Roots – Reflections on Josef Steindl's First Article in an Economic Journal

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Before I come to the subject of this paper as indicated in the subtitle let me try – on this pleasant occasion of honouring my friend Josef Steindl – to speculate on different types of economists. I think one can usefully distinguish three broad types of economists (and similarly in other sciences) with a 'good' and 'bad' specification in each case. Type 1 is the researcher who early in his career gets hold of a very strong view or theory and then sticks to it for the rest of his life. In the positive case this can mean that this person can gain steadily growing special knowledge of certain relationships that he can then bring in as a distinctive contribution to various economic discussions. In the negative case there is a tendency to stick to the chosen theory and to lose touch with new developments and problems. At the other end of the scale we have the economist who turns to every new 'fashion' in the economic field and tries to jump on the bandwagon. With an active mind this can result in useful, though not necessarily deep, contributions and in openness to new combinations and problems; but it can also degenerate into a career orientated to the accumulation of quotable papers which have little to offer and do not permit the author to develop a firm theoretical base for his various views. Finally we have a not so easily definable Type 3, the 'autonomous' economist who has a not too narrowly chosen field of interests and develops a steadily growing 'program' of questions and answers which carry a very distinctive stamp of his personal way of approaching problems and theories. Here the 'good guy' is characterised by his careful choice of emerging problems and theories without falling for every new fashion, and his capacity to make important and original contributions to

ongoing debates. In the 'bad case' such autonomy can cause a person to get lost in a world of his own, to become a pigheaded outsider who is incapable of discussing his or other peoples' ideas in any framework other than his very personal one.

Now my contention would be that Steindl very clearly belongs to the best specimens of Type 3. Firstly, throughout his life he has kept a strong interest in a few highly relevant problems which have wide ramifications over the entire sphere of economic theory and policy – i.e. problems of macro-economic and employment stability, of micro-economic and structural aspects of the firm in capitalist societies, and also – though this is not immediately visible – questions concerning the role and development of technology, which did not only affect his writings on stability and enterprise problems but also formed a firm basis for his valuable papers on educational economics and for the industrial case studies which he prepared for the Austrian Institute of Economic Research.

Secondly, in following the development and changing aspects of his favourite subjects, Steindl shows a remarkable capacity for taking up decisive and carefully selected elements from the growing corpus of economic theory and combining them with his own very characteristic ways of looking at things. Thus one could always find Steindl in the midst of current discussions and moving along the frontiers of new – but not necessarily 'fashionable' – advances and at the same time to deliver 'goods' which carry a highly original 'Steindlian' stamp. This became clearly recognisable (though hardly appreciated in his own country) after his *Maturity and Stagnation* (1952) was published, but it was already present in the papers he wrote at the Oxford Institute of Statistics during the war.

The Steindl of those and later days is a well-known figure and most of you are familiar with many of his writings. I want today to turn to pre-war days and to venture an attempt at the History of Economic Thought by talking about (as far as I can see) Steindl's first published theoretical paper (apart from his unsigned contributions to the monthly reports of the then Austrian Institute of Business Cycle Research). I hope that this will

help to show that indications of later ideas and qualities were already present at that early stage.

The article I am referring to appeared in the Austrian *Zeitschrift für Nationalökonomie* in 1937 and carries the title "Der Konjunkturzyklus". Though the article is meant to be a review of Harrod's book *The Trade Cycle*, which had appeared in 1936 closely on the heels of Keynes' *General Theory*, it is far more than that. It is an extended treatment of several aspects of Harrod's ideas, which are discussed on nine closely printed pages of the journal.

Before I go into a discussion and evaluation of some of the points raised by Steindl, I first of all want to make it clear that to write such a thoughtful and appreciative comment on Harrod's views at that time and that place in Vienna was quite a feat for a young Austrian economist. We must not forget that the Keynesian approach on which Harrod – who had of course been able to follow Keynes's ideas long before they appeared in print – had built his theory of the trade cycle was not an easy thing to digest for the 'normal' run of economists. Today, after fifty years of simplification and modification that has made it possible to teach the Keynesian fundamentals to elementary school children, this is perhaps difficult to imagine. But the fact is that outside the charmed circle of Cambridge that had been forewarned of the coming 'catastrophe' of the Keynesian revolution, the new ideas were met by a mixture of non-understanding, hostility and benign neglect. This was particularly true of Austria, where in the academic world the reception of Keynes hardly got started before 1945. In the University of Vienna the main discussions raged between Hans Mayer, the last representative of the marginal utility school, and Othmar Spann, a preacher of a mystical holistic and corporatist economic philosophy. When I left Vienna University in 1938 – after finishing my Law and Economics studies – I had never come across the name of Keynes, let alone any of his ideas.

The situation was probably a bit different for Steindl, who took his degree in the Viennese School of Economics (Hochschule für Welthandel) where one of his teachers in economic theory, Richard von Strigl, had a special interest in trade cycle theory and had published a book (von Strigl, 1934) in which he developed a neo-Wicksellian type of

over-investment explanation of the cycle which at least touches on some of the problems dealt with by Harrod. And then there was, of course, Steindl's work at the Austrian Institute of Business Cycle Research that brought him into touch with empirical and non-orthodox views, which must have induced an interest for the new ideas. But the fact remains that he had to write the article in an environment that did not provide any help in understanding the Keynesian background. It was either not discussed at all or one still wrestled with questions concerning the equality of saving and investment and similar terminological or conceptual puzzles.

Yet Steindl's article shows quite clearly that at this early stage, he was already able to see the full significance of Keynes' basic approach and the importance of Harrod's expansion of this approach into new fields. Let me quote:

> "[Harrod's] attempt is in itself a step in a direction which goes beyond Keynes […]. A central question of trade cycle theory must be the recurrent nature of cycles and the explanation of turning points, problems which do not get much attention from Keynes […] Harrod has elaborated Keynes' grand concept in a manner and has given us a basis for a better understanding and discussion through the details, modifications and extensions he has added" (Steindl, 1937, pp. 229, 237).

Thus Steindl had caught on to a new development at a very early stage because he recognised that this development would prove important to a field whose economic and social *problématique* lay at the centre of his interests. But though Steindl's early affinity to Keynes was a necessary precondition for enabling him to discuss Harrod's book, it was not a sufficient condition. For though Harrod's book – or 'Essay', as he christened it – was a significant Keynesian contribution to the development of cycle theory and a forerunner of Samuelson's classical contributions of 1939 (Samuelson, 1939a, 1939b), it is at the same time a thoroughly diffuse and vague book in which a great variety of theoretical and practical ideas on the trade cycle is presented without any formulas or diagrams which could help to structure at least some of the main arguments. My personal belief is that the verbosity and vagueness which characterise parts of the book have something to do with the fact that Harrod was at that time probably already groping for the theory of

economic dynamics which he published in 1939 (Harrod, 1939). A critical review, which he wrote of Lundberg's "Studies in the Theory of Economic Expansion" in 1937, points in this direction (Harrod, 1937).

Be that as it may, the fact remains that Harrod had written a somewhat confusing essay. "The reading of the book is somewhat forbidding, if indeed not at times terrifying" wrote Alvin Hansen in his lengthy review of Harrod's *Trade Cycle* (Hansen, 1937, p. 509) and a – in principle more friendly – Joan Robinson remarked in her review: "The style throughout is highly idiosyncratic – somewhat condescending assistance for lame-dog readers alternating with merry asides for the benefit of those well versed in recent monetary controversy" (Robinson, 1936, p. 693). It was this lack of transparency which probably caused Haberler to pay surprisingly little attention to Harrod in the second and third editions of his famous *Prosperity and Depression*, where he added special chapters on "Recent Developments in Trade Cycle Theory" (Haberler, 1937); and this in spite of the fact that Harrod was probably "the first to attribute explicitly the recurrence of depressions to the joint action of the multiplier and the accelerator" (Pasinetti, 1974, p. 50).

Anyway, to deal with Harrod's book required not only a close acquaintance with Keynes' newest ideas but also considerable stamina to cut through to the main arguments. So it was no coincidence that reviewing the book was mostly entrusted either to people who had a long past relationship to Keynesian developments like Joan Robinson or to business cycle specialists like Alvin Hansen. That the young Steindl, living in the cold climate of Austrian economics, took up this task and was able to deliver a long and thoughtful essay shows that already at that time he possessed those characteristics which again and again show up in his later career, particularly in his great works like *Maturity and Stagnation* or *Random Processes*,[1] viz. his capacity to recognise the emerge of important and relevant ideas and then to absorb and utilise them for the analysis of relevant problems.

Let me now turn to Harrod and Steindl's review article. First of all I want to give a short outline of the structure of Harrod's book. The book

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[1] Steindl (1965) [editor's note].

contains four chapters. The first, "The Human Factor," analyses – starting from a Robinson economy and advancing to monetary capitalism – various cost and price influences in a more or less static but fluctuating economy and their stabilising or destabilising effects (e.g. rising marginal costs, prise fluctuations etc.).

This first chapter deals with some traditional material but manages to present it in a rather confusing way. The arguments in this chapter are set forth in an odd fashion, Mrs. Robinson rightly remarks in her *Economic Journal* review (Robinson, 1936, p. 693). So chapter 1 is quite understandably passed over quickly by all reviewers. Steindl, too, spends only one short paragraph on this section, stressing particularly Harrod's remarks on destabilising effects connected with price fluctuations. But he finishes this paragraph with an interesting sentence when he writes: "It is surprising that in this whole attempt to find a bridge from static value theory to trade cycle theory the question of expectations plays so small a part." When we consider that Harrod borrowed (and acknowledged) the multiplier-mechanism as the main element from Keynes's tool-kit for the construction of his trade cycle theory, this remark by Steindl can be regarded – at least from today's vantage point – as the criticism of an early 'true' or 'fundamental' Keynesian vis-à-vis a 'hydraulic' or 'mechanic' Keynesian.

Chapter 2 of the book, entitled "Investment and Output" contains the core of Harrod's 'new theory' ("A new theory of the Trade Cycle!" with an exclamation mark, is the first sentence of Harrod's book). It contains the 'real' stuff that animated Steindl to write his paper. It will be the basis for what is going to follow. The remaining two chapters of Harrod's book, "Interest, Money, and the Foreign Balance" and "The Question of Remedies" deal with extensions to the basic theory and with problems of policy. They contain quite a number of interesting observations and insights, but do not affect the main structure of the original model to which we now turn.

In the preface of his book Harrod stresses the three main pillars on which his theory is built: the accelerator ('the relation' in Harrod's terminology), the multiplier, and the theory of imperfect competition (which in his book appears as a "Law of Diminishing Elasticity of

Demand"). From these three elements he derives the inevitability of the trade cycle.

Let me put his main ideas of the process not in a nutshell but in half a nutshell. I shall only deal with the expansion and upper turning point, partly for reasons of brevity, but partly also because Harrod was rather weak in finding a good 'story' for the lower turning point. It was not until Samuelson (1939) and Hicks (1950) built their Harrod-type cycle theories that this story was added to in a more convincing way.

We start off with an expansionary process in a not fully employed economy. A certain amount of net investment is injected and this leads via the multiplier to increased consumption that in turn – via the accelerator – can lead to further (net) investment etc. In principle one could think of an endless process of expansion. On this we shall have something to say later on. Harrod now introduces his specific assumptions, which have partly retarding, partly expansionary effects on the process, the retarding ones being ultimately decisive. As output and income expand the marginal propensity to consume will decline (as Keynes assumed) and this will act as a brake on the rate of growth of consumption. There may also be a shift from wages to profits as prices rise with recovery and that will act as a further stimulus to saving. Thus a reduction in the rate of growth in consumption is to be expected, and this must lead – via the accelerator – to an absolute decline in investment. This reacts on consumption, which follows the downward direction – the cycle has turned. This brake on the upswing is intensified through Harrod's "Law of Diminishing Elasticity of Demand." As the economy recovers and employment and income increase, people pay less attention to price differences in a world of imperfect competition and this increases the monopolistic element in monopolistic competition. Prices can be raised and the shift to profits and savings is intensified.

Against these retarding factors running from dampened consumption demand to declines in investment, Harrod offers one element which could check this development: the capital coefficient might be increased, or to put it into his words, "The relation of the amount of capital per unit of output" might go up. This may follow, he says, from several causes, particularly from a decline in the rate of interest or from capital-using

inventions. In any case such an increase could maintain investment expenditure in the face of declining consumption growth and thus prevent a downturn. But the belief is that this influence can at best postpone the downturn but not prevent it permanently.

The first question that arose with the publication of the book was whether Harrod had really presented a "new theory" as he claimed. An affirmative answer was not quite obvious – indeed in Haberler's *Prosperity and Depression* Harrod is not given a very special place – because the role of the accelerator had long been stressed in connection with the cycle – e.g. by Aftalion, Clark, Mitchell, Robertson, Spiethoff to name but a few – and the retarding influence of a declining marginal propensity to consume had just been discussed in connection with Keynes albeit in a longer-term perspective. But looking back from today and knowing what role the Samuelson multiplier-accelerator model and Hicks' trade cycle theory played in later discussions, we can easily appreciate the pioneer achievement of Harrod in combining multiplier and accelerator with the result that he obtains a definite cyclical pattern. And there was also his attempt to introduce imperfect competition into a discussion that was dominated by perfect competition assumptions.

Joan Robinson was quick in recognising these achievements. "Mr. Harrod makes two main innovations", she writes, "the first is subsidiary […] it is the "Law of Diminishing Elasticity of Demand" […] Mr. Harrod's main contribution to the theory of the trade cycle lies in his combination of the Multiplier […] with the principle of the Relation" (Robinson, 1936, pp. 691-692). Hansen also recognises these two 'novelties,' but is rather doubtful about the role of the accelerator as a main element in determining investment expenditure, and so comes to a different judgement: "I can find only one new tool in his work-kit: It is the law of diminishing elasticity of demand" (Hansen, 1937, p. 530).

Steindl also recognises the innovatory character of Harrod's contribution but he refers only to the multiplier-accelerator aspect. "We find in Harrod's book," he writes, "not only a refined and detailed elaboration of the propositions which are contained in Keynes' latest work, but also a new point of view" (Steindl, 1937, p. 229). And this is exactly the combination of multiplier and accelerator for explaining

cycles and turning points. In this connection Steindl makes a remark that is interesting. He says: "We can see that in this explanation of the crises the main weight lies on a factor which is connected with technical, non-institutional conditions of production." (Steindl, 1937, p. 229). The stress on the fact that institutional, i.e. social and political conditions are missing in Harrod's theory can be seen as a typical Steindlian criticism, as he did not only know his Menger and Keynes but also his Marx and Schumpeter. This early remark points already to his later style, where he always tried to see the 'mechanics' of economic processes in their social setting. In a 1983 article on "The Control of the Economy" (Steindl, 1983), Steindl castigates economic models that are too mechanistic and believe that pushing a button will start a certain economic process. These gadgeteers, Steindl says, neglect "that there is a society with conflicting interests and conflicting expectations" (Steindl, 1983, p. 238).

As I already remarked, in contrast to Robinson and Hansen, Steindl does not count Harrod's imperfect competition element, the "Law of Diminishing Elasticity of Demand," as a special innovation, he does not even mention it. In this respect he proved to be a better judge than the others, for the way in which Harrod introduces this factor was questionable from the very beginning (this was also recognised by Robinson and Hansen) and it did not play any role in the ensuing developments of trade cycle theory. But it may seem a bit strange that Steindl, in whose later work oligopolistic market imperfections play such a decisive role, completely neglects these passages in Harrod's book. If this requires an explanation I would venture one along the following lines. One reason may have been that Steindl merely regarded this factor as irrelevant in the context of Harrod's theory. But I also think that Steindl, who so quickly and eagerly took up the ideas of the Keynesian and Kaleckian macro-economic revolution, was much less impressed by the Chamberlin-Robinson micro-economic revolution of imperfect competition. This was not due to an orthodox adherence to neoclassical ideas about perfect competition, but to his ever present realistic insight that the shift from pure competition to monopolistic and oligopolistic structures is economically and politically far more important than the

growth of polypolistic market imperfections which form the core of Chamberlin's theory.

Let me now turn to the essence of the cycle process, the interplay of the various factors that lie behind expansionary (deflationary) processes and the turning points. Harrod starts off with the question: under what conditions a steady advance might be possible, taking into account multiplier and accelerator effects? This bit of Harrod's exercise was rather confusing at the time he wrote it. Harrod was obviously already pregnant with ideas for a dynamic equilibrium growth path, which later materialised in his famous "Essay in Dynamic Theory," and he had the ambition to develop this equilibrium idea – contrary to the then usual accelerator theory – without the use of lags.

Thus he starts off with a picture of an economy which can exhibit steady growth if and when (1) the income receivers have a constant (marginal) propensity to save (i.e. the individual multiplier is constant), (2) there is no shift to profits (no distributional effects on the multiplier), and (3) the (marginal) capital output ratio remains constant. Without any further explanation or formal exposition (which would have been a forerunner of his later growth theory) Harrod then declares: "On these conditions consumption on the present day will rise in the same proportion as capital goods are increased and by the same amount as that which the new capital goods were designed to provide, and this experience seems to justify the present rate of advance" (Harrod, 1936, p. 90).

These forebodings of a 'warranted' rate in an ongoing process of equilibrium growth must have sounded mysterious to most contemporary readers who were thinking of multiplier and accelerator effects as lagged sequences following a state of disequilibrium. So it is not surprising that Steindl – on reading this passage – adds the following footnote:

> "I must confess that this point is not quite clear to me. For I am not sure whether the conditions stipulated by Harrod – constant capital coefficient ["Kapitalintensität" in the terminology of 1937!] and constant multiplier – necessarily lead to an identical percentage increase of the stock of capital and of consumption. The increase in consumption is linked to net investment by the multiplier. But there is no clear proof that a similar

> simple relation exists with regard to changes in the stock of capital…. This point is not made completely clear."

But as Steindl himself concedes, this confusion is of little consequence to the further argument in which Harrod derives the inevitability of the cycle from the proposition that the above mentioned conditions for a steady advance are not met in real life: with rising income and production the individual multipliers will fall, the share of profits will rise and though the capital-output ratio may rise somewhat, under the influence of falling interest rates or capital-using inventions, this cannot prevent the advent of a turning point, because normally there are narrow limits for interest fluctuations and technical innovations. It is at this stage that Steindl starts out on a long diversion of his own. While accepting in principle the picture of the two contracting forces (saving propensities and profit shares) and the possibility of a counteracting expansive force (rising capital-output ratio), he diverges from Harrod by giving more and a different weight to the last factor. In this part of the paper he develops some ideas that later became rather prominent both in his own writings and those of other authors.

Steindl starts off by noting that Harrod uses some hypotheses that are not yet empirically decided. This, he says, applies particularly to the assumption that the capital-output ratio will be rather rigid, because interest rates cannot be sufficiently lowered to allow this factor to counterbalance the increasing saving trends. But, says Steindl, even if we accept the argument that not much can be expected from falling interest rates, there is still another possibility for an effective counterbalancing increase in investment expenditure. "Interest rates need not fall in times of recovery," he writes, "in order to raise capital intensity [i.e. the capital coefficient]: rising profit can be used for exploiting possibilities of greater capital intensity even when interest rates are rising. From this point of view one can regard a fall in the size of the multiplier as less dangerous than Harrod does" (Steindl, 1937, p. 233).

Steindl here advances an investment theory that is not based exclusively or even predominantly on the purely technical relation of the accelerator but takes into account a behavioural assumption, viz. desire

for capital accumulation, a driving force. The technical relation does not, however, disappear completely. Steindl remarks explicitly that the extent to which profits lead to investment will be positively correlated with the existing degree of capital utilization. Here – in a nutshell – are already the decisive ideas of the theory of investment decisions that became dominant in Steindl's later writings. In his *Maturity and Stagnation* (Steindl, 1952) he starts off with a basic investment function where profits lead to new investment and vice versa. It is only when shocks or realisation difficulties arise that this growth process is disturbed. In a 1981 paper on Kalecki's theories of the trade cycle (Steindl, 1981) he gives a clearer and more concise account of his views. Profits and the drive for accumulation are the source of net investment but this process is hampered or broken by a growing and under-utilized capital stock. "Why should the ordinary incentives to invest and the accumulation of saving in business not be capable by themselves of creating a trend?" Steindl asks, and the answer can be found in a quotation he takes from Kalecki: "The tragedy of investment is that it causes a crisis because it is useful" (Steindl, 1981, p. 132 and p. 127).

Let us return to Steindl's 1937 paper: after his introduction of a possible alternative to Harrod's "relation" (the accelerator) as the only dominant source of net investment, Steindl begins to speculate about the consequences of his idea of sales and profit-driven net investment. Harrod, he says, has introduced into Keynesian thinking the idea that not only will investment lead to consumption, but consumption can also trigger off additional investment. But Harrod fails to see, Steindl continues, "that by doing this he has introduced a dangerous [sic!] element into Keynes' theory" (Steindl, 1937, p. 233). For now there is no longer the guarantee contained in Keynes' simple multiplier model that in the course of the expansionary process the additional savings volume will catch up with the given additional investment. The automatic self-financing process of investment is endangered. The additional consumption sales lead to further investment and this process might continue right up to the full employment level when the real process of expansion must stop and different conditions prevail (more on this later). Steindl remarks here (in a footnote) that – I quote – "in England this

complication has already been noted several times, but without attaching sufficient weight to it." In this connection he quotes articles from Meade, Shackle, Robertson and Joan Robinson.

Steindl now goes on to tackle this 'complication'. He wants to show that this link from additional consumption (not only from additional consumption growth!) to additional investment does not necessarily destroy the Keynesian mechanism of additional investment converging towards a production level below full employment (and thus leaving room for turning points). In order to do this he introduces a slightly modified multiplier that he develops formally with the acknowledged help of Alexander Henderson of Cambridge. Steindl presents the multiplier process – this should be stressed – in two ways: as a Keynesian instantaneous multiplier and as a (dynamic) period multiplier. This deserves stressing because in those days most people were only acquainted with the Keynesian form and had sufficient difficulties to come to terms with it.

To give you an impression of this part of the paper I give a verbal translation of the 'dynamic' derivation. The symbols used are k for the Keynesian multiplier ($= 1/s$ with s the marginal propensity to save), and n, which he calls the investment factor, which expresses the fraction of the revenue from consumer goods sales that is invested ($I_t = nC_t$).[2] For simplicity it is assumed that k and n are constant. I am going to quote now:

> "Starting with a net investment of 1 we obtain to begin with an increase in consumption amounting to $(1-1/k)$.[3] The producers of consumption goods, who get this money, do not only spend; they spend this more – they invest n-times this sum, so that they spend altogether an amount which is $(1+n)$ times the increase in consumption [i.e. $(1-1/k)\cdot(n+1)$]. This leads again to additional consumption amounting to $(1-1/k)^2\cdot(n+1)$ and so on. The progression of additional consumption expenditures is therefore:

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[2] This differs from the accelerator theory which in its simplest form is $I_t = a\Delta C_t$ with a as the accelerator. n and a need of course not only different interpretation, they are also of different orders of magnitude (with $n < a$). [editor's note: in the original typescript D was used, instead of Δ].

[3] Since $1/k = s$ this expression is equivalent to c, the marginal propensity to consume [K.R.].

$(1–1/k)$, $(1–1/k)^2·(n+1)$, $(1–1/k)^3·(n+1)^2$, …

The condition for convergence of this progression is that $(1–1/k)·(n+1)<1$ or $n(k–1)<1$ […]. We reach the result that only when $n(k–1)<1$ do we have a possibility that the segment of secondary investment and induced consumption growth will become infinitesimally small before full employment is attained" (Steindl, 1937, pp. 234-235).

Using the now more traditional notation and neglecting time lags, Steindl's system can be written as follows (with c standing for the marginal propensity to consume, Y for net national product and G for exogenous demand):

$C = cY$

$I = nC = ncY$

$Y = C + I + G$

$Y = cY + ncY + G$ (in equilibrium) with[4]

$\Delta Y = 1/[1–c(n+1)] \,\Delta G$

The multiplier is $1/[1–c(n+1)]$ and we obtain a converging process when $c(n+1)<1$, which is equivalent to Steindl's solution since $(1–1/k)=c$.

As far as I can see, Steindl has here developed, certainly quite independently, but probably also for the first time, the idea of what some people have later called the "super-multiplier,"[5] namely the multiplier

$1/[1–(\Delta C/\Delta Y+\Delta I/\Delta Y)]$

which takes into account both the marginal propensity to consume and the marginal propensity to invest out of current income[6]. In Steindl's case these propensities are c and nc respectively, leading to his condition for convergence, viz. that $c(n+1)<1$.

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[4] Editor's note: in the original typescript D was used, instead of Δ.

[5] See, for instance, Rettig (1978), p. 293. Hicks introduces a somewhat different super-multiplier in his *Contribution to the Theory of the Trade Cycle* (1950), involving both growing autonomous investment and the accelerator principle.

[6] Editor's note: in the original typescript D was used, instead of Δ.

Having derived this 'critical' value for a non-exploding case of an expansionary process involving sales-and-profit-induced investment, Steindl devotes the rest of his paper to an elaboration of some aspects connected with this special case. Here too, he brings up a number of interesting points, containing several pioneering ideas. First, he stresses that since the size of the investment factor *n* is (in addition to the 'more reliable' propensity to consume *c*) decisive for the question whether an exogenous shock is absorbed before full employment is reached or pushes the economy right up to the ceiling, it is important to form an idea about probable values of *n*. Without stipulating a 'normal' value of *n*, Steindl is rather definite in assuming that in cases of high capital utilisation *n* will take on higher values which could easily surpass the critical value pushing the expansion without break or turning point right up to full employment. The affinity to Hicks' model of 1950 becomes visible, the main difference being that the profit-induced investment in Steindl's case is replaced by growing autonomous investment and a critical accelerator in Hicks' case.

Once full employment is reached, Steindl continues, the multiplier-accelerator process (of Harrod) does not come to an end but now leads to "true inflation in the sense of Keynes," as Steindl formulates it. In a purely formal way, Steindl says, the multiplier process still goes on, but its 'true' content, viz. the automatic creation of additional real savings to cover real net investment, is lost. And then follows an interesting paragraph which I want again to quote in full:

> "One can, however, think of a different way in which an additional net investment might be financed (once full employment is reached). If the increase in demand cannot increase real income but only prices, then we get, with wages remaining unchanged, additional real savings as a consequence of the changing content of the nominal wage unit. Here we are back at the good old theory of forced saving, which in this stage can help us without doubt to bring the process to an end and to arrive at an equilibrium. But such an argumentation is not the theory of the multiplier, quite apart from the fact that it will be wrong under the given circumstances. Because it is difficult to imagine that once full employment is reached the increase in prices will not lead to a rise in wages […]. We must, however, introduce an important admission. When we look at the process of 'true inflation' we see that the continuing investment demand leads – via rising prices – to

> inflationary profits of the entrepreneurs. If the entrepreneurs spend part of these profits for consumption we get a cumulative process of price increases, because additional expenditures leads to a further rise in prices and profits. If we assume, however, that the entrepreneurs save the entire inflationary profits then the 'last' net investment has found the corresponding savings: the (real multiplier) theory remains intact over and above its purely formal validity" (Steindl, 1937, pp. 236-237).

It is not difficult today to see that this passage does not only look back on the "good old theory" of forced saving, but that it already contains a clear, though somewhat crude, premonition of Kaldor's later theory of investment and distribution equilibrium under full employment, and a realistic sketch of a political theory of inflation based on wage-price-escalation under full employment, a situation which was not easily foreseeable in crisis-ridden Austria of 1937.

Steindl then goes on to express doubts about the possibility of reaching a 'smooth landing' once the full employment ceiling is reached, particularly because of monetary reasons. With a rigid monetary system (which was typical for pre-war Austria!), he expects the emergence of restrictive effects once prices and wages rise beyond the 'normal' level. In any case, he comes to the conclusion that,

> "[…] it cannot come as a surprise that the world of the *General Theory* is turned upside down once full employment is reached. The elasticity of production stops, real income can no longer be expanded, savings begin to become scarce, and investment, which used to determine and limit savings, now finds itself limited by the available savings. Once again the rate of interest has to act as a restriction on investment."

And Steindl goes on to say: "It is no contradiction to Keynes' views that the two worlds of unemployment and full employment are so radically different" (Steindl, 1937, p. 237).

Three years later, in 1940, Keynes' pamphlet *How to Pay for the War* proved that Steindl had correctly understood and interpreted the full meaning of Keynes theory, while many other economists then and for many years to come – and they are still in our midst – thought that Keynesian economics could be dismissed as pure 'depression economics.'

I have come to an end. I hope I have been able to convey to you a few of the remarkable characteristics of Steindl's Opus No. 1, its originality and its pioneering insights. What I have done needed some courage. With the author sitting in the audience I am exposed to the danger that he can – authentically! – show that I have completely misinterpreted his ideas or intentions. But since I come today as a congratulator and since we are all in a festive mood I hope he will have mercy on me.

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