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## Voluminous, repetitive, and intractable: Samuelson on early development economics

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VOLUMINOUS, REPETITIVE, AND INTRACTABLE: SAMUELSON  
ON EARLY DEVELOPMENT ECONOMICS

BY MAURO BOIANOVSKY

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# ***Voluminous, repetitive and intractable: Samuelson on early development economics***

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**Abstract.** In the late 1970s Paul Samuelson drafted the outline of a paper, never published, with a critical assessment of the theoretical innovations of postwar development economics. He found the subject essentially intractable. The present paper discusses how that assessment fits in Samuelson's published writings on economic development, throughout several editions of his textbook *Economics* and in some papers he wrote after that assessment. Increasing returns posed a main analytical hurdle, together with the elusive attempt to provide "laws of motion" of economic development. Samuelson's notion of "tractability" may be traced back to Peter Medawar's well-known definition of science as the "art of the soluble."

**Key words.** Samuelson, development economics, tractability, Medawar, increasing returns

**JEL classification.** B20, B40, O10

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Scientists do their best. They deal for the most part with the tractable. When nature and society throw up problems that prove *not to be tractable*, this serves as a challenge and a reproach to the dedicated scientist. But honesty requires recognition of one's limitations. Precisely because the theory of economic development has been so hard to perfect, this is a field that will appeal to the venturesome.

– P.A. Samuelson, “Economic development looked at from the standpoint of a theorist” (1978; italics added)

## 1. A letter from India

In September 1977 Paul A. Samuelson (b. 1915; d. 2009) received a letter from Y.C. Halan (1977) – a professor of economics at the University of Delhi (India) – with an invitation to contribute an essay on “economic development in underdeveloped countries” for a Festschrift in honour of V.K. Rao.<sup>1</sup> Samuelson replied positively a couple of weeks later, but, after further correspondence from Halan inquiring about the delivery of the paper, informed in July 1978 that he had not been able to complete his contribution to the Festschrift due to “terminal illness and recent death” of his long-time wife Marion Crawford Samuelson, who died on 14 February that year. Samuelson, however, did sketch in April 1978 an eight page long draft, never published, of his planned chapter, titled “Economic development looked at from the standpoint of a theorist” (Samuelson 1978b). Samuelson's paper draft provided a critical, if brief, assessment of post-war development economics, which had emerged in the 1940s and 1950s as a new disciplinary field in economics.

Development economics was an exception to Samuelson's claim that he was the “last generalist” in economics, with contributions in virtually every field (see Boianovsky 2019a). So why did he take on the task to produce an essay on the topic? A first reason was precisely the fact that the request came from India. As put by

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<sup>1</sup> Rao was a distinguished Indian development economist, who did his PhD at the University of Cambridge in the mid 1930s. He had an article included in the first-ever collection of readings in development economics (Rao [1952] 1958). Samuelson (1964: 754) was familiar with that anthology. Rao's Festschrift eventually came out in 1982, without a chapter by Samuelson (Gupta, Halan and Desai 1982).

Samuelson (1978a: 1) at the outset of his paper, much of the stimulus to investigate economic development in western universities and research institutes had come from “interest by the advanced nations in India.” He reported a letter from an Indian reader complaining that the usual example used to portray poverty in his influential textbook *Economics* was India, even though there were many other poor (or even poorer) countries.<sup>2</sup> “What do you have against India?” asked the reader. According to Samuelson, the reader had completely reversed his position – “we in America have had something in the nature of a love affair with India” (Samuelson 1978b: 1) since its independence from colonial rule in 1947, due to its features as a large democracy beset by a degree of poverty not so hopeless as to prevent prospects of economic growth by American economists. As put by Samuelson, “Indophilia” was not such an old story in the U.S. as in the U.K., which had long received a large inflow of Indian students. It was only in the 1950s and 1960s that Samuelson’s MIT and other American centres of economic research started to receive a significant number of high-quality Indian economic graduate students and visiting scholars, most of them interested in issues of growth and development.<sup>3</sup>

American academic interest in Indian economic development in the post-war period reflected as well U.S. government’s foreign aid programs, especially after the announcement of President Truman’s “Point Four” in his 1949 inaugural address, as part of the Cold War context. Foreign aid gave American economists “the will and the wherewithal” to grapple with the problems of progress in poorer societies, later called LDCs or “less developed countries” (Samuelson 1978b: 2). That was similar to Hollis Chenery’s (1992: 379) later account – that “the field of development economics emerged as a by-product of America’s aid programs” in the postwar years. Samuelson remarked, in a somewhat ironic way, that that was an instance of Karl Marx’s notion that the “superstructure of science” is shaped by the “basic infrastructure of life’s

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<sup>2</sup> This may be illustrated by the front-leaf – displaying the evolution of income per capita in the United States, Great Britain, the Soviet Union and Japan since 1870 and in India (since the mid-1950s) – added to the sixth (1964) edition of *Economics* and kept and updated until the eleventh (1980) edition with the inclusion of West Germany. Samuelson (1976a: 2) pointed out that the flyleaf made evident the “contrast” between the “Western World’s affluence” and the “poverty of India, whose growth curve barely shows on the chart.” Such contrast had turned men “all over the world” preoccupied with “economic development” (ibid).

<sup>3</sup> A short list would include, e.g., Avinash Dixit, Sukhamoy Chakravarty, Pranab Bardhan, Jagdish Bhagwati, Amartya Sen, Subramanian Swamy, Amitava Dutt and Abhijit Banerjee (the last two did their PhDs at MIT in the 1980s).

material conditions.” The establishment at MIT in 1951 of the Center for International Studies (CENIS) – which gathered together influential development economists such as Walt Rostow, Paul Rosenstein-Rodan and Richard Eckaus –brought development issues close to the MIT economics department, though largely disconnected from its growth economics research agenda led by Samuelson and especially Robert Solow. Indeed, the roots of the separation between development and growth economics may be traced to MIT in the 1950s and early 1960s (see Boianovsky and Hoover 2014). Samuelson was, of course, aware of the breadth and depth of international activities at CENIS, in competition for funding and influence with other American development research centers.

The nineteenth century’s struggle for the partition of Africa was as nothing compared to the scramble among our great universities for particular countries and regions as veritable laboratory specimens for economic study. My own institution, MIT, at one time maintained in its Center for International Studies sizable projects for the three I’s: India, Indonesia and Italy; we left to Harvard Pakistan, Paraguay, and for all I know Poland. (Samuelson 1978b: 2-3)<sup>4</sup>

Samuelson (ibid) observed that, as U.S. government’s foreign aid weakened in the 1970s with the waning Cold War tensions, and the great foundations as Rockefeller and Ford cut their budgets for social science research, the “scale and intensity” of research in development economics fell away (cf. Lewis 1984: 1 and Krugman 1993: 26 on the role of diminishing aid and funding as a factor in the decline of development economics in the 1970s and 1980s).

It was in that context that Samuelson considered that time was “ripe” to assay a survey of what the “scholarly innovations” in development economics amounted to “as looked at from the standpoint of the general economic theorist”, that is, a non-expert in economic development. However, probably due to the unfinished character of his paper, Samuelson (1978b) did not produce such a survey, but offered instead some hints of his discontent with the achievements of development economists so far. In order to fill in the gaps in his 1978 assessment, it is necessary to take into account Samuelson’s writings on economic development before and after the 1978 typescript, particularly in his *Economics*. Indeed, Samuelson (1955) introduced in the third

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<sup>4</sup> See Engerman (2003) on the key role played by CENIS in the economic-political context of American economic aid to India in the 1950s and 1960s.

edition a new chapter on economic development, when the subject became fashionable among students, economists and policy makers alike. He kept and updated it throughout all further editions of the textbook, including the period from 1985 to 2010, when William Nordhaus became his co-author. Breaking the “vicious circle” of poverty and backwardness in underdeveloped areas was listed as a first motivation to study economics; such “new interest” in economic development had made economics “an exciting and vital subject in the capitals of the world, and on Main Street as well as Wall Street” (Samuelson 1976a: 2). The utmost practical relevance of economic development provides another related reason why Samuelson responded positively to the Indian letter.

A few years after Samuelson’s (1978b) typescript, critical assessments of the state and history of development economics started to come out, usually by practitioners (e.g. Little 1982; Hirschman 1981), but also by economic theoreticians like Kenneth Arrow (1986) in his presidential address to the 1986 meetings of the International Economic Association held in New Delhi (India). Arrow’s perspective as an outsider coming from “general economic theory” was close to Samuelson’s. Again, like Samuelson, Arrow’s general tone was defensive of neoclassical economics, with an attempt to discuss economic underdevelopment as the result of market failures and deviations from optimality due to dual labor markets and increasing returns in goods markets. Both Samuelson and Arrow were interested in what the latter called “grand theories” of economic development, which were starting to loose ground to highly specific microeconomic applications and case studies.

The intended contribution to the Rao Festschrift represented also, from Samuelson’s viewpoint, an opportunity to critically assess the notion that the first generation of development economists – approximately 1950-75 – significantly improved the understanding of the development process. The overall tone is defensive of mainstream economics as a modeling endeavor.

Paul Rosenstein-Rodan, Colin Clark, Ragnar Nurkse, Sir Arthur Lewis, Peter Bauer, and Hollis Chenery deserve our praise for their pioneering contributions. After you have read their works, you know more than you did before. But few readers are left with the impression that a quantum jump in knowledge has been obtained in the *voluminous and somewhat repetitive* post-war growth literature. (Samuelson 1978b: 4-5; italics added)

He deployed the term “growth literature” in the sense of the study of economic growth of underdeveloped countries, not models of steady-state growth paths in industrialized economies, of the kind advanced by Solow and others at MIT and elsewhere, which were not discussed in the 1978 typescript. Samuelson speculated whether the absence of “elegance of model building and technicality of mathematical analysis” was a main reason why no development economist had been awarded a Nobel Prize up to 1978.<sup>5</sup> It is implicit that by “quantum jump” he meant formal economic modeling. However, ironically enough, Arthur Lewis and Theodore Schultz would share the Nobel Prize the year after, against Samuelson’s expectations. This might help to explain why Samuelson never came back to his attempted assessment of development economics. He would refer to Lewis’ 1979 Nobel award in the eleventh edition of *Economics* (Samuelson 1980: 698, n.5), but the 1979 prize remained an exception to his habit of writing annual reflections about achievements of Nobel laureates for *Science* and *Newsweek* between the late 1970s and early 1980s.

Years later, Samuelson was asked to contribute – together with other Nobel laureates (Lawrence Klein, Douglass North, Robert Solow and Amartya Sen) – a chapter with thoughts on the future of development economics, as part of the appendix to a World Bank publication about the “frontiers” of the field (Meier and Stiglitz 2001). Unlike his 1978 typescript, Samuelson’s (2001) contribution consisted of a detailed comparison between “new” (endogenous) and “old” (Solovian) growth theories, with no references to post-war development economics. As discussed below, this is explained by his concern with the hurdles involved in the attempt to introduce increasing returns and pecuniary externalities in development and growth models, which is only implicit in the 1978 paper draft.

The timing of Samuelson’s (1978b) draft may also help to explain its critical stance toward development economics. The year before, Albert Hirschman – who was not mentioned among the development economists listed in that draft, but was recognized elsewhere as one of the development pioneers, together with Benjamin Higgins, Everett Hagen, Raul Prebisch and other names absent from the 1978 typescript (see Samuelson [1971] 1972: 708; 1976a: 701) – had argued that Samuelson’s (1948a) famous factor price equalization theorem acted as a

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<sup>5</sup> He acknowledged that past Nobel recipients Tinbergen, Kuznets, Myrdal and Leontief had made contributions that had a bearing on development, but maintained that “still the point stands” (Samuelson 1978b: 8).



“boomerang” against the ability of mainstream trade theory to cope with the facts of international economic asymmetries (Hirschman 1977:67-68). At the same time, Samuelson’s (1948a) theorem, claimed Hirschman (ibid), paradoxically contributed to the emergence of development economics, as it increased the credibility of the less refined but down-to-earth hypotheses about international income inequalities advanced around that time by Prebisch, Hans Singer and others (see Boianovsky 2019b). From that perspective, Samuelson (1978b) may be seen in part as a reaction to Hirschman’s (1977) indictment.<sup>6</sup>

Samuelson’s (1978b: 8) suggestion, as quoted at the outset of this paper, that, unlike successful scientists in general, development economists addressed problems that were “not tractable”, should be read in the broad context of his allusions elsewhere to Peter Medawar’s (1969a) well-known definition of science as the “art of the soluble.” Samuelson (1969: viii; [1982] 1986: 859; 1998: xi) rendered Medawar’s term “soluble” as “tractable”. Together with Brian Loasby (1971: 866; 1976: 107, 194), Samuelson was the economist who paid careful attention to Medawar’s methodology and its implications for tractability issues in economics. Samuelson’s charge that economic development theory tackled intractable questions reflected also his view that development economists – especially but not only those influenced by the “magnificent dynamics” of classical economists and Marx – were attracted to the unfeasible quest for encompassing historical “laws of motion”. This is reminiscent of Karl Popper’s (1944a, b; 1945) criticism – endorsed by Medawar (1969b: 13; 1982) – that social sciences in general suffered from what Popper famously called the “poverty of historicism.” Classical economics’ main contribution to the study of economic growth, according to Samuelson, was Malthusian demography, which did not play a prominent role in post-war development economics. Tractability issues have recently attracted attention from economic methodologists (see e.g. Hindriks 2005; Mäki 2009; Mäki and Marchionni 2012), without references to Samuelson’s previous discussion of the matter though.

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<sup>6</sup> Moreover, shortly before that, Samuelson (1976b: 107) had concluded his criticism of Emmanuel’s ([1969] 1972) thesis of “unequal exchange” between rich and poor countries – Samuelson’s only engagement in debate with a development economist – with the view that “no new light has been thrown on the reason why poor countries are poor and rich countries are rich”.

## 2. Medawar, tractability and increasing returns

Samuelson's 1978b harsh assessment of the theoretical status of development economics restated a judgement he had briefly expressed before in print, when the prestige of early or classic development economics was at its peak. As part of an account of the history and current standing of American economic thought, Samuelson (1960: 37) discussed the preoccupation of 19<sup>th</sup> century American economists – as they tackled structural changes in the industrializing U.S. economy – with issues pertaining to what would in the 1960s be called “growth and development economics.” Like others after him, Samuelson pointed to the poverty of American intellectual achievement in economics before the end of the 1800s, including its inability to formulate a “simple and comprehensive theory” of economic development.<sup>7</sup> However, he stated,

Modern economists of the 1960's are least likely to blame them – since in our own day the philosopher's stone that would unify and illuminate this area has long been sought, but the search has as yet produced only a catalogue of important but not unobvious tendencies and countertendencies. (Samuelson 1960: 37)

Around the same time, George Stigler (1960) shared Samuelson's view that, despite extensive theoretical and applied research efforts, development economists did not go far beyond common sense and data collection. Stigler was interested in the broad question whether economic events and policies had significantly affected the choice of research strategies and the historical evolution of economic theories. The Chicago economist argued that the march of economics (and science in general) was essentially autonomous in respect with historical events and policy debates. As a case in point, he claimed that, regardless of its “current popularity” among economists and policy makers, the study of economic development had “not yielded important theoretical results” and that “the ratio of cliché to analytical creativity in the literature of economic development is awesome to contemplate” (Stigler 1960: 39, 43). The

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<sup>7</sup> That topic would attract later on the attention of William Barber, among other historians of thought (Boianovsky 2019c). For another take on 19<sup>th</sup> century American approaches to economic development see Meardon 2018.

high demand for development economics did not assure that it delivered the goods, Samuelson and Stigler agreed.

The same critical tone concerning development economic theory may be found in *Economics*, sometimes implicitly. The sixth edition brought in a new chapter 35 on “the theory of growth”, one of the first textbook discussions ever of growth theory, covering the classical “magnificent dynamics” of Malthus and Ricardo, Solow’s neoclassical model (the core of the chapter), and, in an appendix, topics such as Schumpeterian innovation, the Harrod-Domar Keynesian growth model(s), the von Neumann general equilibrium growth model and the Cambridge approach of Nicholas Kaldor and Joan Robinson. Chapter 35 provided the “*principles* of economic theory to the process of growth and development,” in preparation for the next chapter’s “application” of those ideas to underdeveloped economies (Samuelson 1964: 721, italics in the original). Samuelson (1964: vi; italics in original) announced in the preface the “new chapter on the *theory* of growth.” While acknowledging that “development problems are all the rage these days among economists,” he stressed the need to “bring economic analysis to bear” instead of boring students with a “hit-and-run analysis of economic theory”, sugarcoated by “dated anecdotes and statistics” about countries in different stages of development. Mere facts had to be supplemented by all the analytical tools of the rest of the book if one wanted to master the “vital subject” of economic development (ibid: 721).

From Samuelson’s perspective, 1960s growth economics was able to account for the “stylized facts” of long-term growth in industrialized economies, but not for economic divergence between poor and rich nations. For the application of growth theory to the “the poor countries in particular, there must be added the important additional concepts of ‘external economies’, ‘social overhead capital or infrastructure’, and ‘increasing returns’ – as is done in the next chapter” (ibid: 752), Samuelson (1964: 752) stated in the closing paragraph of his growth chapter. The main source of the view that “the phenomenon of ‘*increasing* returns’ is at the core of “dynamic economic development” was his MIT colleague Rosenstein-Rodan (1943, 1961), but the notion went back to Adam Smith’s *Wealth of Nations* and Allyn Young (1928), among others (Samuelson 1964: 761-62; italics in the original).

Samuelson also considered in his *Economics* another important component of the emerging post-war development economics: Lewis’s (1954, 1955) concept of perfectly elastic labor supply and its associated notion of disguised unemployment in

economies with labor surplus, which Lewis used to build a model of capital accumulation and growth along classical Ricardian lines, without any analytical role for increasing returns (see Boianovsky 2018a, 2019c; and section 3 below). Development economists, Samuelson (1964: 760) *taxed*, did not elaborate a “unified theory” that could be regarded as an alternative to the neoclassical Solovian full employment growth model under constant return to scale, but added some “special features” such as increasing returns and disguised unemployment<sup>8</sup>

Samuelson’s (1964: 762) view – that increasing returns “can make it possible for dramatic spurts and accelerations to occur in economic development” (ibid: 762) and by that contribute to explain international divergence – was closely related to his conceptualization of economic underdevelopment as a “vicious circle” or trap under multiple equilibria. It was implicit in Solow’s (1956) growth model with diminishing returns to capital the notion of per capita income convergence for countries with the same parameters. From that perspective, steady-state income divergence resulted from differences in parameters (“fundamentals”) such as saving rates and population growth. In the alternative view of underdevelopment as a “coordination failure”, put forward by Rosenstein-Rodan and other post-war development economists, countries with the same fundamentals can move along divergent paths (see e.g. Ray 2008). Samuelson tended to support the latter approach, especially after the 1964 edition of *Economics*, but one may find as well passages reminiscent of the Solovian emphasis on fundamentals.<sup>9</sup>

Hence, it is not surprising that Bauer (1958) would critically refer to Samuelson’s *Economics* as “typical” of the then dominant view that associated economic poverty to market failures and vicious circles which could only be broken through economic policy intervention (especially economic planning) as a way to supplement competitive market forces and bring about Rostovian “take-offs” under

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<sup>8</sup> As for the Harrod-Domar growth model, extensively deployed by development economists at the time as an instrument of long-term economic planning, Samuelson (1964) interpreted it instead in the context of Keynesian macroeconomic cyclical fluctuations along a growth path. This applied particularly to the complex effects of a higher savings ratio on economic growth in the Harrod-Domar framework (see Boianovsky 2018b).

<sup>9</sup> Even so, the savings rate depended on income per capita: “Poverty creates want, want destroys thrift, absence of capital formation prevents improvement, limitation of mass production makes poverty – and so the vicious circle goes” (Samuelson 1955: 722). Productivity affected output, but the size of output also affected productivity because of increasing returns and externalities.

either Rosenstein-Rodan's balanced "big pushes" or Hirschman's unbalanced growth strategies (see also Boianovsky 2019a). Samuelson, however, after asserting that increasing returns and externalities were at the center of underdevelopment phenomena, charged post-war development economics for failing to put forward a theoretical framework or a model able to "unify and illuminate" the links and connections involved. By the time Samuelson drafted his 1978 typescript, he had come to the conclusion that such analytical issues were *intractable*, a concept he developed upon coming across in the late 1960s Medawar's formulation of the goals and limits of scientific activity.<sup>10</sup>

Medawar's innovative discussion of the history of the "hypothetico-deductive" method of scientific inquiry and critique of induction in his 1967 *The art of the soluble* – which reached a large readership in its 1969 paperback edition – brought him a reputation as Popper's "chief apostle" within the scientific community. As pointed out by Neil Calver (2016), Medawar treated Popper not primarily as the high priest of falsificationism, but as the source of the analysis of the role of "creativity and originality" in the formulation of hypotheses by means of imaginative conjectures of what might be true (see also the references to Medawar in Blaug 1992: 31, 62, 64). Scientists should distinguish between "*having an idea and testing it or trying it out*", and stay away from inductive methods (Medawar 1969a: 133). According to Medawar (see his essay on "Hypothesis and imagination" in Medawar 1969a: 147-73), experiments test the logical implications of hypotheses, through the interaction between imagination (conjecture) and criticism that characterizes scientific inquiry. Moreover, the requirement that hypotheses should fit empirical findings imposes a discipline and curbs the imagination of scientists, who have to restrict themselves to what Medawar dubbed the "art of the soluble" (see also Ben-David 1975: 446).

Medawar's depiction of science came out originally in his review of Arthur Koestler (1964), published in *The New Statesman* of 19 June 1964 and reprinted as "The Act of Creation" in Medawar (1969a: 95-108). Medawar reacted against Koestler's (1964, book 2, chap. 10) criticism that scientists often evade the

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<sup>10</sup> Sir Peter Brian Medawar (b. 1915; d. 1987), a Brazilian-born British biologist, shared the 1960 Nobel Prize for Physiology or Medicine for the discovery of immunological tolerance, which became the foundation of tissue and organ transplantation (see Medawar 1986 for biographical information).

investigation of crucial or challenging problems. The reason, he submitted, is that such problems are usually “very difficult” to tackle.

No scientist is admired for failing in the attempt to solve problems that lie beyond his competence. The most he can hope for is the kindly contempt earned by the Utopian politician. If politics is the art of the possible, research is surely the *art of the soluble*. Both are immensely practical-minded affairs. (Medawar 1969a: 97; italics added)

The notion of politics as the “art of the possible” came from Bismarck and Cavour (Medawar 1979: 18). Like politics, scientific research is practical, in the sense that the business of scientists is “to solve problems, not merely to grapple with them” (Medawar 1969a: 11). After all, the spectacle of a scientist locked in combat “with the forces of ignorance is not an inspiring one if, in the outcome, the scientist is routed” (ibid).<sup>11</sup> From that perspective, the perception of some visible means of solution is a key condition for entering the “decision cycle” of scientific research in economics and other fields, as put by Loasby (1976: 107).

Samuelson’s first mention of Medawar appeared in the 1969 foreword to Chakravarty’s book on development planning, written when the Indian economist was visiting MIT, which Samuelson (1969: ix) praised for bringing the tools of optimal control theory to bear upon the problem of economic development – a “vital” matter as “India and indeed much of the world has a desperate need to develop economically.” That was also the first reference to Medawar by an economist. Samuelson (1969) took the opportunity to recant his previous criticism that Ramsey’s (1928) famous result about the optimal rate of saving was a “rather fanciful prescription based on fanciful assumptions”, built on a “mathematically unrigorous, if not wrong” reasoning (Samuelson 1953: 588, n. 26; 1946: 196, n. 8). Chakravarty (1969) helped to elucidate both the role of the form of the utility function in deciding the amount of optimal saving and the meaning of maximizing a Ramsey integral instead of a more general functional.

As a young man I was impatient of “restrictive” assumptions, and

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<sup>11</sup> Medawar’s description of scientific inquiry has been sometimes misunderstood as an argument for the study of easy problems with easy solutions. Medawar (1979: 18) clarified: “What I meant *of course* was that the art of research is that of making a problem soluble by finding out ways of getting at it – soft underbellies and the like.”

insufficiently aware of the element of truth in the maxim enunciated by the Nobel biologist Peter Medawar that science must deal with that which can be managed, eschewing the *intractable*. Sill, along with Sir John Hicks and Chakravarty, I was right to be concerned with the question of how much of the Ramsey result depends upon this peculiar independence [between the marginal utility of one time and that of another along the accumulation path] assumption. (Samuelson 1969: x; italics added)

The relation between theoretical tractability and restrictive assumptions would come back under different guises in the economic literature – e.g. in connection with modeling increasing returns – as discussed further in section 4 below.

Samuelson (1998: xi) endorsed Medawar's distinction between successful researchers and those who spend their lives in the “doomed quest for a proof of how to trisect an angle.” That explained why “Medawar could say that scientists narrow down to focus on *tractable* problems” (italics in the original). As Samuelson observed, that is an assumption about the prudential behavior of (successful) scientists: it is the “rational coward's way, just as those sensible rats are the first ones to leave the sinking ship” (ibid).<sup>12</sup>

That is reminiscent of Thomas Kuhn's (1962) notion of “paradigm”, as Samuelson ([1982] 1986: 859) pointed out while criticizing Axel Leijonhufvud's (1968) distinction between the “economics of Keynes” as exposed mostly verbally in the *General Theory* and “Keynesian economics” as expressed by the IS-LM and other macroeconomic models. Leijonhufvud, according to Samuelson, had failed to grasp the “essence of scientific methodology” contained in the “Kuhnian truth that it is ‘tractable’ paradigms with unambiguous content” which constitute scientific inquiry. Scientific ideas are necessarily expressed through a “prosaic, tractable, describable, testable and understandable model or paradigm.” (Leijonhufvud 1973 anticipated that criticism in his depiction of the high standing of models among members of the Econ tribe, and the consequent low rank of Devlops.) Medawar was right on the mark when he stressed that science “has to grapple with the tractable”, Samuelson ([1982] 1986:

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<sup>12</sup> Herbert Simon (1965: 97-98) had put it in similar fashion: “People (and rats) find the most interest in situations that are neither completely strange nor entirely known ... Nor does creativity flourish in completely unstructured situations. The almost unanimous testimony of creative artists and scientists is that the first task is to impose limits on the situation if the limits are not already given.”

859) observed. Samuelson's claim that "tractable" and "testable" are analogous features of scientific theories may be traced back to his use of the term *operationalism* (under the influence of physicist William P. Bridgman) in his 1947 *Foundations* in the sense of "testability" or "refutability" of hypotheses (cf. Backhouse 2017: 451).<sup>13</sup>

Like Samuelson, Loasby (1971: 866, 873-74; 1976: 107, 194) too detected significant similarities between Medawar and Kuhn. Indeed, Kuhn (1962: 37) pointed out that "normal scientists" who share a paradigm attain a "criterion for choosing problems that ... can be assumed to have solutions ... Other problems are rejected as metaphysical". From that perspective, maintained Kuhn, a paradigm can "insulate the community from those socially important problems" that cannot be stated in terms of the paradigm's tools. Kuhn called such problems a "distraction", as often illustrated by some of the contemporary social sciences. The scientists' insulation from demands from laymen and quotidian life is what allows them to focus on problems they believe are soluble. According to Kuhn (1962: 163), the fact that, unlike natural sciences, practitioners of social sciences tend to choose their research problems in terms of the "social importance of achieving a solution" goes a long way explaining the lower pace of progress of social sciences. Kuhn's argument is consistent with Samuelson's (1960, 1978b) and Stigler's (1960) views about the underdevelopment of development economics, in the sense that the availability of a paradigm is more important than the relevance of a problem.<sup>14</sup>

It may be inferred from Samuelson's treatment of development economics, in his *Economics* and elsewhere, that its apparent intractability and pre-paradigmatic status should be substantially ascribed to the hurdle posed by the formulation of a dynamic model featuring increasing returns and externalities. That was a main element behind his 1978 assessment, quoted above, that post-war development economics had not produced a "quantum jump in knowledge". Surely, the problem

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<sup>13</sup> Interestingly enough, Paul Douglas, who taught Samuelson at Chicago in the 1930s, wrote in the preface to his book on the *Theory of Wages* – which Samuelson probably read at the time – that propositions that are not testable are "non-operational" concepts in Bridgman's sense. "From the standpoint of scientific progress we should primarily concern ourselves with problems which we can solve" (Douglas 1934: xv; quoted also by Backhouse 2017: 83). That fits well with Medawar's later emphasis on soluble problems in science.

<sup>14</sup> Despite some similarities between Kuhn and Medawar, the latter was critical of Kuhn's notions of "normal science" and "scientific revolutions" and preferred instead the view of continuous progress implied by Popper's falsificationism (see Medawar 1978).



presented by “intractable” increasing returns and their incompatibility with perfect competition beset not just development economics but economic theory in general, including Samuelson’s own work as a leading mathematical economist, as he came to acknowledge:

The trouble with increasing returns to scale is that after you have said the first things about it, it is deucedly hard to find second and third themes to develop. Most of pretty modern mathematics lacks elegant application to the combinatorial swamp that nonconvexity irreducibly entails. My conscience was long bothered by our guild’s neglect of a subject on the ground that it is so hard to tackle (Samuelson 2001: 498-499).

Such neglect seemed to have come to an end shortly before Samuelson drafted his 1978 critical account of development economics, when his former students Avinash Dixit and Joseph Stiglitz (1977) put forward what would eventually be regarded as a first tractable model of imperfect competition and increasing returns (section 4 below). Moreover, Samuelson’s treatment of the role of increasing returns in economic development assumed that countries had gone through their demographic transitions, as discussed next.

### **3. History, magnificent dynamics and demography**

In a section titled “The poverty of anti-mainstream analysis”, Samuelson (1978b) discussed whether there were alternatives to the alleged neglect of the dynamics of the “laws of motion” of the world economy by neoclassical economics. His target was the attempt (by Friedrich List, the German Historical School and especially Marx) to understand economic development broadly in terms of long-term predictions about the future economic evolution of societies. He had already addressed aspects of that approach in a section on “stages of history” in the new growth chapter of his textbook (Samuelson 1964: 721-24), which criticized the 19<sup>th</sup> century fashion of regarding economic development as a “timetable” formed by inexorable phases. List and the German Historical School had provided “little more than speculative science fiction

about the alleged timetable and stages of economic and political development” (Samuelson 1978b: 5).<sup>15</sup>

Neither did Marxian predictions, about the long-term path of real wages, unemployment and profit rates in capitalist economies, fire any better – apart from not being a logical corollary of Marx’s economics, claimed Samuelson (1978b: 6-7; see also Samuelson 1976a, appendix to chapter 42). That was different from what Samuelson (1964: 723-24, 733-37) perceived as the role of Solow’s and other growth models in understanding the historical past through the study of basic trends of capitalism in advanced nations, instead of attempting to compress history into schemes that pretend to “predict the future with certainty.” However, with the exceptions of a few paragraphs about economic imperialism and colonial exploitation (Samuelson 1961: 780-82; removed in later editions) and a lengthy criticism of Emmanuel’s ([1969] 1972) theory of international unequal exchange (Samuelson 1976b) – which concluded that “No new light has been thrown on the reason why poor countries are poor and rich countries are rich” (Samuelson 1976b: 107) – written shortly before his 1978 typescript, Samuelson did not engage with Marx’s and Marxian views about underdeveloped economies (see also Perraton 2007).

Popper (1944a, b; 1945) dubbed the belief – that it is possible to identify and promulgate laws of the historic process and of social transformation with the same predictive value as natural science laws – *historicism*. Popper associated it to the methodological practice of social sciences (particularly but not only of Marxian persuasion) and denied that a predictive science of history, featuring laws of “social development” which link up different periods, could exist. He taxed that methodological approach as the “poverty of historicism” (see also Boumans and Davis 2016: 86-87).

Medawar (1969b: 13; 1982: 299-301) interpreted Popper’s criticism of historicism in social sciences as part of a broad attack on inductivism (see also Calver 2016: 305). The influence of inductivism, according to Medawar (1969b: 13), had awakened in social scientists the “ambition to ascertain the laws of social change,

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<sup>15</sup> On List’s extensive influence on economic development theory and policy see Boianovsky 2013a. Samuelson (1964: 761) ironically refereed to Rostow’s (1960) growth stages as an attempt to divide a country’s development in stages the way “Shakespeare divides a man’s life into various ages”; but he did appreciate the turning point economic principle that the take-off “serves to dramatize” and Rostow’s modernization approach as a whole.

above all by the painstaking accumulation of data out of which general principles will you due course take shape” (cf. Ben-David 1975 on “transformational theories” as peculiar to social sciences). Medawar, however did not follow Popper (1945) in arguing for methodological monism and the construction of social science models based on individual behaviour. In his view, the “backwardness” of social sciences was due not to their failure to use the authenticated scientific methods of the natural sciences, but to the “sheer complexity” of their object of study (Medawar 1969b: 13-16). Clearly, his notion of science as the art of the soluble (or “tractable” as rendered by Samuelson) was devised primarily for the natural sciences, as much as Kuhn’s paradigm.

Development economists, however, were not oblivious to Samuelson’s and, especially, Popper’s criticism of “historical predictions” or “historical prophecies.” Lewis (1955), for instance, was at pains to set his analytical framework apart from historical approaches to economic evolution. He shared Popper’s misgivings about historicism, as indicated by a reference to the latter’s discussion of theories of history and prediction (Lewis 1955: 22). “We do not believe that it is possible to say how any particular social system is going to develop, and we do not, therefore, like Ricardo or Marx or Toynbee or Hansen or Schumpeter set out a theory of the laws of evolution of society”, Lewis (1955: 18) wrote. Moreover, he rejected the notion that there are stages through which every society must pass (as in Comte, Marx, Spencer or Weber) or that it is possible to make long-term predictions according to “immutable laws” (ibid). Lewis (1984: 8, 9) would reaffirm those points in his Presidential Address to the American Economic Association, when he claimed that social scientists had long given up models of long-term prediction in the style of Marx, Rostow or Toynbee. Development economics aimed at elaborating testable theories, not providing a vision of “whither history is leading us”, he asserted.

Lewis’s (1954) not fully formalized model of economic development in a dual economy provided the starting-point for the neoclassical growth model(s) put forward by Solow (1956) and Trevor Swan (1956). The view that Lewis’s classical model mapped out the growth process in underdeveloped pre-industrial economies, and that economic growth in advanced countries should be modeled in the way of contrast with that approach, is clear in Solow’s (2005) recollections and in the first section of Swan’s (1956) article (see Boianovsky 2019c: 112-14). By mid 1960s, the MIT separation between the economics department’s growth theory and CENIS’s

development studies gave way to the application of optimal control techniques to investigate growth paths in labor surplus economies by Stephen Marglin, Dixit, Chakravarty and others at MIT (See Boianovsky Hoover 2014 and references cited therein). Samuelson's turnpike theorems of optimal growth theory played a role in that process, as he gladly pointed out in the foreword to Chakravarty (1969). Whether that was compatible with Lewis's classical approach is, of course, debatable.

In the appendix to his growth chapter, Samuelson (1964: 748-9) discussed von Neumann's (1946) growth model together with Lewis (1954) as contributions to the study of economic growth in labor surplus economies under the assumption that wage goods are fed back into the productive process as inputs, the supply of which determines the size of labor supply (see also Boianovsky 2019a, c). Samuelson (1964, p. 749, n. 2) associated the interpretation of the expansion of an industrial sector through the utilization of an unlimited supply of rural labor mainly with Lewis (1954). Japan's economic development from 1890 to World War I could be understood in von Neumann/Lewis terms, as real industrial wages remained relatively stable because of rural migration – the Japanese economy at the time “grew like a colony of yeast, with human labor being as necessary an input-output as cotton or fodder” (Samuelson, 1964, p. 749). Japanese growth experience in the 19th and 20th centuries impressed Samuelson most. Nevertheless, despite his high opinion about Lewis's (1954) suggested model, Samuelson (1978b) would not treat it as an exception to his critical account of post-war development economics.<sup>16</sup>

Unlike Lewis and most other post-war development economists, the aspect of classical economics that attracted Samuelson most in connection with economic development was population dynamics. That was an essential element of what William Baumol (1951, part I) famously named the “magnificent dynamics” of classical economics, a concept he applied as well to Marx, Schumpeter and Harrod. Those authors provided a “magnificent cast, ambitiously attempting to analyze the growth and development of entire economies over relatively long periods of time – decades or even centuries” Baumol (1951: 11). Baumol distinguished magnificent dynamics from “process analysis” discussed in part II of his book. Samuelson's (1948b) survey of “dynamic process analysis”, referred to by Baumol, explained that

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<sup>16</sup> The same may be said of Chenery, who extended the Harrod-Domar to formulate in the 1960s the influential two-gap model of growth in open economies with a balance of payments constraint.

any process that grows continuously at a constant rate is described by a differential equation as a function of time, whose solution is an exponential expression, as first shown by Evsey Domar (1946). Domar's technique of process analysis marked the beginning of growth economics as the study of steady-state paths, and the gradual abandonment of early magnificent dynamics after Harrod's (1948) transitional contribution. Lewis (1955), like many development economists, stayed away from both process analysis and magnificent dynamics, which bore relation to Popper's concept of historicism.<sup>17</sup>

Samuelson (1964: 724-30) paid careful attention to classical magnificent dynamics in his *Economics* and in articles about classical economics and Marx. The classical hypotheses of diminishing returns to land and endogenous population growth fit well with 20<sup>th</sup> century underdeveloped economies (particularly over-populated Asian nations such as India and China), he claimed. "Among the developing nations economics remains what Carlyle called 'the dismal science'" (Samuelson [1971] 1972: 708).<sup>18</sup> Nevertheless, in his 1978 typescript, with its general defensive attitude of neoclassical economics, he downplayed the analytical relevance of classical contributions to the understanding of capital accumulation and economic growth, which he described as "common sense spelled out in detail." We should not exaggerate, he maintained, the "novelty and degree of sophistication involved in what we sometimes call 'the magnificent dynamics of the classical system'." The consequences of neoclassical retreat (until mid 20<sup>th</sup> century) from the classical previous concern with growth were not particularly relevant, as "not much was really lost" (Samuelson 1978b: 4). Malthusian population dynamics was, however, an exception to Samuelson's indictment. Samuelson's lifelong interest in demography may be traced back to his student days in Chicago. In fact, it was a lecture on

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<sup>17</sup> Development economist Ian Little (1982: 18-19) redefined Baumol's "magnificent dynamics" as the quest for a comprehensive explanation of long-term economic growth and development, in terms of institutional, political and cultural variables in a broad historical setting. Little (ibid) associated it to the "structural" approach to economic development, as distinguished from the "neoclassical" one. He mentioned Rostow's as an example of a contribution to this redefined magnificent dynamics; Douglass North and other neo-institutionalists might also be added.

<sup>18</sup> However, David Ricardo and J.S. Mill did not apply the canonical classical framework to "backward" nations (see Boianovsky 2013b: 76-77, 81-82). They distinguished between reduced growth caused either by diminishing returns to land (the case of advanced countries) or by lack of investment due to "bad government" and "insecurity of property" (the case of poor countries).

Malthus' population theory that got Samuelson started as an economics student back in 1932 (Barnett 2004, p. 528) and ignited his interest in demography (on Samuelson's contributions to population theory see Lee 2019).

The notion of the demographic transition – when the Malthusian demographic model featuring population growth as a positive function of income per capita ceases to operate – as a “precondition” for economic development (Samuelson 1964: 761, 765) became especially prominent since the 12<sup>th</sup> edition (1985) of *Economics*, the first co-authored with Nordhaus. The chapter on economic development now opened with a section on “Population growth and development”, including subsections on “Malthus and the dismal science” and “Neo-Malthusianism”, followed by a discussion of the “Population explosion: the legacy of Malthus” (Samuelson and Nordhaus 2010, chapter 26). Some developing countries (e.g. Mexico, South Korea and Taiwan) had gone through demographic transitions, but not Central Africa and other parts.

Samuelson shared with Harrod (1948), Solow (1956) and some other growth theorists, but not with most development economists in the post-war period, the view that demographic issues were essential to economic development. Harrod (1948:19-20, 114) – who assumed away in his dynamic economics the classical law of diminishing returns from land and Malthus's population doctrine – would suggest that the “old classical analysis” applied better to vast “poverty-stricken areas of the world today”, where population “is pressuring upon the means of subsistence.” From that point of view, the economic development of poor countries remained largely outside the terms of reference of Harrod's dynamics.

Solow (1956: 90-91) indicated how, by making the rate of population growth an endogenous variable determined by income per capita, the neoclassical growth model is able to generate multiple equilibria and explain poverty traps. He did that by incorporating demographic transition into the model: population falls for very low levels of income; it begins to increase for higher levels, and decrease for still higher levels (see Boianovsky and Hoover 2014: 204). The stability of the low steady state in Solow's diagram characterizes it as a poverty trap. Such a poverty trap comes out, as Solow (*ibid*) pointed out, for purely demographic reasons, in the “total absence of indivisibilities or of increasing returns” of the kind stressed by Rosenstein-Rodan at

CENIS.<sup>19</sup> Leibenstein (1957) shared Solow's understanding of underdevelopment as the result of demographic multiple equilibria – instead of differences in “fundamentals” – but that view did not catch development economists' general attention (Buttrick 1958 was an exception). That may be partly explained by the fact that development economics did not use the same language of formal building Solow, Samuelson and others deployed (see Solow 1995: 664-65), as implied by Samuelson (1987b).

#### 4. Venturesome development economists

Samuelson's (1978b) critical appraisal – that the extensive post-war research effort in economic development was not a turning point in the knowledge about the subject – was partly influenced by recollections of his student days at Chicago and Harvard.

Any prewar student of Gottfried Haberler at Harvard University or of Frank Knight at the University of Chicago would, I believe, have been able to pass without much briefing the kind of examination in growth and development theory that was to become prevalent in the 1950s and 1960s. (Samuelson 1987b: 4)

He could have mentioned as well Chicago's Jacob Viner, who, like Haberler, would dissent from the view about the perverse effects of international trade on economic development that came to dominate post-war development economics (see Little 1982, chap. 4; Viner 1952; Haberler [1959] 1985). Still, Samuelson's claim that he had learned as a student the tools to understand the main concepts of post-war development economics is somewhat surprising.<sup>20</sup> Samuelson (1996: 1684-85) observed how Viner and Haberler traveled to Brazil and Argentina in the 1950s to

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<sup>19</sup> Hence, Solow did not seem to share Samuelson's (1964) opinion about the centrality of increasing returns to underdevelopment phenomena. For a growth model featuring both increasing returns and demographic transition see Jones and Vollrath (2013, chapter 8).

<sup>20</sup> For example, the notion and the empirical relevance of disguised unemployment, a concept Samuelson endorsed, were contentious issues in the 1950s, drawing criticism from Viner, Haberler and T. Schultz. Samuelson (1961: 783, n. 1) acknowledged Haberler's and Schultz's misgivings, but removed that note after the 1964 edition.

lecture on the positive effects of market discipline on economic development, in reaction to the then influential argument by Prebisch (1950) – and others like H. Singer and G. Myrdal –for import substitution industrialization. According to Samuelson (ibid), Haberler was vindicated by the Asian Tigers’ success with export promotion after the 1970s, as against Latin American import substitution strategy.

Policy issues were not discussed in the 1978 typescript, which evaluated development economics on purely theoretical grounds, without any mention of the economic performance or of policy issues in developing countries. Shortly after, Samuelson’s ([1979) 1983) reacted positively to the World Bank’s *Development Report 1978* (the first of a series of annual reports), with a Foreword by Robert McNamara, Bank president from 1968 to 1981. As put by Samuelson (ibid.), “I take off my hat to Robert McNamara for what has been his unique vision – his recognition that successful market forces alone can’t be relied on to mitigate flagrant inequality and abject poverty.” Samuelson (1984 [1986]: 498-499) at first resisted to studies indicating the superiority of market export promotion over import substitution planning and argued against drawing strong conclusions from “controlled experiments” that were not in fact controlled. Around that time, Samuelson (1980: 717, n. 2) acknowledged Bauer and Yamey’s (1957) “reasoned defense of the market as the main instrument for economic development”, but remained unconvinced.

Whereas Samuelson’s (1978b) target was the “first generation” of development economists (roughly 1950-75), his 2001 essay discussed aspects of the theoretical achievements of the “second generation” (1950-2000), an expression suggested by Meier (2001). With the exception of a brief plea for the “*limited mixed economy*” and a warning about the fragility of “populist democracy” (Samuelson 2001: 495, 503), Samuelson’s 2001 assessment refrained from dealing with policy issues. Indeed, he referred to growth economists only, without any mention of the new development economics’ overturn of the post-war claim that the analysis of underdeveloped countries entailed a separate branch of economics distinct from neoclassical theory (with its concern for the role of prices) as applied to rich countries (see Toye 2018 on the 1980-90s counterrevolution in development economics). Samuelson’s (2001) approach resulted from recognizing that the main new theoretical contributions for understanding economic development came from growth and trade economists who, since the 1980s, had tackled formally the analytic hurdles involved in modeling increasing returns and coordination failures that had eluded post-war



development economists (see Warsh 2006, chapter 19; Ray 2008; and 1998, chapter 5, and references cited therein). By then, the old distinction between growth and development economics, which went back to MIT in the 1950s, had become partly blurred.<sup>21</sup>

As Samuelson (2001: 498) pointed out, the study of constant returns to scale, “sans chance, change, externalities and information uncertainties” was a “finished book” in economics. Authors looking for breakthroughs stressed deviations from that pure model. Hence, increasing returns to scale had received occasional attention in connection with division of labor (Adam Smith), monopoly (A. Cournot, J.M. Clark), international trade (F. Graham, B. Ohlin, P. Krugman and E. Helpman), and “developmental growth” (A. Young), with only a limited degree of success due to the formal problems involved. Samuelson (2003: 2) was only too aware of the difficulty to move beyond “Santa Claus domains of convex sets and differentiable calculus of variations” into the “unpromising lands of increasing returns to scale, asymmetric information, lumpinesses, and all those other imperfections undreamed of in the philosophies of the equilibrium mongers.”<sup>22</sup>

Those were topics that appealed to what Samuelson (1978b) had described as “venturesome” economists who were willing to tackle, *inter alia*, the “intractable” problems of economic development. However, some of the issues pertaining to the analytical treatment of increasing returns became soluble (to use Medawar’s term) after Dixit and Stiglitz (1977) produced a tractable general equilibrium model of monopolistic competition with differentiated goods. Dixit and Stiglitz assumed a consumer with a taste for variety and a utility function with additive separability, which led to price elasticities of demand that are constant and independent of the number of goods.

New growth theory and new economic geography (put forward, among others, by Paul Romer, Paul Krugman and Elhanan Helpman in the 1980s and 1990s), as well as new Keynesian macroeconomics, have extensively used the Dixit-Stiglitz model as

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<sup>21</sup> As John Williamson (1990: 20, n. 5) – in the same book in which he announced the “Washington Consensus” – pointed out, it is ironic that Washington reached a position firmly in support of liberal market reforms just as growth theorists “rediscovered the old idea of externalities that underlay the development literature” and finally gave such notions as the “big push” etc. theoretical foundations.

<sup>22</sup> He made those remarks while praising Edmund Phelps, and also Stiglitz. Stiglitz’s contributions to economic development theory have focused on the role of imperfect information and the absence of markets (see Stiglitz’s 2002 Nobel Lecture).

a building block to tackle the seemingly intractable issues of monopolistic competition and increasing returns. The term “tractable” has been often appended to that model, which went significantly beyond Chamberlain’s 1930s formulation (see e.g. Warsh 2006: 185, 251-52). However, as acknowledged by Stiglitz (2017), that came with a cost, since the model’s tractability depended on its particular parametrizations, which raised questions about its robustness. Krugman (1993: 20) – who, like Dixt and Stiglitz, had studied with Samuelson at MIT – concluded as much in his remark that there are no “plausible tractable models of imperfect competition.” Tractable models such as Dixit-Diglitz always involve “some set of arbitrary assumptions”, which means that “in order to do development theory one must have the courage to be silly, writing down models that are implausible in the details in order to arrive at convincing higher-level insights” (ibid). From that perspective, postwar development economics (or “high development theory” as Krugman calls it) failed because it did not take the same jump.

As discussed above, it was only after reading Medewar (1969) that Samuelson (1969) realized the relevance of restrictive assumptions for enhancing the tractability of scientific hypotheses – even if one must bear in mind such peculiar assumptions while interpreting theoretical results, as in the case of the Ramsey model of capital accumulation. Nevertheless, Samuelson’s (2001: 494) discussion of Solow’s (1957) approach to measuring productivity growth (the Solow residual) indicates that he did not fully appreciate the point. Samuelson referred to Solow’s “heroic marginal productivity approximations” that factor prices are determined by their marginal productivities, which led “not by accident” to the result that the Cobb-Douglas function was a good fit. As put by Samuelson (ibid) somewhat surprisingly, Solow’s MIT office was next door to his but “with a Chinese Wall in between.” Samuelson neither shared Solow’s econometric research program nor was he involved in the elaboration of the 1956 neoclassical growth model with an aggregate production function.<sup>23</sup> Solow’s (1957) assumptions of perfect competition and constant returns to scale may be seen, however, as “empirical tractability assumptions” (Hindriks 2005) that make the measurement of productive growth tractable in view of unobservable variables such as marginal cost.

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<sup>23</sup> Samuelson and Solow co-authored articles and a book in the 1950s in which they dealt with optimal growth paths in theoretical models featuring heterogeneous capital goods.

Mark Blaug (1994: 132) and some other economic methodologists complained that the correspondence of economic models to the real world became increasingly “sacrificed for the sake of analytical tractability”, in the sense that the final aim was to solve theorems and academic exercises that are built “because they are soluble” technically, not because they illuminate observable behavior. However, more recently Uskali Mäki (2009: 81-82) has argued for the compatibility between ontological convictions about the real world and tractability conventions, taking development economics as an example. Postwar development economists lacked the tools for dealing with the feedback mechanisms caused by increasing returns and imperfect competition, which violated their ontological convictions. This situation eventually created a “disturbing tension” between tractability conventions and ontological convictions that led to constructing models that relaxed some of the relevant “tractability assumptions” as in new growth and trade theories based on the Dixit-Stiglitz model. The study of the development and application of the Dixit-Stiglitz model illustrates the interaction between factual beliefs and existing “research technology” (Mäki and Marchionni 2012: 94-95). This is consistent with Krugman’s (1993: 20) view of progress in economics as documented above. It is implicit in Samuelson’s view of the role of tractability in economics.<sup>24</sup>

Samuelson, however, remained skeptical of the economists’ ability to tame increasing returns and imperfect competition. Because imperfect competition was “relatively hard to analyze in an interesting way”, economists still tended to deal with increasing returns in connection with external economies. Moreover, the activity of scientific discovery probably featured decreasing returns to research effort (Samuelson 2001: 499; cf. Jones and Vollrath 2013, chap. 5). Samuelson (2001: 500-502) warned about “pseudo scale economies” and produced arithmetical examples of market failures caused by *external* increasing returns, without referring to Dixit and Stiglitz (1977). Increasing returns to scale continued to be “a bridesmaid but never a

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<sup>24</sup> Medawar (1982: 303-04) doubted the scientific status of economics partly because of the “reflexive” character of economic predictions – in the sense that they can affect the actual course of events (cf. Popper 1945) – and of the instability of economic conditions. It is not their “wrongness as much as their pretensions to rightness that have brought economic predictions and the theory that underlies them into well-deserved contempt”, he charged. Moreover, the “dogmatic self-assurance and asseverative confidence of economists” were seen as “self-defeating traits among people eager to pass for scientists.” It is implied that economics did not fit Medawar’s notion of science as the art of the soluble.

bride”, as it had been throughout the history of economics, Samuelson (2001: 499) claimed. He was in good position to appreciate the emphasis by new growth theory on the non-rivalry of knowledge input as a source of increasing returns, having stated long before that “knowledge is *not* an input such that the more you use of it, the less there is left” (Samuelson 1948a: 181-182; italics in original).

Surely, intractability problems were not restricted to development economics. Indeed, they affected Samuelson’s own 1939 multiplier-accelerator model, but for different reasons, as made clear in his 1972 Nobel Lecture. As most of Samuelson’s macroeconomics, that well-known model was not based on optimizing behavior (see Boianovsky 2019e). The fact that “the accelerator-multiplier *cannot be* related to maximizing takes its tool in terms of the intractability of the analysis”, Samuelson (1972: 258; italics in the original) observed under the impact of his reading of Medawar (1969). Few grand predictions emerged from that model, as there was an extensive range of possibilities of what could happen. Again, the problem was related to non-convexity. The stability conditions, along Samuelson’s “correspondence principle”, did not define a stability region in terms of the coefficient of the system that is a convex region. This problem does not arise in the case of a “well-behaved maximum system”, Samuelson (1972: 259) pointed out. However, that did not prevent Samuelson, throughout several editions of *Economics* and elsewhere, from treating the multiplier-accelerator as a workhorse model of endogenous business cycles.

Apart from exploring seemingly intractable territory, development economists may be labeled “venturesome” also in the sense that they often travel to underdeveloped areas in search of information or to give policy advice. This close contact with poor countries has been an important source of influence or inspiration, as illustrated by many economists who became attracted to economic development upon traveling abroad – e.g. Harry Johnson (Pakistan in the mid 1950s), Jan Tinbergen (India in the early 1950s), Hirschman (Colombia in the early 1950s), Stiglitz (Kenya in the late 1960s). On the other hand, the contrast with foreign economies and cultures brings a new perspective on one’s home country, as Samuelson (1968:1) remarked: “To know your country you must have travelled abroad.” However, with the exception of occasional visits to Japan (partly due to his friendship with Shigeto Tsuru from their Harvard student days), where he witnessed a process of socio-economic transformation that impressed him most, Samuelson did

not go abroad too often. It was only in 1980 that he visited Latin America (Mexico and Peru). He described himself as a “stick in the mud. I get countless invitations to go to China and India and so forth. And I like to stay at home with my lined pad” (Samuelson 2007). Nonetheless, if only from a geographical and theoretical distance, Samuelson kept a critical interest in economic development – which he characterized throughout several editions of his *Economics* in terms of multiple equilibria and poverty traps (see Samuelson 1961: 794-95; Samuelson and Nordhaus 2010: 530-31) – even if sometimes skeptical of development economics’ ability to deal with seemingly intractable problems.

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