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In Search of a Canonical History of Macroeconomics in the Interwar Period: Haberler's *Prosperity and Depression* Revisited*

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Summary: 1. Introduction; 2. Analytical framework; 3. Money, Hayek and over-investment; 4. Underconsumption, Keynes and beyond; 5. Epilogue.

Key words: macroeconomics; canon; Haberler; methodology; history of thought.

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The article investigates the methodological foundations of Gottfried Haberler's 1937 classic *Prosperity and Depression* and their influence on Haberler's assessment of theories prevailing before *The General Theory* and of Keynes's impact on macroeconomics. It is argued that Haberler was influenced by the ideas of the philosopher of social sciences Felix Kaufmann, whose 1936 book on methodology was instrumental in Haberler's emphasis on the distinction between tautological and empirical statements in economics. Haberler's criticism of Hayek's capital shortage explanation of the crisis, as well as his rejection of Keynes' equilibrating mechanism through income changes and his scepticism towards econometric modelling of the business cycle, stem in large part from his views on method.

O artigo investiga os fundamentos metodológicos do clássico de 1937 *Prosperity and Depression* de Gottfried Haberler, bem como suas influências sobre a avaliação feita por Haberler das teorias dominantes antes da *General Theory* e do impacto de Keynes na macroeconomia. Haberler foi influenciado pelas idéias do filósofo das ciências sociais Felix Kaufmann, cujo livro de 1936 sobre metodologia foi decisivo ao levar Haberler a enfatizar a diferença entre enunciados tautológicos e empíricos em economia. A crítica de Haberler à hipótese de Hayek de que as crises são causadas por escassez de capital, bem como sua rejeição do mecanismo de Keynes de equilíbrio via variações no nível de renda e seu ceticismo quanto à modelagem econométrica do ciclo econômico, são provenientes em grande parte de sua visão metodológica.

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1. Introduction

It is now widely recognized that pre-Keynesian economic thought did not fit into the mould of what Keynes used to call “classical economics”. For some decades before *The General Theory*, there had been no simple pattern in the study of economic fluctuations and unemployment, but a plurality of contending explanations. As Terence Hutchison put it, the intellectual situation in economics in the deep depression of the early 1930s “was one of rather complacent, intellectually messy, confusion, not some rigid, consistent, logically clear-cut, and widely-held ‘classical’ orthodoxy” (1978:172; see also Sowell, 1972:201-11; Walker, 1986:14-5; Backhouse, 1995: part II). It was in that context that the League of Nations set up in 1933 under a five-year grant from the Rockefeller Foundation a program of research into business cycles in two stages (see de Marchi, 1991:153). In the first stage (1934-36) Gottfried Haberler carried out an analysis of existing theories of the business cycle, followed by a “synthetic exposition” of the nature and causes of business fluctuations.¹ The outcome was *Prosperity and Depression: a theoretical analysis of cyclical movements*, published in Geneva by the League of Nations in 1937. In the second stage (1936-38) the theories surveyed by Haberler were supposed to be subjected to statistical testing by Jan Tinbergen (see the preface by Alexander Loveday, Director of the Financial Section and Economic Intelligence Service of the League of Nations, in Haberler, 1937a:iv, cf. Tinbergen, 1939).

Prosperity and Depression has since been acclaimed by historians of thought as the authoritative survey of pre-Keynesian business cycle theories (see, e.g., Hutchison, 1953:404; Schumpeter, 1954:1.123, n. 2; Officer, 1982:151; Pribam, 1983:486; Mirowski, 1985:3; Blaug, 1991:180, n. 10; Laider, 1991:118; Samuelson, 1996:1.682). Succeeding editions (1939, 1941, 1958, 1964) incorporated Haberler’s immediate reaction to discussions surrounding Keynes’s *General theory* and his comments on several controversial topics of the 1940s and early 1950s, such as formal post-Keynesian business cycle models, Hayek’s Ricardo effect, the so-called Pigou effect, etc.² According to

¹ According to Moggridge (1995:225, n. 2) the survey was first offered by the League of Nations to Dennis Robertson, who apparently declined the invitation.

² The first edition of *Prosperity and Depression* was published by the League in 1937 in English and in French with two parts (“Systematic analysis of the theories of the business cycle” and “Synthetic exposition relating to the nature and causes of business cycles”, respectively). The second edition came out in 1939 with a new chapter 8 added to part I. The third edition enlarged by part III (“Further reflections on recent developments in trade cycle theory”)

Moggridge's study of articles published between 1936 and 1948 and recorded in classes 2.30, 2.31, 2.230, 2.322 and 2.325 of the *Index of economic journals*, Haberler's book was third in the individual publication citation rankings (1995:232).

The present paper is set out to investigate the methodological foundations of *Prosperity and Depression* and their influence on Haberler's assessment of theories prevailing before *The General Theory* and of Keynes' impact on macroeconomics. Gottfried Haberler (b.1900 in Purkersdorf, Austria; d.1995 in Washington, DC: see biographical information in Chipman, 1987 and Samuelson, 1996) was originally a member of the Austrian school. After graduating at the University of Vienna in 1923, he wrote his habilitation thesis on the economic theory of index numbers (Haberler, 1927). He was, together with Hayek, Machlup, Morgenstern and others, a participant of the famous Mises' private seminar in Vienna in the 1920s and early 1930s (see Haberler, 1981:121-2; Craver, 1986:14-5). After holding since the late 1920s the position of "Privatdozent" at the University of Vienna, Haberler moved to Geneva in 1934 to work as an expert at the financial section of the League of Nations. As Earlene Craver (op. cit., p. 26) suggests, it was probably the publication in 1933 of his volume on trade theory – where he used the notion of opportunity cost to advance the path-breaking concept of production-possibility frontier – which brought Haberler international fame and led to his appointment in Geneva. He left the League of Nations in 1936 for the department of economics of Harvard University, where he remained until 1971.

One of the regular members of Mises' private seminar was "the unforgettable Felix Kaufmann" (Haberler, 1981:122), philosopher of social sciences (b.1895 in Vienna; d.1949 in New York), who was very active in discussions on methodology (cf. Craver, pp. 14-5; see also Haberler, [1923-4]:222, 225). Later on, in the 1930s, Kaufmann's 1936 book on the methodology of the social sciences was instrumental in Haberler's emphasis on the distinction between "tautological" and "empirical" statements in economics, and on the role of induction in the search for the "empirical regularities" that are essential

was published by the League in 1941 and reprinted by the United Nations in 1946. The fourth edition (the first by a private publisher) came out in 1958 with a new part III ("Monetary and real factors affecting economic stability...") and two appendices ("Notes on the present state of business cycle theory" and "The Pigou effect once more"). Appendix I was omitted in the fifth edition (1964) and a new foreword added. Translations were published in Japanese (1938), Swedish (1940), Spanish (1942), Greek (1943) and German (1948, 1955).

for the creation of new hypothesis. I shall argue that Haberler's criticism of Hayek's capital shortage explanation of the crisis (after an initial enthusiasm that showed in his Harris Foundation lecture delivered in Chicago in 1932), as well as his rejection of Keynes' equilibrating mechanism through income changes and his skepticism towards econometric modelling of the business cycle (Tinbergen's and others) stem in great part from his views on method.

2. Analytical Framework

The organizing principle of part I of *Prosperity and Depression* is the classification of business cycle theories into groups according to the explanatory hypotheses deployed (see Haberler, 1946:1,13). This was a urgent task, since, as Schumpeter put it in his dramatic description of the business cycle literature around World War I (which certainly also applied to the early 1930s), "we seem to behold nothing but disagreement and antagonistic effort – disagreement and antagonism that went so far as to be discreditable to the science and even ludicrous" (1954:1.125). The purpose of the survey was "to test the logical consistence" of the several hypotheses and "their compatibility with one another and with accepted economic principles" (1946:1). This would provide the starting point for the much needed "synthetic exposition" of the business cycle in part II. Haberler's methodological standpoint is left largely only implicit throughout the book, though. We must look at some of his other writings in order to find out where he stood in the 1930s controversies on economic methodology.

Most of Haberler's statements on matters of methodology came out of his criticism of the saving-investment terminology of Keynes and Harrod (Haberler, 1936a, 1937b). He pointed out that the "neo-Cambridge" terminology had made it difficult to "decide in this field whether a particular statement involves a really material assumption about the real world (that is, about the behavior of individuals), or whether it is a purely 'syntactic' proposition about the definition of certain terms" (1937b:693). This distinction is, of course, reminiscent of the logical positivism of the "Vienna Circle", but Haberler had learned from Kaufmann (1933, 1934, 1936) that one should distinguish between three, not just two, sorts of propositions. He clarified that in a footnote:

This distinction is here used in the sense in which it has been made quite clear by “Logical Positivism” (Wittgenstein, Carnap). It should be noted, however, that the fact that a proposition has been arrived at by purely logical (tautological) derivation from some other proposition, by no means in itself reduces this derived proposition to a tautology, so long as there are “material” propositions among the premises. On all this compare, F. Kaufmann, *Methodenlehre der Sozialwissenschaften* (Vienna, 1936), *passim*, and esp. pp. 278-9 (ibid).

The “Vienna Circle” distinction between “synthetic” statements (verifiable by empirical evidence) and “analytic” statements (tautologies) would be introduced effectively into economics one year later by Hutchison (1938; but see also his discussion of tautologies in economics in 1933) as a reaction against Lionel Robbins’ apriorism (see, e.g., Caldwell, 1994, ch. 6). In this connection, it is worth noting that Haberler referred to Hutchison (1938) in the new chapter on Keynes in the 1939 edition of *Prosperity and Depression* (see 1946:194n, 215n). Furthermore, despite the similar approaches of Kaufmann (1936, part II, ch. 8) and Hutchison (who referred to Kaufmann; see, e.g., 1938:3), the notion, which goes beyond the Vienna Circle, that there are statements that are “indirectly tested” through the system as a whole cannot be found in the latter.³

According to Haberler (1936a:555, 559, n. 1, where he referred to Kaufmann, 1936:32, 43, 48, 257), “this mistake of treating relationships by definition as causal relationships occurs rather frequently in economics, not only in Cambridge”. As he put it in a letter to Keynes written in 11 April 1936, a relationship by definition is “exact but does not tell us anything about the real world”, while an empirical one “would tell us something about the world but could not be proved a priori” (Moggridge, 1979:252). Haberler reaffirmed later on in a debate at the 1948 meetings of the American Economic Association his overall anti-apriorism:

Of course, the difference between experimental and nonexperimental sciences must not be exaggerated. It is a difference in degree only and an anti-empiristic, aprioristic methodology of economics (or any

³ Cf. Caldwell, 1994:145, who mentions Machlup’s use of Kaufmann’s third type of propositions (called by Kaufmann, 1933:392, “heuristic postulates”: see also Kaufmann, 1942). On the similarities between Kaufmann and Hutchison, see Blaug, 1980:94n, who, however, misses the point that Kaufmann (1944) is a revised translation of the 1936 volume.

other science) cannot be based on the fact that it is impossible to make laboratory experiments with the economy (or society) as a whole (1949a:86n).

In the same vein, there was no straight contrast between the operations of deduction and induction, as Kaufmann (1936, part I, ch. 2; 1934:106; 1933:389) had explained. The premises of deductive reasoning are decided by experience, since they are chosen “with a view to their empirical applications” (1933:389). Hutchison (1933) made it clear against Robbins that “it is no comparative justification... of the deductive method to attack empirical studies on the grounds of the difficulty of forming successful hypothesis inductively. Quite the reverse. The necessary fundamental assumption of all scientists is that there are some regularities about the facts of the world which allow of successful inductive hypothesis” (p. 161).

Haberler treated the various explanations of the business cycle (with emphasis on the upper turning point) as empirical statements and called attention to the empirical regularities that could form the inductive basis for the formulation of alternative hypotheses. The first and crucial “regularity” is the existence of the business cycle itself – that is, cyclical fluctuations in employment and production – which Haberler simply assumed in part I of his report, but went on to argue on empirical basis in part II that it can be divided into four phases: upswing, downswing, upper turning point, and lower turning point (1946:13 and ch. 9).⁴ Following that, he suggests that the two regular features of the cycle are (i) the parallel movement between the physical volume of production (and employment) and the money value of transactions, and (ii) the much higher instability in the capital goods sector when compared to the production of consumer goods (1946:14, 26, 277-8). The last one was not controversial (cf. Schumpeter, *op. cit.*, p. 1.125), but Haberler was at pains to stress that the former feature is not a tautology, since it is conceivable that prices move in opposite direction to the cyclical movement of production (*ibid.*). The statement would be tautological, of course, if prices were absolutely rigid. Haberler later on submitted that this parallelism is suggestive of a causal connection between fluctuations of the money flow (“effective demand”) and oscillations in the flow of goods, which, by the way, is consistent with changes in effective demand provoked by autonomous shifts

⁴ See also Haberler (1932:161), when he assumed “that we have such a thing as a business cycle” and that “we have been able to isolate this movement statistically”. As Lucas (1981:274) points out, the “typical business cycle” analysed by Haberler had been discovered and documented in the well-known empirical studies of Wesley Mitchell.

in, e.g., investment (1953:483-5). He realized that if prices and wages were completely flexible the statement would have to be substantially changed and suggested the concept of “elasticity of supply” of output to express reactions of aggregate production to changes in the money flow (*ibid.*).⁵

In order to explain the business cycle, “we need either a law about a (cyclical) change in certain data or a dynamic theory” (Haberler, 1946:250; this distinction has a long pedigree in economics; see, e.g., Wicksell, 1907:59). This led Haberler to discriminate between, respectively, exogenous (of which Jevons’ “sunspot theory” is the classical example) and endogenous theories (pp. 8-9). In the first group, the upper turning point is explained by an accidental disturbance, while in the other one the boom itself creates a disequilibrium of the economic system which eventually brings about the crisis. In an article that appeared a couple of months after Haberler finished writing part I of *Prosperity and Depression* in December 1935, he used the distinction between “logical” (tautological or purely deductive) and “empirical” propositions to point out that:

From a purely logical point of view, there seems to me to be only a difference of degree between these two types of answers. When it is said that a boom must lead to a crisis, because, say, the expansion of credit is bound to create a vertical maladjustment in the structure of production, this is not to be regarded as a logical but an empirical law. Therefore, exceptions are thinkable and, in fact, we shall see below that it is not difficult to formulate conditions under which the proposition will not hold (Haberler, 1936b:4, n. 1).

He further divided the group of endogenous theories into the following categories, according to the reason given for the upper turning point: purely monetary, over-investment (of the monetary and non-monetary types), under-consumption, and psychological theories. With the exception of the purely monetary one (put forward by Ralph Hawtrey), all these theories explain the upper turning point as a result of what Haberler used to call a “maladjustment in the structure of production”, by which he meant “an allocation of the factors of production in a way that does not correspond to the flow of money” (1936b:4). Furthermore, Haberler’s Austrian background showed in

⁵ Zarnowitz (1985:535, n. 9) has suggested that the positive correlation of movements in prices and quantities stressed by Haberler “would indicate that shifts in aggregate demand dominate the shifts in aggregate supply over the business cycle”. There is no doubt that this is quite close to Haberler’s meaning (*cf.* 1953).

this proposition that there are “vertical” and “horizontal” maladjustments. If the structure of production does not correspond to “the decisions of the population as to spending and saving” there is a vertical maladjustment; if it does not correspond to either the “decisions of consumers as to the distribution of expenditure between various lines of consumption goods” or “the decisions of producers at every stage as to the distribution of their cost expenditure between different forms of inputs”, it is a case of horizontal maladjustment (1946:30; apparently, he introduced this distinction for the first time in 1932:166).⁶ Vertical maladjustment are associated to over-investment and under-consumption, while psychological theories are compatible with both types of maladjustment (1946:150).

I shall comment below on how Haberler made use of this scheme to evaluate the explanatory power of alternative hypotheses. Before that, it should be noted that he was critical of the use of the concept of equilibrium in business cycle theory, since it was not empirically framed, but dependent on divergent definitions given by different theories.⁷

I personally find it advisable to drop the concepts of natural and equilibrium rates... The concept “equilibrium rate” suggests, e.g., that there is one definite rate [of interest]... which ensures the equilibrium [What is the equilibrium? It surely cannot be defined by full employment of all factors of production. It is not defined by the condition that “the market should be cleared” (1936b:3)] of the economic system as a whole. But this rate cannot be determined even if we agree on the definition of equilibrium, unless we know how the equilibrium can be brought about. Only if we accept as the true conditions of economic equilibrium one of the common theories which proclaim stability either of the level of commodity prices, of factor prices, of some other prices, or of MV in one of the many senses of these terms – only on the basis of one of these theories can we tell what the equilibrium rate is.

⁶Haberler (1936b:5, n. 1) further assumed, “as most cycle theories do, that the money saved flows through the capital market and that the distribution among the various investment opportunities is made automatically in a correct way...”, that is, he excluded the possibility of divergences between the composition of the supply of capital goods and demand represented by savings.

⁷In the same vein, Hutchison (1938:107) contended that the use of equilibrium conditions in economics was only justifiable if the tendency towards equilibrium could be postulated as an empirical proposition. Haberler was critical of Schumpeter’s notion that the system passes through an equilibrium position on its way from the lower to the upper turning point (1946:81, n. 1).

Haberler repeated his reservations about the use of equilibrium concepts in business cycle analysis when he put forward his own “synthetic explanation” (cf. 1946:289, n. 1). In the main, he explained the upper turning point as the reaction that follows when – starting from a position of low employment – in the course of a cyclical upswing the economy hits a full employment ceiling. Owing to the operation of the acceleration principle, demand for investment goods will fall abruptly when consumers’ goods industries cease to expand. This hypothesis is essentially identical to the one advanced independently by Harrod in his *Trade Cycle* (published in September 1936a, p. 111, n. 4 and 217, n. 1), as Haberler acknowledged in his review of Harrod and in subsequent editions of his book (1937b:695, n. 13; 1946:369, n. 2). It is also part and parcel of Hick’s well-known non-linear model of the trade cycle, who referred to Harrod and to Haberler in this connection (1950:99, n. 1). Haberler is here very far from his Austrian heritage, reflecting his critical attitude towards the empirical basis of Hayek’s capital shortage hypothesis, as we shall see next.

3. Money, Hayek and Over-Investment

In the early 1930s Haberler was quite enthusiastic about Hayek’s theory of the business cycle (see 1931a:515; 1931b; 1932). This was in part a by-product of his view that Hayek offered a theoretically superior alternative to both the over-saving hypothesis of Foster and Catchings – “so harmful and so popular today,” as he put it in 1931a:515 – and to the “traditional monetary theory” (1931b; 1932) represented by Hawtrey. Let us pause for a moment to discuss Haberler’s (1946:28; 1936b:4) rejection to Hawtrey’s claim that the reason for the crisis is always an interruption of the growth of money supply.

According to Haberler (1946:20-1 and 362-3), Hawtrey based his theory of the upper turning point on the lag of cash reserves of the banking system behind the expansion of credit, which causes the drain of cash to continue after the expansion of credit has been interrupted. That lag is explained by Hawtrey’s assumption that the rise in wages – which largely decides the demand for cash by the public – lags behind the expansion of credit and prices. The upshot is that banks are induced to contract credit – instead of a simple interruption of the expansion – which brings about the crisis (see Laidler, 1991:104ff, for a similar account). What Haberler finds particularly unsatisfactory about Hawtrey’s explanation is the premise that prosperity could be prolonged indefinitely if only money supply continued to expand, without

real maladjustment in the structure of production (especially in the form of the accelerator mechanism described above in connection with Haberler's own exposition of the upper turning point).⁸ Furthermore, he did not accept the notion that banks would keep on making the same mistake of underestimating the cash drain in every upswing (1937:363).

Haberler (1946) left only implicit, though, that Hawtrey's pure monetary had been falsified by the depression of the early 1930s, which was not preceded by rising wholesale prices. This was the main topic of his 1932 Harris Foundation lecture, when he argued that the main features of the years 1924-29 in the USA were price stability and rapid growth of production, accompanied by increasing money supply and vertical maladjustment of the structure of production.

If we accept the proposition that the productive apparatus is out of gear, that great shifts of labour and capital are necessary to restore equilibrium, then it is emphatically not true that the business cycle is a purely monetary phenomenon, as Mr. Hawtrey would have it; this is not true, although monetary forces have brought about the whole trouble. Such a dislocation of real physical capital, as distinguished from purely monetary changes, can in no case be cured in a very short time (1932:172).

This is, of course, a description of Hayek's capital shortage hypothesis, which Haberler called a "more refined monetary theory of the cycle" (1931b:405; 1932:163). What Haberler found especially attractive about this new theory was "Hayek's convincing proof that 'the monetary explanation is not equivalent to the explanation through the variations of the general price level'. In view of the fact that the present depression in the USA has not been accompanied by serious variations of the wholesale and retail price-levels, a theory which does not stress general price changes and yet retain all the obvious advantages and the plausibility of the monetary explanation, should be of considerable interest" (1931b:405). As we could expect from Haberler's application of Kaufmann's framework, it is precisely the investigation of Hayek's ability to explain the American depression that will constitute in *Prosperity and Depression* the crucial test of the capital shortage hypothesis.

⁸ See also Hawtrey's (1937) reaction to Haberler's criticism. Interestingly enough, he accused Haberler's synthesis of being "purely theoretical with no reference to practical experience" (1937:96).

It is well-known (see, e.g., Laidler, 1994; Trautwein, 1996) that Hayek claimed in his *Prices and Production* that it is impossible to bring about a permanent increase in the capital stock of the economy through forced saving. In contrast to voluntary savings, consumers will tend to restore the former proportion between consumption and saving when money income rises during the upswing, with ensuing increase of prices of consumers' goods and abandonment of production in the higher stages. Hayek's reasoning hangs on the crucial assumption that the new roundabout methods of production have not been completed when credit expansion ceases. "But why must there be any incomplete processes at all when the expansion has to end?", asks Haberler (1946:54). Hayek is not able to show that a steady rate of credit expansion would necessarily bring about unsustainable forced savings.⁹ Instead, he argues that credit expansion is halted because of "a progressive rise in prices and the danger of a complete collapse of the monetary system" (p. 55). The relevance of this conclusion for Hayek's claim that his theory explained the 1929 crisis is pointed out by Haberler.

It seems to follow that [Hayek's] theory does not prove, as it claims to do, that a credit expansion which does not lead to a rise in prices but only prevents a fall in prices must have the same evil effect as the more violent type which brings about a rise in the absolute price level. In a progressive economy, where the output of goods in general grows continuously and prices tend therefore to fall, there is scope for a continuous expansion of credit at a steady state... The practical importance of this conclusion is considerable in view of the American prosperity in the twenties, a notable feature of which was the fact that wholesale prices did not rise (1946:55-7).

Fifty years later Haberler (1986:227) reflected once more on Hayek's business cycle theory. He reaffirmed on that occasion his view that there is "no reason why a steady stream of forced saving, the price level remaining stable, should be unsustainable", but he focused his criticism on Hayek's attempted explanation of the depression as a period of readjustment of the structure of production. Haberler (1946:58) had already made it clear that this is "admittedly incomplete and unsatisfactory" and that a general deflationary process (usually referred to as "secondary deflation") is necessary in order to explain

⁹ Haberler had contemplated before the possibility that new processes were already finished when the additional money is spent, but dismissed it in the end (1932:170, bottom).

“why the depression spreads to *all* stages and branches of industry”. Later on, he commented on Lionel Robbins belated acknowledgment that unemployment in the 1930s was largely provoked by fall in aggregate demand (and, therefore, that “Keynesian” measures were justified), which he opposed to Hayek’s insistence (in his Nobel lecture) that the deviation of the actual structure of prices and wages from its equilibrium structure is always the primary cause of unemployment. Haberler’s views on the role of empirical testing in economics are apparent in his reaction to Hayek.

[Hayek] acknowledges that [his] theory has “the unfortunate property of not being verifiable by statistical methods”. In his Alfred Nobel Memorial Lecture he argues at length that in economics there are many propositions that in principle – not because of insufficient data – cannot be verified *or, we may add, falsified...* This is not the place to go into the deep epistemological problems raised by Hayek’s Nobel Lecture. The problem at hand is much simpler... There is sufficient evidence of a general nature to show that Robbins was right (1986: 224-5; emphasis added).

Haberler’s mention of falsifiability should not be a surprise, for he was one of the first economists to read Karl Popper’s 1934 *Logik der Forschung* (see Popper’s 1974 autobiography:69 and 86). Besides, Kaufmann had long stressed that every empirical proposition is “refutable” (1936, part II, ch. 7; he actually referred to Popper in n. 33 to ch. 2, part I, but in connection with probability theory; see also 1933:399, third paragraph).¹⁰ On the whole, the assessment that “the high expectations which were originally entertained [in connection with Hayek’s theory] have given way to a much more cautions and much more sceptical attitude” (Haberler, 1946:67) seems to apply also to Haberler himself.

¹⁰In the third edition of *Prosperity and Depression*, Haberler (1946:481-91) discussed in detail Hayek’s attempt in 1939 to use the so-called “Ricardo effect” to provide an alternative explanation of the upper turning point. According to Hayek’s new version, the crisis is brought about not by an increase of the rate of interest, but by the impact of a rise in the price of consumer’s goods – and the equivalent fall of real wages – on the entrepreneurs’ decision to substitute labour for capital. After a lengthy examination, Haberler rejects Hayek’s new hypothesis on the empirical grounds that the substitution of labour for capital is too slow a process to be relevant for short-run fluctuations, and that real wages actually move pro-cyclically. He concludes that “one cannot help getting the impression that [Hayek] builds his theory on a rather shaky empirical foundation” (1946:491; see also 1986:226, for the same judgment).

Haberler's change of mind about Hayek's approach to business cycles is also visible in the criticism of the notion of equilibrium in *Prosperity and Depression*, which has already been referred to above. In his review of the German edition of Hayek (1933) Haberler had accepted Hayek's view that non-monetary theories of the business cycle are not able to explain their object "without coming into conflict with the principles of the static system, which is necessarily *equilibrium economics* and has no room for general disturbances" (1931b:405; cf. Hayek, 1933:85 and 103). In other words, there is no room in Hayek's framework for what Haberler (1946) called "horizontal" maladjustments of the structure of production and for "psychological theories" of the kind put forward by Pigou. Haberler stressed, however, that the proposition that the upper-turning point is brought about by a "change from optimism to pessimism" can only be turned into an empirical statement (and, by that, "make a positive contribution to the explanation of the cycle") if some hypothesis is formulated about how expectations are formed in terms of "observable factors" (1946:147).¹¹

Wrong expectations are also part of the "non-monetary over-investment theories" of Spiethoff and Cassel, but here Haberler (pp. 32 and 72 ff) was caught in two minds. He tended to play down the differences between these authors and the monetary over-investment theory of Hayek on the grounds that it is largely a matter of the degree of emphasis on real factors (inventions etc) as starters of the upswing. As far as the explanation of the upper turning point is concerned, Haberler suggested that Spiethoff's diagnosis of the disequilibrium at the end of the boom is "substantially the same" as that given by Hayek (that is, excess of the production of capital goods and lack of consumers' goods), even though Spiethoff did not draw (as he ought to) the familiar Hayekian conclusions about the role of the rate of interest (pp. 77-8). However, what Spiethoff contended is that in order for the production of capital goods and the formation of savings "keep pace with each other, the two processes would have to be adjusted to each other, in mutual knowledge. As such knowledge is lacking, the adjustment is impossible..." (Spiethoff, 1953:157; cf. Hayek, 1933:80-1). In another passage, Haberler seems to re-

¹¹He went as far as suggesting that expectations are "non-operational concepts" and that only by asking the individuals we can find out how they are formed (1946:252; cf. Hutchison, 1938:113-4). The way out could be that "from a strictly logical point of view, the psychological link between the past and the present consisting of expectations may be dropped and the theory stated in terms of direct relationship between observable phenomena at different points of time" (1946:253, n. 1).

alize that Spiethoff's account of the upper turning point was distinct from Hayek's: "Expansion comes to an end because it is almost impossible to estimate correctly the supply of savings... the construction of capital goods must be undertaken in anticipation of demand, which in turn is constituted by saving and cannot be foreseen correctly" (Haberler, 1946:84 and 133 where he shows the ambiguity of classifying Spiethoff's theory as either vertical or horizontal maladjustment).¹² With the publication of Keynes' *General Theory*, however, mistaken expectations – whether money induced or not – and disequilibrium leave the scene, not to mention the business cycle itself.

4. Underconsumption, Keynes and Beyond

How did Keynes' *General Theory* fit in Haberler's classification of business cycle theories? In the first edition (Haberler, 1937a) there are only a few references to Keynes (1936:ch. 22), mainly in connection with the "psychological theories". In the second edition (1939) of *Prosperity and Depression* there is a new chapter on "Some recent discussions", which deals basically with the debates that immediately followed the publication of Keynes' book. By then, it was clear to Haberler that Keynes' "Note on the trade cycle" had little to do with the rest of the book, which was not about mistaken expectations and dynamics, but the application of static equilibrium analysis to the study of unemployment (Haberler, 1946:168; cf. Kregel, 1976:213, and Kohn, 1986).

Haberler's immediate reaction to the publication of *The General Theory* in February 1936 appeared in his methodological criticism of the multiplier, which he submitted to the *Economic Journal* as soon as March of that year. After an acrimonious correspondence with Keynes (who was, of course, editor of the Journal), Haberler decided to publish the paper at the *Zeitschrift für Nationalökonomie* (see Moggridge, 1979:248 ff). The central points of the article were incorporated into Haberler (1946:193-4, 224-7), and the article was reprinted in the American Economic Association 1944 *Readings in Business Cycle Theory*, organized by Haberler himself (Haberler, 1944, ch 9). Haberler's criticism was aimed at Keynes' (1936:115, 122) "logical theory of the multiplier, which holds good continuously, without time lag, at all moments

¹² According to Haberler (1946:79) Cassel explained the upper turning point as result of a reduction in the flow of savings at the end of the boom, when the production of capital goods increases. I have argued elsewhere that while this is a sufficient condition for the crisis in cassel, it is not necessary, since the crucial factor is that prices in the market for capital goods are not decided by a tâtonnement process with recontract (see Boianovksy, 1999).

of time". After defining the marginal propensity to consume as $\Delta C/\Delta Y$ (measured in wage units), and the multiplier as \underline{k} in $\Delta Y = \underline{k}\Delta I$, Keynes argues that, since $\Delta Y = \Delta C + \Delta I$, we can write the marginal propensity to consume as $1 - (1/\underline{k})$. Haberler rejected Keynes' claim that the multiplier is, by that, determined by the value of marginal propensity to consume:

It seems that Mr. Keynes has fallen into the trap of treating such a relationship by definition as a causal or empirical relationship between investment and income and that thereby a large part of what he says about the multiplier and its probable magnitude is vitiated. By assuming something about the marginal propensity to consume he assumes something about the multiplier, but this is no more an explanation of the multiplier than *pauvreté* is an explanation of poverty (1936a:554).

Haberler's charge that the relation between the multiplier and the marginal propensity to consume (and between income and investment) in Keynes is a tautology which cannot be used for any causal analysis in the real world has been accepted by many authors (see, e.g., Ackley, 1961:309-12; Kohn, *op. cit.*, p. 1.208; see also Hutchison, 1938:70, n. 45). He did not convince Keynes, though, who asked Haberler (in a letter of 30 April 1936) "what has come over you? I am perplexed", to which Haberler answered (2 May 1936) that "it seems to me that I have a right to being perplexed" (Moggridge, 1979:254). Haberler (1936a:556) further criticized Keynes for using two different concepts of propensity to consume: the "formal" one related by definition to the multiplier (ch. 10 of *The General Theory*) and the "psychological" (and, by that, refutable) one (ch. 8 and 9), based on introspection and observation. He approved to the notion of a consumption function $C = f(Y)$, but insisted that it had no place in the instantaneous multiplier, since "saving and investment have identically the same meaning" (Moggridge, 1979:251) in Keynes' framework. Consequently, Haberler was not prepared to accept Keynes' claim in the 1937 *Economic Journal* that the "initial novelty" of his theory lies in the proposition that the equality between saving and investment is assured by changes in the level of income (see Moggridge, 1973:212).

It is misleading to say that income must change, in order to ensure the equality of S and I . Whatever the level of income may be, S and I must be equal, because they are made so by definition. The change of level of income comes in as a condition only because Mr. Keynes takes the "multiplier" – the "marginal propensity to consume" – as a

constant quantity... Income must change, not because it is necessary to ensure the equality between S and I , but because we have assumed it by assuming the multiplier (Haberler, 1946:193, n. 2; cf. 1936a:558).

Haberler methodological criticism prevented him from realizing that Keynes was driving at the proposition that a decline in output caused by excess aggregate supply will reduce supply more than demand – because of the consumption function – and bring the economy to equilibrium at less than full employment. The equilibrating effect of the contraction in aggregate income has been identified by Don Patinkin (1982:8-11) and other scholars as the “central message” of Keynes’ *General Theory* (cf. Samuelson, 1996:1.683; see also Boianovsky, 1996, for a discussion of the anticipation of the equilibrating mechanism by Frederick B. Hawley). The point was discussed in connection with pre-Keynesian business cycle theory by Lloyd Metzler (1946, 1947), who pointed out that before Keynes a cumulative process of income change was assumed to continue until it was interrupted by some outside force. According to Metzler, the stabilizing influence of consumption upon the cumulative process is the central feature of the “modern theory of employment”, even though he linked Keynes’ equilibrating mechanism to the explanation of the turning points of the cycle, instead of the equilibrium level of economic activity (1947:440-1; cf. Hansen, 1951:493-7).

Besides Metzler’s article, Seymour Harris also included in his book on the *New Economics* a contribution by Haberler which had appeared one year before in the *Review of Economic Statistics* under the title “The Place of *The General Theory* in the History of Economic Thought”.¹³ Haberler did not repeat on that occasion his methodological criticism of the multiplier, but, instead, praised “the multiplier technique, whose usefulness should not be doubted, despite the crudity with which is often used” (1964:582). In part III of the third edition of *Prosperity and depression* he included two sections on “further observations on the theory of the multiplier” and on “the foreign trade multiplier” (1946:455-73) which may help to explain his change of mind. Haberler then distinguished sharply between the “instantaneous” multiplier of Keynes and the “successive-spending” multiplier of J. M. Clark and F. Machlup, which uses period analysis to describe the impact on in-

¹³A German translation of the article was added to the 1948 and 1955 German editions of *Prosperity and Depression*. It was reprinted (with a new section on Keynes’ “Sixteen Years Later”) under the title “*The General Theory after Ten Years and Sixteen Years Later*” in a book on Keynes edited by R. Lekachman in 1964.

come throughout several periods of a once-and-for-all increase in autonomous spending. It is assumed in this case that consumers' expenditures during one period are determined by their incomes of the preceding one *à la* Robertson. The result is that the total income generated in successive periods can be expressed as a sum of a geometric series, but, in this case, as Haberler pointed out, "aggregate saving induced in successive periods by any act of investment approaches the amount of the initial investment, implying that *S* and *I* are different for any finite period, the differences becoming smaller with the length of the period" (p. 458, n. 1). The final formula for the "dynamic" multiplier is identical with the formula for the "comparative-statics" one (cf. Patinkin, 1965:347), but Haberler clearly attached to the dynamic interpretation a non-tautological meaning, since saving and investment differ during the process. Haberler considered Robertson's period analysis as a step in the direction of a "truly dynamic analysis" and went as far as suggesting in the preface to the Spanish edition of *Prosperity and Depression* that "there is no doubt" that the future of business cycle research is represented by Robertsonian dynamic sequences (1942:xv).¹⁴

Regardless of Haberler's attitude to the equilibrating effect of income change in Keynes' system, he maintained that an equilibrium with less than full employment could only exist if money wages were rigid (1946:235 and 238, n. 1; 1964:584). The point was discussed in an exchange with Keynes in May 1938 (see Moggridge, 1979:272-3). Haberler asked:

Would you agree that an equilibrium with involuntary unemployment is incompatible with perfect competition in the labour market? If namely competition there were perfect, money wages would fall all the time so long as unemployment existed and any conceivably desired level of liquidity could be reached. If that could be agreed upon – and I think you say that yourself in a latter part of your book – most classical economists would agree with you, because nobody denies that unemployment can persist, if money wages are rigid.

¹⁴Haberler was not fond of the third approach to investment and saving in the 1930s, that is, the one put forward by the Swedes. "In spite of the appearance to the contrary, no statements about facts are involved" in the description by the Stockholm School of how the ex post equality between investment and saving comes about. It is not a description of a real, causal, process, since "what actually happens if planned saving and investment differ is assumed by way of illustration..." (1946:181, n. 2; cf. Ackley, 1961:325, n. 9).

He had discussed in the first edition of the book (1937a:298-9; cf. the corresponding passage in 1946:403-4 and 242) the positive effects of wage and price reductions on the value of real balances as part of his explanation of the lower turning point. Apart from the introduction of what later became known as the “Pigou effect”, Haberler’s description of the indirect effects of a nominal wage fall was quite close (but written independently of) ch. 19 of *The General Theory*. He submitted, however, that the mechanism was common knowledge (1946:240; cf. the above quoted letter) among orthodox economists such as Pigou, which raised Keynes’ objection:

I am not aware of any passage written before the publication of my book, in which anyone in the classical tradition has said this or anything remotely resembling it... But you are more learned on this matters than I am, and I await a reference from you to a passage where a classical economist has indicated a theory of wages resembling mine in the above respect (Moggridge, 1979:273).

Haberler never provided the references asked by Keynes, though. When Keynes reviewed (anonymously) in the 1939 *Economic Journal* the second edition of Haberler’s book, he complained once more that there were no “precise references to earlier writings” to support Haberler’s claim of essential continuity (Moggridge, 1979:275).

We saw above that Haberler in the first edition of *Prosperity and Depression* included Keynes in the group of “psychological theories” on the basis of ch. 22 of *The General Theory*. He also suggested on that occasion (1937a:111, n. 4) that even though Keynes could not be labelled an underconsumptionist, the concept of the consumption function could be used in support of certain aspects of the underconsumptionist thesis. Haberler was aware that underconsumption theory was, like Keynes’, not primarily about the cycle, but long depressions (1946:119, 234; cf. Bleaney, 1976:13). As far as the explanation of the upper turning point is concerned, his main objections to the over-saving theories of Hobson and Foster and Catchings were, as we could perhaps expect, of an empirical nature: “There is no evidence that an absorption of savings occurs during the boom or before the crisis... There invariably exists a brisk demand for new capital, signalled by high interest rates... There is no evidence for the assumption that the rate of saving rises at the end of the

boom...” (1946:123, 126).¹⁵ There is, according to Haberler (1946:244-5), a distinguished underconsumption flavour to several passages in *The General Theory* where chronic depression is described as a result of the low propensity to consume and vanishing opportunities for new investment. He did not dismiss the possibility of economic stagnation, but, once more, stressed *à la* Kaufmann that “only the careful scrutiny of a mass of experience and the study of historical processes can make the hypothesis more or less probable” (1946:246-7).¹⁶

It is well-known that the so-called “Pigou effect” was brought to the forefront of macroeconomics when Pigou introduced in 1943 the impact of increasing real wealth on consumption as a reaction against Alvin Hansen’s secular stagnation thesis. Haberler was, however, skeptical of the empirical relevance of the “effect” in the actual cycle mechanism (1952:577). He pointed out that the dynamic repercussions of falling prices and nominal wages were far more relevant for the real world:

What Don Patinkin [“Price flexibility and full employment”, *American Economic Review*, September 1949, *passim*] somewhat pompously enunciates as “the Pigou effect”... is just common sense which must have been in the minds of many writers. At any rate, when I stated in the first edition of my *Prosperity and Depression*, I thought that I expressed an obvious fact... It is true, of course, that all this is not the whole story and that it does not settle all problems of policy. Dynamic repercussions upon the marginal efficiency of capital, changes in the income distribution due to the increased real value of money debts and similar frictions are vastly more important than the mechanics of the

¹⁵ Haberler was attracted to another version of the underconsumption theory, based on the effect of a rise in the supply of consumers’ goods (instead of a fall in demand) at the end of the boom. He ascribed that hypothesis to Aftalion (who, in his turn, influenced authors such as Robertson and Schumpeter) and considered it to be, when combined to the acceleration principle, the best alternative to and the “direct opposite” of the shortage-of-capital explanation of the crisis (1946:32, 103, 127ff). He characteristically concluded that only “extensive empirical studies” could decide which hypothesis is correct (1946:376). Empirical testing is not a straightforward matter, though, as can be gathered from Haberler’s reactions to Tinbergen’s (1938) negative statistical findings regarding the acceleration principle and, in another context, to Leontief’s famous empirical refutation of the Heckscher-Ohlin theorem (the so-called “Leontief Paradox”). In both cases, Haberler criticized the tests for lack of correct specification of the respective theories (1946:87, n. 13; 1961:71-2).

¹⁶ Guthrie and Tarascio (1992:402-3) have concluded from their survey of the opinions regarding the interpretation of Keynes’ position on secular stagnation that Haberler (1946) was ambivalent on whether Keynes actually subscribed to that view. Haberler was quite unmistakable about that in his other writings, though (see, e.g., 1986:221).

static Keynesian system... It is high time that Keynesians recognize the inability of the Keynesian system to cope with the problems of wage and price flexibility so that the discussion can be moved out of the dead-end track on which it has been shunted by those writers who take as the last word the static Keynesian system which its author, if he had lived longer and had not been preoccupied with other matters, would have abandoned long ago as untenable intermediate station (1949b:571).

The answer to the empirical question “does flexibility of prices and wages promote economic stability?” was (or should be) according to Haberler (1946:243, 491ff; 1953) the most pressing item in the business cycle research agenda after Keynes. He suggested that Keynes himself (despite a few hints in ch. 19 of *The General Theory*) had no definite answer to that question (1953:485). Formal “Keynesian” models of the business cycle put forward in the 1940s and 1950s usually assumed rigid wages and prices, and had nothing to say about that.¹⁷

5. Epilogue

The early 1930s variety of divergent explanations of the business cycle was largely gone in the 1940s, not because of Haberler suggested “synthetic” explanation, but because of the proliferation of mathematical models often cast in terms of the multiplier-accelerator mechanism. The “modern theory” of the business cycle based on Keynes found its “highest expression”, as Haberler (1953:486) put it, in Hick’s 1950 volume, whose explanation of the upper turning point was close to Haberler’s, as we saw above. Hicks and others only dealt with the problem of “capital widening”, though, signaling a “change in the centre of gravity” of business cycle research when compared to Hayek’s emphasis on the problem of “capital deepening” (1953:490). Haberler pointed out, however, that the accelerator cannot be found in *The General Theory* and that “it is almost certain that [Keynes] would have rejected modern business cycle theories. He was too much of a business cycle practitioner and had a deep distrust in complicated mechanical models” (1953:485, n. 2).

¹⁷ *The relation between price flexibility and instability has been thoroughly examined by the macroeconomic literature since the late 1980s (see, e.g., Blanchard and Fischer, 1989:546-8).*

Haberler was not fond of “mechanical models” himself. He had noticed in the second edition of *Prosperity and Depression*, in connection with the work of Ragnar Frisch and Jan Tinbergen, that a dynamic theory of the business cycle “if fully elaborated in precise terms... requires a highly complicated mathematical technique and present formidable problems from the purely formal logical point of view” (1946:254). He discussed the matter further in part III of the book, while commenting on the models put forward by Samuelson, Lundberg, and especially the econometrics of Tinbergen. Haberler compared a “general dynamic theory comprising the economic system as a whole” to “Laplace’s famous world formula”.

Given the state of the economic universe at a single point of time (or during a short period) such a formula would enable the human mind to reconstruct the course of economic events into the remote past and to foresee its evolution in the distant future; to an intellect equipped with such a theory “nothing would be uncertain and the future as well as the past would be present before its eyes” (Laplace) (Haberler, 1946:478).

Haberler was aware that this is a “distant ideal”, but contended that, in any event, “some extrapolation, some prediction of the future (or of the past) must be possible, if [Tinbergen’s] scheme has any value, not only from the practical, but also from a purely scientific, point of view”. He refrained from pursuing the matter in detail, on the grounds that it would require an extensive discussion of “fundamental epistemological questions” (1946:479). From the perspective of Kaufmann’s philosophical framework (which has many points in common with Popper’s) prediction is essential to science. Haberler reaffirmed his position in his 1956 critique of “certain tendencies in modern economic theory”, when he stated that the proliferation of formal models of the business cycle (linear or nonlinear) have been so far “most disappointing”, especially because, even if fitting well the data, none of them “stood up to the test of extrapolation beyond the period from which the data were taken” (1946:464).¹⁸

One of the corollaries of the econometric approach to the business cycle – whether of the purely endogenous or of the (damped) impulse/propagation type – is that no special theory of the turning point is needed, which was

¹⁸ *Milton Friedman was also critical of the poor predictive power of macroeconomic models in the NBER Conference on Business Cycles edited by Haberler in 1951 (see Kim, 1988:78).*

firstly noticed by Tinbergen (1940:78). This would render the whole analysis carried out in *Prosperity and Depression* obsolete, but Haberler (1946:479-80) maintained that, given the “theoretical stage” of formal models, turning point analysis is necessary for testing and construction of theoretical models, without dismissing the possibility of an eventual general theory “capable of explaining the cycle in all its phases” (1946:480; cf. Kim, 1988:61-2). By the middle 1950s Haberler became convinced, though, that the main problem with the formal models produced so far was the neglect of monetary factors, in contrast with their importance in historical episodes such as the Great Depressions of the 1870s and 1930s (see 1956). The publication of Friedman’s and Schwartz’s *Monetary History* in 1963 confirmed that, and Friedman’s presidential address of 1967 provided an explanation for the comovement between money and output in the business cycle. This led Haberler (1980:605-7) to suggest that the empirical proposition that there is a short-run (not permanent) trade-off between unemployment and inflation formed the basis for a “Post-Keynesian Consensus” in the 1970s.

Prosperity and Depression has been sometimes described as a history of the development of business cycle theory (see, e.g., Mirowski, op. cit., p. 3), despite Haberler’s explicit disclaimers (1946:1, 12). There are a few historical references in the book – such as Wicksell’s adoption of Spiethoff’s non-monetary explanation (1946:72),¹⁹ Marx on reinvestment cycles (p. 84), Labordère’s 1908 elaboration of the capital shortage hypothesis (1946:46, n. 1), Malthus, Sismondi and Lauderdale as founders of the underconsumptionist tradition (1946:118), and a list of the authors who developed the acceleration principle from T. N. Carver on (1946:87) – but they are not pursued. Nevertheless, as Link (1959:186) has shown in his well-known study of theories of economic fluctuations in the first half of the 19th century, Haberler’s classification scheme can be profitably applied to other periods as well.²⁰ But is *Prosperity and Depression* the canonical history of interwar macroeconomics? A quick search through macroeconomic textbooks (especially those written in the 1950s and 1960s, the heyday of “Keynesian economics”) shows that Keynes’ caricature of “classical economics” as a straw man to be knocked down still survives

¹⁹ While Wicksell was quite positive about Spiethoff’s work on business cycles, it is hardly accurate to say that he “adopted” the latter’s theory (see Boianovsky, 1995).

²⁰ Link suggests the following matches: Hawtrey/Thomas Attwood; Hayek/James Wilson and J.S. Mill; Hobson/Thomas Malthus; Keynes/Thomas Joplin and Malthus; Pigou/Thomas Tooke and J.S. Mill (and, one could add, John Mills).

(cf. Haberler, 1964:594).²¹ On the other hand, the development of the “new classical school” in the 1970s has led to the opposite view that Keynesian economics was just a detour in the history of business cycle theories (see, e.g., Lucas, 1981:273-7; Kim, 1988:2-3), which is also far from the account found in *Prosperity and Depression*. What Haberler has given us is a comprehensive and critical assessment of the complex interwar macroeconomics grounded on a clear methodological standpoint.

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²¹ See, e.g., Ackley, 1961, ch. 5 and Cochrane, 1970, ch. 5. Ackley warns that his model of “classical economics” is “historically inaccurate” and is devised for pedagogical reasons only, but his warnings are not always taken into account by students or even by other authors (cf. Cochrane, 1970:92).

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