) on his “amusing and penetrating article on Keynes.” Samuelson had joked, “I hasten to add – as who does not? – that I am not myself a Keynesian, although some of my best friends are” (Samuelson 1946: 188). He questions Keynes’s analytical abilities: “Indeed, until the appearance of the mathematical models of Meade, Lange, Hicks, and Harrod there is reason to believe that Keynes himself did

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<sup>1</sup> We wish to acknowledge Donald J. Harris for insightful comments based in part on first hand discussions he had with Joan Robinson and Frank Hahn, among others, during the time he spent at Cambridge University. Prue Kerr has also read our drafts and made important points about how mathematics has affected and infected methodological debates.

<sup>2</sup> Our main source of letters and other communications is Box 63 of the Paul A. Samuelson Papers, 1933-2010, located in the Rubenstein Library at Duke University. The Papers of Richard Ferdinand Kahn at King’s College Archive, Cambridge University, contain some of that same correspondence. All underlining in the quoted letters was in the original.

not truly understand his own analysis” (ibid.); and faulted Keynes’s most famous work for failing to deliver on its promise: “As for expectations, the General Theory is brilliant in calling attention to their importance and in suggesting many of the central features of uncertainty and speculation. It paves the way for a theory of expectations, but it hardly provides one” (ibid.: 192). Samuelson acknowledged that Keynes, on at least one occasion, had shown interest in “an esoteric theoretical problem, [giving] a rather intricate interpretation in words of a calculus-of-variations differential-equation condition of equilibrium” (ibid.: 196, fn. 8). He was referring to Ramsey (1928) – which presages Samuelson and Solow (1956) and Dorfman et al. (1958) – referred to on numerous occasions in Samuelson’s correspondence with Robinson. There is no mention of Keynesian uncertainty or speculation in this later work, whereas Ramsey is given star billing (see Samuelson and Solow 1956: 537).

Suggesting (2 January 1947) that Robinson make a “grand tour”, Samuelson writes, “Along with all American economists, I have always been intensely interested in your writings.” He repeats the invitation to “visit our shores” (23 May 1949), adding: “I read with great pleasure your review article on Harrod [Robinson, 1949] ... Perhaps if Harrod’s lectures serve to provoke interesting discussions such as yours, I will have to modify my critical judgement.” She returns the compliment (11 December 1950): “I much enjoyed your piece in *Economica* (I didn’t try the appendix).” The charming style of Samuelson (1950) on the problem of integrability of utility functions involving more than two goods no doubt appealed to Robinson.

In the last of these initial letters (17 May 1951), Samuelson acknowledges hearing that Robinson is working on a new project: “I take it as the best possible news that we can

soon look forward to another book of yours, and one on so interesting a subject.” Her famous critique of the production function in the theory of capital (Robinson, 1953-1954) was followed by that book, her magnum opus (Robinson 1956)<sup>3</sup> and its sequel (Robinson 1962a). In all of this work, Keynesian uncertainty, though not always central to the discussion, is never lost sight of.

Seven years pass before the first rumblings of disagreements are heard in three rapid-fire letters in early 1958 – disagreements that would, in the end, never be resolved. The last of these finds Robinson concluding on a despairing note (9 February 1958): “But I think you and Solow are a case of None so deaf as he who will not hear – so I shan’t make myself hoarse shouting at you. Much look forward to seeing you later this year.” A further pause ensued, followed by five letters from 1959 dealing with questions arising from Samuelson’s famous paper on consumption-loans (Samuelson 1958).

In 1961, Robinson finally made the “grand tour”. Following a seminar at MIT, Samuelson recorded that she had asked him “when you say the real wage is the marginal product of something, what is that something and what is it that is ‘being held constant’?” His initial answer:<sup>4</sup> “Er, er. The kings of England were William...and

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<sup>3</sup> For the long history behind this book, see Harcourt and Kerr (2013).

<sup>4</sup> In a letter (31 March 1961) to Kahn following the MIT seminar, Robinson remembered it this way: “He replied – you have a point there – I do not know the answer ... [I]t seems he just was disconcerted ... [A]ll very amiable tho’ hard hitting and plenty of good humoured banter.” In a follow-up (2 April 1961) to Kahn, she added: “[I]t is remarkable that he should never have asked himself this simple question before...and turn it to a joke against himself. But I wonder if the point will sink in.”

besides the interest rate or profit rate is identifiable as a certain physical marginal product concept, namely the tradeoff between today's and tomorrow's consumption goods." Her response: "Come, come, sir. Answer the question." Samuelson's memo set forth three answers dated 6:30 AM on the day after the seminar, implying a sleepless night struggling to cover all his bases! The protagonists were evidently enjoying the

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Robinson's doubts are recalled in her 1977 Reminiscences: "It was great fun to tease Samuelson, but this debate took attention away from the main issue" (Robinson 1978: xviii, cited in Turner 1989: 171-172). There, she reported that "the answer [Samuelson gave to the question] is that either you keep all physical inputs constant or you keep the rate of interest constant." In the latter case, the main issue between them was what determined the rate of interest. For Samuelson, it was "the tradeoff between today's and tomorrow's consumption goods." This would never satisfy Robinson because it only made sense in a world where output is continuously constrained by an economy's production possibilities. She wanted a more general theory of the economy-wide rate of profits, such as she would offer in Robinson (1962a: 48-51). As for the short run, "keeping all physical inputs constant" was not the traditional basis upon which to define the marginal product of labor. Indeed, it was an old question, long discussed in Cambridge and elsewhere. Stigler (1952: 117, 1987: 136), echoing Robertson (1931: 48), found it reasonable to hold constant total *expenditure* on non-labor inputs while allowing their *form* to vary. Whether or not one accepts this way around the problem, estimates of, say, the marginal rate of substitution between more and less computer-literate workers – the ratio of their marginal products – would be quite meaningless without *also* knowing the prior level of investment in computer equipment (for empirical work, see Lichtenberg 1995).

joust, but whether Samuelson's answers satisfied Robinson is unclear.<sup>5</sup> In any case, over the following six years, some thirty-four letters attest to the seriousness with which each attempted to understand and to criticize the other's positions on various matters relating to capital theory and the distribution of income. It all came to naught. On 11 May 1967, Robinson dashed off a final volley, "I fear it's hopeless. Goodbye."

Four years later, communication resumed in some two dozen letters written over the period 1971-1975. Robinson argued for an essential difference between various systems of prices and their associated schools of thought. Samuelson saw only a set of variations on a general equilibrium theory of supply and demand. In a response to her published note on model types (Robinson 1973), he threw up his hands, empowering "the reader...to strike out the label 'Walrasian,' substituting for it 'Ricardian,' 'Bortkiewiczian,' 'Sraffian,' or other similar adjectives" (Samuelson 1973: 1367). His well-known penchant for stringing together the names of famous progenitors of modern theory was only intended to give credit where credit was due. He did not agree with Robinson that different types of problems required different theories of price. Her own fully developed thesis first appeared in Robinson (1958) to which she made reference (21 January 1972): "I would be very much obliged if you would read a piece called The Philosophy of Prices." Samuelson wrote back (29 January 1972) saying only that he had "found it as I remembered it."

Much attention was paid in the letters of this period to the problem of constant returns

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<sup>5</sup> In her own theory of marginal productivity pricing, under conditions of imperfect competition, "the marginal net product of labour (after allowing for raw materials, power and maintenance of plant) is equal to the wage plus profit" (Robinson 1967: 77).



in Sraffa. Samuelson initially asked Robinson in early 1972 if Sraffa's "constant returns" means constant returns to *scale* or (as becomes clear in a subsequent letter) constant *opportunity* cost. A notation in the margin, "What else could it mean?" is in Sraffa's hand and initialed "P." Exasperation soon set in on both sides. Robinson wrote (18 April 1972), "There is none so blind as he who will not see." Samuelson responded, "I feel I am wasting time better spent on other things. Here are my final words on this matter." But this was not the end of it. Robinson had submitted "The Unimportance of Reswitching" to the *Quarterly Journal of Economics* (Robinson 1975a), and sent a draft to Samuelson (15 June 1973). For her, it marked a final turning away from many of the formal problems that had so interested protagonists on both sides of the capital theory controversy. Samuelson saw the article as an opportunity for him and Solow to set matters straight in the pages of the journal wherein the famous "Paradoxes in Capital Theory: A Symposium" had first appeared.<sup>6</sup> In one of four long letters, Samuelson remarked to Solow: "[A]s any examination of the dirt on the pages of the bound volumes of the QJE...will verify, [the 1966 *Symposium* issue] was in fact one of the most read in its history." These letters were, indeed, drafts of their replies to Robinson (Samuelson 1975; Solow 1975). Another, longer paper (Samuelson 1976) also emerged from this correspondence with Solow, filling out Samuelson's response to what he had described to Solow (9 July 1973) as Robinson's "disconnected remarks."

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<sup>6</sup> The editor of the *Quarterly Journal of Economics*, Richard Musgrave, wrote to Samuelson (3 July 1973), in reaction to Robinson's submission: "We don't think it very novel, but suppose that the prominence of the author suggests we take it in any case. We shall, of course, be glad to have a brief rejoinder by you, Bob [Solow] or, perhaps, the two of you jointly."

Again, it all came to naught. Asking Samuelson for permission to include part of his response to “The Unimportance...” in her *Collected Economic Papers*, Robinson again despaired (30 March 1978): “I am sorry we have never been able to get to the bottom of this dispute.” Samuelson replied: “You would not believe the hours I spend reading your writings ... You should take satisfaction that you have had a major impact on three generations of economists. Ever, Paul.” Four years later (12 March 1982), upon her return from India, Robinson sent her last letter to Samuelson, evidently puzzled by what he had written: “I was somewhat mystified...but I am glad to know there is no ill will. Yours, Joan Robinson.”

Section 2 of this chapter takes up the five letters between Robinson and Samuelson concerning his consumption-loan model. Section 3 covers the bulk of their letters, setting forth conflicting points of view in three overlapping areas, all concerned in one way or another with capital theory and the distribution of income. What often gets lost in the weeds of their back-and-forth is the simple fact that Keynesian uncertainty is central to Robinson’s understanding of what constitutes an acceptable economic theory of accumulation and growth within a capitalist system. Samuelson, on the other hand, sees the same problem as one to be addressed with the tools of dynamic programming, which inevitably involves an embrace of the curious notion – for economics, at least – of perfect foresight. Section 4 considers the role of mathematics in explaining this conundrum.

## 2. Pure Consumption Loans or Saving without Investment

Shortly after the publication of his classic paper on consumption-loans,<sup>7</sup> Samuelson wrote to Robinson (18 May 1959): “I have been rather inundated by correspondence concerning my JPE article.” He thanked her for her own reaction, “A Simpler Way of Putting It”,<sup>8</sup> noting that “a comment from you is always most welcome and I am

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<sup>7</sup> Karl Shell (1971: 1002) expressed the view of many: “Paul Samuelson’s (1958) paper on consumption loans is to my mind one of the most original and stimulating contributions to modern economic theory.” It has long since become standard textbook fare (e.g. Azariadis 1993) and remains a stimulus for research (e.g. Azariadis and Smith 1993 and Azariadis and Lambertini 2003). Samuelson’s purpose, it should be recalled, was “to give a complete general equilibrium solution to the determination of the time-shape of interest rates [associated with] a rational consumer’s lifetime consumption-saving pattern ... This sounds easy, but actually it is very hard, so hard that I shall have to make drastic simplifications in order to arrive at exact results” (Samuelson 1958: 219).

<sup>8</sup> There is a brief indication in an accompanying (undated) note that Robinson’s comment had been sent to the *Journal of Political Economy*. The note included a diagram, constructed with the help of Richard Kahn (as may be inferred from Kahn’s papers in the King’s College Archives), showing lending minus borrowing and income minus consumption as functions of the interest rate. It was not included in the final version which appeared only in Robinson’s *Collected Economic Papers* under the title “Saving Without Investment” (Robinson 1960).

looking forward to reading it with the usual benefit and enlightenment.” At the time, Samuelson was working on a response to Abba Lerner who “has written no less than two comments, each revised twice and I gather that the editors have already accepted some other comment.” That response (Samuelson 1959) to Lerner (1959), who wrote a “Rejoinder”, and another (Samuelson 1960) to Meckling (1960), who also fired back, were just the beginning of a burgeoning literature.

In two further exchanges, Robinson (22 May 1959) insisted that “the system requires excess consumption by the young adults.” Samuelson (29 May 1959) answers a different question, stating that “any pure loan scheme is very likely to produce a negative interest rate.” This prompted Robinson (12 June 1959) to write again: “If the young...don’t want to overspend, it won’t work,” at which point, Samuelson (30 June 1959) simply answered: “I agree with your recent statement.” This typifies the way in which their questions and answers often seemed to elide one another. In any case, Robinson’s main point was straightforward. Retirees (assumed to have no income) cannot borrow because lenders know they will not be alive to honor their debts (assumed to be zero at the end of life).<sup>9</sup> To survive, they must have assets to liquidate and so, as middle-aged adults in the previous period, they must be lending – but to whom?

“The essential feature of the situation is the excess consumption of young Jones. If there are no spendthrifts and all are determined to start saving up as soon as they begin to

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<sup>9</sup> Robinson put it this way, focusing on the late stage of working life: “An elderly man cannot afford to borrow even at a negative rate, for he has little time left to repay and to save up for his retirement” (Robinson 1960: 192).

earn, there is no equilibrium and there is an indefinitely great negative rate of interest [minus 100% per diem]...making it impossible to carry consumption forward from one day to the next” (Robinson 1960: 192). It was this dire situation that prompted Samuelson to ask, “Is it true, in a growing or in a stationary population, that twenty-year-olds are, in fact, overconsuming so that the middle-aged can provide for their retirement?” (Samuelson 1958: 474). Using the simplest example, he gave a negative answer and then proved his famous Impossibility Theorem: “[T]he social optimum configuration can never here be reached by the competitive market, or even be approached in ever so long a time” (ibid.: 477-478; italics in original). A further example showed that the only meaningful and locally stable solution “implies that consumption-loans lose about two-thirds of their principal in one period. This is here the competitive price to avoid retirement starvation” (ibid.: 478).

Samuelson’s model “points up a fundamental and intrinsic deficiency in a free pricing system, namely, that free pricing...by itself has no tendency to get you to positions on the [efficiency] frontier that are ethically optimal in terms of a social welfare function; only by social collusions – of tax, expenditure, fiat, or other type – can an ethical observer hope to end up where he wants to be” (ibid.: 479).

In the same vein, Robinson concluded: “When conditions are such that the free market rate of interest is heavily negative, the social insurance system seems more humane than the consumption-loan system, and more consonant with traditional morality” (Robinson 1960: 196).

In a discursive penultimate section, “Social Contracts and the Optimum,” Samuelson

also alluded to a social insurance system: “Once social coercion or contracting is admitted into the picture, the present problem disappears. The reluctance of the young to give to the old what the old can never themselves directly or indirectly repay is overcome. Yet the young never suffer, since their successors come under the same requirement. Everybody ends better off. It is as simple as that” (Samuelson 1958: 480).

A coercive transfer by the State is not, however, the note on which the article ends. Rather, it is the use of Money as a Social Contrivance: “In our consumption-loan model nothing kept. All ice melted, and so did all chocolates ... There is no arguing with Nature. But what is to stop man – or rather men – from printing oblongs of paper or stamping circles of shell. These units of money can keep” (ibid.: 481). Robinson offers a Keynesian caveat: “The introduction of money tokens which could be hoarded would be disastrous unless the first generation of retired men were given a free issue” (Robinson 1960: 195). By spending it, the retired cohort guarantees what Samuelson then asserts: “Now the young and middle-aged do have something to hold and to carry over into their retirement years” (Samuelson 1958: 481). He adds another condition: “[A]s long as the new current generations of workers do not repudiate the old money, this gives the workers of one epoch a claim on workers of a later epoch, even though no real *quid pro quo* (other than money) is possible” (ibid.: 482). Thus, coercion may still be necessary to ensure that all follow the traditional golden rule: “Do [as workers] unto others [now retired] as [the older] you would have them [future workers] do unto [the retired] you” (after Matthew 7:12).

Besides adhering to the rules of the game, there is a further matter of theoretical importance – the amount of money introduced into the system must be *exactly* right. As

David Gale showed, with too much money, “the model heads toward a kind of economic collapse in which eventually people will be demanding more consumption than the economy can provide ... [Unless the issue of money is exactly right], the model will move back away from the golden rule toward the balanced steady state” (Gale 1973: 16-17). In Gale’s terminology, a “balanced” steady state is one in which the aggregate of assets is zero. Samuelson alludes to the problem of getting the money injection just right at the beginning of his article: “Some interesting mathematical boundary problems, a little like those in the modern theories of dynamic programming, result from this analysis” (Samuelson 1958: 467). Writing to Robinson (29 May 1959), he is a little more explicit: “[T]he pure loan system if somehow engineered into the optimum configuration would generally be highly unstable,” as is also true of convergent yet *unstable* saddle paths in neoclassical models of capital accumulation.

So-called “boundary problems” in infinite-time-horizon models received a great deal of attention in the subsequent literature.<sup>10</sup> Less attention was paid to Abba Lerner’s insistence that, with either positive or negative interest, there will be less *total* utility than under an optimum zero rate of interest, owing to the wedge that is driven between marginal utilities of consumption of contemporaneous workers of various ages (young, older, and retired, in his three-period model). Consumption and real income are the same thing in this model. Thus, Lerner is taking the old-fashioned, radical view that social welfare is made higher by equating marginal utilities of income across individuals. Otherwise, “much of the good juice of utility [simply evaporates]” (Robinson 1962b: 52).

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<sup>10</sup> For a summary of results for the consumption-loan model, see Azariadis (1993: 245-247).

For Lerner, the concept of a biological rate of interest equal to the growth rate of population merely allows “the authorities [to] pretend that ‘social security’ is not a ‘socialistic’ tax and give-away program by the government but a ‘saving’ by each worker out of his current income to provide for his old age” (Lerner 1959: 514). In a stationary economy, the imaginary rate of interest associated with an imaginary transfer over time just happens to equal “the zero biological rate of increase in population” (ibid.). With a population that doubles every twenty-year period the “proportions of the populations in the three age groups would be 4:2:1...[so that] the one-seventh of his wage that a young worker pays grows to four-sevenths of the wage during the two periods that elapse before he gets his pension ... The true reason for this wonder is that there are at every point of time *six* workers to support each pensioner. The fictitious nature of the ‘biological’ interest stems from the impossibility, by assumption, of transporting consumption through time in an economy where no products are durable” (ibid.: italics in original).

Lerner elaborates, noting that: “In our society many people feel that social security by redistribution of income by the government is alien to the pure essence of the individualist capitalist system so that, if ‘social security’ has to be provided, it should take the form of individual saving for old age. This has led to the belief that a social security system cannot operate honestly unless it has acquired a fund actuarially corresponding to the savings of all those members of society who have paid in their contributions in the past and who will be taking them out as benefits in the future ... The essence of the matter is that the fable of the time-travel of consumption is accepted with implicit faith by the accountants, as guardians of the private point of view ... It is



the duty of economists, as guardians of the social point of view, to explode this fairy tale ... The only real problem from the social point of view is the allocation of current output of consumption goods between current consumers of different ages. This can never be achieved by any kind of trading or lending, but only by a one-way transfer...with no genuine quid pro quo” (ibid.: 516-517).

All this suggests that Samuelson (1958) may not have provided a sound social justification for national superannuation schemes, whatever other merits the paper may have had.<sup>11</sup> Certainly, the myth of a “trust fund” backing US pay-as-you-go Social Security benefits attests to the “fairy tale” nature of the scheme that Lerner was eager to explode.

In writing to Robinson (29 May 1959), Samuelson observes that, “Lerner would probably argue that you should see that pensions and taxes are right and not worry about what the *quid pro quo* is between youthful payment and aged receipts.” He says nothing about Lerner’s main argument concerning the *fragility* of a system in which the interest rate is not effectively set equal to zero. Nor does he remind Robinson of what he saw as the important theoretical problem motivating his analysis: “[I]n order to define an equilibrium path of interest in a perfect capital market endowed with *perfect certainty*, you have to determine *all* interest rates between now and the end of time; every finite time period points beyond itself!” (Samuelson 1958: 467; italics in original). This may explain why Robinson titled her response, “A Simpler Way of Putting It.” Samuelson

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<sup>11</sup> For further arguments along these lines, see Asimakopulos and Weldon (1968: 704, fn. 9) who wrote: “We have found the Lerner way of looking at the problem persuasive.”

stays the course, replying to Lerner: “Such a dynamic problem, involving as it does the infinity of time and the almost philosophical problems raised by the presence or absence of ‘perfect certainty,’ is to me one of the most difficult in all economics” (Samuelson 1959: 518). The same issues – an infinite time horizon and perfect foresight – would arise in his analysis of capital accumulation<sup>12</sup> and, yet, in his often long letters to Robinson, Samuelson mentions his denial of uncertainty on just two occasions (23 October 1961 and an undated communication of early 1972). There is far more candor expressed in communications with Solow during the summer of 1973 when their response to a draft copy of Robinson (1975a) was under intense discussion (see Section 4 below).

Samuelson returned to Lerner’s critique, which had been further analyzed and supported by Asimakopulos (1967),<sup>13</sup> acknowledging: “It is indeed paradoxical that the policy which maximizes social utility in every period should, when applied to a permanently growing exponential, results in a configuration such that every man who ever lives is worse off. This paradox, like most paradoxes,<sup>14</sup> arises from the infinity assumption

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<sup>12</sup> Samuelson (1958), it may be noted, is reprinted under “The Pure Theory of Capital and Growth” in his *Collected Scientific Papers*, rather than in the sections concerned with consumption theory.

<sup>13</sup> Cited by Samuelson (1967a: 280), this paper was written in 1966 (see Asimakopulos 1967: 189).

<sup>14</sup> Remarkably, Samuelson acknowledges that, “Up to *any* instant of time, the L-A [Lerner-Asimakopulos] state has produced more social utility than the S-D [Samuelson-Diamond] state. And departing from an ever-held S-D state in favor of an L-A state leads to an increase in social utility in *every* subsequent period” (Samuelson 1967a: 270,

involved ... Lerner presumably would bypass the pitfalls of divergent geometric series by insisting that the assumption of a permanently growing exponential is an absurd and monstrous one, involving a Ponzi-game or chain-letter swindle. If golden-age assumptions are successfully ruled out by Lerner as monstrous, he will in one swoop kill off a sizable fraction of the modern growth-theory literature” (Samuelson 1967a: 270).<sup>15</sup>

### 3. Capital Theory and Income Distribution

Samuelson’s correspondence with Robinson on matters relating to capital theory and income distribution began about ten years after their first exchange of letters following the death of Keynes. Samuelson repeatedly made three points. First, the mathematical theory of dynamic optimization is the proper foundation for a theory of capital accumulation, the pricing of capital goods, and the determination of the spectrum of own rates of interest. Second, constant returns to scale leaves indeterminate the number of transactions in final goods taking place between owners of various produced and non-produced inputs. Third, reduced consumption at a point in time causes a subsequent outward shift in production possibilities, under conditions of near enough full

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fn. 2; italics in original).

<sup>15</sup> Samuelson went on to out-do Lerner-Asimakopulos by proving a Catenary Turnpike Theorem. The most efficient path to a welfare-maximizing golden-rule equilibrium at some finite future date entails just such over-accumulation of capital as to bring the rate of interest down to *zero* for most of the path: “The optimum...presupposes direct intervention by the state to redistribute consumption by lump-sum taxation” (Samuelson 1967a: 279).

employment.

### **i. Dynamic Optimization as the Foundation for Capital Theory**

Robinson (22 January 1958) thought the assumptions she discerned in Samuelson's "piece on Marx" (Samuelson 1957) were "very unnatural" and wrote that he had merely "escaped[d] the puzzles connected with measuring capital" by making the length of life of all capital goods "uniform and very short." Samuelson (3 February 1958) answered the charge by referring to his work on dynamic optimization, eschewing any concern with the value of capital as a whole:

I avoided all puzzles connected with the measurement of capital...by specifying a complete (albeit simple) technological model. The world never grapples with the problem of reducing capital to a homogeneous measure: it uses complicated vectors of inputs to produce complicated vectors of output ... [I]n the November 1956 QJE Solow and I showed how any number of heterogeneous capital goods are to be handled; and also in our Dorfman-Samuelson-Solow book *LINEAR PROGRAMMING AND ECONOMIC ANALYSIS* (1958), of which I've sent Richard Kahn a copy.

Without taking a stance "on the question as to whether it was natural or unnatural to consider a model in which...the system remains not too far from full employment," Samuelson made his objective clear: "I do consider it a highly interesting problem to analyze the probable pattern of goods and factor pricing of a model in which activist fiscal and monetary policies did succeed in keeping chronic unemployment from being

large and growing.”<sup>16</sup>

Following Robinson’s 1961 visit to MIT, Samuelson (10 October 1961) again writes:

“The MIT School view that I have always adhered to maintains that one has to work with vectors of heterogeneous physical capital goods and time-consuming processes – that use of a homogeneous capital stuff is never necessary even though the parable<sup>17</sup>

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<sup>16</sup> Citing Meade (1961), Hahn (1966: 646) also suggests that the assumed success of Keynesian demand management policies is an “alternative interpretation” of a competitive equilibrium growth path under conditions of full employment.

<sup>17</sup> When the “parable” was published, Samuelson expressed his gratitude to Pierangelo Garegnani “for saving me from asserting the false conjecture that my extreme assumption of equi-proportional inputs in the consumption and machine trades could be lightened and still leave one with many of the surrogate propositions” (Samuelson 1962: 202, fn. 1). See also Asimakopulos and Harcourt (1974: 483), where it is pointed out that, “If there are many techniques, the differences in the value of output per head and the value of capital per man between adjacent points may be very small ... These small differences in value may mask substantial differences in the types and composition of capital goods in each system. Hence, if an attempt were made to move from one long-period equilibrium position to another, much more than a marginal change in the capital goods would be required. The existing equilibrium would be ruptured, and there is no guarantee that the behavior of the individual decision-makers in the economy would be such as to enable the transition to the new equilibrium position to be made.” Robinson put it this way: “[T]he machines...may be completely different for each [technique] ... [so that to] ‘change’ the technique in use...it would be necessary...to go back into the past and rewrite the history of the investment” (Robinson 1975a: 34, 53). Rewriting

may have some useful pedagogical functions.” He concludes on a somewhat patronizing note: “I do not enclose a copy of the Dorfman-Samuelson-Solow book showing how programming handles such diverse vectors of goods because no one can be expected to read so big a book.”

The determining role of “existent supplies of physical capital goods” (23 October 1961) is repeatedly emphasized, in opposition to the aggregate value of capital (3 May 1965; 13 August 1965; 5 May 1967). Answering Robinson’s query (14 October 1971) concerning the difference between “what Marx called ‘prices of production’ ...[and] what you now propose to call Walrasian prices based on supply and demand for scarce means with alternative uses,” Samuelson wrote in the same vein:

[I]n the DOSSO book [an acronym for Dorfman et al. (1958)], if you look carefully, you will find examples of complete, logically non-contradictory models in which heterogeneous capital-goods’ pricing and interest rate determinations are defined over time. Whether these models are realistic or even interesting depends on the eyes of the reader.

He maintained this position – with its hint of ambivalence – throughout their correspondence, adopting Harrod’s terminology (23 April 1973) when he wrote of “the optimal-programming rules for describing accurately a warrantable dynamic accumulation process from one steady state to another.” Samuelson was no doubt aware that a non-mathematically trained economist would be unable to follow the argument in Samuelson and Solow (1956), which forms the basis for chapters 11 and 12 of DOSSO.

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history is *her* way of ensuring equilibrium, but not, of course, Samuelson’s.

However, neither the paper nor the book is entirely mathematical. There is, for example, a half-page of interpretation in DOSSO at the end of a section entitled *Competitive Markets and Dynamic Efficiency*:

The truly remarkable thing about the intertemporal invisible hand is that while it results in efficiency over long periods of time, it requires only the most myopic vision on the part of market participants. Just current [equilibrium] prices and [their] current rates of change need to be known, and at each moment long-run efficiency is preserved. But for society as a whole there is need for vision at a distance (Dorfman et al. 1958: 321).

Had Samuelson drawn attention to these few lines in his correspondence with Robinson, she would surely have pressed him to explain what society's "need for vision at a distance" actually entailed. The authors of DOSSO are clear on this; namely, setting the initial price of each capital good exactly right so that the equilibrium trajectory of accumulation will follow a convergent saddle path,<sup>18</sup> or, in language that Robinson would have understood, a knife-edge. Samuelson nowhere acknowledges a connection between this formal problem – the problem of ensuring that *transversality* conditions are satisfied – and the literary arguments set forth in Robinson (1953-1954) concerning the problem of reconciling past, present, and future valuations of capital. Robinson (1959) clarified the issues from a Keynesian perspective (part of her effort to extend Keynesian analysis of effective demand to the long period). Samuelson, in contrast, acknowledges no formal connection between dynamic optimization and Keynesian

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<sup>18</sup> In Dorfman et al. (1958: 321), capital-good prices at some *terminal* date must be set precisely – hence the "need for vision at a distance", but a footnote states that setting *initial* prices *correctly* will do just as well.

analysis. He simply assumes that fiscal and monetary policy actions – Keynesian aggregate demand policies – always succeed in keeping the economic system operating at (or near enough to) a point on its production possibility frontier, a standard assumption of neoclassical capital theory (see Solow 1956: 91-94 and Swan 1956: 335).<sup>19</sup>

In their correspondence, Samuelson often tells Robinson how much he has learned from her published work, but on the matter of making clear the role of perfect foresight in his own theory, he is less than candid.<sup>20</sup> She addressed the issue directly in a response to Samuelson's suggestion (27 May 1964) that she write an essay on the question:

What difference does it make for the behavior of a mixed economy if its book of technological blue prints has a great many pages of slightly varied technical opportunities...as compared to an economy whose technology has great 'gaps' and which has choice only of a limited number of fixed coefficient alternatives?

Robinson answered (1 June 1964; italics added):

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<sup>19</sup> For extensive commentary on the differences between the growth models of Solow and Swan, see Pitchford (2002) and Dimand and Spencer (2009).

<sup>20</sup> Solow does not mince his words: “[T]he real difficulty...comes not from the physical diversity of capital goods. It comes from the intertwining of past, present and future, from the fact that while there is something foolish about a theory of capital built on the assumption of perfect foresight, we have no equally precise and definite assumption to take its place” (Solow 1955-1956: 102).



[S]o long as we are concerned with the positions of equilibrium where each economy has become adjusted to its own rate of interest with *correct foresight*, it does not make the smallest difference to the argument how large or small are the steps in the book of blue-prints. What is important is the actual collection of physical capital goods, knowledge and qualities of labor produced by the past history in which there has *not been correct foresight* or a *single* book of blue-prints. The response of the economy to monetary policy and questions of that kind depend upon this short-period situation.

As for eschewing the importance of “reducing capital to a homogeneous measure” (22 January 1958), it is curious that Samuelson also fails to alert Robinson to a point that he and his co-authors had made:

One interesting sidelight before we leave the subject of intertemporal pricing: Consider any efficient capital program and its corresponding profile of prices and own-rates [of change of prices]. *At every point of time the value of the capital stock at current efficiency prices, discounted back to the initial time, is a constant*, equal to the initial value. This law of conservation of discounted value of capital (or discounted Net National Product) reflects, as do the grand laws of conservation of energy in physics, the maximizing nature of the path (Dorfman et al. 1958: 321-222; italics in original).

An interest in “Conservations Laws” marks his later research,<sup>21</sup> and yet Samuelson

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<sup>21</sup> Samuelson (1990) proves a theorem on the constancy of the capital-output ratio along an optimal path, where *both* prices and quantities of capital goods are changing continuously, i.e. along a non-steady state path of accumulation. He then asks, recalling Robinson’s arguments: “Will these neoclassical results hold for a neo-classical model of

granted no importance to the “conservation” of aggregate capital value in Dorfman et al. (1958) or elsewhere. He simply responded (3 May 1965) to Robinson’s question concerning the relationship between the aggregate capital output ratio and the interest rate by saying: “As soon as one leaves the simplest model, we need a general equilibrium approach in which everything depends upon everything else (but in a virtuously-Walrasian way and not in a viciously-circular way). The system cares nothing for aggregative totals and few invariant relations can be found for them.” The exception, for “even the most complicated set of blueprints” is that “if intrinsic joint-products...are ruled out, it remains true that: ‘The higher the interest rate  $r$  the lower the real wage expressed in terms of any (and hence every) consumption good.’”

Interpretive passages in other published work reinforce the view that Samuelson might well have met Robinson on her own ground, but, for whatever reason, did not do so. A clear case arises in connection with one of two articles Samuelson published in the 1967 volume edited by Karl Shell entitled *Essays on the Theory of Optimal Economic Growth*. Under the heading, “Re-aiming Behavior of Speculators”, Samuelson sets forth, with striking imagery, his own misgivings concerning the “MIT School view that

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Joan Robinson type, where marginal productivities in the form of partial derivatives are not definable because only a finite number of activities are technologically possible?” (ibid.: 58). His affirmative answer is qualified, and so “this law is not trivially true ... I conclude that, at best, if discrete-time intervals can be taken so small that there is little error in approximating the system by a continuous-time model, we can hope to state an ‘almost-constancy’ of the capital-output ratio along any optimal path” (ibid.: 69).

Champernowne (1953-1954) had also faced the problem of dealing with discrete versus smooth changes.

[he had] always adhered to”:<sup>22</sup>

My analysis...has confirmed Hahn’s finding<sup>23</sup> that merely to postulate over-all saving propensities leaves a heterogeneous system indeterminate in its development ... Now how do actual economic systems resolve these indeterminacies? ... The image in my mind is that of a bicycle. The rider of the bicycle is the bulk of the market, a somewhat mystical concept to be sure – like its analogue, the well-informed speculator who gets his way in the end because his way is the correctly discerned way of the future ... Even

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<sup>22</sup> A *belief* in the stabilizing role of speculators – neither backed up by an argument nor questioned in the way that Keynes (1936) [1973] and Kaldor (1939) had done – informs Samuelson’s other published work with Solow: “This re-aiming is, so to speak, what an optimizing society is constantly doing” (Samuelson and Solow 1956: 548). How such “re-aiming” occurs and what damage is done before the ship is righted are, of course, the fundamental questions that Robinson wanted to discuss.

<sup>23</sup> The issues raised by Hahn recall the theoretical stance taken by Irving Fisher: “When prices find their normal level at which costs plus interest are covered, it is not because the past costs of production have determined prices in advance, but because the sellers have been good speculators as to what prices would be” (Fisher 1906: 188). “Thus Fisher shifted all economic reasoning to the future ... It was a complete reversal of the classical causation, as Fisher himself put it” (Bharadwaj 1985: 18). Hahn found in this reversal a profound problem: “The fact that the description of the present involves the future in an essential way must bear the responsibility for the unsatisfactory behavior of the equilibrium path” (Hahn 1966: 645-646). Samuelson quickly realized the importance of Hahn’s argument, but nowhere is it mentioned in his correspondence with Robinson.

if there is something valid in this heuristic reasoning, one must admit that the system need not – and, generally, will not – move from its present position to the golden age in the most efficient way: it will hare after false goals, get detoured, and begin to be corrected only after it has erred ... One feels that the real world of tools, plant, and inventory contrasts with the purely financial dream world of indefinite group self-fulfillment. But can this feeling be given documentation and plausible explication? (Samuelson 1967b: 228-230).

The pages from which this passage is taken – had they been sent to his sparring partner – might have opened up a more fruitful dialogue. Obfuscation may actually have been a strategy, however, as suggested in a letter to Solow (19 July 1973):<sup>24</sup>

[W]hen we face up to heterogeneous capital goods, we do get into terrific problems of needed foresight (and indeed the ‘Hahn’ problem of how to cut the indeterminacy of the detailed composition of capital goods within the aggregate saving-investment budget set by baby-simple class propensities to save). Of course, Joan can’t understand, or at least never keeps in mind for as long as 30 seconds running, that there is a logical way of defining warrantable dynamic paths that are not balanced exponentials (sometimes for a socialist planned society she concedes a little) ... And think how horribly difficult it would be to get this into a reasoned discussion of the so-called Two Cambridges issues

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<sup>24</sup> In an addendum to an earlier letter (9 July 1973), Samuelson provides Solow with a list of eleven points he is considering in connection with a response to Robinson. There, he states categorically: “I doubt that she understands what a price-quantities warranted dynamic path is or that it is logically possible – albeit not always [if ever] realized in real life.” The eleven points, plus two more, are found in Samuelson (1976).

... Perhaps never admit anything and claim everything?

Certainly, Samuelson alludes to assumptions that he knows are unacceptable to Robinson, but they are not spelled out in his letters to her. Earlier (10 October 1961), he had written:

One of the few apparent differences that still remain is that I think the study of transition states is not a hopeless one for the economic theorist. I am prepared to make certain assumptions about uncertainty and expectations in the presence of fast and slow changes that you will think useless assumptions; I expect some little light to be reflected on such transient dynamic states by the study of stationary states, and I understand you to be more skeptical in this regard. I think we can hope to make some slow progress toward understanding the behavior of an economy undergoing technological change by first isolating some of the properties of a system that undergoes no technical change. In all this I may be wrong, and time may tell.

It is rather in his correspondence with Solow concerning the draft of Robinson (1975a) where one finds (19 July 1973) a clear statement of Samuelson's position on the relationship between equilibrium theory and the market process:

What you and I realize is that it is a tough problem for a market economy to have all the perfect futures markets that are needed for Arrow-Debreu optimal dynamic paths. Yet, when you review the Pareto-Mises-Lerner-Lange-Hayek debates about socialism versus capitalism, one realizes that Hayek's point about the need to mobilize and utilize imperfect information actually wins the debate: no planning algorithm or computer can

come anywhere close to approximating intertemporally-efficient paths, whereas the market (helped by the Justice Department and bankruptcy) makes a workably-adequate pass at the problem.

It is hard to countenance the suggestion at the beginning of this passage that lesser mortals – including Robinson – are quite unable to appreciate the fact that intertemporally-efficient paths entail a set of perfect futures markets. As for allusions to actions by the Justice Department and to bankruptcy, these bring into question the whole theoretical argument, yet Robinson was never brought into Samuelson's confidence on this fundamental caveat.

## **ii. Constant Returns to Scale Eliminates the Need for Transactions in Final Goods**

On the theoretical implications of constant returns to scale, Samuelson (28 January 1972) wrote to Robinson:

For 70 years at least students of Wicksell have realized that in a world of constant returns to scale, there is no need for exchange of final goods. I see no contradiction to that in the view of Smith, Ricardo, Sraffa and me that the equilibrium price ratio of goods readily reproducible at constant costs (and produced in positive amounts) is their cost ratio, without regard to the number of transactions that take place at that equilibrium ratio.

Samuelson was referring to Knut Wicksell's *Lectures on Political Economy*, in particular, "The Landowner as Entrepreneur," and "The Labourer (or a Third Party) as

Entrepreneur,” which, it should be noted, fall under the heading “Non-capitalist Production” (Wicksell (1934) [1977]: 110-133). Samuelson (1953-1954) had also granted the indeterminate *organization* of production under constant returns to scale, arguing that it is immaterial which “factor” hires the other(s):

Throughout I make the simplifying assumption most appropriate for viable perfect competition – namely constant-returns-to-scale...; under this strict assumption the composition of industry output among firms becomes indeterminate and of no importance, so that the factors can be thought of as hiring each other in a Darwinian process of ruthless natural selection which severely punishes any momentary deviation from the static optimum ... [T]he *same results* would follow if I supposed, with Leontief and others, that each good requires in its production every *other* good as an input. Goods would then have a double function – as inputs as well as outputs, as intermediate as well as final goods. Moreover, the only way to characterise a ‘primary factor’ in such a system would be by the fact that it cannot be produced and reproduced by a homogeneous production function (ibid.: 1; italics in original).

Constant returns to scale is, for Samuelson, theoretically tractable rather than empirically justified, as is clear from his further remarks about additivity.<sup>25</sup>

Alternative hypothesis: We can always independently carry on production in two separate processes and there will not be any necessary ‘external’ inter-action between these processes that prevents us from getting as a total the sum of their separate outputs.

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<sup>25</sup> “Conservation Laws,” too, require the assumption of constant returns to scale (see Samuelson 1990).

Like the assumption of constant-returns-to-scale this is an empirical hypothesis rather than a truism; not only are both of these hypotheses conceptually refutable, but in addition there is considerable empirical evidence, in connection with technology and the breakdown of perfect competition, that in large realms of economic life these are poor hypotheses to make (ibid.: 2).

In other communications as well, Samuelson had already gone to considerable lengths to insist that, under constant returns to scale, no transactions are strictly required. He sent Robinson (3 April 1971) a numerical example of an economy using coal and corn to produce coal and corn. In a familiar notation, the price equations (not given by Samuelson) are:

$$(.5p_2 + 2w)(1 + r) = p_1$$

$$(.5p_1 + 2w)(1 + r) = p_2$$

Neither good is a direct requirement in its own production, but both are basic in the sense of Sraffa. The wage is paid at the beginning of the production period. Samuelson sets the steady-state rate of profit at 50% to obtain a real wage of 1/12 in terms of either good, and a relative price of 1. Each price is \$12, given a \$1 nominal wage. Quantity equations (also not given) are:

$$X_1 = .5X_2(1 + g) + C_1$$

$$X_2 = .5X_1(1 + g) + C_2$$

$$2X_1 + 2X_2 = 1,200$$

There are 100 workers, each providing 12 hours of work per day; the growth rate is zero. Gross outputs are set equal:  $X_1 = 300 = X_2$ , so that the gross value in each sector is \$3,600, for a total of \$7,200. Workers spend their whole \$1,200 income on corn, for a total of 100 units, or 1 for each worker. Circulating capital is replaced using 150 units of



each good, accounting for \$1,800 of each sector's output. This leaves \$600 worth of corn (50 units) and \$1,800 worth of coal (150 units) for consumption. Samuelson assumes that capitalists spend their profit in such a way as to create an equilibrium of supply and demand. As a check, profit income is \$1,200 in each sector for a total of \$2,400 (50% of the sum of the value of circulating capital plus the wage bill). In terms of corn this is 200 units. In equilibrium, one-quarter of profit must be spent on corn (the above 50-unit residual). In terms of coal, total profit is also 200 units. In equilibrium, three-quarters must be spent on coal (the above 150-unit residual). These are the fractions in Samuelson's example.

Samuelson posits a no-transactions "island of feudalism" headed by one (of ten) capitalists who "uses 10 workers 12 hours a day" to produce outputs for replacement of used-up corn and coal, as well as consumption for himself and his workers, exactly equal to 1/10 of the steady-state quantities already set forth. (No mention is made of the fact that the capitalist also needs 15 units of coal and 15 units of corn to produce 30 of each good, plus enough corn to pay wages "at the beginning of the period." Simply having 120 worker-hours at his disposal is not enough to operate in isolation from other capitalists, whether or not corn-wages are paid in advance.) Samuelson then remarks that, "If the rest of society used competitive capitalist exchange, this island of feudalism could not be tempted out of its isolation by economic advantage." His main point, however, concerns the alternative arrangement in which "workers want the illusion of autonomy." Now, ten workers form a "cooperative or kibbutz." To get going, "They find a rentier-capitalist and strike an arms-length bargain or social contract with him once and for all in the year 1066: he gives them 15 initial corn (plus corn subsistence) and 15 initial coal, in return for their remitting forever 15 coal per period plus 5 corn per

period.” Nothing has really changed, for, “As Wicksell put the matter so long ago, once interesting matters of uncertainty, change, and deviations from constant returns to scale are idealized away, the perfect competition that remains is indifferent as to which factors ‘hire’ the others.” Importantly, there is no acknowledgement that Wicksell’s argument concerned *non*-capitalist production or that, when Wicksell does take up capitalist production, he does *not* repeat or refer to the argument set forth earlier.<sup>26</sup>

Robinson’s reaction (11 April 1972) is brief, reflecting her view that economic theory ought to recognize an essential difference between various systems of prices – in particular, Walrasian versus Sraffian (see Robinson 1962a: 1-21). In their correspondence, she repeatedly asks Samuelson for his theory of the rate of profit; and, in this letter, asks: “What determines the rate of interest when workers hire inputs from

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<sup>26</sup> Samuelson also ignored Wicksell’s remarks on capital as a factor of production:

“Whereas labour and land are measured each in terms of its own technical unit (e.g. working days or months, acre per annum), capital, on the other hand, as we have already shown, is reckoned, in common parlance, as a sum of exchange value – whether in money or as an average of products. In other words, each particular capital good is measured by a unit extraneous to itself. However good the practical reasons for this may be, it is a theoretical anomaly which disturbs the correspondence which would otherwise exist between all factors of production ... If capital also were to be measured in technical units, the defect would be remedied and the correspondence would be complete. But, in that case, productive capital would have to be distributed into as many categories as there are kinds of tools, machinery, and materials etc., and a unified treatment of the role of capital would be impossible” (Wicksell (1934) [1977]: 149, cited by Eatwell 2012: 5, fn. 3).

capitalist-rentiers and why do they pay in perpetuity for an advance made in 1066?” In the example, the rate of profit (interest) is simply given, and the assumption of perpetual payments eases the calculation of implied capitalized values of the associated income streams. In this exchange, neither protagonist dwells on the implicit assumption of perfect foresight of those income streams.

Samuelson then addresses (14 April 1972) what he likely regarded as the underlying political problem at the heart of Robinson’s queries:

There is no disagreement that, in a stationary state with only one page-of-blue-print technology which has long been giving to workers only a fraction of the total net product, an act of political expropriation that gave the workers collectively ownership in the raw materials would successfully result in euthanasia of the capitalist class. But Allende will of course face a more complicated model.<sup>27</sup>

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<sup>27</sup> Samuelson wrote in the same vein to Solow in his letter of 19 July 1973:

“[R]eswitching and backward switching and all the important things that are possible in n-vector models but not in 1-vector models, do not provide a powerful test or refutation of ‘marginal-productivity income determination’ or of what is even more basic of the fundamental insight that supplies of capital goods, relative to unproducible labor, do importantly affect the pricing of the owned factors of production and the market-imputed income of property-less laborers in comparison with propertied people. This is the basic denial of Robinson, Kaldor, Pasinetti, etc. Only if they assume their overly-simple models – fixed capital/output ratio, etc. – do they get the indeterminacies that leave a way for simple power acts by unions to drastically alter distribution and the indeterminacies that can be filled in only by macroeconomic tautologies of the Kalecki-

[In cases] with many pages of technology, a once and for all expropriation which was shared equally by all workers might well, if each worker was subsequently permitted to sell off his share of intermediate goods, result after a time in unequal ownership as between the more- and less-thrifty and the lucky or unlucky ... If the system ever comes into a new steady state – which I would regard as doubtful – it could be one again with positive profit rate (not necessarily unique) just high enough and just low enough so that the capitalized market value of non-labor resources averaged over people of all ages (the retired dissavers and working savers) would work out to equality with their cost of production total values. In short, Modigliani’s life-cycle model of saving provides one possible way of finding the missing equation [for the profit rate]. But there are others.

A final, undated letter<sup>28</sup> on this topic begins, as in the above numerical example, by assuming that “the rate of profit is (provisionally) taken as given.” Samuelson reviews his “non-substitution theorem” (steady-state prices of final goods independent of the composition of final demand), emphasizing its dependence on the special assumption of a single non-produced input. With more than one such input, increasing opportunity cost immediately comes into play so that relative prices under competition do generally depend on the composition of final demand. However, the main thrust of the letter is to argue that in all cases where constant returns to scale prevail, there is “no occasion or

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Robinson-Kaldor type.” See Asimakopulos (1975, 1980-1981) for a discussion of how accounting identities – presumably what Samuelson was referring to as “tautologies” – provide a starting point for non-tautological macroeconomic theories of distribution.

<sup>28</sup> The date is likely March or April 1972, as Samuelson is responding to a point that Robinson had made (15 March 1972) about labor-value prices in Smith.

necessity to trade” final goods, “since anyone can use his ownership of primary factors and of intermediate goods to hire all that is needed to produce the final goods.”

Regardless of the technological assumptions made, “what must be said is that, after a single initial exchange, ever after in the steady state each of us can exchange only primary factors against primary factors and can produce all of our unchanging final goods for ourselves.” A footnote repeats a point made in connection with the numerical example, adding that “this initial exchange would consist of borrowing the wherewithal to buy the intermediate goods you will need forever, paying interest out of your gross sales; in exponential growth, a new act of borrowing will be needed to finance each increment of output reached for the rest of time thereafter.” Finally, a brief answer is given to Robinson’s repeated query on what determines Samuelson’s “provisionally” given rate of profit:<sup>29</sup>

To understand how the competitive rate of profit gets set under these conditions,

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<sup>29</sup> This is very much like the “provisional” assumption, in static theory, that relative prices are *given*. First, one works out what the value-maximizing composition of output *would* be; then, what the utility maximizing composition of demand *would* be, in light of the factor prices (and therefore the distribution of income, given factor ownership) implied by the given relative prices, assuming there is a unique answer. If they match, the “provisionally” given relative prices are equilibrium prices. If not, one tries again. Where no solution algorithm is known, i.e. in most cases of any degree of complexity, fixed-point theorems can establish existence, but not necessarily uniqueness. There may be multiple, isolated solutions or even a continuum (when a changing distribution of income just happens to leave the composition of final demand unchanged at the point where the value of output is maximized).

whether high or low, one must weigh the conditions of supply that make intermediate goods available in plenitude or scarcity [sic] as against the motivated commercial demand for them. The capitalists, like the landlords, have something the workers haven't got; the workers have something the others don't have, etc.

A generalized theory of supply and demand was always in the back of Samuelson's mind. Robinson, in contrast, insisted on distinguishing among various systems of prices, on one occasion (15 March 1972) setting forth a tripartite division, altered somewhat when later published (Robinson 1973). Robinson had written to Samuelson, "The important thing is to get the question clear before trying to answer it. Three cases have been mixed up in this correspondence." She then set forth her distinctions:

(1) "Scarce means with alternative uses..., fixed amounts of specific factors and a production possibility frontier"; (2) "Sraffa. The composition of output is whatever it is. There is a single complex technique. Inputs in use, all produced within the system, are those appropriate to the technique and the composition of output; (3) "Beavers and deer [a reference to Smith]. There are constant returns for each separately in the sense that output per man hour is independent of the amount of labour engaged. If, furthermore, labour is interchangeable between occupations and divisible, there is no occasion to trade. Therefore, I say that Adam Smith was wrong in saying that trade takes place at labour value prices. On the other hand, if each kind of labour is fully specialized to its own product, there is no general unit of labour ... If trade takes place, prices must be influenced by supply and demand.

There was no essential point of contention concerning Smith's deer and beavers,

although Samuelson insisted that *if* trade did take place, it would have to be at labor value prices, otherwise, “infinite arbitrage trade would be set into motion.” Presumably, he also would have had no quarrel with Robinson’s allusion, in the case of specialized types of labor, to his own comment on the role in trade of specifically localized factors of production: “We would be giving the show away if we were to descend to such fatuities as: the tropics grow tropical fruits because of the relative abundance there of tropical conditions” (Samuelson 1948: 182).

In her published list of model-types, Robinson insisted that, in the case of scarce means with alternative uses, “there are ‘rentals’ for particular physical ‘factors of production’ but *no overall rate of profit*. This is the basis of Walras’ general equilibrium” (Robinson 1973: 1,367; italics added). She is here making no allowance for the possibility that particular physical factors of production may be reproducible, as in the world of Sraffa, where such inputs have prices, as *outputs* of the system, but no explicit rental rates for their services. It is in this world of production of commodities by means of commodities where one can find an overall rate of profit under conditions of competition, the starting point for an analysis of capitalism, which can then be modified to account for elements of monopoly power, among other complications to the basic model.<sup>30</sup>

Rental markets for the *services* of capital goods may, in fact, be thin or non-existent, but, for Samuelson, rental rates are *always* defined, if not observable, as *implicit* shadow prices. Each such rate,  $R_i$ , together with the price of the corresponding capital good,  $P_i$ ,

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<sup>30</sup> See, for example, chapters 11 and 12 of Kurz and Salvadori (1995). An extensive discussion and critique of the classical “law of a uniform tendency in the rate of profit” is found in Harris (1988).

and its time derivative,  $\dot{P}_i$ , defines an own rate of return, net of (for simplicity, exponential) depreciation,  $\delta_i$ . All rates rate of return are equalized under competition across the whole spectrum of capital goods:<sup>31</sup>

$$R_i/P_i + \dot{P}_i/P_i - \delta_i = R_j/P_j + \dot{P}_j/P_j - \delta_j = R_k/P_k + \dot{P}_k/P_k - \delta_k = \dots$$

In short, where Robinson sees a basic difference between Walrasian and Sraffian prices – the former involving rental rates for scarce factor services along a production possibility frontier, the latter involving a uniform competitive rate of profits – Samuelson sees none. For him, only the special case of constant relative prices yields a simple proportional link between rental rates and prices:  $R_i = rP_i$  for all  $i$ , where  $r$  is *the* rate of profit (interest) in a steady state.

Constant returns to scale is central to Samuelson’s understanding of Sraffa, but not, it

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<sup>31</sup> See Bruno (1969: 49), where the *common* value of such rates of return is the growth rate of the labor force *plus* the rate at which future consumption is discounted, both in *and* out of steady-state equilibrium. In a footnote, Bruno states that, “an optimal growth model avoids some of the ‘causal indeterminacy’ problems recently raised by Hahn (1966).” Recent work undermines this optimism: “From the outset, it is admitted that, in all the models considered, the path of prices and quantities is, from the point of view of market adjustment, dynamically highly unstable – namely saddle-path stable” (Burgstaller 1994: 38). See also Gram (1996) and Burgstaller (2001). As for Robinson, Samuelson’s remark to Solow (9 July 1973), doubting that she understood “what a price-quantities warranted dynamic path is,” may not be quite fair. Referring to intertemporal equilibrium theory, she wrote: “[F]or my part, I have never been able to make that theory stand up long enough to knock it down” (Robinson 1980a: 128), an apt quip, in view of the knife-edge property of a convergent saddle path.



would seem, to Robinson's. Following Sraffa, she takes *both* the technique and the composition of output as given when referring to a Sraffian price system.<sup>32</sup> Sraffa himself equivocated on the role of returns to scale: "If [the assumption of constant returns in all industries] is found helpful, there is no harm in the reader's adopting it as a temporary working hypothesis. In fact, no such assumption is made" (Sraffa 1960: v). In correspondence, Samuelson did not engage Robinson on the essential/inessential role of constant returns to scale in Sraffa (ibid.). Yet, in his published work, he is adamant: "In sum, if a Sraffian denies constant returns to scale, the one-hundred-page 1960 classic evaporates into a few paragraphs of vapid chit-chat" (Samuelson 2000: 123). In "Sraffian economics" for *The New Palgrave*, he expressed the same viewpoint:

Although Sraffa reserved judgment for half a century on whether he wanted to assume constant returns to scale, experiments with returns laws that depart from that property will be found to rob his algebra of any interesting economic applications, as the paucity of results on this point in the literature of the last quarter of a century attest (Samuelson 1987: 455).

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<sup>32</sup> As noted in the Introduction, Samuelson had asked Robinson if, when Sraffa wrote "constant returns," he meant "to scale." It becomes clear (28 February 1972) that Samuelson took it to mean constant opportunity cost, i.e. a linear production possibility frontier, in contrast to "more general models." Robinson (1 February 1972), for her part, widened the question: "I do not know whether 'scale' applies to a plant, a firm, or an industry. I assume you mean the total output of a specified commodity, but then are you discussing a change in the composition of output or an increase in total output?" She reiterates her position that "there are two distinct kinds of prices."

This indictment has not gone unchallenged. Dependence of prices and the wage on *both* outputs and the rate of profit in a Sraffian system is recognized, formally, as a “correspondence...not yet fully explored” (Kurz and Salvadori (2000: 154), responding to Samuelson (2000)). More generally, this dependence is seen as an essential part of a reconstructed modern classical theory consisting of:

[A] union of theories – theories of output, of accumulation, of technical development, of the real wage, and so on – from which the data of the theory of value are derived. A great strength of the classical framework derives from this separation between the determination of output and the theory of value. Not only does this separation free us from the need to assume constant returns, but also it embodies great potential for theoretical development. For example, the direct integration of the classical theory of value with Keynesian principles of effective demand presents none of the difficulties encountered in attempts to establish a relationship between the neoclassical theory of value, in which values and the employment of factors are determined simultaneously, and Keynesian analysis (Eatwell 1977: 66; see also Eatwell 2012).

A “paucity of results” from Samuelson’s vantage point is, at least for other students of Sraffa (1960), a potential not yet fully explored, a lacuna with rich and generative possibilities for the development of a classical theory of competition (see Harris 1988). In any case, reading Robinson (1956, 1962a) as a systematic effort to integrate the classical theory of value with Keynesian principles of effective demand is surely a more promising point of departure than one that holds her magnum opus up to the formal standards of intertemporal equilibrium analysis under perfect foresight, which, for Robinson, pulled the rug out from under any realistic analysis of capitalism.

### iii. Reduced Consumption Increases Future Production Possibilities

Towards the end of *The General Theory*, Keynes remarked, “[I]f our central controls succeed in establishing an aggregate volume of output corresponding to full employment as nearly as is practicable, the classical theory comes into its own again from this point onwards” (Keynes (1936) [1973]: 378). A door had opened to the development of what Robinson saw as a backward step: pre-Keynesian theory after Keynes. Thus, in the wake of Robinson’s “memorable visit” to MIT, Samuelson wrote to her (10 October 1961), arguing, in effect, that the presumption of *successful* aggregate demand policies justified an intertemporal equilibrium analysis based on such traditional concepts as the production possibility frontier:

But I assure you that whatever my delight in abstract models for their own sake, I have always in the front of my mind such important real questions as this: If by private thrift or Kennedy-overbalancing-of-the-budget-coupled-with-*successful*-expansionary-credit-policies a mixed economy succeeds in lowering the fraction that consumption is to its *full employment income*, how much will its *future* production possibilities improve as against what would be expected to happen under other policies (italics added).

Later in the same month (23 October 1961), the connection between a change in the time profile of consumption and the average level of interest rates is referred to in a general way:

[T]he rate at which the society is willing to consume below its potential in the interest

of physical net capital formations could (as could any demand-composition factors) affect the average level and spread of (own) interest rates. You will realize that I do not mention the interest rate (or the profit rate) because in the regime I speak of there is no necessary equilibrium stationary over time which keeps relative prices the same and thereby turns the complex of own rates of interest into a single rate.

He then “throw[s] in a bonus” on the overall *direction* in which interest rates will move:

[A]ny society which stints itself in consumption goods, making sure that no Keynesian abortions of ‘saving’ result from induced unemployments [sic], can be expected to (1) increase its future production possibilities and (2) to lower the general position of its structure of own interest rates and (3) to find itself more easily in a position to swing into a new low-profit high-output high-wage stationary equilibrium if it should want to do so – all this in comparison with its having made the alternative decision to consume more generally. No more [is] implied by any position I have ever maintained – and I have maintained that general position for some decades now.

Samuelson’s 1961 presumption that reduced consumption, under full employment, will lower interest rates in a new stationary equilibrium with expanded production possibilities is, by 1966, recognized as generally incorrect – a result that Samuelson came to regard as the sole significant theoretical advance to emerge from the capital theory controversy. In a footnote to his famous “Summing Up,” he acknowledges that:

The reversal of direction of the [interest rate, net national product] relation was, I must confess, the single most surprising revelation from the reswitching discussion. I had

thought this relation could not change its curvature if the underlying technology was convex, so that there had to be a concave, basic Fisher (intertemporal) production-possibility frontier ... I had wrongly confused concavity of [the production possibility frontier] with concavity of the [interest rate, net national product] steady-state locus ... [R]eswitching reveals this possible curvature phenomenon, but is not necessary for it (Samuelson 1966: 577, fn. 6).

Samuelson further acknowledged a general exception to the underlying idea of diminishing returns along the transition path to lower interest rates:

Lower interest rates may bring lower steady-state consumption and lower capital/output ratios, and the transition to such lower interest rates can involve denial of diminishing returns and entail reverse capital deepening in which current consumption is augmented rather than sacrificed. There often turns out to be no unambiguous way of characterizing different processes as more ‘capital-intensive,’ more ‘mechanized,’ more ‘roundabout,’ except in the *ex post* tautological sense of being adopted at a lower interest rate and involving a higher real wage.<sup>33</sup> Such a tautological labeling is shown, in the case of

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<sup>33</sup> A year earlier, Samuelson wrote to Robinson (23 August 1965): “If the Levhari proof is valid, for such systems it is not possible to find contradictions and flaws in a definition of more-mechanized, more-round-about, more-capital-intensive configurations of the system at lower profit rates.” The reference is to Levhari (1965), later shown to have contained an error (Levhari and Samuelson 1966). With the false proof withdrawn, the “contradictions and flaws” Samuelson had referred to were granted to be entirely possible, as shown by various examples of re-switching, using indecomposable technologies.

reswitching, to lead to inconsistent ranking between pairs of unchanged technologies, depending upon which interest rate happens to prevail in the market. If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life (ibid.: 582-583).<sup>34</sup>

Samuelson was intent on correcting his own error concerning the direction in which interest rates were likely to move following an act of “abstaining” from consumption. He dubbed it “Samuelson’s false theorem – the rate of interest will be (if anything) lower at the higher plateau of consumption” (Samuelson 1976: 14), acknowledging that “traces of this view” could be found in “my elementary *Economics*, somewhere around the sixth edition...in the appendix on interest theory [Samuelson 1964: 595-596, as noted in Samuelson 1966: 579, fn. 8] and in one or another of my letters to Robinson one might find this false theorem.” His most detailed discussion is found in the published papers presented of a Conference on Capital and Macroeconomics held in Buffalo, New York, in March 1974:

Even if that other false theorem, which for my sins I planted in the mind of Levhari, had not been false...Samuelson’s false theorem would be false since

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<sup>34</sup> Robinson dismissed the possibility of finding empirical evidence of reswitching:

“Nothing could be more idle than to get up an argument about whether reswitching is ‘likely’ to be found in reality” (Robinson 1975a: 38). She had earlier made the same point: “[E]quilibrium positions with different rates of profit and the same ‘state of technical knowledge’...are not found in nature and cannot be observed” (Robinson and Naqvi 1967: 591).

indecomposability...cannot rule out the innumerable contradictions to Samuelson's false theorem. Why the false theorem is false, which is to say why I should have discerned this fact from the beginning, is indicated briefly in footnotes 6 and 9 of Samuelson (1966: 577, 579)" (Samuelson 1976: 14, fn. 5). Mea culpa, on multiple occasions, meant only<sup>35</sup> that, "the most general neo-neoclassical case of a concave homogeneous, Fisher-Solow intertemporal [production] frontier...does not, repeat not, have *all* the familiar properties of the simpler Clark-Ramsey-Solow-Swan-Meade parable" (Samuelson 1976: 15).

Samuelson lists thirteen propositions true for the parable. Observing that only three can remain standing in his most general case, he goes on to say that, on the question of the relationship between steady-state consumption and steady-state interest rates, there is one definite result that emerged from the capital theory controversy. The *necessary and sufficient* condition for steady-state consumption to be higher, when the excess of the steady-state interest rate over the growth rate is also higher, is that the real Wicksell effect should "fail to be negative" (Samuelson 1976: 19), a result proved by Burmeister (1976). Samuelson takes pains to point out that an inverse relationship between the capital-output ratio (the capital-labor ratio) and the rate of interest is *not* the issue. Only the *sign* of the real Wicksell effect, which is the price-weighted sum of changes in the

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<sup>35</sup> Under the heading, WHAT THE CAPITAL THEORY CONTROVERSY TAUGHT ME, *only* this false theorem (Samuelson 1989: 137-139) is mentioned. None of the other issues – and certainly not the central problem of uncertainty in economics raised by Robinson in her published work and in her letters to Samuelson concerning the controversy – were deemed worthy of comment. This is remarkable in an article devoted entirely to his memories of their interactions!

vector of physical capital goods, matters. When the effect is negative – as in the parable – the economy is “regular” (see *ibid.*).<sup>36</sup>

#### **4. General Assessment of the Correspondence**

Robinson’s arguments concerning income distribution and the meaning of capital, the process of capital accumulation, and, most importantly, the problem of “getting into equilibrium” were never fully embraced by Samuelson, but, in his correspondence with her, he was unfailingly polite – if a little wary of her influence. Certainly, he was taken aback by the interest of students in the capital theory controversy and what he saw as its political overtones, writing to Solow (9 July 1973):

If this were all merely a tempest in the advanced seminar room of capital theory, it would all be comical. But, honest to goodness, the new left thinks that the ‘other Cambridge’ breakthroughs have shown up the emptiness of mainstream economics when it comes to a theory of distribution. Three times I’ve given general lectures on new trends in economics ... The first two times, at the New School and Wisconsin, I never even mentioned the Robinson-Sraffa critique of marginal-productivity

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<sup>36</sup> In more recent work, Burmeister observes: “There is some irony in the conclusion that a well-behaved aggregate production function exists when (and probably, for all practical purposes, only when) the Marx EOCC [equal organic composition of capital] condition holds. Unfortunately, the conditions required for EOCC are so exceedingly stringent that they are unlikely to be realized in any actual economy” (Burmeister 2008: 344). Nevertheless, the aggregate production function lives on as a seemingly valid theoretical construct in myriad textbooks.



distribution; only to have students come up to me after the lecture and reproach me for not having mentioned this great new wave of the future. The third time I knew better and brought it up myself.

In a subsequent letter (19 July 1973), Samuelson muses on the best way that he and Solow might respond to the “misstatements” and “misrepresentations” he found in the draft of Robinson (1975a) which she had sent him:

This could of course be done in a joint note...but that might be construable as a united front by a desperate and beleaguered official MIT and, to use Schumpeter’s phrase, one wouldn’t dream of using so big a cannon to crush so little a peanut ... Murray Brown...is running next March a 2-day symposium on all this. I’ve little desire to go...the thought of being in a room with...some of those who’ve made a career of this subject will drive the strongest man into the field of plumbing.

Stepping back from the frustrations evident in some of their letters, the question remains: Why did Robinson and Samuelson fail to come to *any* sort of mutual understanding? The answer turns on the problem of uncertainty and how each protagonist viewed this long-standing bane of the formalist. Solow was clear: “The fundamental difficulty of uncertainty cannot really be dodged; and since it cannot be faced, it must simply be ignored” (Solow 1963: 15), while Samuelson, in a letter to Solow (19 July 1973), was uneasy about making such a drastic move, actually granting Robinson her point, while seeming to keep up the faith: “She is on to a real problem here; but she goes overboard in her nihilism that no kind of roughly good foresight is possible in dynamic paths outside golden age balanced growth.”

Lack of candor aside, formalism also played a role. While Robinson acknowledged her limitations,<sup>37</sup> she understood that assumptions underlying a formal technique are not always recognized for what they are: “The account which the economist gives of the assumptions required for his technique is often very misleading. ‘The formulae are wiser than the men who thought of them,’<sup>38</sup> and the technique knows a great deal more

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<sup>37</sup> When Ragnar Frisch invited her to be a Vice-President of The Econometric Society, “I said that it was no good for my name to appear on the cover of the journal when I could not understand anything inside it” (Robinson 1975b: iii). “The Rate of Interest” (Robinson 1951) was, however, published in *Econometrica* on the insistence of Frisch who wanted “to get more prose into [the journal, and who] paid me the very valuable compliment of saying that I had Ricardo’s instinct for making realistic simplifications” (Robinson 1975b: iii). In “Thinking About Thinking,” Robinson remarked that, “I had a very literary education and to this day I know only the mathematics that I was able to pick up in the course of trying to formalize economic arguments” (Robinson 1979: 115). She *did* seem to have had a knack for seeing “the resolution of complex logical and even mathematical problems without any knowledge of mathematics,” as Richard Goodwin experienced first-hand (see Goodwin 1989: 916), but the mathematics that appeared in her writings was generally relegated to Appendices, written by others.

<sup>38</sup> Robinson was quoting Heinrich Hertz, who established the existence of electromagnetic waves, theorized by Cambridge mathematician James Clerk Maxwell. Hertz had written: “One cannot escape the feeling that these mathematical formulae have an independent existence and an intelligence of their own, that they are wiser than we are, wiser even than their discoverers, that we get more out of them than was originally put into them” (Hertz cited in Sheldrake 2012: 87-88). Hertz celebrated the

about the assumptions that it requires than the economist who is expounding it” (Robinson 1932: 8). Potts (2000: 73), referring once to Robinson (1962b), effectively addresses the point she had made thirty years earlier, arguing persuasively that in the algebraic structure known as a *field*, one finds the root cause of many unresolved controversies.<sup>39</sup> The *field* concept is essential to the analytical economics Samuelson did so much to establish, but nowhere in discussions of its entailments (e.g. Samuelson 1983: 148-153; de la Fuente 2000: 24-37),<sup>40</sup> does one find a critical discussion of its implications for the *connections* – mutual and complete – that are assumed to *pre-exist* among all economic agents over space *and* time:

Field theory allows complete off-the-shelf mathematical formalisms [e.g. dynamic programming] and analytical principles ... [I]ts appropriation was much influenced by the temptations of mathematical respectability, and, it must be acknowledged, this has...brought enormous success. But the fact remains that unlike physical space, economic space is not amenable to a field representation because it is not integral in the sense that every element (that is, agent) affects *every other element*. This is a wholly

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power of mathematics to generate unexpected results and novel problems. Robinson was wary of the ways in which it can restrict the types of questions that can be asked in a subject like economics.

<sup>39</sup> His bold thesis is that *all* heterodox economics, of which he presents a sweeping survey, can be broadly understood as a rejection of the *field* concept as the foundation for analytical economics.

<sup>40</sup> For an everyman’s guide to algebraic structures, rings and fields, there is nothing to match the grace and elegance of Allen (1962), a book written with all the depth and insight of his better-known text, Allen (1938).

satisfied empirical condition for physical field theory but for reasons both definitional and observational it is a most audacious step to define an economic system as anything remotely resembling a perfect integral (Potts 2000: 25; italics added).

A complete exegesis of Robinson's arguments with Samuelson in terms of this opposition between *field*-based theory and the imperfect, incomplete, mutable connections that characterize the economic life of firms, households, and other institutions would take us far afield. Nevertheless, this opposition – once appreciated in its full complexity – provides a clarifying backdrop to the otherwise ill-understood aspects of Robinson's efforts to grapple with "Time in Economic Theory" (Robinson 1980b) and with "History versus Equilibrium" (Robinson 1974). In the end, she did not think that she, or anyone else,<sup>41</sup> had accomplished much, concluding her life's work on a despairing note: "[T]he 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, unmeasurable quantities and indefinable concepts and reconstruct a logical

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<sup>41</sup> Here, one must mention Brian Loasby, whose work was known to Robinson. She evidently enjoyed learning of his discovery that, "'Pretending to forecast the future' shall be classified as disorderly under...the [New York State] Code and liable to a fine of \$250 and/or six months in prison" (Robinson 1977: 1,322). There is, however, no evidence that Robinson had read the first of his many books (Loasby 1976) published during her lifetime. It offered just the sort of critique of the information structure of mainstream economic theory that would have addressed her Keynesian-inspired concerns about knowledge and expectations. Loasby's work is cited throughout Potts (2000).

basis for analysis with what, if anything, remains” (Robinson 1985: 160).<sup>42</sup> Certainly, a never-ruptured equilibrium of supply and demand maintained over an infinite time horizon under conditions of perfect foresight would, for Robinson and others,<sup>43</sup> qualify on all three fronts! And, yet, one wonders, is there no *other* logical basis for analysis that mathematically oriented economists might embrace?

A resurgent evolutionary economics, relieved of its association with the now discredited biological notion of a competitive selection process effecting optimal outcomes (see Potts 2000: 34), treats *connections* among economic *objects* on a par with the objects themselves. Local and partial interactions are all that occur. Analysis of such *interactions* requires the use of a non-integral space, replacing the algebraic structure of a *field* with something else. As Robinson recognized, however, tractability of assumptions prompts “the optimistic, analytical...economist [to] choose the manageable set” (Robinson 1932: 6) and certainly the *field* assumption is manageable. Where would the differential and integral calculus be without it? But nowadays, combinatorics and, in

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<sup>42</sup> This posthumously published paper, originally dated December 1980, was a work-in-progress, “The Theory of Normal Prices, Spring Cleaning.”

<sup>43</sup> “Should complete foresight be an indispensable postulate...there results that wider paradox that the science has already posited the object that it is first to investigate; that, without this assumption, the object could not exist at all” (Morgenstern (1935) [1976]: 175). “The intertemporal equilibrium is not an object of analysis, it is not a statement of what is to be determined. It is simply the name attached to the solution of a set of equations. Modify the equations – say by working with expected prices rather than a full set of futures markets – and a new ‘equilibrium’ emerges as the solution of the new equations” (Eatwell 2012: 3-4).

particular, graph theory are sufficiently well developed to provide something else, a logical basis for a new type of economic analysis which casts aside the integral space<sup>44</sup> upon which so much current theory has been built (see Kirman 1987: 559). Of course, the path forward will not be an easy one, but with a fuller understanding of how the *field* concept and associated integral space has restricted the types of economic questions that can be formulated, perhaps enough young economists will perceive another “lovely vacuum” (Samuelson 1983: xxv) into which to pour their energies. The potential payoff is enormous to the extent that it lays a foundation for an evolutionary growth theory<sup>45</sup> fulfilling Robinson’s goal of generalizing Keynes’s *General Theory*, a project that neither Samuelson nor Solow saw as having anything to do with the theory of capital accumulation.<sup>46</sup>

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<sup>44</sup> The word “integral” sometimes appears in the context of evolutionary analysis so that the question of whether such analysis *requires* a non-integral space remains open. See Donkers (2016).

<sup>45</sup> The traditional foundation for increasing returns as found in Smith (1776) [1976] and Young (1928) is described as “wholes into parts” or *downward* complementary by Dopfer et al. (2016: 757), who then place “parts into new wholes” or *upward* complementary on the same footing.

<sup>46</sup> Samuelson makes one concession in a letter (9 July 1973) to Solow: “If from relative factor supply side no distribution of income theory is possible, then a Kaldor-Keynes macro theory might be in order to try to fill the vacuum. That’s the only sense in which ‘generalizing the General Theory’ and ‘post-Keynesian versus pre-Keynesian processes of accumulation’ are relevant.” Solow (3 July 1973) had been more adamant: “Keynes has nothing to do with it one way or the other, since nobody is being Keynesian in the sense of having output limited by effective demand rather than by supply side

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considerations."

<sup>47</sup> The dates of papers by Robinson are followed by corresponding volume and page numbers from her five volumes of *Collected Economic Papers*, published by Blackwell, Oxford, in 1951, 1960, 1965, 1973, and 1979; and reprinted with an index by MIT Press, Cambridge MA, in 1980. New Introductions to volumes 2 and 3 are reprinted as chapters 9 and 10 in Robinson (1980a). The dates of papers by Samuelson are followed by corresponding volume and page numbers from *The Collected Scientific Papers of Paul A. Samuelson*, published by MIT Press, Cambridge MA, in 1966 (volumes 1 and 2), 1972 (volume 3), 1977 (volume 4), 1986 (volume 5) and 2011 (volumes 6 and 7), with various editors. Robinson and Samuelson added occasional postscripts or addenda to their reprinted papers. Among Robinson's cited papers, two were shortened in their reprinted versions (Robinson 1953-1954 and Robinson 1958). For assessments of the work of Robinson and Samuelson, see, respectively, Harcourt and Kerr (2002) and Backhouse (2017), the first volume to be published of a multi-volume work.

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