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Paul Samuelson and Macroeconomics

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> Alvin Hansen led the revolt, with lumpen lecturers, assistants, and students in support. Harvard's giants – Haberler, Schumpeter, Leontief, and others – resisted the new paradigm, but *the golden age of macroeconomics* emerged from the ferment. (PAS 1988a: 32; italics added)

By macroeconomics I shall mean, often, macroeconomic dynamics or, simply, macrodynamics, in the light of Paul Samuelson's incredible productivity on practically all aspects of economic theory. The seven volumes of *The Collected Scientific Papers of Paul A. Samuelson* (1966–2011) contain a total of 7068 numbered pages and 597 chapters, not counting the xii + 447 pages of the *Foundations of Economic Analysis* (PAS 1947) and (even) the first (or third—where the much-maligned phrase neoclassical synthesis was coined) edition of the stupendously successful textbook on *Economics*!

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1 Introduction

> *More can be less* … But the fine garments *sometimes* achieved fit only by chopping off *some* real arms and legs … Easy victories over a science's wrong opponents are hollow victories – at least *almost* always. (PAS 1986a: 850; italics added)

I think this is what PAS[1] means (ibid.) when he refers to the "new" mathematical economics of the "theory of cones, polyhedral, and convex sets made possible". The qualified predicates—"can", "sometimes", "almost"—show PAS at his critical best; he is *almost* always[2] generous and measured in his evaluation of those who are, implicitly, critical of his chosen methodology.

Of the almost 95 years of his life, he wrote and rewrote about the macroeconomics of fluctuations and growth for 68 years of those, and also at least once every decade, since he began his scientific authorship. It was about deterministic, determined, fluctuations of aggregate income, consumption and investment, in the context of—mostly—discrete-, but also of continuous-time, fluctuations, within a growth framework, i.e. of growth cycles. The names of his elder, contemporaneous and younger colleagues, like Joseph Schumpeter, Maynard Keynes, Alvin Hansen, Roy Harrod, Ragnar Frisch, Michał Kalecki, Erik Lundberg and many others, dot these critical, appreciative and influential contributions by PAS.

On the words, "macroeconomic" and "macrodynamics", PAS (1986b: 858; italics added) writes, with disarming frankness:

> These papers [vol. 1 of the Japanese translation of *The Collected Scientific Papers of Paul A. Samuelson*] deal with what today we call *macroeconomics*—a surprisingly recent term. A dozen years ago, Dr. Edwin Nourse, President Truman's venerable first Chairman of the US Council of Economic Advisers, wrote to Professor Alvin Hansen of Harvard, asking: "Who invented the word '*macroeconomics*'?" Hansen wrote back: "I don't know. *Probably Samuelson.* I'll ask him"… I must answer for much. But *I don't believe it was I*

[1] I shall almost always refer to Samuelson as PAS in this chapter.

[2] With notable exceptions (especially those economists, and others, who are more or less contemporaneous)—see Samuelson (1983, 1986b: 858–859).

> *who coined the now-standard word* … [Frisch], Tinbergen, and Kalecki used the expression "*macrodynamic*" and had not yet contrasted *microeconomics* and *macroeconomics*".[3]

These modest pages contain the consideration—both economic and mathematical—of the analysis and extension of PAS's fundamental contributions of 1939, as it changed and evolved, during the period from 1939 to 2007, via his observations about Frisch's rocking horse methodology, allegedly following Wicksell (1907).[4] In the process, PAS aligned himself firmly as a Keynesian in the US and (eventual) Canadian traditions (see Timlin 1949)—very different from the (Cambridge) UK tradition of Kahn and Robinson. Although inspired by Alvin Hansen, it must be said that it was not an exclusively North American tradition—at least Pigou, Robertson, Hicks, Meade, Harrod and Lange[5] also interpreted Keynes's multiplier analysis of short-run equilibrium macroeconomics in a way that was congenial to PAS.

Hansen inspired PAS not only to become a Keynesian macroeconomist, but also decisively in the way he considered equilibrium fluctuations against a backdrop of (predicted) secular stagnation—i.e. as the obverse of growth—contexts (as Nikaido 1987 emphasizes).[6] Of course, Hansen (1939) was more influential than Harrod (1939) for PAS. The former was true to Schumpeter and Wicksell, and therefore the idea of "No Growth,

[3]Although Frisch did also use the word macroeconomics, it was in an internal university memorandum. Lindahl (1939: 52) did use the word macroeconomics, in contrast with microeconomics (cf. PAS 1997: 157).

[4]Samuelson (1939a, b, 1974, 1988b, 2005). Wicksell (ibid.) does not refer to any rocking horse.

[5]However, the economics of the Stockholm School, led by the neo-Wicksellians (PAS (1952a) [1966]: 591), Lindahl, Myrdal, Hammarskjöld and Lundberg, was a combination of short-run and long-run analysis, with disequilibrium and equilibrium dynamics, respectively. PAS (1959: 183) acknowledged that the American Keynesians should take seriously the disequilibrium, model-sequences, general numerical, approach of Lundberg. Hansen adopted this numerical model-sequences approach because he did not know the elements of formal dynamics; Lundberg did so (partly) because he was not sure that the postulated relationships (between aggregates) can be assumed to be invariant. The formal dynamics of Lundberg's (and Lindahl's) numerical model sequences are best analysed in terms of general nonlinear dynamics (and structural stability).

[6]See, in particular, the chapters by Higgins and Goodwin in the Alvin Hansen Festschrift (Metzler et al. 1948a); PAS corresponded with his lifelong friend, Richard Goodwin, endorsing wholeheartedly this particular contribution (he—PAS—was one of the effective editors of the Festschrift, for his mentor and friend, but not, in any formal way, his teacher, Alvin Hansen).

No Fluctuations" (the title of Nikaido's unfortunately neglected paper); Harrod was trying to separate growth from cycles (Harrod 1936) despite the importance given to the relation, i.e. acceleration, in it.

In Sect. 2, the themes at the heart of PAS's contribution to economics, in general, and macroeconomics and macrodynamics, in particular, and ignored in this paper—and some of the reasons for ignoring them—are developed, no doubt inadequately. Section 3 is an examination of the background methodological discussion of dynamics in *Foundations of Economic Analysis*, its allied mathematics (and logic) and the way it (may have) influenced PAS's vision of macroeconomics. Several topics, all of them now part of the vocabulary of the macroeconomist, delineate this section, but all in the context of macroeconomics and macrodynamics. The different sections are, inevitably, uneven—perhaps not in quality, but in the pages involved and the underlying theoretical basis (the latter, perhaps, a reflection of my own narrow focus; after all, not all can be as universally competent in economics and classical mathematics[7] as PAS!).

Section 4 is on the macroeconomics of fluctuations and growth, within the context of PAS's vision of the interaction between the multiplier and the acceleration principle, as the mathematics of dynamical systems itself developed. The first part is written with a historical flavour, macrodynamically. PAS himself kept updating his knowledge of the changing horizons of dynamics, but somewhat classically. In the second part of Sect. 4, a more extensive and technical analysis of the Hansen–Samuelson multiplier-accelerator model(s) of 1939, as it became the Keynes–Hansen–Samuelson multiplier-accelerator model of secular stagnation, is the focus. I do not believe, even in this extension, PAS went beyond Nikaido (op. cit.); but PAS (1988b) is a splendid exercise in cyclical models with, first, a strict accelerator, and later, with a flexible accelerator,[8] leading to a stable (from the inside) limit cycle in phase space. However, in

[7] I should add modern physics, encompassing both classical and quantum aspects of the subject.

[8] Even though the paper, as a whole, brackets Goodwin, Hicks and Kaldor as simultaneous sources for nonlinear dynamical modelling of aggregate fluctuations, seeking limit cycle(s), this particular section is a handsome tribute to his friend, Richard Goodwin (see PAS 1988b: Section 2, p. 5 and Section 6, pp. 11–14).

PAS (1990) he had shown admirable familiarity with mathematical dynamics more intricate than limit cycles.[9]

In Sect. 5, I try to draw some threads together of the mathematical economics of this universal scientist. Of course, the limitations of my knowledge and ability in economic and mathematical analysis limits the scope and nature of what I can competently discuss the astonishing prowess and productivity of PAS. It is, therefore, nothing more than an outline—it cannot be anything else!

2 Some of the *Instruments* that *I* Don't *Sound*

> My soul is a hidden orchestra; I know not what instruments, what fiddlestrings and harps, drums and tambours I sound and clash inside myself. *All I hear is the symphony.* (Fernando Pessoa, *The Book of Disquiet*, 1930: 8; italics added)

A caveat on one of the (many) important "roads not taken" (pace Robert Frost), on PAS's monumental contributions even to the core of macroeconomics (and monetary theory) and its concepts and (mathematical) tools must be mentioned. I do not touch on the rich monetary macrodynamics of the famous overlapping generations, consumption loan model. As PAS (1986b: 860; italics added) himself acknowledged: "I took pride in the fact that my 1958 'Exact-Consumption Loan Model' had *immeasurably more depth and sophistication*…"[10] There are many expository and fundamental contributions to PAS's 1958 paper. I myself have found Blanchard and

[9] Although not existence (or uniqueness) proofs of dynamical systems, of any order from algorithmic or constructive mathematical viewpoints. This is what I mean by his focus on coming to terms with mathematical developments classically (see Hales [2014] and my comments on Flood [1950] in the next section).

[10] On this paper, Samuelson observes, in addition (ibid.: 861; italics added): "You might think that a scientist's best papers receive instant recognition and approbation. Actually, some of mine that have come to gain most renown were *first refused by editors and referees* … ([T]he *1958 paper* mentioned above encountered *rough weather* from the *Journal of Political Economy*)". It is very similar to what Lucas experienced with his "Expectations and the Neutrality of Money", a foundational paper on new classical macroeconomics, at the hands of editors and referees (at the *American Economic Review*;

Fischer (1989, Chapter 3), Solow (2006) and Weil (2008) most illuminative—but there is the proverbial *L'Embarras des richesses* on this exceptional model. It leads, after Gale (1972), via the work of Day (1983)[11] and others, to a utilization of the mathematical tools of the frontiers of nonlinear dynamics,[12] to elucidate economics, in general, and macrodynamics, in particular. PAS did not take part in this adventure—perhaps a reflection of his fundamental philosophy, expressed in PAS (1959: 183): "Scientific theories are like children in that they have a life of their own. But, unlike children, they may have more than one father".

That it forms the foundation for the underpinning of the lack of the familiar welfare properties—particularly that of the first fundamental theorem of welfare economics, quite independent of the usual market-failure reasons of a competitive Arrow–Debreu economy—in a multiplier-accelerator model is, in my opinion, the reason for Samuelson adopting non-maximum systems for macrodynamic analysis (see below).

It is, however, woefully inadequate to only acknowledge, here, the solid and detailed work of Cord (2009: especially pp. 115–116) and Clower (1996: in particular pp. 42, ff),[13] on the next two topics, namely IS-LM and the Keynesian cross, and their important role in orthodox—i.e. American—textbook Keynesianism. My inadequate excuse for this is the lame one of blaming my personal interest in macrodynamics rather than orthodox macroeconomics.

Given this lame excuse, the other "instrument I don't sound", apart from the ramifications of the overlapping generations, consumption loan

see Gans and Shepherd 1994: 172), before it was, finally published in the *Journal of Economic Theory* in 1972. By the way 'Loan' is spelled 'Long' in the original of the quote above!

[11] I have chosen a late article by Day, who developed the ideas in Gale, Samuelson and nonlinear dynamics, from at least the early 1970s; in fact, Gale (ibid.) was published in a book (jointly) edited by Day.

[12] Barnett (2004: 538; italics added) speculates that PAS may not have mastered, or contributed to, "the recent literature on complex unstable nonlinear dynamics", but does cover himself by adding: "But I would not be surprised, if [PAS] were to *correct* those speculations as misperceptions, if I were to ask". Samuelson may cite PAS (1990), if asked, "to correct these…misperceptions". I had a minor role in getting PAS to write about complex unstable nonlinear dynamics, which was not difficult for someone like him who had heard the unstable nonlinear dynamical symphony played by Henri Poincaré, George Birkhoff, Alfred Lotka and Andrej Kolmogorov.

[13] Say's law—supply creates its own demand—is fruitfully, and in the context of PAS's stance on the Keynesian cross contrasted with Hansen's law—demand creates its own supply (at least in equilibrium states), and brilliantly espoused in Clower (ibid.: 44).

model, in the context of macroeconomics, leading to macrodynamics, is the framework of the neoclassical synthesis, encompassing the Hicks–Hansen (IS-LM) model,[14] augmented by the Samuelson–Solow work on the Phillips curve(s).

It was in the third edition of his famous textbook that a synthesis between Keynesian macroeconomics and Walrasian[15] microeconomics, called neoclassical[16] economics, was identified by PAS (1955: 282; italics added)

> In recent years, 90 per cent of American economists have stopped being "Keynesian economists" or "anti-Keynesian economists". Instead, they have worked towards a *synthesis* of whatever is valuable in older economics and in modern theories of income determination. The result might be called "*neoclassical economics*" and is accepted in its broad outlines by all but about 5 per cent of extreme left-wing and right-wing writers.

In Samuelson and Scott (1968: 226; italics added), the following is added to the paragraph above: "Modern economists are '*post-Keynesians*,' keen to render obsolete any theories that cannot meet the test of experience and applicability".

During a conversation with Robert Clower in the early 1970s, PAS confessed that the term "neoclassical synthesis" was coined primarily to get "McCarthy off my back"! The phrase did not signify anything dynamic, in particular, macrodynamic (at least according to Samuelson).

Finally, on the Keynesian cross, a staple of introductory texts on (neoclassical) macroeconomics (see Cord 2009: 110), PAS (1986b: 858; italics added) felt that he had to: "*[P]lead guilty to* when I approach St. Peter's Gate to Heaven—if it is a crime—*first devising the 45°-line diagrams* in which C + I Keynesian *schedules* of consumption-plus-investment intersect with the 45°-line to determine Keynes's simplest 'multiplier' model of (unemployment) equilibrium determination".

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[14] Hicks, in his classic 1937 article labels IS-LM as SI-LL curves. In 1987, in Aalborg, I asked Hicks whether it stood for SILLY curves? His answer was to hide behind chuckles—and a pull of the pipe! Perhaps this is the reason for PAS referring to Hicks—after paying due respect—as an "egoist" (PAS 1998: 1381).

[15] With hindsight—it leads, via Clower, to the microfoundations of macroeconomics movement.

[16] The term neoclassical, hyphenated as neo-classical, was coined by Veblen (1900: 261).

If I had been literal in my interpretation of the static underpinnings of the Keynesian cross, as it became the core of the Hansen–Samuelson version of American Keynesianism, I would rely on the powerful critique of Clower (op. cit.) and dismiss this as an exercise in macroeconomics without macrodynamic implications. However, the above quote by Samuelson, buttressed by Fig. 1, on page 790, of PAS (1939b), provides an explicit way to understand the connection between the static and the dynamic in the Correspondence principle.[17]

To this, and other issues of a methodological[18] nature in macrodynamics, I now turn.

3 Mathematics, Logic, Proof, and *Foundations of Economic Analysis*

> On the title page of my *Foundations of Economic Analysis*, I quoted the only speech that the great Willard Gibbs was supposed ever to have made before the Yale Faculty … Gibbs, who was not a loquacious man, got up and made a four-word speech: 'Mathematics is ***a*** language' … I wish he had made it 25 per cent shorter – so as to read as follows: '***Mathematics is language***' … I mean this entirely literally … For ***in deepest logic***…the two media ***are strictly identical***. (PAS 1952b: 56; bold and italics added)

Neither Wittgenstein, representing, perhaps, the philosophy of mathematics, nor Brouwer, from a foundations of mathematics interpretation, would have agreed with this Samuelsonian viewpoint of the identity of logic and language. Brouwer, in particular, would not have agreed at all with Flood (1950: 267; italics added) that PAS (1947: 116, fn. 18) sketches "a *constructive proof* "; as a matter of fact, PAS's proof is not constructive in any sense, not just from the point of view of Brouwer's intuitionistic

[17] In this context, see Cord (ibid.: 116, fn. 53) on Swan's 1945 introduction of the AD/AS model.

[18] In the sense of Metzler et al. (1948b: 905; italics in original): "[*Foundations*], as its title indicates, is a study of the *foundations* of economic method, but it is by no means a book on methodology in the customary sense. It is methodological only in the sense that the author is more interested in illustrating a means of solving economic problems than in developing a complete and self-contained theory of the working of the economic system".

constructive proof. Samuelson's (mathematical) logic, mathematics and the proofs of his theorems—both mathematical and economic (broadly conceived)—were always classical.[19] However, it is necessary to point out that the proof of existence of general equilibrium in Walras and Pareto is mathematically constructive; the latter is for an analogical computable model, while the former could be for a digital or analogue model.

No competent mathematician (or physicist)—classical or not—(or even Samuelson) would agree with the misleading sketch of mathematics (and, by implication, mathematical physics) in Stigler (1948). Stigler's strictures on stability, differential and difference equations display his monumental ignorance of the distinction between linear and nonlinear systems (among other things of a similar nature, e.g., nonstationarity).[20] Stigler's ignorance of the homilies in Boulding (1948: 189, fn. 5) is fairly clear from the following two quotations: "It is the central task of…chapter [IX] to show how the problem of *stability* of equilibrium is intimately tied up with the problem of deriving fruitful theorems in comparative statics. This *duality* constitutes what I have called the *correspondence principle*" (PAS 1947: 258; italics added)[21] and "How many times has the reader seen an egg standing upon its end? From a formal point of view it is often convenient to consider the stability of *nonstationary* motions" (ibid.: 5; italics added). Metzler (1948b: 906; italics added), in a brilliant review of *Foundations*, refers to these quotes as, "[T]he part dealing with the *stability* of *a dynamic system* [being] its most novel feature and is perhaps *Samuelson's greatest contribution to economics*".

[19] Warts and all! PAS, himself, is on the record as acknowledging the mathematical mistakes in *Foundations* as well as confessing (PAS 1998: 1378; italics added): "Even the *book's mistakes generated a history*" and "[A] busy author who had no relish for proofreading *complicated mathematics*". Allen (1949) and Savage (1948) catalogue lists of the mistakes in *Foundations* illuminatingly. Incidentally, PAS writes, incorrectly, George Birkhoff, when he means, of course, Garrett Birkhoff (see PAS ibid.: 1377).

[20] Obviously, Stigler's ignorance of Boulding (1948: 189) and Hart (1948) does not embarrass him in the least. Hart (ibid.: 912; italics added), points out: "Broadly speaking, Samuelson is *not* out to discredit anybody". See also p. 911 of this splendid review of the classic PAS (1948), on the connection between the relentlessly (classical) mathematical and logical approach of *Foundations* and the predominantly (classical) geometric and verbal rendering of economic propositions in PAS (ibid.).

[21] Niels Bohr had defined a Correspondence principle, linking quantum and classical physics, at least as early as 1922 (see Velupillai 1973).

There were many competent, sympathetically critical and enlightening reviews of *Foundations*, by well-known economists, mathematical economists and statisticians, Allen (op. cit.), Baumol (1949), Carter (1950), Metzler (ibid.), Savage (op. cit.) and Tintner (1948) in addition to the dubious ones by Flood (op. cit.) and Stigler (op. cit.)[22]—all of them in leading journals. Savage (ibid.), in particular, concentrates on the mathematics of *Foundations* and observes, correctly in my view, that "In this book…[*mathematics*] is almost exclusively employed to *deduce qualitative* conclusions from *qualitative* assumptions" (ibid.: 201; italics added). This is most evident in PAS's propositions, conclusions and possible generalizations regarding the stability of dynamical systems, in a qualitative sense:

> [I]n the absence of *precise* quantitative data [the economist] must infer analytically the *qualitative* direction of movement of *a complex system*. What little success [the economist] has hitherto achieved can be classified *in large part* under two headings: (1) theorems proceeding from the assumption of *maximizing behaviour* on the part of firms or individuals and (2) *stability conditions* relating to the interaction between economic units. (PAS 1947: 258; italics added)

PAS was never tired of emphasizing the importance of initial conditions in the stability of generalized—i.e. even nonlinear—dynamical systems. Thus, implicitly, the future—and past, buried in the works of Poincaré and Cartwright-Littlewood—notions of stability, qualitatively conceived, of nonlinear dynamical systems, sensitive to initial conditions.

Figure 1, adapted from Ekeland (1988: 74), shows much—not all—of PAS's concept of stability and *SDIC*[23] of nonlinear dynamical systems. In view of *SDIC*, the (nonlinear) dynamical system, initialized at **P**, can lead to two trajectories that, in the *long-run*, diverge from each other (like the thick blue curve and the dotted curve). However, this dynamical system will also have trajectories (like the thin curve), although respecting *SDIC*,

[22] The reviews by Tintner and Stigler are in the same journal, but different numbers. It may be interesting, in this context, to mention that Hart (op. cit.) and Metzler (ibid.) are in the same issue of the *American Economic Review*—the former succeeds the latter!

[23] *SDIC*: Sensitive Dependence on Initial Conditions.

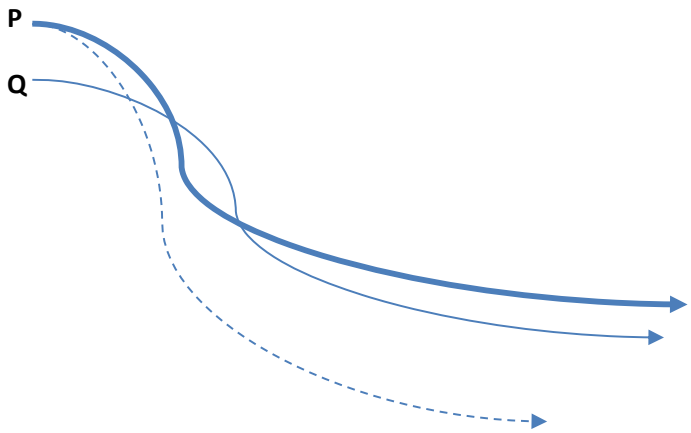

Fig. 1 Instability and approximate stability

will, in the long-run, be approximately (depending on the *quantitative* definition used) close to the thick curve. In other words, even though starting from an initial condition slightly different from **P**, say **Q**, it will, in a sense mathematically[24] definable, stay approximately close to the thick curve, starting from **Q**. One loses the possibility of exact short-term prediction of individual trajectories but gains long-term prediction feasibilities of a *whole* system of trajectories.

The notions of instability and approximate stability for dynamical systems subject to *SDIC*, summarized in the dynamics shown in Fig. 1, are based on Anosov's results of 1951 (which were inspired by Stephen Smale's invited lecture at Kiev in 1961). This is a perfectly appropriate way to honour Samuelson (cf., PAS 1986c). Thus, combining what Metzler (ibid.) calls the "greatest contribution to economics"—i.e. stability of a dynamic system—with Savage's characterization of the qualitative analysis of mathematical systems, one gets the above figurative and explanatory definitions implied in PAS's notions of the duality between stability and meaningful theorems—i.e. the Correspondence principle—as valid, but in terms of long-term feasibility of accurate (interval) predictions.

[24] Classically, constructively, infinitesimally, non-standardly or whatever mathematical framework one desires to use!

Perhaps PAS erred in not being specific about the time dimension of the dynamical system's predictability; but that does not mean his concepts are mathematically evanescent. PAS did not, however, err in the vistas of mathematical analysis that its future posed; he was fully aware of his own human short-term possibilities and the long-term possibilities of conceptual developments in mathematics. These are particularly evident in chapters 367 and 368 in PAS (1986b, 1986c), but it is also evident that he was constrained by the Gibbsian and (for want of a better name) Whitehead-Russell views of mathematics and logic, and the identity of the latter, by way of the use of classical mathematics, with language.

This did not entail a development of formal language theory, in the Chomskian sense, towards the mathematics of computability theory; nor did the role of theorems and proofs in the kind of mathematics he used in economics, lead to ideas on the alternative concepts of proof—or even the kind of mathematics he can use, in formalizing economics, beyond a future which, for example, emphasized Bourbakism—i.e. topology (mainly its combinatorial variety, leading to a specific kind of fixed-point theorem), group theory, manifold theory, etc. and non-standard analysis of the Abraham Robinson variety. He remained in the time warp that Bourbakism was in logic (see Matthias 1992). PAS did not foresee developments, at least, in the varieties of proof[25] (cf. Abramsky 2015).

I do not think this invalidates the mathematical and logical framework within which PAS formalizes dynamic economic concepts. On the other hand, I agree with Mas-Colell (1985) and Balasko (2009) that PAS (1947) is a culmination of a research programme in the application of calculus, from Cournot, via Marshall, Walras, Fisher, Pareto, Edgeworth and Hicks. It is a culmination with a continuation—say via a mastery of Spivak (1965) and Abraham and Marsden (1978)—with a resurrection of the fertility of calculus in formalizing economic concepts at the frontiers. This is, surely, what PAS (1986a: especially p. 849) means by *Newtonian Paradise Regained*.

The supreme knowledge PAS had in the mathematics of dynamical systems, of the time, and their applications in the foundational development

[25] Clower (op. cit., p. 5; italics added) is surely incorrect to state that: "If the argument is valid, *one proof should suffice*".

of economics, when *Foundations* was published,[26] became part of the core of analytical and mathematical macrodynamics for at least the proverbial Golden Age of (American) Keynesianism. Macroeconomics after *Foundations* became a fundamentally analytical subject.

4 The *Inexactness* of Macrodynamic Multiplier-Accelerator (M-A) Models

> In any sufficiently rich system
> including the present mire
> statements are possible
> which can neither be proved
> nor refuted within the system.
>
> *Those are the statements*
> *to grasp, and pull!*
>
> —(*Homage to Gödel* by Hans Magnus Enzensberger, translated from the original German by Enzensberger; italics added)

This first part of this section is partly historical, in which I deal with four aspects: the role of Harrod in dynamics, in general, and in M-A models, in particular; the Frischian framework for macrodynamics; the roles of Keynes and Hansen in making the M-A formalization a growth-cycle model; and the distinction between discrete- and continuous-time M-A models. The latter two parts are technical.

Let be begin with the declaration of what I call *inexactness*[27] in the macrodynamics of the M-A model by Samuelson in his Nobel Prize Lecture (PAS 1972: 258–259; italics added):

> My point in bringing up the *accelerator-multiplier* here is that it provides a typical example of *a dynamic system* that can in *no useful sense* be related to *a maximum problem* … The fact that *the accelerator-multiplier cannot be related to maximizing* takes its toll in terms of the intractability of the

[26] See Samuelson (1998).

[27] I use the word inexactness in contrast to the way it is used in PAS (1958); hence, Enzensberger's '*statements to grasp, and pull*', against the backdrop of Gödel's (second) incompleteness theorem.

> analysis … [P]erhaps the hardest part of my 1947 *Foundations of Economic Analysis* had to deal with the statics and dynamics of nonmaximum systems.

As both Goodwin—in private correspondence with me—and Samuelson (PAS: 1939b: 795) emphasized, Roy Harrod's intuition was far ahead of his (technical abilities for) "reasoned conclusions".[28] His notions of dynamics never went beyond that which one obtains from classical mechanics (see, e.g., Harrod 1939: 14, fn. 1; 1937). Harrod did not have *any* understanding—either conceptual or technical (as he acknowledged to Tinbergen in Harrod ibid.)—of the nonlinear dynamics of Poincaré, van der Pol, Birkhoff, Levinson or von Kármán, or even Lotka, Volterra and Kolmogorov. Samuelson, through PAS (1947, 1967, 1971), makes clear that he is fully aware of the classical mathematical framework of the nonlinear dynamics of the above eight. I would even venture to say that Harrod's understanding of, and distinction between, statics and dynamics did not transcend that which was in the contemporary textbooks on these subjects by A. S. Ramsey.[29]

So, it is with surprise and bewilderment I read Heertje and Heemeijer (2002),[30] their reflections on PAS (1939a)[31] and propositions on Harrod. There are many inaccuracies in their paper, but I will point out only the glaring ones:

1. The (alleged) PAS M-A model they describe with the three equations and the fourth equilibrium relation, on p. 209, is *not* a differential model; Samuelson's reply (in his 87th year!), PAS (2002: 221), makes this very clear;
2. Obviously, these authors are *not* aware of Tinbergen (1937: especially p. 90), where it is shown that Harrod's model of the cycle in

[28] Quoted in Heertje and Heemeijer (2002: 214), but without a page source!

[29] A. S. Ramsey was the father of Frank Ramsey. His textbooks on statics and dynamics were those from which I was taught these subjects in high school in 'old' Colombo, now over fifty-five years ago.

[30] It is only because of PAS (2002) that I discuss here their paper. There are many other articles by Harrod (1936, 1937, 1939, 1948) that are equally worthy of comment, but Samuelson's role in them is minimal or non-existent (to the best of my knowledge).

[31] Or PAS (1939a).

> Harrod (1936), due to it being a (linear) first-order differential equation, cannot give rise to oscillations.

There are other—and many—infelicities in this paper, but let that pass.[32]

As for the Frischian framework for macrodynamics, in PAS (1974: 10; italics added) Samuelson confesses:

> In leaving Frisch's work of the 1930s on stochastic difference, differential and other functional equations, let me point out that a great man's work can, in its impact on lesser men, have bad as well good effects. Thus, by 1940, *Metzler and I* as graduate students at Harvard fell into the dogma…that all economic business-cycle models should have *damped* roots. *We* accepted Frisch's criticism of the Kalecki procedure of imposing constraints on his parameter-estimating equations so that roots would be neither damped nor undamped; to explain Kalecki's supposed constancy-of-amplitude-of-capitalism's-fluctuations, *Frisch's mechanism of exogenous shocks* seemed preferable.

Fourteen years later, in PAS (1988b: 17, fn. 2),[33] it became:

> I, and…Lloyd Metzler…took it more or less as a dogma that our dynamic systems should be 'stable', in the sense of having damped rather than anti-damped characteristic roots … [F]rom 1937 on, I rejected the multiplier-accelerator explosive exponentials that kept thrusting themselves at me in my research notebooks. My effect on Hansen in this regard was baneful.

Samuelson, in PAS (ibid.: especially Section 6), gave up the dogma; this may well be regarded as an atonement for the baneful effect on Hansen's stubborn refusal to accept the dogma, Frisch notwithstanding!

The vacuous nature of Frisch's mechanism of exogenous shocks generating observed oscillations, for the chosen values of the parameters and

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[32]The authors use "none" and "all", in conjunction with "others", and given that the relevant time specified by them includes Kalecki (and the early Tinbergen of the ship-building cycle), makes their assertion on external shocks inaccurate (even Frisch's well-known—for all the wrong reasons—model in the Cassel Festschrift, does not oscillate, even with external shocks, as Zambelli (2007) has shown convincingly.

[33]Interestingly, in Section 9, titled, '*The road not taken*'!

initial conditions, has been amply demonstrated by Zambelli (2007).[34] However, the caveat(s) are cogently provided by PAS (2005),[35] in which, in the capacity of a generous referee at age 90, Samuelson wrote (bold and italics added): "More important at this date would be to show that *no* such [Frisch Mechanism] can ***exist***. (*That* is neither true ***nor*** I guess ***claimed by the author to be true***)".

The proof[36] of non-existence of such a Frisch Mechanism is impossible—unless the dynamical stochastic processes that it can give rise to, for given characterization of the mechanism, is well-defined (or definable). For now, such a definition of the entirety of possible stochastic processes that a Frisch Mechanism can generate is impossible.[37] It can be stated as a theorem, in honour of Samuelson, as follows:

Samuelson's Frisch Mechanism Theorem A Frisch Mechanism for the generation of a stochastic process, characterizing the macrodynamics of M-A models, is impossible.

Proof A Frisch Mechanism for the generation of any stochastic process is formally equivalent to an Oracle Turing Machine. It is then straightforward to derive a result on the non-deterministic halting problem for an Oracle Turing Machine (see also footnotes 40 and 41).

Remark The proof, as it stands, is incomplete for at least four reasons:

i. Where, and why, it is non-constructive, is not specified;
ii. The notion of Oracle Computations, and its relation with Turing Reducibility is not developed;
iii. The way non-determinism encapsulates stochastic and probabilistic formulations of sequences needs to be exactly defined; and

[34] As pointed out in fn. 32 above.

[35] I am greatly indebted to Professor Zambelli for providing me access to this letter/referee's report and also for giving me full permission to quote from it.

[36] I cannot envisage a constructive impossibility proof of this sort!

[37] This is akin to the proof of the halting problem for Turing Machines, based on the definability of algorithms, encapsulated by, for example, Hilbert's tenth problem (see Hilbert 1900: 21).

iv. The observation by Maury Osborne regarding nonstationarity and the way it relates to any kind of scientific computation of real sequences, has to be considered.

Detailed consideration of these issues will take us beyond the stunted Frisch Mechanism and Samuelson's own interests. Moreover, the proportion of pages of the overall chapter to the size of the full proof may not be useful.[38]

Samuelson, again in PAS (2005), delineates three possibilities for macrodynamics:

A. The M-A model(s) of PAS (1939a, b) in terms of damped linear difference equations;
B. Kaleckian (early) dynamics based on coefficient values that lead to centre-type dynamics;
C. The single limit cycle model (essentially of Goodwin 1951).

Figures 2 and 3, summarize the macrodynamics of (i) and (ii) above; (iii) cannot, even for a single limit cycle, be summarized in a figure in the coefficients plane. The general distinction to be respected, and observed, is that between (some kind of) linearity and nonlinearity. Moreover, the single limit cycle (in, say, Goodwin (ibid.: 15, Fig. 9) is entirely due to a truncated Taylor series approximation of a mixed difference-differential equation[39]; higher-order approximations of Taylor series expansions lead to a multitude of cycles, not necessarily finite (see Strotz et al. 1953).

Before I return to PAS (1988b), and the generalized Keynes–Hansen–Samuelson model—Samuelson calls it the *KHS model*—which is supposed to integrate Keynes (1937) and Hansen (1939) with the single limit cycle of the Goodwin (1951) model of the business cycle, I would like to point out the economic dubiousness of a (linear) discrete-time M-A model. In Goodwin (1989: 250–251; italics added), he wonders:

[38]These will be discussed, and elaborated, elsewhere.

[39]Of Eq. (5c), on p. 12, of Goodwin (ibid.), [y: income (real); O_A: "sum of the autonomous outlays β and l"; θ : "one half the construction time of new equipment"; $d\varphi(\dot{y})/d\dot{y}$: "is the acceleration coefficient". This gives: $\epsilon\ddot{y}(t+\theta)+(1-\alpha)y(t+\theta)=O_A(t+\theta)+\varphi[\dot{y}(t)]$

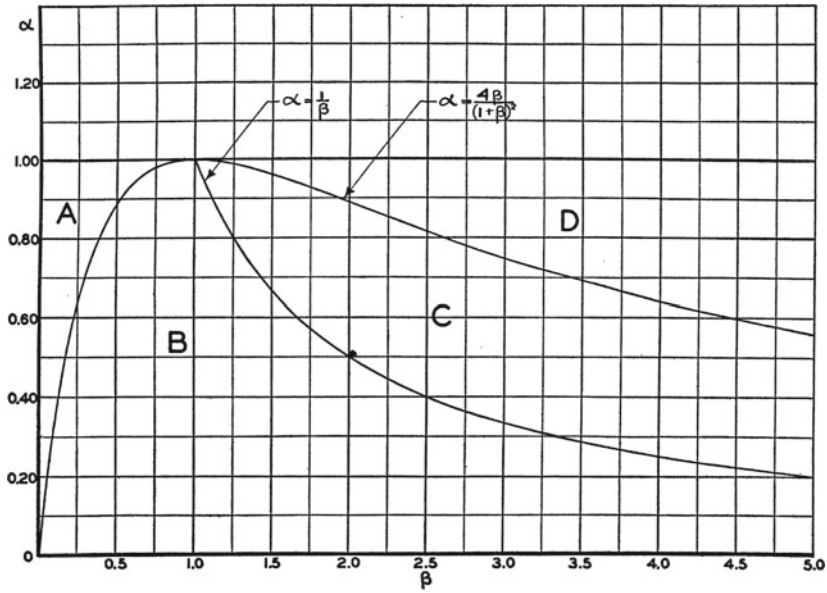

Fig. 2 Stability diagram for a linear difference equation (*Source* Samuelson [1939a: 78])

The problem with *difference equations* is: *What do we mean by them*? Is time a continuous or a discrete variable? *Does one assume that nothing happens between t and t* + *1*? This is grossly unrealistic, but the alternative involved in a finite difference with continuous time, means horrendous difficulties. I find it acceptable *only if* we regard such *aggregative macromodels* not as realism, but as illustrative of the nature of the problem and indicative of possible *solution types*. With that proviso, we are then dealing with a discrete time dynamic model. It has in recent time become known, what was unsuspected in the great number of 'period' analyses, that frightful problems arise even with the simplest of such models. Such an endogenous, completely deterministic model…can give rise to highly erratic, totally unpredictable behaviour. [I]ts solution may *depend on initial conditions*; it can bifurcate from oscillatory to monotone behaviour and then bifurcate back again as also *from stability to instability*. This even in the absence of exogenous shocks, the solution can be erratic and quite unpredictable.

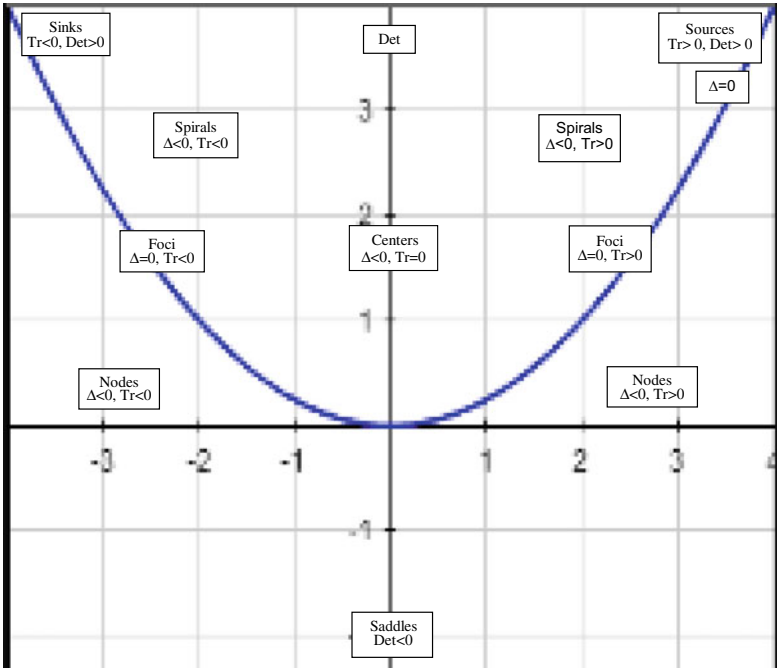

Fig. 3 Coefficients space of the phase portrait of a linearized nonlinear dynamics (*Source* Based on Hirsch and Smale [1974: 96])

Four Samuelsonian remarks, based on the above quote, may well be in order:

I. In a sense, the first part of this observation is a vindication of Harrod's disdain for lags and his relentless emphasis on differential equations, for the modelling of cycles and growth (I do not think he ever succeeded in integrating the two into a growth cycle).
II. The very last sentence of the Goodwin quote is the substance of what I have called Samuelson's Frisch Mechanism Theorem.
III. The substance of the penultimate sentence is that which was depicted in Fig. 1 and explained below it.
IV. Aggregative macro(dynamic) models, studied as solution types for illustrative problems is what was practiced by Samuelson—following Lundberg (1937: Chapter 8)—as studies of model sequences. He

> was lucky that the model sequences were for M–A models of linear difference equations.

In PAS (1988b: 8; italics in original), he makes the point that[40]: "[A]s Tinbergen warned Harrod in 1937, without time lags a *crude* multiplier-accelerator model can generate no oscillations but rather only exponential trends".

It is clear that by "crude" Samuelson means that Tinbergen's formalization of Harrod's system of the M-A model in Harrod (1936) is a first-order ordinary differential equation; adding time lags to the crude model makes it a linear difference-differential equation of appropriate order, i.e. depending on the nature and extent of the lags. The linearity makes every aspect of its dynamics (almost) completely characterizable (particularly if the lags are finite or its effects, if infinite, converge monotonely). On the other hand, referring to the KHS model, Samuelson observes (PAS: ibid.; italics in original): "For the KHS model it…*matters little* whether I use *differential equations* or *finite-lag difference equations*".

Whatever it is that "matters little for the KHS model", this proposition is true only if the differential equations or the finite-lag difference equations are linear. It is not true for the KHS–Goodwin M-A model, generating a single limit cycle.

The KHS-M-A version of the Goodwin-inspired single limit cycle model:

> The pure KHS-M-A model is a supposed integration of economic progress – in the form of stagnation – and fluctuations in (national) income (and unemployment), based on Keynes (1937) and Hansen (1939). In other words, it is Samuelson's integration of growth and fluctuations, i.e. growth cycles, where the former is of the order of decades and the latter of a time scale between the Schumpeterian Kitchin and Juglar cycles—approximately between inventory cycles of duration extending to a little beyond two years and the industrial cycle of (advanced) economies of an approximate period

[40] Referring to Tinbergen (1937: 90). However, see also Harrod (1937), especially the role of lags in his theory of the cycle, generated by multiplier-accelerator interaction. This letter to Tinbergen also reflects Harrod's ignorance of the mathematical properties of formal differential equations, the (formal) distinction between linearity and nonlinearity and that between difference and differential equations.

> length of ten years. I should observe that it should also be an integration of Keynes (1930), as long as its claims of being a growth cycle model is to be taken seriously. However, in spite of claims to the contrary – like the incorporation of Modigliani's life-cycle savings hypothesis to replace Harrod's purely exogenous propensity to save (shared with all the early neoclassical growth frameworks) – the KHS model (in any generalization) treats growth exogenously, especially in generating a limit cycle fluctuation in income and unemployment.

Therefore, I identify the key difference in the mathematics of PAS (ibid.) and the original PAS (1939a, b) as the transition to (nonlinear ordinary) differential equations modelling from a reliance on linear difference equations; as mentioned, almost in passing, thus downplaying its significance, Samuelson assumes (1988b: 4; bold and italics added): "What remains [in KHS] of the Harrodian razor's-edge is the inherent *anti-damped* instability of a laissez-faire equilibrium based on accelerator-multiplier ***differential equations***".

The significant economic change in assumptions (from a KHS model) is from a tight accelerator (ibid.: 8, Eq. 1) to a flexible accelerator (ibid.: 12, Eq. 15). These two assumptions—one mathematical, the other economic—generate the equilibrium single limit cycle fluctuation of the KHS model, but does not make the growth path endogenous. This—whether stagnation due to limits of population rise, or growth due to the obverse of this and the ensuing rate of accumulation[41]—remains exogenous (Fig. 4).

The only ordinary differential equation model of (macrodynamic) endogenous growth, but not necessarily generating a single, locally unstable, limit cycle, I know of is that of Nikaido (1987), who concludes:

> [C]apitalist economies are capable of evolution over time, but inevitably in the form of *cyclical growth*. This is not a pure cyclical movement superimposed on an autonomous growth trend, nor is the endogenously determined

[41] As Keynes (1930: Part II, p. 98; italics added) concludes: "[O]ur destination of economic bliss will be governed by four things — *our power to control population*, our determination to avoid wars and civil dissensions, our willingness to entrust to science the direction of those matters which are properly the concern of science, and *the rate of accumulation* as fixed by the margin between our production and our consumption; of which the last will easily look after itself, *given* the first three".

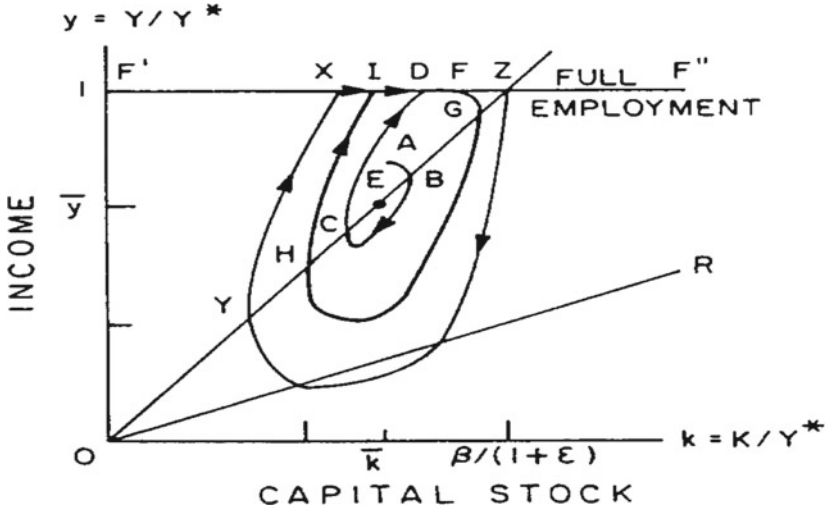

Fig. 4 The (one-sided) single, locally unstable, limit cycle (*Source* PAS [1988b: 13, Fig. 3])[42]

growth trend without cycles. Rather it ensues from the instability inherent in the workings of the capitalist economies as an entangled complex of growth and fluctuations mutually acting as causes and effects. (ibid.: 442–443; italics in original)

Thus, Nikaido – at least in this article – remains agnostic between Schumpeter's vision that "fluctuations are a form that any growth takes" (Ragupathy et al. 2013: 124) in an industrially capitalist economy and Hudson's precept that "growth is an inevitable consequence of the macrodynamic fluctuations of a capitalist economy". (ibid.)[43]

[42]The single, one-sided, locally unstable, limit cycle, in the phase space of capital and income, moving along the ceiling of full employment [given by XIDFZ], is shown as the trajectory, XZY. The phase space is reduced to a timeless ratio of $\frac{Y}{Y^*} = y$ and $\frac{K}{Y^*} = k$, where $y = n$. The latter assumption makes the growth part exogenous! I think the proper way to pay homage to Keynes and Hansen is to make sure that the [single] locally unstable limit cycle does not hit the full employment ceiling. After all, both Keynes and Hansen—especially after Hansen became the American Keynes—were of the unshakeable faith that a free enterprise economy will always be in a phase space with unemployment (see also PAS ibid.: 12, fn. 1).

[43]I am personally persuaded that Hudson allows non-industrial economies to be of a capitalist nature; cf. Ragupathy et al. (2013: especially p. 124).

5 Concluding Thoughts on a Universal Scientist

> Dynamics is a multidisciplinary study in mathematics and physics … In physics, *belief* is a strong principle: we *believe* in the laws of physics or we *believe* in the results of an *experiment. Proof* is more the concern of mathematicians; *logic* and *mathematics* enable the *possibility of proof*. When we work with *simulations* of *dynamical systems*, *question of belief and proof become confused.* (Weissert 1997: 133; italics added)

Paul Samuelson was a mathematical economist, in the mould of a mathematical physicist; he was neither a pure mathematician, nor a physicist, pure or applied. He believed in many of the observed facts of market capitalist macrodynamics; those he believed in, he tried to formulate as propositions or theorems—and thus, tried to prove them. Samuelson was admirably consistent in his search for rigour, in proving these propositions or theorems, but was fully aware that he failed in this endeavour.

He believed in the truth of the propositions or theorems about the market system that he formulated, but never displayed any dogmatism in defending the truth of them, if the proofs were shown to be mathematically incorrect or inadequate. Samuelson read widely, and absorbed deeply, the classical and neoclassical economists—Smith, Ricardo, Malthus, Marx and the two Mills; Jevons, Marshall, Walras, Pareto, Edgeworth, Fisher and Wicksell and many more; he was assiduous in mastering the economic works of his immediate predecessors and contemporaries—Schumpeter, Leontief, Robertson, Chamberlin, Keynes, Hansen and Hawtrey, but also Sraffa, Joan Robinson and Kahn, too—and, thus, also critically.

Samuelson was extremely generous to those younger than himself, especially when he disagreed with them. He neither did experiments with the market system, to test some of its properties, but studied, mostly theoretically, based on falsifiable hypotheses, the actual functioning of the market system, and extracted, in a Bayesian fashion, revised hypotheses, always relying on the principles of macro and micro that were enunciated in *Foundations*, i.e. maximization and equilibria for microeconomics, and stability of dynamical systems for aggregative economics (macrodynamics).

However, to the best of my knowledge, he did not ever even try to simulate the market economy or any aspect of it; so the kinds of questions asked by Weissert (ibid.), on the epistemology of simulation, were never asked in the Samuelsonian oeuvre. He was, of course, almost passionately interested in epistemological questions, but he was not averse to suspending hypothesis formation in the mathematical sense to try to answer them. His knowledge and understanding[44] of the philosophy underpinning the classics in economics was most helpful in these matters.

As pointed out above, he more or less specialized in classical mathematics that had at its core the differential and integral calculus—perhaps in the mould of a Hardy or Smirnov—of pure mathematics, in the physics of mathematics, so that he was comfortable with, say, Birkhoff on dynamical systems and familiar with something like the Hilbert-Courant two volumes on the *Methods of Mathematical Physics*. In applying this kind of mathematics and mathematical physics, he was explicitly influenced by E. B. Wilson and, implicitly, by Willard Gibbs.

Personally, I have only two regrets. Samuelson was a pioneer in the mathematics of Lotka–Volterra, struggle-for-existence, literature—as is evident from PAS (1967, 1971). His knowledge of this literature extended to a familiarity with the Kolmogorov (1936) classic. Kolmogorov inspired a definition of the structural stability of dynamical systems (in the phase plane).[45] Obviously, Samuelson was also fully aware, and familiar, with the economics of Goodwin (1967). Given all this, my first regret is, as an example of PAS (1985), he did not use his impeccable knowledge to model the KHS system, as giving rise to endogenous growth cycles.[46]

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[44] Warts and all—*again*, I nearly said. Samuelson engaged with so-called specialists in classical and neoclassical economics, all the way from the mid-1930s to the end of his life. In my opinion, his stance on new classical economics was, at best sceptical; at worst, dismissive. He was critical of Chicago economics, so much so that, when he spoke to me, by phone, in early January 1987, when I had, for the first time, crossed the Atlantic and taken up a Visiting Professorship at UCLA, he asked (the bold in the word and phrase reflects the way he talked): "What is a person like **you**, doing in **a place like that**!" The composition of the Department of Economics at UCLA prompted it to be known, to many, as the University of Chicago at Los Angeles!

[45] Many macrodynamic models—by Yasui, Morishima, Ichimura, Rose, Schinasi, Chang and Smyth, for example—formalized as phase-plane models, assumed (implicitly and explicitly) structural stability of such systems (proceeding to use non-constructive proofs of the Poincaré-Bendixson theorem to demonstrate the existence of cycles in the plane).

[46] Growth is an exogenous factor, in the Harrod-Domar sense, in Goodwin (ibid).

My second regret, which is related to the first, at least mathematically, in the sense of interesting hypothesis formation of the behaviour of the stability (local and global) of dynamical systems, is that Samuelson does not seem to have been interested in Kolmogorov–Arnold–Moser (KAM) theories and theorems (see Abraham and Marsden 1978, particularly the appendix translation of Kolmogorov's 1954 lecture, Scott Dumas 2014 and Weissert 1997). Since the essence of KAM theory is to analyse the persistence of quasi-periodic motions under perturbations, it is eminently suitable for a Samuelsonian analysis of aggregate economic dynamics, especially those in which ergodicity is prevented and invariant tori can be observed.

Samuelson's esteem for Fermi, Kolmogorov[47] and Ulam is both legendary and well-recorded. His understanding of the mathematics of KAM, without any serious need for updating the classical mathematics of dynamical systems, would have been easy. The application of the dynamical hypothesis and theorems of KAM to the macrodynamics of *Foundations* may have shunted economists from concentrating on the economics of Bourbakian mathematics, quite apart from stimulating encouragement to simulational hypothesis testing of the stability of novel dynamical systems, thus avoiding the current dominance of experimental behavioural economics.

Paul Samuelson was a universal economist—he was interested in, and contributed to, every aspect of economics (of his time), but always within the framework he defined and explored in *Foundations*. I will always remember him, his many-faceted contributions, his generosity and his unsullied humanity, as I remember Rumi's verse, in his *Rubaiyat*:

> I sought a soul in the sea,
> And found a coral there;
> Beneath the foam for me,

[47]The Marshallian Samuelson may approve Shiryaev (2000: 5; italics added), quoting Kolmogorov: "In mathematics, I [Kolmogorov] was one of the first in my class, but *the principal more serious scientific passions* for me *in school* [the E. A. Repman private gymnasium] were initially *biology* and then Russian history". In relation to the topic of my first regret, see also Sigmund (2007). In fact, both of my regrets have, as a common element, Kolmogorov! The mathematics of population dynamics, as developed by Kolmogorov, must be embedded in KAM theory. In this way, macrodynamics can—must—be unified with biology and mathematics.

An ocean was all laid bare.
Into my heart's night,
Along a narrow way,
I groped; and lo! the light,
An infinite land of day.

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