The Kieler Kreis, GPSS 7 & Social Research
The First Decade

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* Reflective notes on the contributions of some of the distinguished members of The University in Exile, in general, and the Graduate Faculty – now, the New School for Social Research – in particular, to economics in the pages of Social Research (SR) during its first decade of vigorous life. This year marks also the tenth anniversary of the original University in Exile becoming The New School. These notes are meant to commemorate these two ‘couples’, at the two ends of a noble endeavor to challenge a variety of orthodoxies, but without shunning tradition. In particular, I emphasise the contributions of Fritz Lehmann, who died at the very young age of 39 (cf., Social Research, 1940), Emil Lederer – who also died relatively young (cf., Social Research, 1941), Hans Neisser; Gerhard Colm, Eduard Heimann, Adolf Löwe and Jacob Marschak, They formed the core of a Kieler Kreis, with Lowe at the centre and, then, became what I call members of a fertile GPSS 7. Although Frieda Wunderlich’s contributions – particularly in the very first issue of SR (Wunderlich, 1934), but also Wunderlich (1943) - and achievements were remarkable, regrettably I do not feel competent to include her work as part of the formative contributions to economic theory and macroeconomic policy. She, like Karin Koch in Sweden, was a pioneering woman academic in a milieu that was then – and still is, now – dominated by a disproportionate number of men. In the context of the Graduate Faculty, her distinction of being its first Female Dean – not only at The New School, but in any serious department or faculty of social or political sciences in the Western World, has to be handsomely acknowledged.
Abstract

The reflective notes reported in this paper are meant to commemorate the two ‘couples’ of decades, formed at the beginning by the contributions of what I refer to as the *Kieler Kreis*, in the pages of *Social Research*, and at its other end, by the current contributions by recent and current members of the department of economics at NSSR. I refer to the latter as the *NSSR7* and the former as the *GPSS 7*. Their contributions, at both ends, were a source of rich alternatives, in political economy, macroeconomic policy, monetary theory, macrodynamic fluctuations and social accounting. An attempt is made to weave together a coherent narrative of this tradition – which is in danger of being subsumed by irrelevant orthodoxies.

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§ 1. By Way of a Background to (almost) *A Decade of High Theory*

“On rare occasions in literary history a new publication appears not as a result of long, conscious planning, not a product of particularistic ambitions, but a spontaneous generation within a dominant circle of circumstances. *Social Research* is such a spontaneous growth.”

Johnson, 1934, p.1; italics in the original.

Thus, Alvin Johnson, opened the first issue, of the first volume, of *Social Research*, in February, 1934. Most of the scholars of the *Kieler Kreis*, who had formed around Adof Lowe at Kiel, became – sooner or later – members of, first, the *University in Exile* and then, of the *Graduate Faculty of Political and Social Science*, which transformed itself into today’s *New School for Social Research*.

Although the events that determined the year *Social Research* was launched were tragic, yet there was something serendipitous in having to begin in 1934. In that year, and the few years around it, much of what eventually came to be codified as mathematical economics, neoclassical microeconomics and macroeconomics, emerged as independent disciplines, almost as ‘a spontaneous generation within a dominant circle of circumstances’.

Before the ‘*Tilton Laundry Hamper*’ papers were discovered, in the mid-1970s, the ‘first surviving complete contents to’ the *General Theory* (Keynes, 1936), was that from December, 1933 (Keynes, 1973, p. 421). Lindahl had visited Keynes at Cambridge in January, 1934, and, in fact, had appraised him of the way the Swedes – particularly he and Myrdal – had introduced the concepts of *ex ante* and *ex post* to come to terms with that tortuous, almost controversial, issue of Savings, in the aggregate, *always equals* Investment.

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1 In analogy with the almost simultaneously formed *Wiener Kreis*; I coined the phrase *Kieler Kreis* during my preparation of the Lecture Notes for Advanced Macroeconomics 1, this forthcoming Autumn.
2 He was born Adolf Löwe, but changed it to Adolf Lowe on becoming a naturalized citizen of the UK, in September, 1939.
3 A characterization first introduced by one of the original founders of *The New School*, Thorstein Veblen (1900, p. 261).
4 Joan Robinson (1939, p. 492), with her usual perspicacity, was the only ‘Anglo-Saxon’ – at least of the period – who realized this, in her superb review of Myrdal (1939), that the *ex ante/ex post* distinction was a way of making sure that savings, for the community as a whole, is *always equal* to investment.
Frisch’s famous Cassel Festschrift article had been published in 1933 and the coining of the word Macroeconomics, from being macro-economics, first at the hands of the great Norwegian’s unpublished lecture note at Oslo, finally as the unhyphenated word in Lindahl (1939, p. 52), came to delineate what the subject became.

Then there were Hayek’s important contributions, even if controversial, of those years of ‘High Theory’ (pace Shackle, 1967); above all, Hayek (1933a, 1933b, 1934 & 1935).5

The years of high theory were those defined, at one end, by Sraffa (1926) and, at the other end, by Hicks (1939), Lindahl (1939), Myrdal (1939), and Leontief (1941) i.e., by the theory of value, on the one hand, and Monetary Macroeconomics, and Inter-industrial economics, on the other, with Keynes (1936) and the nascent formal dynamics of growth and cycle theory7, interspersed in the years in between. Shackle’s entertaining – but doctrine historically seriously flawed – narrative was not particularly informative on the intensive development of cycle theory – both formal and informal – that was going on, during those years.

5 Of these, particularly for the purposes of this essay, Hayek (1933a & 1933b) are the most important. Both of these significant books were reviewed, not uncritically, in the then LSE ‘House Journal’, Economica, quite immediately after publication, by eminent reviewers, Bresciani-Turroni (1934) and Hicks-Gilbert (1934), respectively. In particular, the Hicks part of the review of Hayek (1933b) was a masterly introduction to Myrdal’s essay, which introduced, for the first time, the ‘ex-ante/ex-post’ machinery of the Swedes. On the other hand, Hayek (1933a) is an ‘immanent critique’ of Lowe (1926), and is the link with the equilibrium methodology of Lucasian Business Cycle theory (Lucas, 1980), on thoroughly unfounded grounds. Moreover, Lucas quotes from Hayek (ibid) quite haphazardly, mostly out of context.

6 Although this classic by Leontief was published two years beyond the formal end of The Years of High Theory, Shackle was writing retrospectively, in 1967; hence, there was a place for Leontief in his ‘years’, also because those that spanned the analysis of The Structure of the American Economy, ended with 1939. In any case it falls squarely in The Decade of High Theory!

7 Of the Harrod-type (1933, 1936 & 1939), and not of the von Neumann (1938) genre – at least from the viewpoint of formal mathematics. Two important points must be remembered: one, Harrod (1933) is the forerunner to Harrod (1936), meaning, in essence, it had been conceived before the GT was published (which may account for the fact the reference to the title of Keynes’ magnum opus is incorrect in Harrod (1936)); two, von Neumann (1938) was originally presented in Princeton, already in 1932. Many – if not all - of these ‘facts’ would have been known to many members of the Kieler Kreis, particularly to Adolf Lowe and Jacob Marschak.
Both Haberler (1937) and Tinbergen (1939) had, in the early 1930s, already been commissioned by the ILO. Hammarskjöld (1933), a forerunner to Lundberg (1937) was one foundation upon which Swedish Monetary Macrodynamics rested. And, then, there was, of course, the Kaleckian approach to Macrodynamics, even if as seriously flawed, from a strictly mathematical point of view, as Harrod (1936), whose original work, like those of the Swedes, were in a language that was not easily accessible to the Anglo-Saxons. There was also Hansen & Trout (1934).

The theory of value, here, refers to Sraffa’s critique – of the core elements of the Marshallian apparatus on competition - of the Laws of Returns under Competitive Conditions (ibid), and the ensuing imperfect/monopolistic competition ‘revolutions’ of Joan Robinson (1933) and Edward Chamberlin (1933).

As recently as less than one year ago, Martin Wolf, in an influential article in the Financial Times of 24th April, 2014 (Wolf, 2014), resurrected the Chicago Plan of 100% Money (endorsed, most importantly, by Irving Fisher):

“One of the most important such proposals [i.e., to give the state a monopoly on money creation] was in the Chicago Plan, advanced in the 1930s by, among others, a great economist, Irving Fisher. Its core was the requirement for 100 per cent reserves against deposits. Fisher argued that this would greatly reduce business cycles, end bank runs and drastically reduce public debt.”

It is little known that the Chicago Plan was subject to searching criticisms by Fritz Lehmann (1936) – and led to an interesting exchange with Irving Fisher (Fisher & Lehmann, 1936), in the pages of Social Research, exactly during the fertile first decade. That this ‘plan’ is now being advocated by the libertarian wing of the Republican party means that concerned liberal economists should have at least some ‘nodding’ acquaintance with Lehmann’s brilliant – and

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8 There was also Tinbergen (1937).
9 It would be apposite to remember the sarcastic remark, by Myrdal (1939, p. 8) on ‘the attractive Anglo-Saxon kind of unnecessary originality, which has its roots in certain systematic gaps in the knowledge of the German language on the part of the majority of English economists.’ Just imagine how much ‘worse’ it was for those who wrote in Polish and Swedish – then, and now!
10 Reflect, eventually, in those now famous – even controversial - remarks on constant returns assumptions, Keynes’ admonishment of 1928 and THE book being a ‘Prelude to a Critique of Economic Theory’, in Sraffa (1960); the starting point, of course, is Marshall (1890: 1924) – but the more proximate impulse was given by Clapham (1922).
fearless – interpretation of the lacunae in it, as well as the thin theoretical reeds on which Fisher based his defense in favour of it (cf., however, Allen, 1993).

Finally, there is Hayek (1935) and its pervasive influence in economics, at least from a methodological and epistemological point of view, if not also providing an ideological underpinning for welfare theoretic foundations for claims on the superiority of the ‘market’, over ‘planning’. There is a story to be told here, on the path from the Socialist Calculation Debate – and its origins in Pareto – to computable general equilibrium theory, and its origins in Walras; and, thereby, also the link with the two fundamental theorems of welfare economics.

The ‘bird’s eye view’ of the background, largely of The Decade of High Theory, was framed with the intention of providing a context for the nature and content of the high quality of articles on economic theory and theoretically founded (macroeconomic) policy in Social Research, during its first decade. In particular, in terms of the contributions of the remarkable latter day Kieler Kreis members. More specifically, I consider here Colm (1934, 1937), Colm & Lehmann (1936), Heimann (1934, 1939), Lederer (1935, 1936, 194111), Lehmann (1936, 1938, 1938a12), Lerner (1943, 194413), Lowe (192614,1942), Neisser (1941, 194615 ) and Marschak ( 194016,11 An appreciative summary of Emil Lederer’s intellectual life, the first part, on Lederer the Economist, was written by Marschak (whose doctoral dissertation advisor was Emil Lederer). My own teachers at Cambridge, Maurice Dobb and Richard Goodwin were fully appreciative of Lederer’s work; that the latter was, goes without saying, given that Schumpeter, too, thought very highly of Lederer.
12 I think very highly of this, Lehman’s pedagogically brilliant expository piece on Keynes’ General Theory, which was published in a booklet, but within the ‘decade’ of relevance. It has been a staple reading assignment in my courses on Macroeconomics at many Universities.
13 The Lange-Lerner alternative to the Hayek-Mises-Robbins dogma of the virtues of the market was convincingly presented in many of the chapters in this volume, which – although a PhD dissertation at LSE - was published during Lerner’s five-years as a member of the GPSS.
14 I include this important reference here, although it was neither published in Social Research, nor within the ‘decade’, but it was the work which was subject to Hayek’s immanent critique (Hayek, 1933a) and, eventually to Lucas’ faulty equilibrium, micro-founded, business cycles.
15 This is a reference to Neisser’s sympathetic obituary of Keynes, though published in Social Research two years beyond the ‘decade’.
16 Strangely, this article does not appear in Arrow’s otherwise comprehensive bibliography, in his characteristically competent and sympathetic account of Marschak’s life, and times (cf. Arrow, 1991, p. 144).
1941a, 1941b, 1941c, 1941d\textsuperscript{17})\textsuperscript{18}. The intention, from the outset, was to concentrate on the contributions of this remarkable group of latter day Kieler Kreis stalwarts solely to – and in – \textit{Social Research}, during the years, 1934 – 1944. However, I have made some ‘excursions’ beyond these years, and outside the confines of \textit{Social Research}, but always in context.

Thus, in § 2, an attempt is made to summarise the latter day Kieler Kreis contributions, in terms of the background context of \textit{The Decade of High Theory}, ‘more-or-less’. §3 is an attempt at paying homage to the triple gems of \textit{The Decade of High Theory}: Lehmann (1936), Heimann (1939) and Lowe (1942) – all of which appeared in the pages of the \textit{Social Research}. The concluding section, § 4, is written in praise of traditions, heritages and the historical sense – but, in my aged mind, it is the hymn that is to be chanted, occasionally, against the backdrop of the contributions to \textit{Social Research}, by the latter day Kieler Kreisists, in the decade of relevance..

\section*{§ 2. A Decade of Critical Economic Theory – and Policy - in Social Research}

“A general increase in production follows only when the increase in buying power has stimulated an increase in demand. The policy of public works is in accord with economic laws, except that the initiative of private enterprise for long term investments, which is now lacking, is replaced by an act of the state. .. [I]t is impossible to overcome depression by means of banking policy only or by means of a general stimulus to production. Only recovery through a program of public works, the theory of which has been developed in recent years by English and German economists, has, at least, not failed, even if it has not yet proved its efficacy.”

Colm\textsuperscript{19}, 1934, p. 96; italics added.

The foundations in sound economic theory, of the advocacy of efficient (macroeconomic) policies, was the prime, almost singular, research strategy of the GPSS 7, above all during the first decade of contributions to \textit{Social Research} (SR). Moreover, the primacy of effective

\textsuperscript{17} Marschak was a classic interdisciplinary scientist – and an eclectic economist; this reference is within the ‘decade’ but not an article in \textit{Social Research}.

\textsuperscript{18} I would like to refer to them as the ‘GPSS 7’ (it is 7, and not 8 – because Lerner, to the best of my knowledge, was not a member of the original Kieler Kreis, except in ‘spirit’ – where, of course, GPSS, in analogy with, and as a continuation of, NSSR, stands for the Graduate Faculty of Political and Social Science.

\textsuperscript{19} Gerhard Colm succeeded Adolf Lowe at Kiel, when the latter left for a Chair at the University of Frankfurt, and thus led the \textit{Kieler Kreis} for the rest of the period – till 1933 - before it was forcefully disbanded by the Nazis. Colm was one of the five economists Alvin Johnson recruited – among nine faculty – for the initial formation of the \textit{University in Exile}.
demand – and, concomitantly, a rejection of Say’s Law – was a hallmark of the work of the GPSS 7 during the relevant decade. With this came the Lowe (1926) inspired critique of a reconciliation of rational, equilibrium, foundations for any theory of aggregate fluctuations, a critique enthusiastically adopted by the latter day Kieler Kreis members, forming the GPSS 7. This also implied a monetary equilibrium was not the same as the general equilibrium of orthodox economic theory (cf. Myrdal, 1939, §5, chapter III).

But who did Colm have in mind when he referred to ‘German economists’? This becomes clear in Marschak’s typically topical (for the time) essay, in SR, in 1940 (Marschak, 1940, p. 285, fn. 3; italics added):

“The ideas [of mitigating aggregate fluctuations, employing resources as efficiently as possible, but the efficiency not necessarily determined exclusively by market mechanisms or rational, equilibrium, dynamics, and aggregate growth with full employment of resources] may be said to have originated mainly, though by no means exclusively, in the Cambridge of Keynes and Robertson, and in Sweden; younger German economists of the early thirties – in Kiel and Frankfurt .. “

Marschak’s reference to ‘younger German economists of the early thirties’, puts in context Colm’s reference to ‘German economists’ and easily helps one identify them as the members of the extended Kieler Kreis, especially after Lowe left Kiel, for Frankfurt, in 1931.

Macroeconomics, at any frontier, at any time since the early 1930s, could be characterized in terms of the way the problems of the fallacy of composition, a cumulative process underpinning the discrepancy between a ‘natural’ and a ‘money’ rate of interest, Say’s Law, and the accounting conundrums of the way Savings was brought into equality with Investment, for the community as a whole. Keynes, the Swedes and Kalecki did it one way; Hayek, another way. Broadly speaking, the GPSS 7 adhered to the former – particularly Keynes’ way.

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20 I do not subscribe to Ohlin’s absurd characterization of the work that Lindahl and Myrdal (and their followers, primarily Hammarskjöld, Lundberg and Svennilsson) developed, on the basis of Wicksell’s pioneering macroeconomic contributions, in terms of a ‘Stockholm School’. Moreover, neither Wicksell nor Lindahl, were ever based at any of the Universities in Stockholm. Above all, there was the preposterous notion in Ohlin (1937; p. 221, fn. 2; italics added) that:

“The Swedish books contain only a scanty analysis of interest theory, so I do not know to what extent [this part] is accepted by my Stockholm colleagues.”
Marschak (1941, p. 45, fn. 3), in his inimitable style, described the mechanisms and definitions that underpin the ‘eternal’ savings = investment relation, and went on to observe:

“This hare was started by Mr. Keynes, and economists, including Mr. Keynes, have worked hard to catch it.”

Keynes developed the triptych of the multiplier, the notion of the marginal efficiency of capital and a liquidity preference schedule for the determination of the interest rate – and, in the process, had to disown his own Ricardian-Marshallian heritage. The Swedes, starting from Wicksell, highlighted the cumulative process, the \textit{ex ante/ex post} mechanism and the notion of a monetary equilibrium. Kalecki, beginning from a flow accounting system, based on aggregate \textit{income} categories, had little reason to depart from the stepping-stones provided by Marx (especially in Volume II of \textit{Das Kapital}), Tugan-Baranovsky and Rosa Luxembourg.

As mentioned above, even although the GPSS 7 adhered to the Keynes-Swedish-Kalecki vision of macroeconomics, they, each in his\textsuperscript{21} own way, did so to varying degrees of agreement. Lehmann (1938a) and Lerner, in general, were unreserved adherents of Keynes\textsuperscript{22}; so was Neisser (1946), but tempered by critical disengagement in Neisser (1936), where his critique also encompassed, what he referred to as ‘the unfortunate concept of the multiplier’ (ibid, p. 459)\textsuperscript{23}. On the other hand, Lerner (1940), Marschak (1941b) and, in particular, Neisser (1941, 1941a) were less enthusiastic about the contributions of the Swedes, essentially of Myrdal (1939) and Lindahl (1939).

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\textsuperscript{21} Alas, ‘they’ were all masculine!

\textsuperscript{22} The exception, of course, is Lerner (1952) which was, mercifully neither published in \textit{SR}, nor within the relevant period! On the other hand, Lerner’s functional finance (Lerner, 1943) is a supreme exposition of the economics of the \textit{GT} in its policy aspects.

\textsuperscript{23} Although the paper acknowledges an indebtedness to ‘Dr. F. Lehmann …. for valuable suggestions’; Lehmann, of course, fully understood, and approved of, the virtues of the multiplier (cf. Lehmann, 1938a) – and comprehended also its ‘vices’, especially of ‘telescoping’ dynamics (which Keynes understood perfectly well).
Apart from an indiscriminate reference to Kalecki, together with Frisch, Tinbergen and Roos, in a brief footnote on what he referred to as ‘the so called macro-dynamic school’, Neisser (1937, p. 441), and Marschak’s thoughtful remark on the relative constancy of factor shares, even in the turbulent years of 1929-1929 (Marschak, 1941, p.44), there is no explicit consideration of a macrodynamics along the lines broached by the great Pole.

However, I feel I have to expand, at least tangentially, on what I consider to be invalid interpretations of Myrdal (op.cit) and Lindahl (op.cit), by Neisser and Marschak. I believe one reason for their ‘invalid interpretations’ was that they viewed the Swedish contribution, with its rich underpinnings in Wicksell’s monetary macroeconomics, from the standpoint of their reading of Hicks (1937, 1939). Hicks became increasingly skeptical of the IS-LM model of the GT, as Neisser – an unreserved adherent of Modigliani (1944) and, indeed also a co-author with Modigliani, who obtained his doctorate at The New School, working under Marschak, at the end of the relevant decade – became more insistent on its fertility for macroeconomic modelling, in general. This was also true of Marschak (op. cit). But both of these stalwarts of the Kieler Kreis, were more influenced by Hicks (1939) and, in this writer’s opinion, on completely untenable grounds – especially mathematically, but also conceptually and methodologically. Yet, two of the most perceptive observations on the Lindahl-Hicks connection and Wicksell, respectively, were made by Neisser and Marschak, in the context of their skeptical discussions of the Swedes. Neisser (1941a, p.452; italics added) wondered whether:

24 The other – or ‘another’ – reason is my conviction that neither of them have read and mastered the nuances and changes that were part of the final appearance of the 1939 books by Lindahl and Myrdal, from the original Swedish works by these Neo-Wicksellians, from 1924 to at least 1931 and, surprisingly, also of the German version of Myrdal (op.cit). In a personal conversation, Mrs. Gertrud Lindahl told me, in July 1983, that Hayek first invited Lindahl for a contribution to his book, but due to other pressing commitments it was, finally, Myrdal’s essay that appeared in Hayek (1933b).

25 First expressed in his remarkable Lindahl Festschrift contribution (Hicks, 1956) and, later, more focused on the role of IS-LM as an interpretation of the core message in the GT, in Hicks (1980-81).

26 Neisser (1940) is a sympathetic, but completely misguided, review of Hicks (1939). Even the title of the review, The Economics of the Short Run is nothing short of absurd, and I regret to have to say that practically every mathematical assertion is untenable, almost all of which are made somewhat pretentiously. The only saving grace in this sorry saga is that Neisser was not, as yet, a member of GPSS – although he was to become that for the last two years of the relevant decade.
“The dynamic system [outlined by Lindahl in the third study by using the concept the ‘plans’ of entrepreneurs] … is so closely akin to the line of thought in Hicks’s book (to which Lindahl refers several times in footnotes) that it would be of interest in the history of economic doctrine to know whether the two approaches were worked out independently.”

That they were ‘not worked out independently’ came to light, in a public way, only during Hicks’ typically unassuming contribution in 1987, at the Conference to commemorate the 50th anniversary of Ohlin’s Stockholm School papers (Hicks, 1991); it was made clear, by Hicks himself, that the influence was from Lindahl (and Myrdal) to Value & Capital.

Marschak (1941, p. 473; italics added), was unambiguous in his endorsement of Wicksell:

“Wicksell may have expressed himself vaguely on occasion but he has seldom talked nonsense.”

The only two caveats I would like to add are that Wicksell never – as Keynes never – ‘expressed himself vaguely’, nor did he ever – again as Keynes never did – talk ‘nonsense’. Any vagueness, or apparent nonsense, anyone reading anything by Wicksell (or Keynes 27) felt was a case of the proverbial ‘in the eye [and mind] of the beholder’ 28.

But it was Emil Lederer (1936) 29 who, in my opinion, was most tempered in his evaluation of the GT. It has the all the hallmarks of the Marxian underpinnings of a Kaleckian macrodynamics, although he is not as sure-footed in his discussion of the role of risk in Keynes’ definition of liquidity preference. Somehow, Lederer seems to have forgotten that Keynes invoked that fertile distinction between risk and uncertainty, which he had brilliantly developed in his Treatise on Probability (Keynes, 1921) 30.

27 I am motivated to add Keynes here because of the opening paragraph in Lerner (1952).
28 As Amartya Sen (1974, p. 329 ), once wrote, ‘Venerable Solow may make peculiar assumptions, but he never makes a mistake.’ This applies, pari passu, to Wicksell (and Keynes – although he may have changed his mind many times).
29 For whom Schumpeter (1954, pp. 850-1), had nothing but admiration. It was Maurice Dobb – bracketed with Lederer in the paragraph by Schumpeter (ibid) - who first introduced me to the importance of Lederer, during my early student days at Cambridge.
30 Marschak (1941, p. 53, ff.) did not forget A Treatise on Probability, but he did not seem to have understood the distinction between a partial and a total ordering in the Keynes-Ramsey differences.
Lederer, interpreting – in a Marx-Kalecki fashion – Keynes from a justifiably persistent business cycle point of view, also forgets that in Notes on the Trade Cycle (Keynes, 1936, chapter 21, p. 314), the mechanism of the cycle that is advocated is precisely that which Lederer puts forward, most cogently. The idea – formally – is equivalent to what the nonlinear Keynesian theorists of endogenous fluctuations later called a relaxation oscillation; the kind of oscillator which allowed for a non-repetitive asymmetry between the upturn and downturn phases of the cycle – sudden in the downward direction, slow, and almost smooth, in the upward direction. Lederer (ibid, p. 487), in a most Kaleckian way, ends his thoughtful reflections on the GT, invoking Keynes’ famous closing lines on ‘ideas, not vested interests’, by a very appropriate, typically Kieler Kreisian way:

“The idea put forward by Keynes would entail a revolution in power and property, but we can hardly believe that mankind will accept a new economic and social set-up by persuasion.”

One of the most important pre-GPSS 7 decade of contributions to SR was Lowe (1926) – on the impossibility of reconciling a theory of the business cycle with any concept of rational equilibrium dynamics. Hayek (1933), as I have suggested, should be considered a Myrdalian Immanent Critique of this classic by Lowe. The current newclassical, equilibrium, approach to business cycles, descends directly from these classics by Lowe, and then Hayek. Strangely, the only mention of this classic by Lowe, by any member of the GPSS 7, in any significant contribution to SR in the relevant decade, is in Neisser’s review article, Neisser (1937), on Harrod’s flawed, but profoundly stimulating – even if controversially so – approach to The Trade Cycle (Harrod, 1933, 1936). Before I begin to conclude this section with two of Neisser’s contributions to SR, this one on Harrod, and another on Hayek’s theory of industrial fluctuations (Neisser, 1934), I would like to make three personal observations.

First of all, my own vision of a theory of trade cycles\footnote{I have always preferred the phrase trade cycle theory to the currently ‘fashionable’ business cycle theory.} was heavily, even decisively, influenced by the direct teaching of Goodwin, Kaldor and Hicks – the undisputed pioneers of an endogenous, nonstochastic, nonlinear, theory of aggregate fluctuations. Secondly, even apart from this, the direct inspiration, teachings and interchanges with – and of – Richard Goodwin
were most important in my formative years as a research student and, eventually, a teacher and contributor to the mathematics of nonlinear macrodynamics. Finally, I owe a considerable debt to my collaborative work and research on cycle theory, from a Harrodian standpoint, with Stefano Zambelli and Ragu Ragupathy (cf. Ragupathy & Velupillai, 2012 and Velupillai, Ragupathy and Zambelli., 2013, for example).

In the latter connection, I was introduced both to Lowe (op.cit) and to Harrod (op.cit) via Goodwin’s lectures at Cambridge, personal conversations with him and in a series of letters, exchanged with him, one of which is particularly important for the ‘light’ it shed on the way he felt he had to model the cycle in terms of nonlinearity. It was this letter, almost more than anything else, that was instrumental in my attempts to understand the nonlinear, endogenous, nonstochastic way of modeling aggregate fluctuations – and, above all, introduced me to Tinbergen (1937a).

But Richard Goodwin also repeatedly emphasised, both in his lectures and in personal conversations, of the importance of Marschak’s seminars on trade cycle theory, during his Oxford research student days (and just before Marschak moved from the Oxford Institute of Statistics, of which he was the Director, to become yet another of the Kieler Kreis stalwarts who formed the GPSS 7).

32 In Velupillai (2015), my vision, much conditioned by the above pioneering trio, in addition to that by Ralph Abraham, Richard Day and Otto Rössler, also as teachers and friends, and Stefano Zambelli – both as a colleague and a friend – and Ragu Ragupathy, as a friend and former student, is described in considerable detail.

33 I have quoted from this letter, most recently in Velupillai (2015a).

34 This is most poignantly reflected in Harcourt (1985, p. 415; italics added), as follows: “This conviction [that the ‘real’ behavior of the economy … would be wave-like, quite without any help from the banks] was acquired in [Goodwin’s] last year at Oxford when he went to Jacob Marschak’s ‘superb seminar’ where they went through Tinbergen’s survey of business cycle theory.” Yet, I feel I must record my deep dissatisfaction with Marschak’s mastery of nonlinear dynamics – which was practically non-existent (anyone interested in a substantiation of my melancholy assertion could do no better than study, seriously, the correspondence between Harrod and Marschak, all of which are now available in the Harrod Papers, in the years and months prior to the publication of Harrod (1939)).
With this background in place, it is easy to see why I am deeply and seriously displeased with Neisser’s interpretation of Harrod’s theory of the trade cycle, as comprehensively given in Neisser (1937). It is not a model of clarity, although he ‘accuses’ Harrod (1936 – of which his is a review article, (I do not believe he had access to, or even knew of, Harrod (1933)) - of being ‘obscure’ in his explanation of what Neisser calls a ‘monistic-theoretical approach (ibid, p. 440)\(^{35}\).

It may seem churlish to interpret critically a serious and detailed review of a flawed work, as Harrod’s was, but it is my view that Neisser’s – and Marschak’s – deficient mastery of nonlinear (\textit{planar}, at this stage of the development of the subjects) dynamics which was the prime cause of his – their - misguided reading of the economics of the trade cycle – whether monistic-theoretical, or not – in Harrod (1936). That Tinbergen’s (1937) ‘brutal review’\(^{36}\) of Harrod’s book\(^{37}\) was as misguided as Neisser’s is no consolation to any theorist with an applied, policy oriented, stance.

Neisser’s denunciation of the ‘monistic-theoretical’ approach to trade cycle modelling, and extolling the virtues of the ‘pluralistic theoretical-instrumentalist’ approach to theorising about industrial fluctuations, is – in this writer’s opinion (which is easy to substantiate) – entirely based on the methodological arguments presented in his own ‘obscure’ vision for an understanding of

\(^{35}\) Neisser’s classification of trade cycle theories – or theories of industrial fluctuations – are based on the tripartite scheme of the \textit{empirical or correlation}, the monistic-theoretical and the \textit{pluralistic-theoretical or instrumentalistic} approaches. His own preference – used also in writing Neisser (1936a), which was cogently criticized for being ‘obscure’ by Mosak (1937) – is for the pluralistic-theoretical or instrumentalistic way of constructing any model of the trade cycle, defying any reliance on an Occamist principle in theory. Essentially, any and all things ‘go’, in Neisser’s world of trade cycle theorising – although I must admit it is admirable that he dismisses the ‘empirical or correlation’ approach to modeling, fully in accordance with the general critical outlook of the \textit{GPSS 7} towards ‘measurement without theory’.

\(^{36}\) As referred to by Goodwin in his letter of 17\textsuperscript{th} June, 1985, to this author, quoted in Velupillai (2015a).

\(^{37}\) There is no evidence whatsoever that Neisser was aware of Tinbergen (1937) – and, a fortiori of the correspondence between Harrod and Tinbergen. I am convinced that Neisser would have harnessed Tinbergen’s unusual ‘brutality’ to reinforce his own critique of Harrod, without understanding an iota of the mathematics of dynamical systems.
cyclical phenomena in an international context (Neisser, 1936a – despite recent attempts by Trautwein, 201538, to lift the veil of obscurity that hangs over it).

I think I owe the ‘interested reader (that elusive creature)’, in the wry phrase used by John Kelley (1955, p.vi), an explanation of why Neisser failed to see that there was an implicit, inevitable, nonlinearity in Harrod (1936), a recognition of which is what distinguished Goodwin, Hicks (and Ichimura (1955), from Tinbergen (Marschak, and many others). This explanation is a tribute to Neisser’s perceptive understanding of part of Harrod’s (weak mathematics) and methodology. Neisser’s critique of Harrod’s ‘monistic-theory’ of the trade cycle is supposed to be based on ‘the second part of [Harrod (op.cit)]’ (Neisser, ibid, p. 442; italics added):

“My [i.e., Neisser’s] presentation in the text is based on the second part of [Harrod’s] book, which may be supposed to offer the theoretical explanation for the observation concerned.”

‘The observation concerned’ are listed as five items, of which the fourth and fifth are (ibid, p. 442; italics added), respectively:

“[I]nvestment stops, income shrinks, investment for replacement is postponed, income shrinks cumulatively; [and] investment for replacement is taken up a cumulative upswing is started.”

Neisser goes on to observe, most perceptively (ibid, p. 442), and correctly in this writer’s opinion:

“In the first part of his book Mr. Harrod does not explain by theoretical analysis the cumulative process of steps four and five.”

The nonlinearity that Neisser ‘misses’ is due to the observation that ‘investment stops’ – and that was where the famous ‘ceiling’ of Hicks (although it was Harrod who first introduced the concept) and the ‘flexible accelerator’ of Goodwin, generalized and derived the dynamic multiplier-nonlinear accelerator model of the trade cycle. Harrod’s ‘step five’ (à la Neisser) is what became, at the hands of Goodwin and Hicks, autonomous investment (later abandoned by

38 Which is, itself, seriously marred, not only by an absence of the obvious international aspects of the business cycle considered by Hammarskjöld – a foundation upon which the Swedish approach to economic dynamics was built – but also by not emphasizing the so-called rational, optimization-based, micro-founded, open economy macroeconomics of the latter day theorists (I nearly wrote ‘saints’- there is no risk they would be mistaken for ‘scholars’!).
the former, when he learned how to introduce Schumpeterian innovations in a Rössler 3-dim model of the dynamics of industrial fluctuations – since the ‘accelerator is dead’ at the bottom of the cycle).

Hicks incorporated the ‘stops’ via a piecewise liner model of 2-dim dynamics; Goodwin introduced the flexible – nonlinear – accelerator, in conjunction with the dynamic multiplier – as did Hicks – to generate, eventually, a model of aggregate output fluctuations, without the Hicksian ‘floor’ (again, because the ‘accelerator is dead’ at the trough).

Neisser did not have the ‘theoretical technology’ of nonlinear mathematics to make formal sense of Harrod’s model; nor, perhaps, the will to learn the mathematics that was necessary to achieve this important ‘generalisation’. That Marschak also failed must, surely be due to his non-mastery of nonlinear mathematics, and a lack of understanding of the different possibilities of linear vs. nonlinear dynamics. 39

My reading of Neisser’s interpretation of Hayek’s theory of industrial fluctuations (Neisser, 1934) is, alas, as critical as his unsympathetic review of Harrod. This time, however, my skepticism is not independent of the thoroughly inappropriate theoretical underpinnings of Hayek – and, thereby also Neisser – of any notion of the average period of production 40 to

39 Paul Samuelson (1974, p. 10; italics in the original) was, as always, completely candid on this: “[B]y 1940, Metzler and I as graduate students as Harvard fell into the dogma - … - that all economic business-cycle models should have damped roots. … [W]hat was so bad about the dogma? Well, it slowed down our recognition of the importance of non-linear autorelaxation models of the van der Pol-Rayleigh type, with their characteristic amplitude features lacked by linear systems.” Neither Neisser, nor Marschak, ever ‘recognised the importance of non-linear auto-relaxation models of the van der Pol-Rayleigh type’. This is why Neisser made the inappropriate – indeed, irrelevant – observation about ‘periodically’ (ibid, p. 443). Incidentally, the Goodwin’s Harrodian dynamics, in a Keynesian Macrodynacic model, did ‘recognise the importance of non-linear autorelaxation models of the van der Pol-Rayleigh type’.

40 As cogently argued by Sraffa (1960, pp. 44-5). Neisser’s gratuitous reference to Marschak’s mathematical definition of the average period of production (ibid, p. 454; Marschak, 1934) is evidence of the fact that neither understood the necessity of much more powerful mathematics than they had at their disposal. Moreover, the untenability of a ‘correlation’ between an ‘average period of production’ and any notion of ‘length’ in an investment process, as discussed by
measure capital, as well as the peculiar absence, in Neisser (ibid), of any reference to Knight’s powerful, capital theoretic, critique of any Austrian approach to this thorny field (not even to Knight, 1934).

However, let me end on a positive note! Every member of the latter day Kieler Kreis, as the GPSS 7, contributed copiously to both a theory of industrial fluctuations and policy measures to mitigate the undesirable effects on employment. Many of them also made important forays into a theory of the possibilities of capitalistic development – as distinct from any narrow concept of aggregate growth; in this they developed Schumpeterian notions of innovation, both critically and in their respective own ways. It is unfortunate that hardly any of this saw the light of day in the pages of SR (except, perhaps, Lederer, 1935), in the relevant decade.

§ 3. Appreciative Notes on the Triple Gems

“For the simultaneity of several distinct themes is not something that is unique to music. Typical things in general do not belong in one place alone; rather they are rooted anywhere and everywhere, embedded organically.”

Paul Klee

The triple gems in SR, in the relevant decade, from the pens of the GPSS 7, were those by Lehmann (1936), Heimann (1939) and Lowe (1942) – not only from any Whig history point of view. Lehmann’s essay was about a topic of ‘current’ monetary macroeconomic policy interest – ‘current’ then, 1936, and now, 2015. Heimann’s is a contribution to a topic at the frontiers of applied economic theory, but is rarely, if ever, referred to – in any context. Lowe’s classic is simply never mentioned, despite its fundamental importance to developments at the frontiers of economic theory. The Heimann and Lowe papers are on themes that should be related, but are not – the former, on what has come to be called (inappropriately, in this writer’s opinion) the socialist calculation debate; the latter on both formal general equilibrium theory and generalised versions of tâtonnement, or the implicit dynamics of the law of supply and demand.

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Böhm-Bawerk, and used by both Neisser and Marschak, shows also their lack of attention to Fisher (1907), and the acute critical remarks therein.

41 I am not referring to the Triple Gems of Buddhism (the Buddha, the Sangha and the Dhamma), nor the triple ethical jewels of Jainism – nor of any other religious triptych.

David Gale’s classic on *The Law of Supply and Demand* (Gale, 1965) was one of the pioneering contributions to static mathematical economics. It was squarely in the tradition of the ultra-static Arrow-Debreu approach to Walrasian equilibrium theory. There were other traditions of static mathematical economics, developing ostensibly different strands of the law of demand and the law of excess demand, respectively; the former, exploiting similarities with a law of production in the aggregate, was very much a product of Werner Hildenbrand’s research, in the early 1980s (Hildenbrand, 1983). The latter, chronologically the earliest, is what I call the excess-demand tradition in static mathematical economics, pioneered by Wald (1933-34). A related, much hyped version of the excess demand approach to static mathematical economics is the so-called Sonnenschein-Debreu-Mantel theorem (Sonnenschein, 1973). This much maligned result can be found in the earlier work of Dimitriev (1902; 1974) and Clower (1965)\(^4\).

It is a pleasure, however, to acknowledge a *Kieler Kreis* priority here – i.e., over the conventional acknowledgements to Wald! It was Neisser (1932) that catalyzed Menger’s *Koloquiums* into an action that resulted in the Wald results. At the time he derived, and published his results in the ‘house journal’ of the Kiel Institute of World Economics (on the eve of its dismantling by the Nazis), the *Weltwirtschaftliches Archiv*, Neisser was Lowe’s deputy director of what I have called the *Kieler Kreis*. Neisser does not mention his priority over Wald in his review of Hicks’ *Value & Capital* (Neisser, 1940, p. 35 – but to Wald (1936)), but he does so in Neisser (1968, p.1199; italics added):

“The question of ‘existence’ was attacked no earlier than 1932, when I drew attention of theorists to the fact that mathematical solutions to an equation system may be economically meaningless, since negative (or complex) prices and input magnitudes do not exist.”

What was, in Walras, Marshall and Pareto, each in his own way, the issue of solvability, became in its modern formalization a question of non-constructive, uncomputable, ‘proof’ of existence – not of non-negative rational or integer-valued prices and quantities; but of merely non-negative

\(^4\) Dimitriev’s result was for the analogous excess profit function; Clower’s result was a byproduct of his thought experiment on a dual-decision hypothesis. This is most likely the reason why Sonnenschein’s most accessible expository version of the S-D-M theorem, appeared in the then *Western Economic Journal*, which was edited by Clower! One has to remember, I suppose, George Stigler’s acid observation (Stigler, 1966, p. 77): ‘If we should ever encounter a case where a theory is named for the correct man, it will be noted.’
prices and quantities. This was a handmaiden to the kind of ‘law of supply and demand’ all of
the modern worthies – from Arrow & Debreu, via Gale, Nikaido, McKenzie and Scarf, all the
way to the purveyors of computable general equilibrium theory and stochastic dynamic general
equilibrium theory.

This is where the richness of Lowe’s analysis of the law of demand offers new hopes. Lowe
analyses the consistency of microdynamics – of prices and quantities – with a macrodynamics of
the interaction of supply and demand. He begins, ab initio, so to speak, with a dynamic definition
of the law of demand – its tendencies towards one or another ‘attractor’ (not all of them
mathematically characterisable on the basis of any known economic theory, or, indeed, of
mathematics). His is a world of many splendoured tâtonnements of the Walrasian, Paretian,
Marshallian and even Edgeworthian types, sometimes in combination, too\textsuperscript{44}.

Surely, any teaching of microeconomic foundations of macroeconomics, emphasising dynamics,
has no better place to start than this absolute classic by Lowe – which, to the best of my
knowledge, has been comprehensively neglected in every kind of teaching, whether orthodox or
heterodox.

The other side of the Lowe coin is that which is elegantly discussed, quite as completely as I
know how, by Heimann using the framework of the ‘story’ of what is now referred to –
inappropriately, in my opinion – as the Socialist Calculation Debate (SCD). Heimann’s elegant
and comprehensive survey of the literature on the SCD pays homage also to a neglected classic
by the young Jacob Marschak (1924)\textsuperscript{45}, before his Kieler Kreis days – as well as, of course, to
Lerner (1944), in his GPSS 7 period, but to the earlier arguments put forward by this fascinating,
even mercurial, expositor of Keynes.

\textsuperscript{44} I had, never, in almost forty five years of academic economics, ever read any reference to
Samuelson as ‘Paul. N. Samuelson’ (Lowe, p. 450, f.n., 27)! The footnote in its entirety is most
relevant in delineating Lowe’s approach to formal dynamics and that of Samuelson (and
Marschak). The similarities and differences between Samuelson’s formal dynamics and Lowe’s
more microeconomic and behavioural dynamics is made crystal clear in this footnote.
Unfortunately, it is Samuelsonian dynamics that was embraced by the profession – even when,
ostensibly, it was ‘denied’ by the Newclassical theorists.
\textsuperscript{45} It is, however, listed in Appendix B of Hayek (1935).
One of the most telling observations by Heimann is his convincing demonstration of the
doctrine-historical and conceptual distortions by Mises and Hayek. For this enlightenment alone,
his beautiful survey – it is neither a survey, nor just a cataloguing of the ‘literature’ on SCD - is
an independent and critical analysis of the eminently theoretical possibilities of setting up a
practicable scheme of mechanisms to determine the dynamics of equilibrium of supply and
demand. It is, in this sense, but not only in this particular sense, the obverse of the even more
neglected classic by Lowe (1942).

I come, finally, to Lehmann (1936), Fisher & Lehmann (1936) and Fisher (1936). It will not pass
unnoticed that the GT was published in February, 1936, with its ‘revolutionary’ theory of a
monetary production economy. Lehmann had, quite immediately, mastered the message of the
GT and was an unreserved admirer and adherent of the framework and policy proposals
contained in Keynes’ magnum opus. It is, therefore, not at all surprising that he was theoretically
skeptical of the Chicago Plan, particularly in the variant endorsed by Fisher (ibid). I consider it
one of the triple gems of the GPSS – 7 contributions to SR in the decade of relevance particularly
because the proposal itself, of the Chicago Plan, is now a pet project of the libertarian wings in
the Republican Party of the US. The paradox here is that an adherence to this plan limits the
much vaunted freedom of the market (and, thereby, of commercial banking) and extolls the
virtues of centralized banking!

As usual, the libertarians would like to have the cake – and eat it (cf., Block & Barnett, 2005). I
have never found one consistent, skeptical, argument, whilst also cogently summarizing the core
elements of the Chicago Plan, superior to this youthful classic by Lehmann. I feel I could easily
conduct a seminar on monetary macroeconomics, using only the classics on this topic by Lederer
(1937), Lehmann (1935, 1936, 1938 & 1940) – together with just the books by Wicksell (1898),
Keynes (1936) and Myrdal (1939). I do believe they, and their contents, are completely
consistent with the philosophy, methodology and epistemology of sound theoretically based
policy oriented monetary macroeconomics, of the kind first fashioned by the Kieler Kreis, then
developed in the pages of SR, by the GPSS 7.
§ 4. Traditions, Heritages and the Historical Sense

“Yet if the only form of tradition, of handing down, consisted in following the ways of the immediate generation before us in a blind or timid adherence to its successes, ‘tradition’ should positively be discouraged. .. Tradition is a matter of much wider significance. It cannot be inherited, and if you want it you must obtain it by great labour. It involves, in the first place, the historical sense …; and the historical sense involves a perception, not only of the pastness of the past, but of its presence; the historical sense compels a man to write not merely with his own generation in his bones …. This historical sense .. is what makes a writer traditional. And it is at the same time what makes a writer most acutely conscious of his place in time, of his contemporaneity.”


The noble traditions of the Kieler Kreis had, as its heritage – even although under tragic circumstances – the economics, political philosophy and methodology of the GPSS 7. With a sure-footed historical sense, they are the dominant research, teaching and epistemology of what I would like to refer to the NSSR 7: John Eatwell, David Gordon, Duncan Foley, Ed Nell, Anwar Shaikh, Duncan Foley and if I am allowed to be presumptuous, myself as the direct descendents of the GPSS 7, all of us respecting the now distant traditions of the original Kieler Kreis.

The monetary macroeconomics of Ed Nell, the dynamics underpinning the adjustment of Social Accounting Matrices of Lance Taylor, the Political Economy of Anwar Shaikh, the dynamics of general equilibrium theory of Duncan Foley and my own approach to the theory of macrodynamic fluctuations, with a basis in a rich history of economic, political and philosophical doctrine which all of us and the departed four, Eatwell, Gordon, Nell and Taylor, have developed along the noble traditions broached by the Kieler Kreis. This research and teaching makes Eliot’s wise observations fully relevant for the courageous graduate students who choose to come to The New School for enlightenment and a way to command new tools and concepts to meet the challenges of a virulent orthodoxy.

Much has happened in economics and its frontiers keep shifting in all sorts of unpredictable and unfathomable ways. Understanding the contours of a dynamic capitalism requires, now, a mastery of new and evolving theoretical technologies. One of the modern pioneers of what is,
unfortunately, called chaos, has outlined, with admirable conciseness, the task facing an analyst of spatial and temporal patterns\textsuperscript{46}:

“Sometimes the phenomena being described are things that appear to have random arrangements in space rather than random progressions in time, like wildflowers dotting a field. On other occasions, the arrangements or progressions are simply very intricate rather than seemingly random, like the pattern woven into an oriental rug.”

For reasons that are not clear to me, neither the Kieler Kreis, nor the GPSS 7, adhered to the growing dominance of mathematics in economics. The relevant decade was, after all, when almost all the theoretical technologies of mathematical economics, macrodynamics, game theory and the theory of industrial organisations came into being – and advanced almost unchecked. It was also the period when the systematization, on the basis of solid theory, of national income flow accounts and balance sheets, respecting stock-flow consistency, became a major research enterprise. Then, there was the burgeoning filed of econometrics – after all, the works of Frisch, Tinbergen and Haavelmo were very well known to the GPSS 7!

Why, then, did the GPSS 7, and the Kieler Kreis before them, shun these new developments, in favour of what was even then considered outdated methodologies? Perhaps it was because of the intended readership of Social Research; or it may have been the intrinsic propensities of the individual members of the GPSS 7.

Whatever the reason, the NSSR 7 seem to have a full mastery of the theoretical technology at the frontiers of economic theory; they – we - certainly understand the dynamics underpinning the adjustment mechanisms of national and regional accounts. This implies new demands on the courageous students who join them in challenging the tools, concepts and frameworks of orthodoxy. I believe they will succeed – but they will need to pay close attention to the words of Edward Lorenz.

\textsuperscript{46} In his most readable book on Macroeconomics (Leamer, 2009) referred to the student of economics as pattern-seeking, story-telling, animals. The telling of plausible stories, not always necessarily true ones, on the basis of observations of intricate spatial and temporal patterns, generated by the contours of capitalistic dynamics, is the art of developing good theory, on the basis of which enlightened policies can be based. This was the mission of the original Kieler Kreis; it was continued by the GPSS 7 – and it is nobly carried out by the NSSR 7.
Independent India’s first Vice President, and second President, the philosopher-statesman, Sarvpalli Radhakrishnan reflected wisely, in his *Hibbert Lectures* at the University of Manchester, in 1929, on an Idealist View of Life (Radhakrishnan, 1932), when he noted (italics added):

"If we are not to lapse into individualistic rationalism and ultimate negation, if we are not to be led astray by our wandering whims, if our personal intuitions are to be guided by the accumulated wisdom of the race, only tradition can help us. It takes centuries of life to make a little history, and it takes centuries of history to produce a little tradition, and we cannot lightly set it aside ..... *But loyalty to tradition is one thing, and bondage to it quite another.*"

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