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THE REALITY AND MYTH OF BUSINESS CYCLES

THE NATURE AND REPRESENTATION OF SHORT–RUN ECONOMIC FLUCTUATIONS

PhD.

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December 1995

Prosperity is not without many fears and distastes;
and adversity is not without hopes and comforts
..........Francis Bacon (1561-1626)

Abstract

Business cycles as a distinct type of economic behaviour originated in the severe instabilities experienced by European banking systems in the nineteenth century. A large number of divergent explanations of the phenomenon were proposed by contemporary economists; but by 1900 a consensus had emerged about how they were propagated, if not about causes. Business cycles were thought to be induced by disequilibrium relations among real and monetary variables. This quantity-theoretic view was formulated as self-sustaining sequences of phases of prosperity, recession and depression in 'general conditions' by Wesley Mitchell in 1913. Mitchell, with Arthur Burns, attempted to document these 'comovements' between the wars, but found that actual behaviour was complex and that all episodes were effectively unique. Their results were taken as 'proof' of the comovement hypothesis by later economists, and most current research assumes such behaviour. Econometric research proposes an *a priori* decomposition into 'trend' and 'cycle' on the identifying assumption of separate data generating processes for each component, following the standard interpretation of Burns and Mitchell. Most empirical studies find that such decompositions either are rejected by the data or else fail to capture important empirical properties. Theoretical research assumes comovements to be the effects of random shocks propagated through moving average processes. This model is not in general supported empirically owing to the difficulties in identifying shocks from time-series data. The current literature mostly describes growth-rate rather than levels fluctuations, and models are increasingly being formulated explicitly in terms of growth. Evidence from undecomposed time series in levels suggests that the comovement hypothesis is not supported and further, that timing relations among economic variables are not stable.

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CHAPTER 1

INTRODUCTION: THE PROBLEM OF 'BUSINESS–CYCLE' ANALYSIS

In the late summer of 1945 reports of impending economic crisis began to appear in the national press in Britain and the United States.[1] Government departments were confidently predicting, so it was said, ten million unemployed in the United States by the winter, as personnel were released from the forces. In the event this doomsday forecast was not fulfilled, though output did fall in the US by about one quarter in real terms from its wartime maximum and more than ten million men were actually demobilized from September 1945 to the end of 1946. The chronic mass unemployment of the 1930s, which everyone had reason to fear, did not return, and economic growth quickly resumed, stimulated by rearmament and sustained by postwar reconstruction. It then became fashionable to claim that the short-run economic fluctuations known as 'business cycles' had been 'conquered', the proof being the strong growth experienced by most national economies up to the late 1960s. Such a fashion was not new: during three of the last eight decades — the 1920s, 1950s and 1980s — such claims have been made, usually in support of an ideological position. The capitalist economies, so the argument goes, will by rational allocations of resources provide growth sufficient to prevent economic collapses. Any mild fluctuations experienced along the way are the natural market adjustments of a dynamic equilibrium. These three decades all coincided with governments of the Right; and two of them were characterized by speculative bubbles ending in stock market crashes. Business–cycle research, which was ignored in the expansive periods, attracted fresh attention in the aftermath of these events.

The phrase 'business cycles', as denoting a distinct class of economic behaviour, was first used by Wesley Mitchell in the early years of the current century. The empirical findings of his seminal work with Arthur Burns at the National Bureau of Economic

[1]See, for example, the front page of the *Observer* VJ–Day edition.

Research between the wars have been taken as the point of departure in most subsequent research. These results appeared in *Measuring Business Cycles* [1946] (MBC), summarized in the now-famous 'definition' on page 3: business cycles are continuous sequences of 'recurrent but not periodic...expansions occurring at about the same time in many economic activities' followed by contractions of similar character, with durations of approximately one to twelve years. The definition, however, was not intended by its authors as more than a working hypothesis. Mitchell was in fact primarily a theoretician, a point which was later forgotten, and his view of 'business cycles' as self-perpetuating sequences of expansions and contractions, each phase being 'caused' by its predecessor, arose from the history and intellectual climate of the nineteenth century. Mitchell's quantitative methods were the means of formulating and verifying this theory, rather than an end in themselves; yet despite an intensive search over 60 years, he never found unambiguous empirical evidence of the general 'comovements' among 'many variables' predicted by the hypothesis. Indeed, he emphasized in all his writings that short-run fluctuations are very complex, and that interpretations other than business cycles are available to characterize and explain them: fluctuations in growth patterns, the effects of exogenous shocks, of structural changes, etc.

For one reason or another these caveats were largely forgotten, and late twentieth century perceptions are chiefly founded on the 'empirical evidence' of MBC, a work originating in ideas, methods and data originating in the years before 1914. Behavioural assumptions evident in the current literature thus rely on an hypothesis formulated as early as 1913 and considered definitive by 1946. They furthermore ignore the extraordinary circumstances of the US economy in the period 1918-1939 which conditioned it: the occurrence of three exceptionally severe downturns in levels of output and a collapse of growth (see Figures 1.1 and 1.2).[2] The latter had not been experienced previously, nor has it occurred subsequently; for as authorities such as Blanchard and

[2]These data are taken from the Balke and Gordon estimates given in Appendix B of R.J. Gordon (ed.) [1986].

Fischer [1989] have pointed out, the dominating feature of industrial economies in the past 200 years has been persistent growth.

Economic historians, with their interest in the nineteenth century, have always been concerned with short-period instabilities; but with the renewed disturbances of recent years there has been a revival of interest among macroeconomists and econometricians. The original econometric approach to the modelling of business cycles — moving averages of random shocks — was proposed by Slutsky [1927] and Frisch [1933]. So persuasive were these propagation mechanisms that, according to Blanchard and Fischer, they are now preferred by macroeconomists to Mitchell's idea of self-sustaining phases. However, although 'simple linear propagation mechanisms appear to [explain] the stochastic behaviour of economic variables...beyond this shared general framework there is little agreement as to the main sources of disturbances'.[3]

Business-cycle theory has been subject to much revisionist thinking since Mitchell's death in 1948. His hypothesis was re-interpreted by the Cowles Commission in the 1950s as a single, unobserved, common stochastic variable driving all macroeconomic fluctuations. In the 1970s Lucas proposed a 'rational expectations' model of dynamic equilibrium, subject to Slutskyian exogenous shocks, as a theoretical explanation of the empirical 'findings' of MBC; and this has been followed by a large theoretical literature known as 'real business cycles' (RBC). 'Neo-Keynesian' macroeconomists have disputed the RBC understanding of the nature and persistence of these shocks. Among econometricians Stock and Watson [1988, 1989, 1991, 1992] and Hamilton [1989] have proposed probability models as formalizations of the 'indicator variables' which supposedly track and anticipate the dates of cyclical 'peaks' and 'troughs'. Stock and Watson, while taking the Burns and Mitchell definition as 'primitive', argue that the 'comovements' in many activities 'can be captured by a single underlying, unobserved variable [which] in the abstract...represents the general "state of the

[3]Blanchard and Fischer [1989] p. 277.

economy"'.[4] Hamilton considers business cycles to be 'the broad-based swings in economic activity'. While not directly alluding to Burns and Mitchell, he compares his results with the 'reference cycle' chronology compiled monthly by the NBER based on Mitchell's original methods from the 1930s which supposedly represents a set of dates when 'business cycles [reach] troughs and peaks'[5]. Recent econometric studies of the observed asymmetries between amplitudes and durations of expansions and contractions also quote Burns and Mitchell as authority for the empirical behaviour of business cycles. Most macroeconomists accept the 1946 definition as 'proved'. Lucas, for example, while differing in certain respects from Mitchell in his theoretical views, asserts in a 1977 essay that 'business cycles are all alike'; and again in 1981, that Burns and Mitchell found 'the similarity of all peacetime cycles with one another'.[6] The RBC literature has always assumed that the 'comovement' behaviour was established as 'fact' in MBC. Historians, on the other hand, have become sceptical of any such homogeneity.

Burns and Mitchell did not identify 'reference' with 'business cycles', but rather argued that any one-to-one correspondence must be an empirical issue. They never claimed to have found satisfactory evidence to resolve this issue, mainly because of the inadequate coverage of the available time-series data; and hence any argument relying on an assumption of such a correspondence from MBC is circular. Moreover they always insisted that business cycles are diffused throughout the economy. If Mitchell's 'many activities', or 'comovements', criterion is thus to be satisfied, then the significant fluctuations (in both the general and the statistical sense) in observed time series will either be in phase or else have their phase shifts predictable. Such conditions cannot be assumed, as some of the current literature seems to do.

[4]Stock and Watson [1989] p. 1.

[5]Hamilton [1989] p. 352. The 'reference cycle' is defined in MBC p. 24.

[6][1977] p. 10; [1981] p. 274.

The understanding of historical business cycles thus appears to be at the least incomplete, if not in error, relying as it does on empirical results now nearly 50 years out of date and upon time-series data sets that are unrepresentative, or inaccurate, or both. In order to put recent research in its proper context, as well as to assess its value, it is essential to re-examine the Mitchell hypothesis with the perspective of half a century of additional theoretical research and time-series data. The following specific questions are suggested by the literature:

1. How did the experience of the nineteenth century condition later theoretical, historical and statistical business-cycle research?
2. What were Mitchell's original intentions and objectives and how did they influence his quantitative research programme?
3. What were the results of Burns and Mitchell's investigations?
4. How have these ideas been interpreted subsequently? Do these interpretations accurately reflect the authors' views? How have they affected later research?
5. What has been the contribution of developments in the econometrics literature?
6. What empirical methods are available for identifying 'business cycles' as distinct behavioural types? How objective are these methods? What assumptions are required? What scope is there for improving them? What light does the analysis of modern time-series data shed on the problem? How successful are the theoretical explanations?
7. What, therefore, can be said about the nature of short-run economic fluctuations? Is the comovement hypothesis supported empirically? Is there a relationship between short-run and long-run behaviour? What are the implications for policy?

These questions imply a reexamination of the historical record for quantitative evidence; but they also suggest the need to re-examine the historical and theoretical prior perceptions that influenced Mitchell's research programme, and to assess whether the increased information available since 1946 alters the historical as opposed to the econometric view.

This dissertation is organized into four substantive chapters in an exploration of these issues. Chapter 2 discusses the cultural background in the period up to 1914, the prolific explanations of business cycles engendered in this period, their eventual coalescence into a single consensual explanation and its effect on Mitchell's approach. Chapter 3 analyses the work of Mitchell and the NBER, how it has been interpreted and followed, and offers new interpretations of its results. Chapter 4 assesses the econometrics literature on business cycles and presents the results of a Monte Carlo study of short-run 'cyclical' asymmetries and their link with long-run secular behaviour. Chapter 5 implements an amended algorithmic approach to turning-point analysis and reports the results of its application to US and UK statistics. Chapter 6 discusses the general conclusions of the study and their implications for future 'business-cycle' research.[7]

[7]Statistical tables are placed in the text of each chapter. Because of their extensive nature, charts and graphs appear in appendices to each chapter.

Figure 1.1. US Quarterly Real GNP, 1875-1983

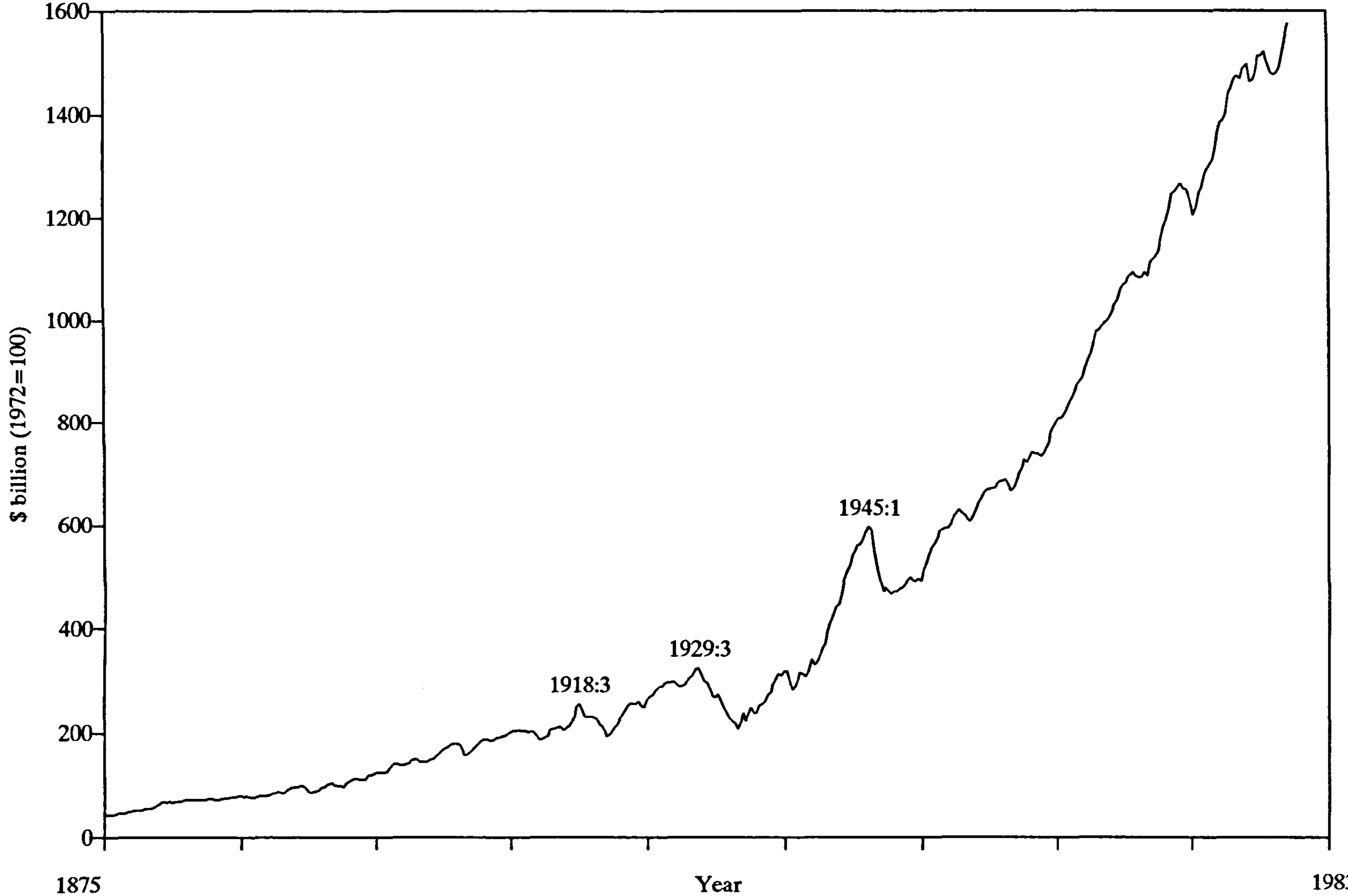
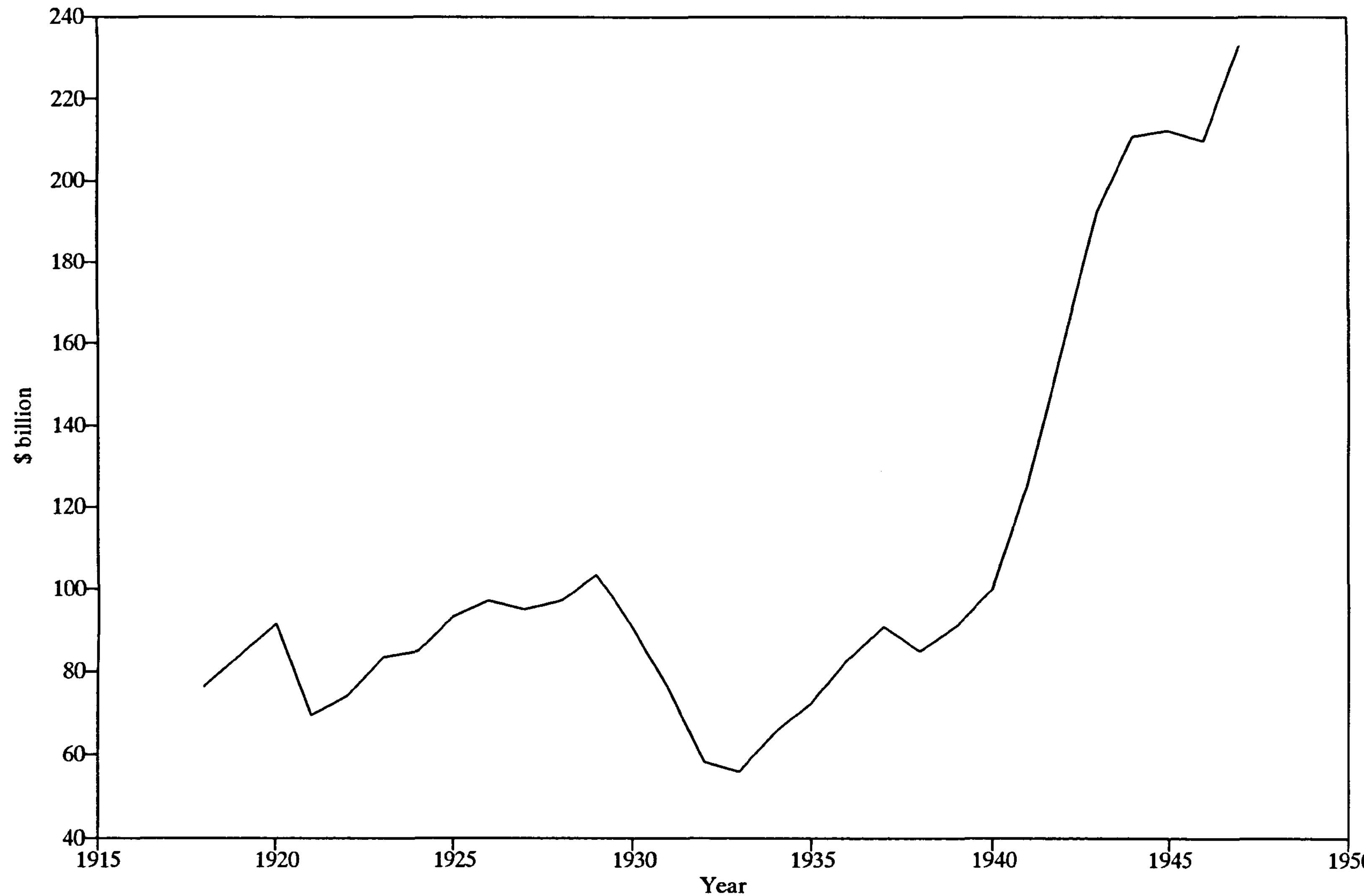


Figure 1.2. US Nominal Annual GNP, 1918-1947



CHAPTER 2

BUSINESS CYCLES BEFORE 1914: THEORY AND EVIDENCE

2.1 INTRODUCTION

Interwar commentators such as Keynes held an optimistic opinion of nineteenth and early twentieth century conditions as a 'Golden Age' of peace and economic stability, especially monetary.[1] This is not a readily explicable view: far from being economically or politically stable, the history of that era is a recurrent sequence of manias, panics, crises, financial collapses and depressions, shortages and 'gluts' of money and goods and rises and falls of prices, all maintained against a background of war, famine and revolution. Popular memory of crisis goes back only as far as 1929, and even economists tend to accept the Victorian myth when confronted by authorities like Keynes. The only 'golden' feature of the earlier epoch was the monetary standard, which was what Keynes may have had in mind.

It is evident on closer examination that actual conditions in the 'long nineteenth century' were complex, and that the concurrence of long-run growth and short-run instability is not necessarily contradictory. On the contrary, the record shows that they appear often to have coexisted, and the consequent difficulties in interpreting 'general economic conditions' have shaped the development of both theory and empirical data sources.[2] In particular, the relative weights given by contemporary observers to these juxtaposed behavioural strands were largely responsible for understanding of 'business cycles' that emerged in the late nineteenth century; and these weights themselves depended on beliefs about both 'causes' and transmission mechanisms. It was the latter that formed the basis of Mitchell's 'business-cycle' hypothesis, and hence of beliefs about the nature of short-run behaviour down to the present day.

[1]See 'Inflation and Deflation' in *Essays in Persuasion* [1930].

[2]Mitchell later noted that the 'multiplying solutions' proposed to explain these complexities were 'confusing' rather then 'illuminating'. See Chapter 3 below.

This chapter examines historical conditions prior to 1914, their interpretation and its intellectual provenance, that Mitchell would have considered when he began his research in the early years of the present century. Section 2.2 traces the development of theory from Ricardo to Irving Fisher. Section 2.3 examines nineteenth century business-cycle chronologies, which reveal diverse historical behaviour. Section 2.4 discusses the time-series methods that influenced Mitchell.

2.2. THE EVOLUTION OF BUSINESS-CYCLE THEORY: THE SEARCH FOR EXPLANATIONS

As the industrialization of England gathered pace in the nineteenth century, so also did the frequency of 'crisis', 'mania', 'panic' and 'depression'. Although the classical economists of that period preferred to address problems of long-run equilibrium, they were generally forced to concede that short-run phenomena had different dynamic properties, and thus from time to time found it necessary to discuss links between the two types of behaviour. Explanations for instabilities proliferated. The problem was that equilibrium was unobservable, while observed phenomena were perceived by the end of the period to be so complex as to defy generalization. Nineteenth century economic analysis of short-run behaviour thus became a search for unifying explanations of markedly divergent processes.

2.2.1. The Classical Economists and the Debate about Capital

Theoretical debates about 'causes' are ancient and have always been motivated by contemporary conditions. As early as the seventeenth century Sir William Petty commented on 'cyclical' mechanisms and distinguished the various signals emitted by real, nominal and psychological effects — the 'intrinsick' and the 'extrinsick' — the former being related to productivity, and the latter, or 'accidentall', a function of money. Petty may have been the first economist to use the word 'cycle' to mean a sequence of 'Dearths and Plenties' in crop yields, thus indicating its Biblical origins.[3] Chapter IX of his other great work, the *Political Arithmetick*, is an early example of an (implicit) quantity relation, equating the total circulation of money with total output (i.e., volume × value). Also, from the *Treatise*, $P=k\times C$, the level of prices ('rates of commodities') is proportional to the stock of money.

With the development of banking and credit systems in England came a succession of severe economic crises, which in the hundred years after 1790 occurred at frequent intervals, a disquieting state of affairs that stimulated much heated debate. Say, Sismondi, Ricardo, Malthus and J.S. Mill all attempted to explain the phenomenon. Say is remembered for his *loi des débouches*, largely because it was criticized by Keynes in the *General Theory*. Ricardo interpreted this law to mean that demand and supply are always in equilibrium 'because demand is only limited by production'.[4] Sismondi was among the leading opponents of Say's law, and in several works published between 1803 and 1837, he advanced three separate explanations for the occurrence of economic crises. The first, an 'oversaving' argument, formed the core of his analysis of disequilibrium between production and consumption. The second held that wages were too low to permit workers to consume their own product, the causes being the unequal distribution of incomes itself as well as unemployment arising from the mechanization of industry; and the third introduced the idea of lags in the adjustment of purchasing power to the level of output.[5]

Questions of disequilibrium in the classical literature usually centred on the role of prices and profits, and their effect upon the rate of capital formation ('accumulation'). The issue was whether scarcities or 'gluts' in all commodities could be general or at most partial. Ricardo thought that long-run general disequilibrium was impossible: although there may be a 'glut' of a particular commodity in a high-profit regime, simultaneous

[3] *Treatise of Taxes* (c1662). See *Collected Works* [1899], C.H. Hull (ed.), Volume I, p. 43.

[4] *Principles of Political Economy*, Volume I, p. 290.

[5] See the discussions in Schumpeter [1954], p. 740 and Mitchell [1927], p. 6.

gluts in all markets cannot occur owing to general unsatisfied demand for other commodities. When profits temporarily fall, however, the better-off save and reduce their demand for luxuries, leading to a short-run 'universal glut' of all goods.[6] Apart from this case, exogenous shocks, or 'sudden changes in the channels of trade' could induce crises.[7] These arose from 'the influence of fashion, prejudice, or caprice' on the purchasers of commodities, or from the levy of a new tax, or 'the effects of war' which increased the transport costs of exports. Shocks caused 'considerable distress and...loss', not only at the time but during the whole transition period, and could lead to a permanent diminution of the national capital. The dominant condition was nevertheless growth: national economies had a 'tendency to continue for ages, to sustain undiminished their wealth, and their population'.

Mill's *Principles of Political Economy* (1848) gave greater attention to commercial crises — which by the time this work was published were increasing in perceived severity — than did either Ricardians or Malthusians. He distinguished two types, the first of which occurred when 'in speculative times money-lenders [and traders were] inclined to extend their business by stretching their credit'.[8] During the 'revulsion...there [was] a disinclination to lend' — a 'panic' — and 'a most pressing need to borrow', the result being a severe and rapid contraction of the monetary and credit base. In the intervals between crises, a 'gradual process of accumulation' took place, with a consequent drop in interest rates. The latter was 'sufficiently rapid to account for the almost periodical recurrence of these fits of speculation' because investors were inclined to take excessive risks to protect their incomes.[9] The other type of crisis occurred, as in 1847, when 'circumstances [tended] to withdraw from the loan market a considerable portion of the

[6]*Principles of Political Economy*, p. 293.

[7]*Ibid.*, Chapter XIX.

[8]Book III, Chapter XII, Section 3.

[9]*Ibid.*

capital which usually supplies it'. In that instance, a mania for railway shares on the stock market locked up capital in fixed assets which, combined with a large export of gold to pay for food imports in the wake of the Irish famine, put severe pressure on the supply of loan capital. Interest rates rose, share prices collapsed, short term credit became unobtainable and firms stopped payment.

Mill's conception of the 'almost periodic' crisis, although influential, was by that time far from novel. (In particular, Tooke's great empirical work of the 1840s, the *History of Prices*, had become the reference standard for empirical evidence of recurrent crisis.[10]) Mill's greater concern was with theories of value and price determination. Analysis of the former was along classical lines, that is, related to labour-costs of production. However, when considering the 'value of money' (i.e., its 'purchasing power') the discussion became quantity-theoretic: although in the short run 'other things being the same [it] varies inversely as its quantity' in the long run, value depends upon the costs of production. In other words, prices (the inverse of the 'value of money') are directly proportional to quantity or, more precisely, vary directly 'as its quantity multiplied by what is called the rapidity of circulation' (i.e., velocity).[11] This relation applies to a system in which gold and silver money are the only instruments of exchange, and is a rudimentary 'equation of exchange', $P=MV$, where M, V are the quantity and velocity of money respectively.[12] There are no terms for physical volume (T) as in later formulations; nor are bank deposits included, because Mill thought the relation applied only to currency.

Complications arise mainly through the medium of trade credit, which produces an effect on prices proportionately greater than that of ready money, 'and this is produced, though none of the written instruments called substitutes for currency may be

[10]See Section 2.4 below.

[11]*Ibid.*, Chapter VIII, Section 3.

[12]Ricardo proposed the quantity relation in a pamphlet of 1811. Its origin has also been attributed to Hume. Wicksell claimed to have found the idea in the Roman literature of the second century A.D.

called into existence'.[13] Inflations of credit are induced by expectations of rising prices which in turn increase profit expectations (an hypothesis whose eventual ubiquity in the literature declined only after World War II). Such speculation can occur in all commodities at once. In the ensuing 'mania' or 'epidemic fit of gambling', shortages induce price inflation even without an increase in money or credit, 'the ideal extreme case of what is called a commercial crisis'.[14] Mill's theory predicted that all commodities gravitated towards a 'Natural Value', that is, 'that at which it exchanges for every other thing in the ratio of their cost of production'. Prices, however, coincide with natural values 'only on an average of years', varying otherwise according to market conditions above and below equilibrium. Although market forces are self-correcting, resulting in a 'general convergence' in the long-run, 'dearth...and...glut are incident to all commodities'.[15]

2.2.2. *'Business Cycle' Theorists and the Debate about Money and Prices*

'Revulsions' of trade in England, in Ricardo's phrase, were noted by contemporary observers in 1815-6, 1825, 1836-41 and 1847.[16] As the century progressed the debate about both causes of and remedies for these episodes intensified. Instability of prices and credit was in one sense a subsidiary issue in the deeper controversy about the nature of money and the means of regulating it. The debate about the cyclical nature of crises took place in the shadow of this more fundamental controversy and was, at least to begin with, inseparable from it. Hence an overlap exists between the bank-regulation literature and that dealing with the causes of cyclical fluctuations, because of the apparent periodicity of crises. Interest in the first half of the nineteenth century was concentrated

[13]Book III, Chapter XII, Section 2.

[14]*Ibid.*, Chapter XII, Section 4.

[15]*Ibid.*, Chapter XIV, Section 1.

[16]See, for example, Tooke, *Op. cit.*,Volumes II and III.

on the role of the Bank of England in controlling the supply of currency — bullion, specie and bank notes — which by the Quantity Theory was essential to price stability. As well as issuing notes, the Bank operated at this time a commercial deposit and discount business. The bridge between the technicalities of Bank practice and the general condition of trade was the gold standard, which necessitated the export of bullion from time to time to pay for imports, normally after poor harvests, which often resulted in a critical shortage of credit.

The debate about the amount of 'money' available to the banking system (the 'circulation') divided commentators into two factions, known as the 'banking' and 'currency' schools. Their most prominent partisans were Loyd (later Lord Overstone) for the currency school, and Tooke and Bagehot for the banking school. In common with other English political disputes, the debate is rather obscure, not only because the two positions intersect at some points, but also because the members of the respective factions disagreed amongst themselves. The controversy ranged over such issues as how the term 'money' should be defined, how its value was determined and how its markets should thus be regulated. Advocates of the 'currency' position argued that inflows or outflows of gold on the foreign exchanges would automatically adjust the circulation in corresponding amounts. The banking school thought that, because of the existence of bank deposits and bills of exchange, neither a pure metallic standard nor its mixed variant would behave as the currency school believed. On the contrary, the needs of the ordinary commercial banking system would adequately control supply, providing that convertibility (with gold) was maintained.[17] Since crises were seen as effects of fluctuating money supply, prices and profits, their control was a function of money-market regulation. The immediate outcome of the controversy was the Bank Charter Act of 1844, a measure distinguished only by its lack of effect: during each succeeding crisis it had to be

[17]See Viner [1932], pp. 221-238.

suspended in order to save the banking system from its harshest effects, and the economy from collapse.

The important point for present purposes is what the debate implies about contemporary conditions, rather than which of the two positions is correct.[18] Opinions about what caused crises, as expressed in the emergent business-cycle literature, tended to be conditioned by the school to which the writer adhered. For example, Overstone described the relationship between the state of trade and the volume of credit as early as 1819, in evidence before a House of Lords committee.[19] 'Private credit', which can be seen as the printing of 'virtual money' by private individuals (in the form of bills of exchange), inflates the volume of trade and eventually induces the revulsion. By 1837 Overstone had a more explicit view of crisis as a recurrent sequence of phases — a cycle — and of the role of the Bank of England as either irritant or emollient.

> The...'state of trade' is...subject to various conditions which are periodically returning; it revolves apparently in an established cycle. First we find it in a state of quiescence, — next improvement, — growing confidence, — prosperity, — excitement, — overtrading, — convulsions, — pressure, — stagnation, — distress, — ending again in quiescence.[20]

Wade, in his *History of the Middle and Working Classes*, expressed the popular view of crises as manifestations of divine law, as periodic in their occurrence as 'the seasons, or as the plague, sweating-sickness, and the cholera-morbus'[21]. Over the previous 70 years the period had been 'ordinarily completed in five or seven years', with 'alternate periods of prosperity and depression'.[22]

[18]The answer to the latter question is likely to be 'neither' or 'both'.

[19]As cited by Wade [1833], p. 45.

[20]Overstone [1837]. This passage was quoted by Bagehot in the essay 'Investments'. See his *Collected Works*, St. John Stevas ed.,Volume IX, p. 272-5.

[21]*Ibid.*, p. 46.

[22]*Ibid.*, p. 211.

The problem of the 'value of money' was integral to the developing theory of the 'trade cycle'. The *History of Prices*, published in six volumes between 1841 and 1857, attempted to examine price behaviour empirically from the point of view of the banking school. Chapters IX and X of Volume II describe a recurrence mechanism in terms of an excess of demand in the 'phase' of rising markets, and of an excess of supply in the phase of 'stagnation'. These phases, discussed in Volume VI, are 'alternations between periods of confidence and discredit, of the spirit of enterprise and despondency'. In an 1844 pamphlet Tooke argued that (1) the money supply was directly proportional to the price level; (2) that prices are limited by the amount of the 'revenues of the different orders of the State, under the heads of rents, profits, salaries, and wages'; and that (3) rather than inducing price increases, reduced rates of interest tended to reduce prices by reducing production costs.[23] (The latter point was in response to a collapse of investment yields in the 1840s.)

Walter Bagehot advocated the banking-school position regularly through the pages of the *Economist,* of which he was editor from the mid-1840s. Bagehot's strongly held views about commercial crises originated in his concerns about the stabilization of the 'money market', as summarized in *Lombard Street* [1873]. He was perhaps the first to distinguish two types of crises: the purely monetary and those originating in the great technological changes of the second half of the nineteenth century. His main concern was that the growth of the banking sector had increased the severity of fluctuations through the price effects of changes in investment yields and in the accessibility of credit. Because the English had become accustomed to deposit their surplus capital in banks, in contrast to other European countries, a very large loan fund was available for commercial and investment purposes, and credit was thus nearly always available. This increased risks to the stability of trade by reducing barriers to entry, so that established merchants in each

[23]Republished as Appendix XV, Volume VI of the *History*, p. 636.

generation were 'pushed out, so to say, by the dirty crowd of little men'.[24] The 'phases' of the money market, prosperity and depression, arose not only from exogenous shocks ('accidental events'), *viz.*, 'a bad harvest, an apprehension of foreign invasion, the sudden failure of a great firm which everybody trusted...', but also from 'regular internal changes'.[25] The source of the latter lay in the lags which occurred in the operation of trade as firms began to produce goods for a general market. The development of the banking system further increased price instabilities as a result of over-accumulation and chronically depressed investment yields — the so-called 'era of two percent'.

Bagehot also distinguished real and nominal effects related to price changes.[26] The causes of inflation were threefold: 'cheap money, cheap corn, and improved credit'. An increased money supply was a necessary but not sufficient condition. In most periods of industrial expansion, the three causes were seen to have acted together. However, in an echo of the classical position 'real' prosperity came from increased physical output, whereas prosperity in which the only effect is increased price was 'imaginary'. The question was a moral one: a real prosperity might on occasion be destroyed by a single bad harvest; but an 'apparent' prosperity, during which plentiful credit inflates prices, would invariably be destroyed, because more loan capital would be needed to sustain given level of trade, and eventually a sudden and steep rise in interest rates would occur, bringing about a 'reaction'. This 'severe alternation of opposite causes' produced both 'cycles' and panics, largely owing to the 'delicate' balance of supply and demand and its action on discount rates.[27]

It is not clear whether Overstone, Tooke or Bagehot understood cycles as a continuous process of depression, revival, prosperity and crisis, although all of these

[24] *Lombard Street*, 1915 edition, p. 9.

[25] *Ibid.*, Chapter VI.

[26] See *Economist*, 30th December 1871, leading article.

[27] *Lombard Street*, 1915 edition, p. 149.

elements seem in one form or another to have entered their analysis. If not continuous, then by 1850 crises were at least seen as regular occurrences. Observers were increasingly struck by the apparent irrationality of these episodes. Both Bagehot and John Mills saw pathological behaviour in the great speculative booms of the period.[28] John Mills, in 1867, argued that 'the periodicity of commercial crises is at any rate *a fact* [emphasis in original]'. The instances of crisis 'are already too numerous, regular, and persistent, to allow...for a theory of fortuitous coincidence'. It being logical to try to classify the various regular 'phases', the idea of continuity in commercial cycles appears to have arisen directly out of the search for a general taxonomy. Following this approach, Mills proposed an early version of a generic, phased business cycle: revival, speculation/crisis and post-panic. However, although all cycles followed the same sequence, the common element was the evolution of commercial psychology, the only real uniformity that Mills could identify over the preceding 60 years.[29] Foreign trade had 'enormously increased in volume'; there had been regimes of inconvertible paper currency, of free issues of convertible paper, and finally of regulated convertible issues; the legal framework had been 'modified in every conceivable way'; world commerce had moved from monopoly to free trade; and the gold backing for currencies had received 'a vast and sudden addition' from discoveries in California and Australia. Amidst this 'bewildering diversity' the only regularity appeared to be that of recurrence, suggesting that the phenomenon was one of 'mind' rather than 'purse'.

During the 1870s and 1880s, the debate about causes of economic variation came increasingly to encompass theoretical relations among money, prices and physical output, supported by growing volumes of economic statistics. Giffen, Marshall and Jevons were the principal theoretical protagonists. Jevons was an innovator of empirical methods, especially the use of index numbers and early forms of correlation analysis, but is perhaps

[28]'A panic, in a word, is a species of neuralgia...' *Lombard Street*, p. 51.

[29][1867] p. 29.

best remembered for his attempts to find an empirical link between commercial and sunspot cycles.[30] Fluctuations from 'commercial causes' (speculation) were considered to have a 'more sudden and considerable effect' on the fortunes of the propertied classes than the depreciation of gold (inflation).[31] Despite the price and commercial instabilities, Jevons believed that the economic changes of the previous 20 years had produced a 'general increase of wealth and of mercantile industry and profits', implying that growth and cyclical instabilities could coexist. The benefits of these changes forbade any assertion that anyone had 'suffered positive loss of the necessaries and comforts of life by the depreciation of his income'.[32]

The essay 'Periodicity of Commercial Crises and its Physical Explanation' (1878) was a response in part to the trade depression of the 1870s but more generally to an awareness that the

> present depression is...only one [of] a long series of events of the same kind, occurring with remarkable regularity at intervals of about ten years. ([1884] p. 187)

The essence of Jevons's theoretical work was an attempt to link this decennial cycle causally to crop yields as determined by sunspot variation. From a study of statistical sources he asserted that '...trade reached a maximum of activity in or about the years 1701, 1711, 1721, 1732, 1742, 1753, 1763, 1772, 1783, 1793, 1805, 1815, 1825, 1837, 1847, 1857, 1866'. Owing to his obsessions Jevons has been dismissed as a crank by later commentators; yet it must be remembered that his sunspot hypothesis was not seen *a priori* as implausible. For example, the socialist writer H.R. Hyndman appeared to regard Jevons' hypothesis as an attempt to avoid embarrassing explanations of an increasingly serious problem, by referring 'the whole of our social troubles...to these strange changes in that great body [the Sun]'.

[30]On which see Morgan [1990] Chapter 1.

[31][1884] pp. 74-5. These comments may also indicate the difficulty of separating real and nominal effects.

[32]*Ibid*., p. 90.

> This theory was actually accepted for a time, until...the worst crisis of the century came in the same year as one of the finest harvests ever known on the planet, and when also the sun's disc was exceptionally afflicted with spots.[33]

Jevons agreed with Bagehot that *temporary* rises in commodity prices are induced by expansions of credit through bills of exchange and other banking instruments. Prices and credit 'mutually inflate each other'. The 'exhaustion' of loanable capital limits credit and eventually checks price increases. Unlike Bagehot, however, Jevons argued that the loan fund is limited by the 'reserve of notes, equivalent to gold, in the banking department of the Bank of England'. Although temporary price fluctuations may be independent of monetary gold, '*ultimately they must be governed by this quantity* [emphasis in original]'.[34]

2.2.3. *'Multiplying' Explanations 1873-1914*

Commentaries in the fifty years before the First World War imply increasingly complex economic conditions. After the crisis of 1873 the 'state of trade' appeared generally to deteriorate, led by commodity prices falls and followed to a debatable extent by declines in physical volumes. Three royal commissions in the UK, the Bureau of Labor and several congressional committees in the USA investigated the underlying 'causes' of the apparent deterioration, which by the mid-1880s was characterized as a 'Great Depression'. In 1886 the results of these inquiries were reported in both countries.[35] The British commission found no single decisive factor, but that the 'chief features' were:

> (a) a very serious falling off in the exchangeable value of the produce of the soil [i.e., the 'agricultural depression'];

[33]Henry Mayers Hyndman (1842-1921), *Commercial Crises of the Nineteenth Century*, [1892], p. 9. According to the *Dictionary of National Biography*, Hyndman became acquainted with Marx's works about 1880 and was chiefly responsible for introducing them to a popular readership in England. (See D.N.B., 1921 Supplement, p. 180.)

[34][1884] pp. 24-29.

[35]*Final Report of the Royal Commission on the Depression in Trade and Industry*, Parliamentary Papers 1886, Volumes XXI-XXIII. The analyses of 'alleged causes' of depressions by US congressional committees and the US Commissioner of Labor, are reported in Hull [1926] Appendices B and C.

(b) an increased production of nearly all other classes of commodities;

(c) a tendency in the supply of commodities to outrun demand;

(d) a consequent diminution in the profit obtainable by production; and

(e) a similar diminution in the rate of interest on invested capital. (P.P. Volume XXIII, p. xv)

The fall in the profitability of industry and agriculture 'has given rise to a widespread feeling of depression among all the producing classes', whereas those on fixed incomes or in regular employment 'have apparently little to complain of'. The 'distress' caused by the displacement of workers by technological innovation and by the 'fluctuating' nature of the demand for labour had, in the previous year or two, been 'more marked than usual'; but 'the increasing consumption by the working classes prove[s] that their general prosperity has not materially diminished in recent years. This complacency was not entirely unjustified, because such indicators as existed were contradictory. The difficulty lay in distinguishing changes in output from structural shifts, at a time when output data were almost entirely unavailable for the real sector, and both from price effects. Thus the commissioners believed that production in general had increased through the period 'described as depressed', with decreasing costs owing to the decline in prices; and that moreover, capital 'continued to accumulate'.

Such apparent confusion perhaps implies not one but several factors, short-run and long-run, real and nominal, acting concurrently, whose effects were impossible to separate. 'Causes' of depression in America as found by the congressional committees numbered some 178, and by the Commissioner of Labor, 143. A sample of these from the 1886 report, as catalogued in Hull [1926], reveals the drift of contemporary thought and suggests that not only did observers fail to understand what was going on, but that they did not even agree that anything at all was happening.[36] The Royal Commission, for example, were not unanimous in their acceptance of the existence of a general trade

[36] [1926] Appendix B.

depression. There was in fact a dissenting minority report, and the scepticism was shared by other commentators.

The issue seemed so obscure that Giffen, in an 1885 article aimed at a lay audience, interpreted commercial sentiment as being almost entirely a psychological reaction to the protracted price falls. He agreed with the Royal Commission that the meaning of 'trade depression' itself was by no means unambiguous and did not necessarily indicate that

> the whole industry of the country is being ruined... 'Depression'...may exist when almost all the statistical signs point the other way; when production and consumption are on a large scale and there is real prosperity, although without the glow of a period of inflation. ([1885], p. 801)

Giffen argued that in a deflation, the 'leaders of industry', accustomed to look at nominal values, 'are all poorer, and feel even poorer than they really are'. Thus 'the moral is that economists and public men should beware to some extent of the outcry from the market-place'.[37]

The article presented a theory of prices as determined by changes in the quantity of monetary gold. The other possible factor, 'a great multiplication [in the supply] of commodities and diminution of the cost of production' was thought to have little weight. In a paper given before the Royal Statistical Society in 1888, Giffen remarked further on 'the theory of the relation of the quantity of money to prices'. He argued that prices may induce changes in money supply and vice versa. The direction of this action became an issue in later debates about the role of prices, but at the time Giffen thought that the 'quantity of money in supply and the demands upon it were certainly assumed to have some connection with prices'.[38] He was more interested in long-run than short-run movements in purchasing power, and was trying to show that once the 'ebb and flow of

[37][1885], p. 803.

[38][1888] p. 165.

credit' were filtered out, a longer run deflation ('rise in the purchasing power of gold') had taken place since 1850. That a scarcity of specie accounted for it was demonstrated by a diagram ([1888] p. 214) showing the supply of 'money' stationary after 1873, with commodity volumes rising and prices falling.

In written evidence to the Commission on the Depression of Trade and Industry, Alfred Marshall also expressed scepticism about 'any attempt to divide the recent fall...into that part which is due to the changes in commodity supply and that which is due to available gold supply'. He, like most commentators, saw those 'causes' not originating purely in 'changes in the supply of money' as a combination of factors, short-run and structural, real and financial: (1) increased foreign supplies of gold, (2) cheaper transport costs, including tariffs, and increased speed of distribution, (3) technological developments in production and (4) a reduction in interest rates. However, 'changes in the available supply [of gold] are not accountable for more than a small part of total fluctuations in the purchasing power of money'; since 1850, for example, only about one-sixth of the variation in aggregate prices could so be accounted.[39]

Marshall's response to the commissioners' question about effects of inflation and deflation on the state of trade was that

> *other things being equal*, an increase in the supply of precious metals inflates credit, because it goes in great measure into the hands of the dealers in credit... (*Official Papers*, p. 23)

However, the 'volume of business and the methods of payment...are of importance commensurate' with changes in the supply of precious metals. Moreover, technological developments would induce a fall in production costs and an increase in the volume of output, and this fall was a 'true cause' of the general deflation, in line with classical theory. The evidence at this point becomes ambiguous, but Marshall seemed to be

[39] *Official Papers*, pp. 8-13.

proposing a quantity relation under which total output is proportional to the quantity of monetary gold.

As to the true severity of trade fluctuations, Marshall (in evidence to the Gold and Silver Commission in 1887) thought that claims of both prosperity and depression were exaggerated. Thus in 1872, for example, when Gladstone described the prosperity as increasing 'by leaps and bounds...real prosperity was not increasing at anything like the rate at which it appeared to be if one neglected the fact that prices were rising...'[40] The current depression (1887) was thus nominal — i.e., of 'prices, interest and profits' — but Marshall could not 'see any reason for believing that there is any considerable depression in any other respect'. He argued, as had Giffen, that exaggerated claims of the severity of depression were lobbying attempts by commercial circles whose felt their interests compromised. Because periods of price inflation produced 'exceptionally high returns' they were considered prosperous, but when prices were falling, real incomes actually increased, so that nominal and real effects had become confused. Furthermore, unemployment had not been higher 'during the last ten years than during any other consecutive ten years'. However, 'irregularity' of employment was emerging because of the 'transitional stage in which a great number of industries are', i.e., structurally from advancing technology.

The same problems were examined in the monetary context by Clement Juglar, who from 1862 to 1900 produced empirical studies of the balance sheets of the central banks of France, England and the United States. Juglar, as much concerned with theoretical explanations as with data, expatiated on the underlying mechanism of a continuous cycle and its 'causes' (an idea which strongly influenced Mitchell). In the 1889 edition of his main work, *des crises commerciales et de leur retour périodique en France, en Angleterre et aux États-Unis*, he concluded that 'we move ceaselessly from a prosperous period to a period of crisis, and reciprocally'.[41] Such crises 'only appear in societies in

[40] *Ibid.*, p. 98.

which commerce is highly developed'.[42] Prices were the primary indicator, with their 'turning points at the maximum...coinciding with crises'.[43] The movement had three phases and was found to be continuous: national economies were always placed in a recurrent 'cycle' of 'prosperity, crisis...or liquidation.[44] The features of commercial crises were so universal that they could not be confused with 'simple accidents' and panics which result from shocks or exogenous influences.

From a survey of the theories then current, Juglar distilled the following common features of crises:

> first, the price increases that precede them; second, the drain of gold that determines the explosion; third, the price falls that allow and facilitate the liquidation. ([1889], p. 20)

The three phases follow each other always in the same order, but with different historical features, depending upon the epoch. The critical determinant of instability is credit rather than money (but here credit includes bank notes). The importance of money is in its 'rapidity' of circulation through credit media (bills of exchange, letters of credit, etc.) rather than its quantity. From the official accounts of the Banks of France and England, it could be established that 'there has been no prosperous period without an increase', virtually continuous, of the prices of the 'principal commodities', and that invariably the crisis occurs when prices are at their maximum. Credit is the 'prime mover' of the mechanism. The crisis ends when prices, having fallen, begin again to rise.

Juglar denied the 'periodicity' of crises and strongly contradicted Jevons's notion that they are linked to sunspots. It would be stretching the point to fix the 'periodic recurrence to five or ten years'. The *retour periodique* can thus be interpreted, not as

[41]All translations mine — P.E.

[42]Cf. Mitchell's institutionalist ideas about the 'money economy' (next chapter).

[43][1889] p. 10.

[44][1889] p. 10.

meaning 'periodic recurrence', but rather as 'recurrent but not periodic'.[45] Juglar also did not hold with the idea that crises could arise from exogenous influences, the 'accidents' or 'extraordinary events'. Each industry had its own special history which could not be fitted into a recurrent pattern: such events could only act as a 'fuse' and only when 'the charge is already primed'.[46] Crises were by this argument always predictable because the precondition of 'unhealthy speculation' is always observed before the 'tempest'.[47]

By the turn of the century Juglar had begun to examine fluctuations in the real sector more closely. In a pamphlet of 1900, *Des Crises Commerciales et Financières et les Crises Économiques Générales*, he distinguished two types of crisis: those arising from credit-market conditions and those in the real sector generated by 'overproduction' (*crises de surproduction*) or from structural change. The latter, 'general', crises were described as 'breaking when...improvements, sudden and deep, in production and in land and sea transport, modify the usual proportions of supply and demand'. Nevertheless, a

> crisis of overproduction brought on from a very rapid change in production techniques, in means of transport or in public tastes, brings an ordinary commercial crisis with its accustomed phases. ([1900], p. 18)

Prices fell in either case, and the years after 1873 were explained as a combination of the two types of crisis. Juglar thus acknowledged, in common with other observers, the diversity of both the causes and effects of instabilities. Because he considered the velocity of bank-note circulation to be an important factor, his ideas about money do not differ greatly from later quantity-theoretic formulations.

[45] *Ibid.*, pp. 162-9.

[46] *Ibid.*, p. 165.

[47] *Ibid.*

2.2.4. Early Twentieth Century Writers: the Acceptance of Quantity-Theoretic Explanations

Every nineteenth century writer so far discussed believed that short-run fluctuations were effects of monetary and price instabilities. By the end of the century a consensus had developed that in the steady state, nominal output was directly proportional to the intensity of the monetary circulation. The idea, of course, was an ancient one, as Wicksell had found. An 'equation of exchange' appeared explicitly in 1857 in the tract *The Present Crisis* by the American writer George Dutton, who was attempting to distinguish real and nominal effects in the events of that year; and again in 1885 in Newcomb's *Principles of Political Economy*. This work was significant because Irving Fisher later took Newcomb's equation as the basis for his own. Although the purpose of quantity equations was to describe the steady state, most authorities attempted to adapt their expressions to account for cyclical instabilities. Since the repeal of the Usury Laws in England and the United States in the 1830s and 1840s, the role played by interest rates in economic disturbances had attracted increasing attention as a component of price determination. The most influential of such theories was probably Wicksell's idea of a 'natural rate of interest', defined as 'neutral in respect to commodity prices [tending] neither to raise nor to lower them'.[48]

The provenance of the natural rate lay in the Quantity Theory, to which Wicksell devoted a full chapter. He accepted that *ceteris paribus* the determination of prices by the quantity of the circulation could be valid. However, the 'things' required to 'remain equal', especially velocity, were so often found in practice to be volatile that he judged it 'impossible to decide *a priori*' whether the theory could hold. His chief reservation was the direction of causality:

> Incomes determine prices; but we might just as well say...that the former are determined by the latter... [There] is no category of income that is not, to a

[48]*Interest and Prices* [1898] p. 102.

greater or lesser degree, dependent on, or regulated by, the prices of goods and services. ([1898] p. 45)

The problem of validating Quantity Theory thus became a question of 'chicken and egg' and the solution lay in disturbances caused by differences between the 'natural rate' and market, or 'money rate' of interest: if 'the money rate is relatively too low all prices rise', and vice versa. Such a mechanism could in theory be self-equilibrating; in practice a 'fairly constant' difference between natural and market rates could be maintained for long periods, although they would 'eventually coincide'.[49]

To Thorstein Veblen, the great Institutionalist, crisis and depression were

primarily phenomena of price disturbance [affecting] industry because industry is managed on a business footing, in terms of price and for the sake of profits.[50]

Increases in money supply, arising for example from an increase in gold or government purchases, resulted in price rises through increased demand and speculation. Increased prices brought an expectation in the markets of future increases in profits which, through the medium of the stock exchanges, increased the total capitalization of industry. This increase in the asset base allowed an expansion of commercial credit, a process repeated until it was undermined as costs began to rise faster than prices. A crisis developed as the speculation began to appear excessive to the markets, usually precipitated by the calling in of loans by a major creditor. Veblen argued that the depressed conditions following a crisis would perpetuate themselves from a combination, first of technological innovations which forced the scrapping of partially amortized equipment and second, of the capital restructuring which reduced fixed costs and turned the hitherto weakest competitors into the strongest ones.

George Hull published *Industrial Depressions*, a study of economic fluctuations in the United States, in 1911, with a revised edition in 1926. His writing was somewhat

[49] *Ibid.*, Chapter 8.

[50] See *The Theory of Business Enterprise* [1904], Chapter VII. Institutionalism was the philosophical school to which Mitchell adhered.

eccentric and is in the tradition of mid-nineteenth century American polemical literature of the rather than of the later, more scholarly approach of, say, Irving Fisher. Hull dissented from the quantity theory, emphasizing conditions in the real sector, especially the construction industry. Like Juglar, with whose works he was familiar, he distinguished between commercial crises ('panics') and depressions, the first psychological and the second material; and like Juglar, he criticised Jevons's sunspot theory, whose untenability rested in Hull's opinion on empirical rather than *a priori* grounds. The two types of instability operated independently, the evidence of the government commissions emphasizing that on many occasions one had occurred without being followed by the other. Periods of industrial prosperity were marked by increases in manufacturing and construction, and of depression by the decrease in these sectors. The phase indicator was the price of manufactured goods (a function of supply conditions), the onset of depression being signalled the when it reached a maximum. Overproduction did not cause depressions. The 'checking of manufacturing and construction' because of high prices caused the slump in demand for industrial goods. If anything, underproduction was the real cause of depression, a view Hull seems to have shared with earlier American writers. The idea that industrial depressions could be periodic was considered 'absurd...the actual divisions [ranging] all the way from one to fifteen years'. Other theories, such as excess of fixed capital, were also rejected.

In 1911 Irving Fisher published *The Purchasing Power of Money*, a study of the quantity theory. The relation $MV + M'V' = \Sigma pQ = PT$ —where M, M' are currency and demand deposits respectively, V, V' their respective velocities, p and Q individual commodity prices and quantities, and P, T their aggregates — was borrowed from Newcomb, to whom the book is dedicated. In its pure form it is a theory of the determination of the price-level (or its inverse, the 'purchasing power of money'). The five variables ('groups of causes') affecting price levels are 'money, deposits, their velocities of circulation, and the volume of trade'. However, the 'five causes in turn [are found to] be themselves the effects of antecedent causes...' such as diversification of industry, increased efficiency and decreased cost of transport, and increased gold supply and bank credit. Behind these 'antecedent causes', if pursued to 'remoter stages' emerged yet an increasing number of causes as 'the number of one's ancestors increases with each generation into the past'. Causality in Fisher's version of the quantity theory is expressed by the proposition '...*the normal effects of an increase in the quantity of money is an exactly proportional increase in the general level of prices* [emphasis in original]', all other things being equal.[51] However, this holds as a '*normal* or *ultimate* effect after transition periods are over'.

Chapter IV discussed the dynamics of disequilibrium effects. A disturbance may occur, for example, through an increase in the quantity of gold: prices increase, inducing a rise in profits, because even if costs rise proportionally, increases in the costs of borrowing lag behind the general price rise. Loans expand, and hence deposits rise faster than currency, which induces further price rises.

> In other words, a slight initial rise of prices sets in motion a train of events which tends to repeat itself. Rise of prices generates rise of prices, and continues to do so as long *as the interest rate lags behind its normal figure* [emphasis in original]. ([1911] p. 60)

Furthermore, the volume of trade will be disturbed by relatively cheap loans, as will the velocities. In such a 'boom' the debtor will benefit and the creditor suffer. With money 'spoiling...like ripe fruit', the tendency is a flight into goods, which further disturbs the Q's. Because prices are further inflated, the process repeats itself, with what appears to be an implicit multiplier. During the inflationary period real interest rates fall: the nominal rate rises, but prices rise faster. In due course, the banks raise rates faster than prices to protect their reserves and eventually the boom collapses into a credit crisis as profits vanish and debtors default. The period of this 'commercial pendulum' is seen to

[51] [1911] p. 157.

be about ten years; and these periods of transition are the rule, with periods of 'equilibrium the exception'.[52]

Natural and man-made disasters were identified as the main features of crises in earlier centuries; but as late as 1914, Prof. M.T. England argued that exogenous disasters, both natural and man-made, could provoke crises distinct from ordinary cyclical phenomena.[53] The most complete summary of pre-industrial conditions (which Mitchell later distinguished from those of the 'money economy') is given by William Scott in his 1912 study of late-medieval commerce (see Section 2.4 below).[54] Scott found crises on average every 11½ years from 1558 to 1720, but was sceptical of Jevons's periodicity hypothesis because 'disturbances of this kind were so frequent in the sixteenth and seventeenth centuries that it is possible to extend the list very considerably'. In the end, based on the data for the whole period, Scott came down in favour of an explanation resulting from

> the joint action of subjective and objective conditions, which in the period under review takes the form of what may be described as the occurrence of 'unforeseen' conditions... Things that are unpredictable are liable to cause crises, if of sufficient importance. ([1912] Volume I, p. 469)

2.3. CHRONOLOGIES UP TO 1914

The transition to a theory of continuous business cycles was not complete until the 1920s. Most of the pre-1914 evidence was historical rather than statistical, emphasizing events rather than processes. Thus, as late as 1913, writers such as England

[52]*Ibid.*, Sections 2 and 3.

[53]England still considered crop failure to be important. See [1914] p. 351. A footnote to this discussion indicates that Jevons's sunspot theory might even at that date have had some residual credit. Prof. England taught at the University of Nebraska, which may partly account for this interest. Before dismissing the effects of agricultural disasters too quickly it is well to remember the 'dust bowl' conditions of the 1930s in America.

[54]*The Constitution and Finance of English, Scottish and Irish Joint-Stock Companies to 1720*. Scott was a lecturer in political economy at the University of St. Andrews.

could attach significant weight to 'unforseen' calamities. Scott's chronology of economic conditions from 1558 to 1720 gives a good sense of the emphasis on crises as explanations of trade fluctuations which still flourished at a comparatively late stage in the prewar period. Of the 33 occurrences of crisis between in that long period, all but 29 had some non-economic factors (famine, plague, war, etc.). This was thought in the 1920s, by Wesley Mitchell and others, to demonstrate that commercial business cycles did not emerge until the banking system (the 'money economy') had become fully developed (see next chapter).

Jevons's chronology is suspect because he was inclined to ignore years of crisis that did not coincide with bad harvests and sun-spot maxima; but numerous other accounts were kept in the nineteenth century, and, beginning with the annual survey of trade in the *Economist* in the 1840s, these developed into statistical series. The chronologies which have been cited in recent studies, and which were compiled for most of the events of the century, are those of Juglar/Thom and Hyndman, for commercial crises in various countries, and of Hull, for industrial 'depressions' in the United States.[55] The dates of crises (or 'panics'), 1815-1892, are given in Table 2.3.1, and show broad agreement. Hull's dates of depressions (for the US) are: 1836-9, 1855-8, 1865-70, 1882-5 and 1890. This bare chronology conceals vivid accounts of almost continuous distress, an interpretation of events which would have had wide acceptance at the time. It is well to bear in mind, however, that historical accounts, with their preoccupation with events, sometimes fail sufficiently to emphasize the underlying processes that distinguish economic from political history: in this case continuous growth throughout the period.[56] An analysis of the histories of Hyndman for British, and Juglar/Thom for United States

[55]Decourcy W. Thom, Juglar's American acolyte, published a posthumous English language version of *Crises commerciales*, updated to 1915, incorporating ideas and statistics of his own.

[56]No one, from Jevons onward, underestimated the expansion. The chronologies emphasized crisis because growth was seen as 'natural' and beyond regulatory control.

'crises', and Hull for US industrial disturbances suggests that although sharing some common features, these episodes were each unique in most important respects.

Close reading of Hyndman especially shows that he was aware of multiple economic states coexisting during each of the episodes. He emphasizes the transition in the British economy during the first 40 years of the nineteenth century from an agricultural economy to the 'workshop of the world', with its share of sea trade similarly extended to a position of near monopoly. All factors grew at an accelerating rate: population (e.g. by one-sixth from 1821 to 1831), agricultural yields, industry, infrastructure and the banking system. Throughout the whole century the 'well-to-do classes...were becoming steadily richer' as the economy continued to grow despite the disturbances. Annual rents of land increased by £40 million in the period, and wheat yields increased at the rate of 44 million bushels per annum (far more in percentage terms than the increase of farm labour).[57] Growth was both monetary and real: for example, the years 1847-1857 saw annual increases in UK domestic gold stock of about £25 million. This, combined with price increases and increases in industrial output combined to mislead businessmen into believing that the economy was richer than was really the case. The instabilities thus induced, largely by nominal factors, led to a crisis when a speculative bubble burst in 1857. The real distress was concentrated upon the labour market where hundreds of thousands were temporarily thrown out of work. The crisis was thus 'industrial' but precipitated by irresponsible behaviour in the financial sector.

[57][1892] Chapter III.

Table 2.3.1. Dates of Nineteenth Century Crises		
Juglar/Thom Reference Dates		Hyndman Reference Dates
US	UK	UK
1814	1815	1815
1818	1818	–
1826	1825	1825
1829-31	1830	–
1837-9	1836-9	1836-9
1848	1847	1847
1857	1857	1857
1864	1864-6	1866
1873	1873	1873
1884	1882	1882
1890-1	1890-1	1890

After 1850 the underlying process became the explosive growth of foreign trade, which among the industrial powers 'doubled' from 1857 to 1873, making the respective economies increasingly interdependent. As the royal commissions had found, 'a great advance in the production of wealth', a fall of transport costs and an improvement of the speed of movement coexisted with the 'bad trade and stagnation'. Unemployment was thought to be caused by technical improvements, and a theory of 'overpopulation' again became fashionable.[58] Productivity improvements cited by Hyndman include, for the ten years to 1883, a 16 percent increase in coal production with a fall of 4 percent in the number of miners; and in iron and steel, an increase of 26 percent in production with an increase of 0.3 percent in the work force. Income tax receipts grew by 4 percent per annum. The same trends were manifest in America, where 'productive power' increased 58 percent in the same period, while the employed labour force increased by only 33 percent, with trading conditions 'stagnant' overall.[59]

[58]It yet again became so between the wars, in the guise of 'Eugenics'. Although at its most extreme Germany, the movement attracted respectable figures in the Western democracies (including Keynes and Dr. Marie Stopes).

[59][1892] Chapter VIII.

Hyndman argued that although crises were of diverse origin, they were commonly associated with some failure of capitalism. The following table shows a summary of his analysis.

Table 2.3.2. Hyndman's Account of Nineteenth Century Crises	
Year	'Cause'
1836-9	Collapse of credit speculation
1847	Exogenous shocks and collapse of boom in capital markets
1857	Collapse of speculative boom
1866	Bank failure
1873	Collapse of Vienna stock market diffused though world economy
1882	Bank failure diffused though world stock markets
1890	Collapse of Barings Bank

The Juglar/Thom version was similar in its account of events to Hyndman's, but implicitly followed Juglar's theories of price/credit behaviour rather than Marx's critique of capitalism. Hull was more concerned with supporting his own theory of price cycles in the construction industry. Economic growth and the multiplicity of 'causes' was acknowledged by all, either implicitly or explicitly.

2.4. TIME SERIES BEFORE 1914: DATA AND METHOD

Although aggregate output series, even at annual frequencies, were hardly known before the 1930s, price records were available from very early times. The first writer systematically to analyse large amounts of data was Tooke in the 1840s, and this work became the basic reference for later research. An example Tooke's approach is the series of maximum and minimum cotton prices, 1782-1820, reproduced in Mitchell [1988], and plotted here as Figure 2.4.1. These prices were sampled at unequal intervals and so are not directly comparable to modern time series. Tooke, moreover, did not transform his data — e.g., by moving averages or indexation — but merely presented them as an historical record. In addition to prices he also compiled data on banking and currency operations, including the balance sheets of the Bank of England. Jevons's price data were based on Tooke's and that of the *Economist*, and writers as late as Rostow refer to those series to support their arguments.

2.4.1. Jevons's Time-Series Analysis

Jevons published a tract on purchasing power in 1863 which used both index numbers and averages to investigate possible changes following the gold discoveries in Australia and California. His method was to construct indices of commodity prices and the note circulation for the years 1845-1862. Jevons thought that they showed a general fall of purchasing power from 'about 1853', and also that 'an expansion of the currency [occurred] one or two years previous to a rise of prices'.[60] An 1865 tract amplified the statistical method, using Tooke's time series; but Jevons thought that such raw data 'were in want of some method of [reducing] and elucidating the general facts contained in them'. The proposed method was modified by the use of a geometric mean to calculate an index of price changes, i.e., $\sqrt{\frac{b}{a} \times \frac{q}{p}}$ for two commodities, where the price of the first commodity changes from a to b, and the second from p to q.[61] Index numbers were calculated for 12 individual commodity groups and an aggregate of 40 commodities, examples of which are shown in figures 2.4.2(a)-(d). For the individual commodities both money-price and relative-price changes ('actual' and 'comparative') are given. Jevons's purpose was to find evidence to support his belief that prices rose in the Napoleonic Wars while specie payments were suspended, fell after 1815 and then rose again with the gold discoveries of the 1840s. He also experimented with seasonal analysis, using note-circulation statistics for England, Scotland and Ireland, and found evidence of

[60][1884] pp. 76 and 101.

[61]*Ibid.*, pp. 113-114. Laspeyres apparently criticised this, arguing that the simple arithmetic mean was more appropriate for measuring changes in 'purchasing power'.

recurrent fourth-quarter pressure on the reserves of the Bank from seasonal increases in the circulation.[62]

2.4.2. Later Nineteenth and Early Twentieth Century Data

Until the first decade of the present century, nearly all time-series analysis was of price behaviour, which is comparatively easy to measure. Beginning in 1871, the United Kingdom Board of Trade collected information and published indices of wholesale prices for most commodity groups, as well as an aggregate index. Nine of these series are shown in Figure 2.4.3. The Sauerbeck index of commodities and precious metals appeared annually in the *Statist* from 1886 (Figure 2.4.4). Beveridge published a UK wholesale price index in his 1909 book *Unemployment*, covering the period 1871-1907, reproduced here as Figure 2.4.5. He also plotted a number of real and nominal variables in per capita terms: prices, employment, bank rate, foreign trade, etc. — perhaps the first instance of output estimates. These were plotted as 'The Pulse of the Nation', an early example of graphical trade-cycle analysis.[63] Beveridge believed that employment levels rose and fell as waves, and at very much the same times in all his industrial groups. The price series shown here all appear to confirm the commonly held view that prices fell from about 1873 to about 1896 and rose thereafter.

2.5 DISCUSSION

Keynes's assertions not withstanding, and despite the 'multiplying' explanations, economists of the 'long nineteenth century' were preoccupied with the problematical stability of purchasing power. The debate was as much social and political as economic. Recurrent financial crisis, with associated price volatility, undermined the banking system,

[62] *Ibid.*, pp. 151-174.

[63] *Unemployment*, p. 44.

and its backwash regularly embarrassed large sections of the middle class. Debasement of the currency was hence seen as a threat to the 'natural' social order which was still thought, at least by conservatives, to be equivalent to divine right. This is the import of Bagehot's commentary (and perhaps of Jevons's). The efforts of the 'dirty crowd of little men' to elbow their way into established power was seen almost as a species of economic and social warfare — an attempt to usurp the existing order by the unrestricted printing of money, threatening the ruin of both usurper and usurped. Up to the publication of Mill's *Principles* at least, economic debate about the stability of money — and in extreme form about finding an intrinsically valuable medium of exchange — was, in fact, really a coded debate about the distribution of wealth. The formal link between prices, real output and money in the various quantity equations, each of which was an expression of an equilibrium relation, were designed to suggest an immutable order, and were therefore unsuitable for explaining observed phenomena.

The Quantity Theory was an attempt to demonstrate that economies were stable because both prices and output were completely determined 'in the long run' by monetary conditions. This formal idealization had some intuitive appeal: it was plausible and could be used to justify a fairly severe monetary regime if required. However, it suffered, as did all neo-classical theory, from being static, which is surprising in view of the acknowledged dynamic properties of the nineteenth century economy: the persistence of growth and continuous structural change. Thus even the most committed quantity theorists were forced to modify their ideas to take account of disequilibrium.

Apart from price and financial data, statistical sources were limited in the nineteenth century. Quantity Theory was primarily interested in the *M*'s and the *P*'s rather than the *Q*'s, which were considered exogenous. Accounts of trade fluctuations, as in Hyndman, Juglar/Thom and Hull, were anecdotal in the sense that they did not make use of real output data sampled at regular intervals, but rather adduced ad hoc quantitative evidence in support of particular points, much in the way of legal briefs. Beveridge, for the first time in 1909 plotted real variables, but these were in per capita terms because of his preoccupation with labour market conditions. The advantage of time-series plots is that they give an impression of longer-run processes, if any, as well as of short-run irregularities. Even if long-run tendencies may have been dominant, the absence of information about them gave the false impression that economies were governed solely by short-run disturbances. This is the point Marshall was trying to make in his royal commission evidence: that the depressed state of the British economy in the 1880s was as much apparent as real, and that depressed prices, yields and profits masked persistent real growth and structural, technical and demographic change. Juglar's 1900 pamphlet speculated that the long price declines were really evidence of increased technological efficiency, a consensus shared by most late nineteenth century writers. By 1914 the debate had focused on transmission mechanisms rather than underlying 'causes' and there was a firm consensus among economists that monetary instabilities drove prices and that prices drove profits and their expectations. While prices continued to have long swings, sometimes rising and sometimes falling, this consensus continued; and the debate about 'causes' became a non-operational debate about the existence of equilibrium. When pressed, however, all writers of the period conceded that a number of different processes might be going on concurrently. The consensus about lagged adjustment implied that 'causes' were 'chickens and eggs', a view which later came to reside at the core of Mitchell's business-cycle hypothesis. In other words, the 'cause' of current conditions was previous conditions.

There remains, however, the question whether the state of any economy can ever be described historically by a single phrase: as 'depressed', 'prosperous', etc. Juglar's belief in two types of crisis — financial and the industrial, each arising from a different set of conditions — indicates that by 1900 he no longer supported the single-state model. Although price and credit-market behaviour indicated that unitary 'states' could occur, anecdotal information from the real sector suggested that such conditions were unlikely. Marshall and Giffen, as well as Juglar, were sceptical of a 'general state of trade', and thought that the recurrent credit 'manias' and collapses were only part of the story. As early as the 1660s Petty had distinguished between real depressions of trade and apparent depressions merely of mood, and this psychological theme emerged again in the work of Bagehot, Mills and Juglar. Both Juglar and Hull thought that real and nominal variables could follow independent paths. The 'state of the economy' at any time was thus largely a subjective assessment dependent upon the strength of mood of particular interest groups, as Marshall and Giffen argued. The difficulty for nineteenth century economists was lack of information: although all accepted (even those on the Left) that growth continued strongly throughout the period, none had much quantitative evidence of where and how much growth there actually was. Information on fluctuations was nominal rather than real, and so equally unreliable. Both Left and Right emphasized the disturbances, the former because of their effects on employment, and the latter because of their implications for profits. Neither view was objective or balanced; yet their juxtaposition constituted a political sub-text to nineteenth century economic analysis.

Even allowing for the known lacunae in data sources, the impression given by all contemporary writers is of perceptions of behaviour in the period to 1914 as having complex and multifarious patterns, among which they distinguished

(1) short-run fluctuations in output, prices and the money supply;

(2) long swings in prices, which as contemporary sources show, fell consistently from 1815 to the mid-1890s, and then rose until 1914;

(3) the long-run tendency of most sectors to grow;

(4) the structural changes, e.g., technology, the decline of agriculture, etc., which appeared to accelerate towards the end of the century, leading many observers to distinguish their effects from those of financial crises;

(5) and the economic effects of demographic changes, which were controversial.

In addition were the nominal versus real effects which price movements rendered difficult to separate or interpret, and the psychological effects — the 'outcry from the market-place'; and these 'multiplying solutions' had serious implications for later students of short-run fluctuations.

Appendix 2

Chapter 2 Graphics

CONTENTS

Figure 2.4.1. Tooke's Cotton Price Series 1782–1820

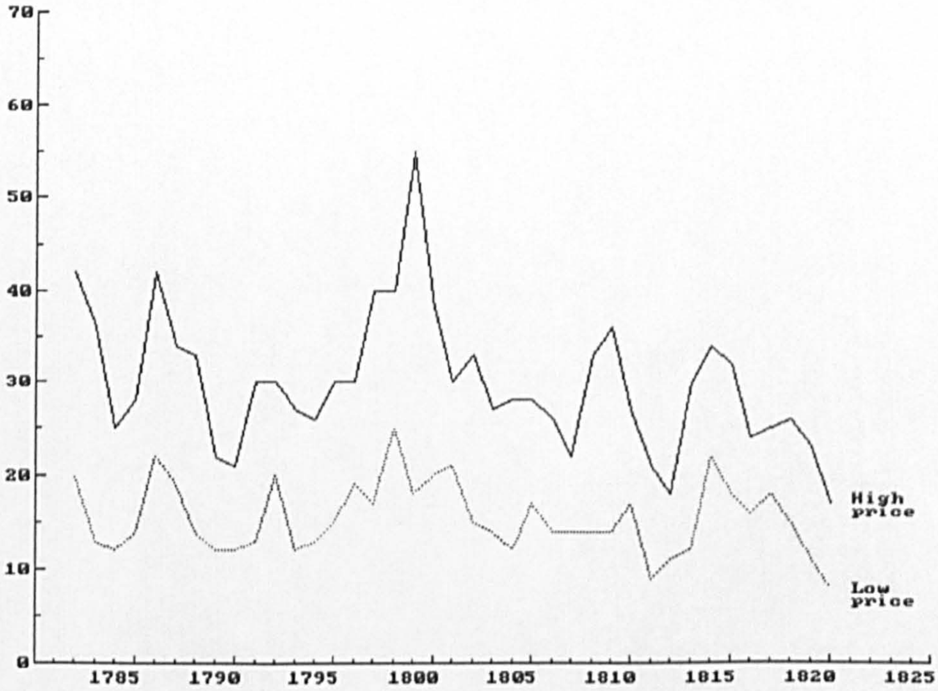


Fig. 2.4.2(a). Jevons's Aggregate Price Index 1780–1865

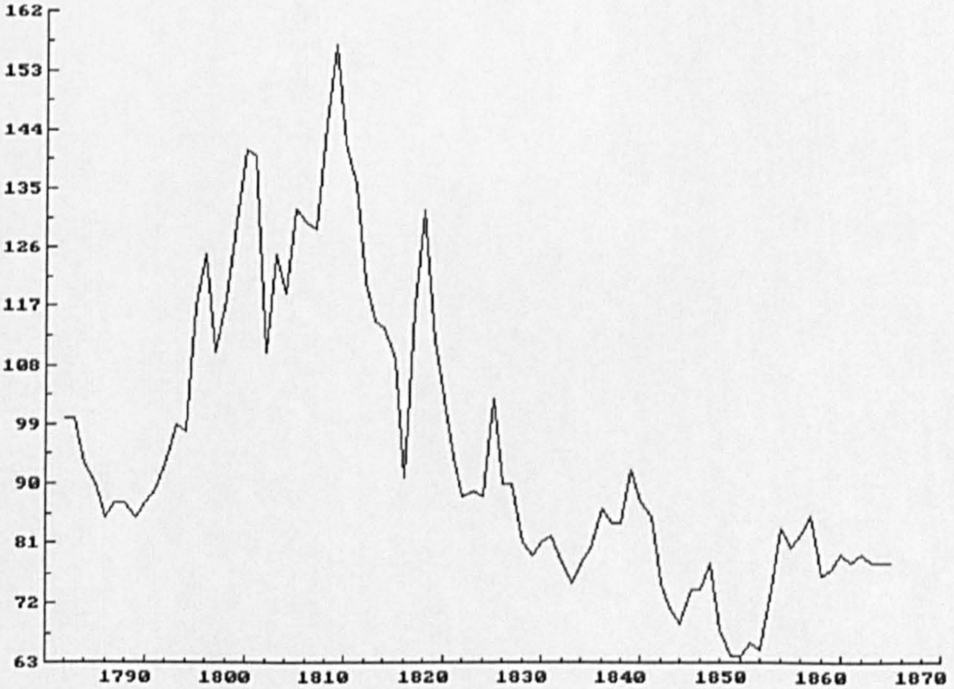


Figure 2.4.2(b). Jevons's Iron Price Series 1780–1865

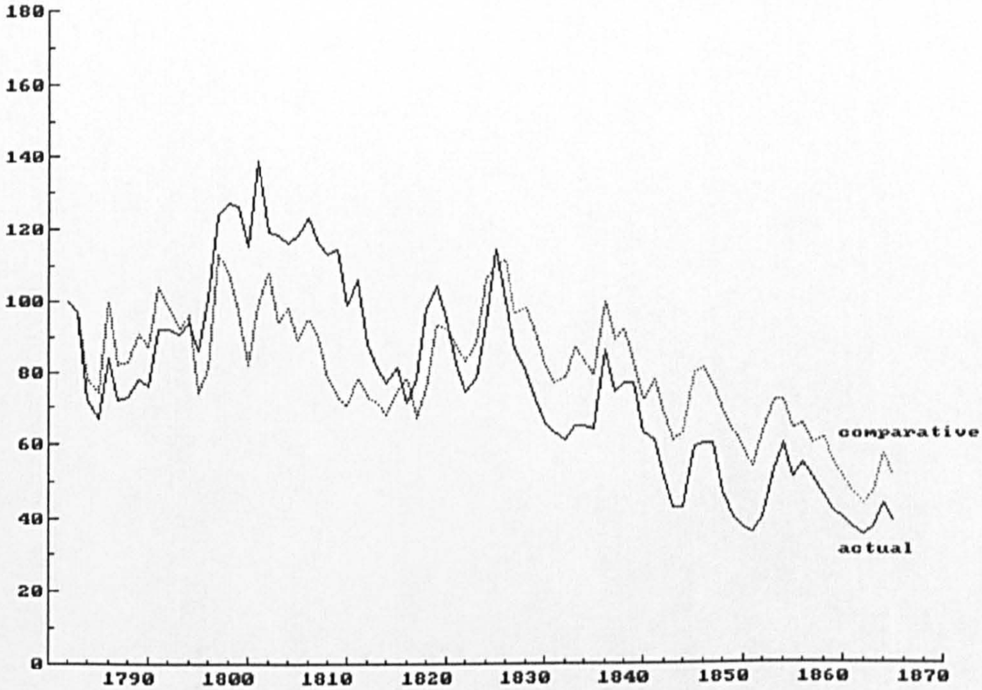


Fig. 2.4.2(c). Jevons's Cotton Price Series 1780–1865

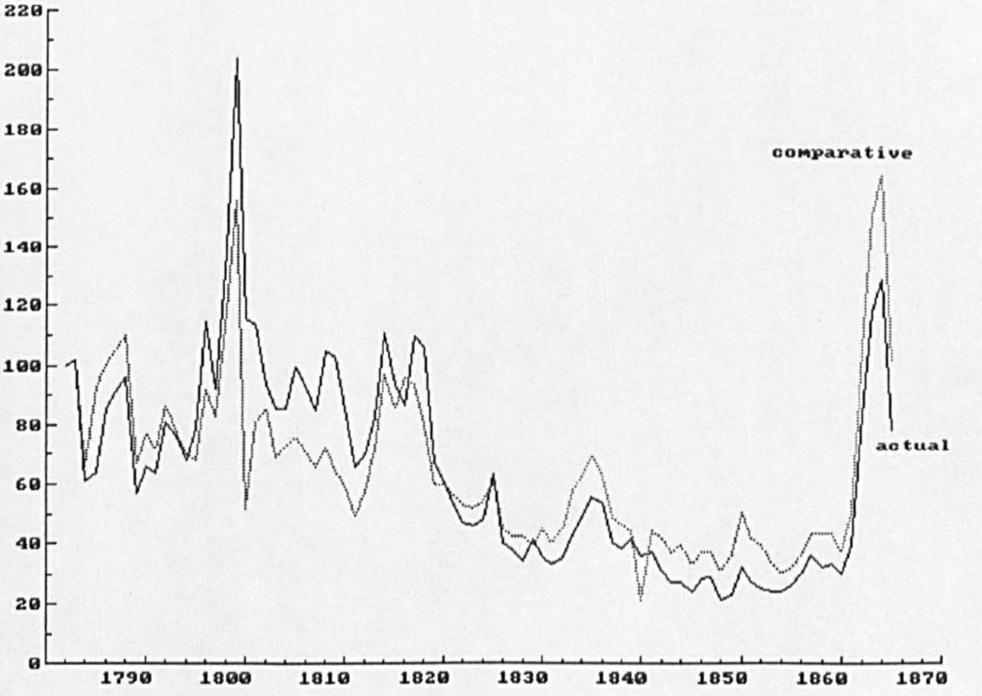


Figure 2.4.2(d). Jevons's Wheat Price Series 1780–1865

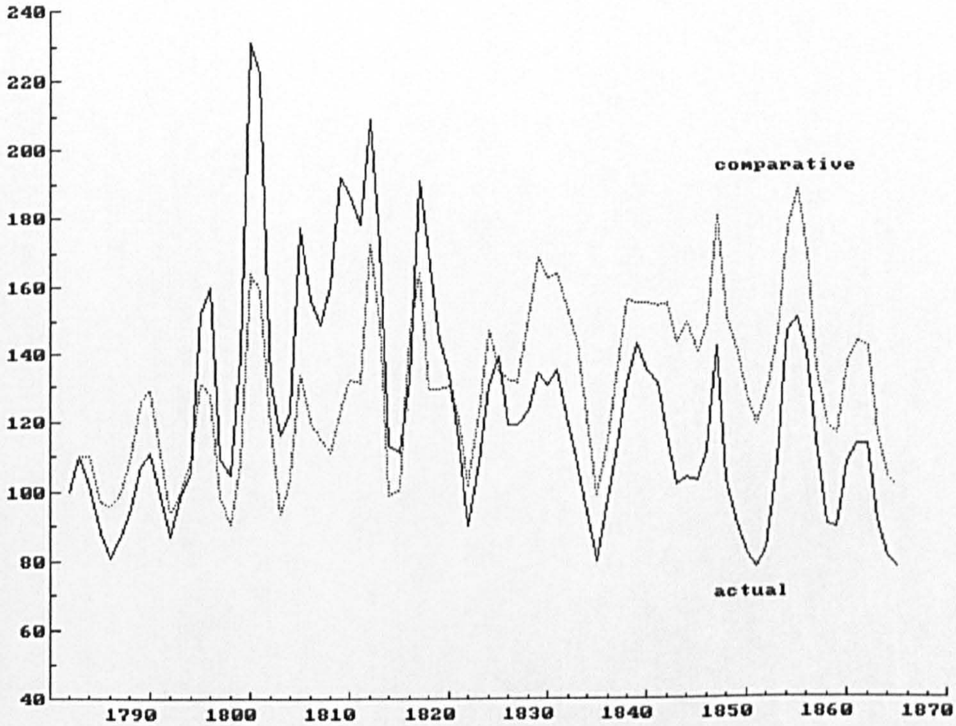


Figure 2.4.3. United Kingdom Board of Trade Price Series 1871–1913

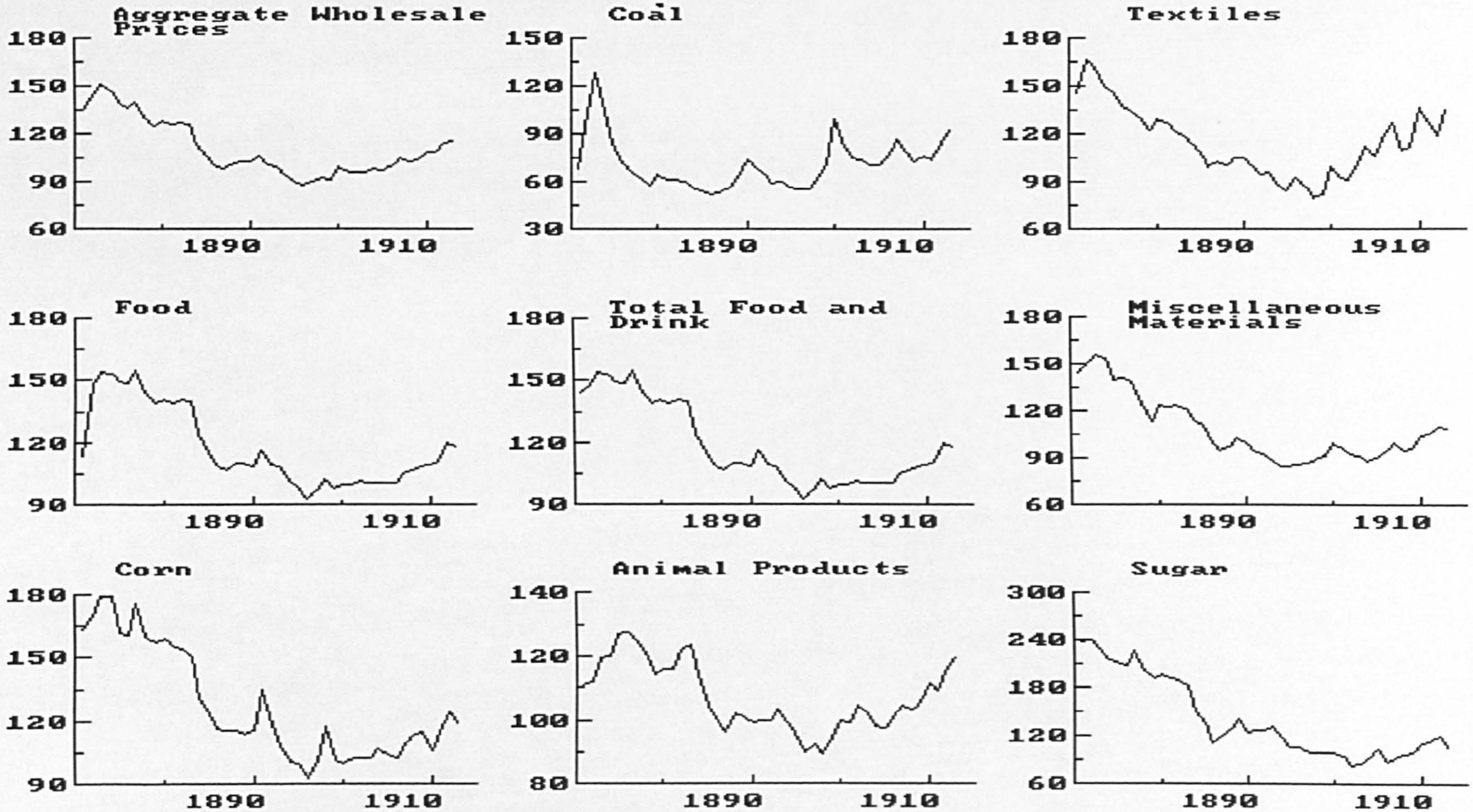


Figure 2.4.4. Sauerbeck's Aggregate UK Price Index 1851–1913

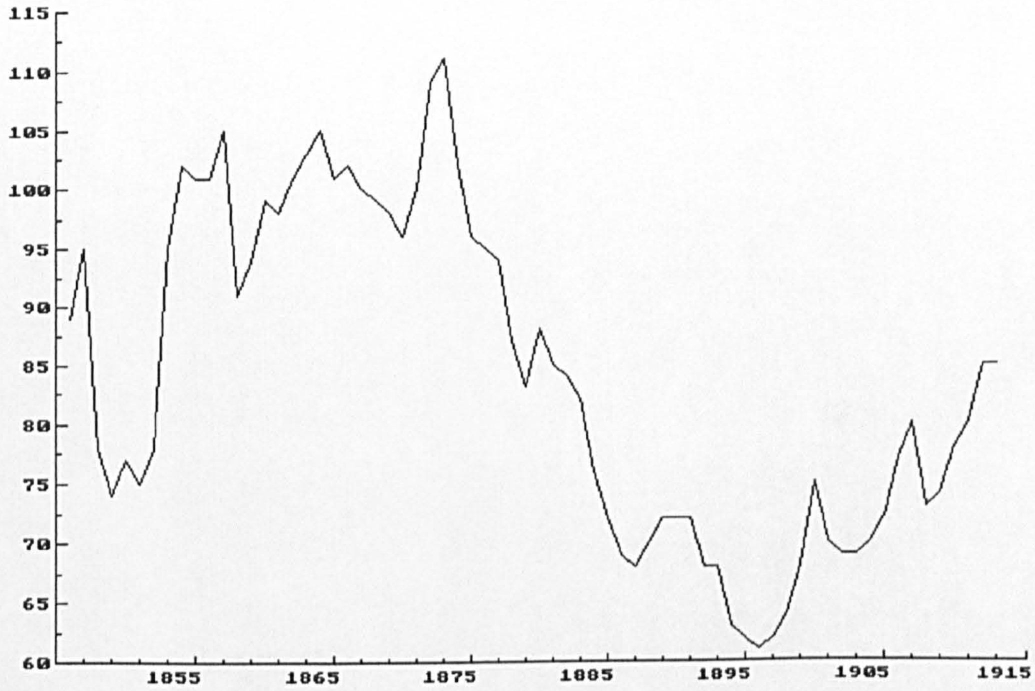
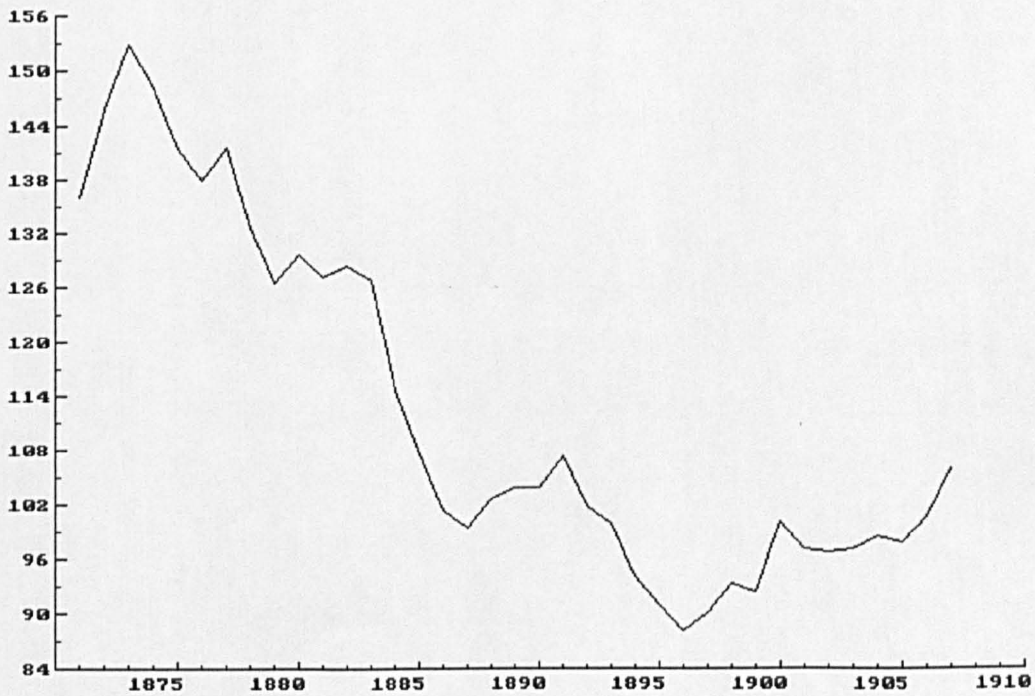


Figure 2.4.5. Beveridge's UK Wholesale Price Index 1871–1909



CHAPTER 3

BUSINESS CYCLES, MITCHELL AND THE NBER: THE 'EMPIRICAL' VIEW

3.1. INTRODUCTION

The political and economic conditions that Wesley Mitchell found when he first considered the problem of business cycles at the turn of the century were at the least ambiguous in character. Theoretical explanations had proliferated in the previous hundred years to the extent that they threatened 'to be confusing rather than illuminating'.[1] On the other hand statistical evidence was 'sufficiently abundant...to make hopeless a purely empirical investigation'.[2] The 'multiplying' explanations, each 'inadequate' on its own, were in the main mutually exclusive. Catalogued under three main and 25 sub-headings, they provided 'persuasive demonstrations that each one of a dozen different processes reveals the factor of crucial importance'.[3] Mitchell therefore saw his work as an attempt either to choose between them, or to synthesize them in a coherent theoretical framework, or else to find a more satisfactory theory of his own based on empirical evidence rather than on the neoclassical practice of deduction from first principles. His research programme and results filled three large volumes, *Business Cycles* [1913] (BC), *Business Cycles, the Problem and its Setting* [1927] (BCPS) and *Measuring Business Cycles* [1946] (MBC).

The complexities of the topic and the encyclopedic nature of Mitchell's writing have obscured his objectives and findings from later view. Nowadays, *Measuring Business Cycles* is thought to have established 'definitive' evidence of recurrent fluctuations diffused throughout the economy. It is also generally believed that Mitchell had no interest at all in theory, and indeed that the techniques employed are 'measurement without theory'. This interpretation is wrong; yet nowadays practically every research

[1][1927] p. 47.

[2]*Ibid.*, p. 3.

[3]*Ibid.*

paper on business cycles cites the 'definition' on p. 3 of MBC and implicitly accepts an empirical interpretation of that work. It is the purpose of the present chapter to show what Mitchell's real intentions were, the extent to which his work has been misunderstood, and how this misunderstanding has led to distortions in the assumptions and results on which all recent business-cycle research is based. For it is can be demonstrated that in many ways Mitchell's ideas were surprisingly close to the mainstream economic theory of his day, notably Professor Irving Fisher's quantity theory, but that this was forgotten in the wake of cultural shifts in economics after 1945 — the Keynesian revolution and its rejection of monetarism.

The most perplexing aspect of Mitchell's work is that despite the expenditure of enormous intellectual resources on theoretical argument and empirical investigation, he constantly speculated against the very existence of the phenomena described by his own hypothesis. Antiphonal voices of argument and counter-argument are evident on practically every page of the three volumes. The clue to what he really believed and found lies in these spasmodic expressions of self-doubt. It is shown below that rather than finding clear-cut evidence of 'business cycles', Mitchell's analysis was inconclusive. Unfortunately such an inference is itself buried in a voluminous and discursive text and obscured at every turn by the very ambiguities he was seeking to avoid.

Section 3.2 of the current chapter discusses Mitchell's approach to theoretical explanations of business cycles and how this approach was influenced by his Institutionalist views and by the prevailing quantity-theoretic consensus of the period. Section 3.3 examines the results of his empirical work and the extent to which its direction was pre-programmed by the search for a theory. Sections 3.4 and 3.5 discuss interpretations of Mitchell's work by other economists, the errors propagated by these interpretations, and their consequences for later research.

Social conditions in the United States during Mitchell's formative years in the 1890s were far from ideal. The background of events was depression, falling prices and incomes, monetary instability and social unrest; agrarian revolt whose extreme expression was the Populist movement; bimetallism and a panic out of dollars into gold; and finally the restoration of 'sound money' after the defeat of the Populist candidate in the presidential election of 1896. Pressure for more moderate reforms crystallized in the so-called 'progressive' movement, which has been described in a large literature beyond the scope of the present study. The true motivation for change, according to a recent account in McElvaine [1984], was the social effects of economic distress, especially in agriculture. So alarmed were the middle classes by growing unrest that by the turn of century even conservatives had been forced into a reforming mode in an attempt to thwart the gathering revolutionary forces. The scope of the proposed measures was strictly limited, designed as it was to deflect the 'discontent that had been so evident among farmers and workers in the nineties' rather than to promote social justice. Reforms would thus be imposed from above, rather than being 'won by the struggle of the lower classes'.[4]

Manifestations of this philosophy can be seen, for example, in Theodore Roosevelt's anti-trust legislation and the expansion of the national parks (a movement that Mitchell actively supported), and later in Woodrow Wilson's moralistic view of America's European policy. The First World War had an additional effect on the American political consciousness: the US government, in order to mobilize the nation,

> had embarked, for the first time, on serious social and economic planning. Indeed, its has been...argued that the models for many programs and practices of the New Deal [FDR's political programme of the 1930s] are to be found in wartime Washington rather than in the proposals of progressive reformers. John

[4][1984] p. 9.

Dewey, for one, took note in 1918 of 'the social possibilities of war'. ([1984] p. 11)

Dewey was one of Mitchell's professors at the University of Chicago in the 1890s. 'Social possibilities' meant, for example, increases in real wages and the growth of collective bargaining, all objectives with which progressives would have agreed. (They also implied, controversially, some form of public direction of resources and control of prices and incomes.)

3.2.1. Progressivism and Institutionalism: Mitchell's Ethical Foundations

Mitchell had the progressive's strong sense of rational optimism and social justice. The former is shown by his faith in science for both positive and normative purposes, the latter by his dissent from the rigours of classical theory which he thought dealt too lightly with the disturbances threatening to overwhelm the existing order. These values were acquired in the economics and philosophy faculties of the University of Chicago from 1890. Arthur Burns, in a memorial essay published in 1952, asserts that this turbulence in 'large affairs...imparted a monetary slant to Mitchell's early economic thinking'.[5] It was, however, the philosophers Thorstein Veblen and John Dewey who exerted the most profound influence. Dewey argued that because economic agents were not in general rational it was impossible to predict their actions from theoretical first principles. Economic behaviour must rather be 'a matter of observation, which the economic theorists take all too lightly'.[6] Veblen's virtuoso performances 'got nothing more certain [about the motivation and behaviour of agents] with another set of premises'. Mitchell objected to the anti-empirical nature of economic theory from the very beginning; yet adopted much of Veblen's thinking, especially the ideas (a) that economic behaviour was habituated by institutional influences such as the 'money economy', and (b) that dynamic

[5]*Wesley Clair Mitchell, the Economic Scientist*, p. 10.

[6]Described by Mitchell in a letter to J.M. Clark, 1928, reprinted in *Ibid.*, p. 95.

paths were evolutionary sequences rather than rational adjustments to market conditions. Classical value and distribution theory were unsatisfactory because they only dealt with 'what happens in an imaginary "static state"...[and] therefore [possess] but meager scientific interest'.[7] Habit created customs and conventions, principles of conduct and preconceptions 'cumulatively', with only indirect reference to native instincts (i.e., the hedonistic model of pain and pleasure). This emphasis distinguished Veblen's approach from the neo-classical. Such processes implied adjustment-lags as conditions changed, and explanations of current behaviour were thus found in 'preceding instalments of the story', rather than in the idealized, immutable and platonic properties of individual behaviour.

Mitchell shared the Veblenite position that the interposition of money and financial institutions between the agent and his need for goods had distorted economic behaviour.

> Money becomes a most significant thing in the economy of society, because it shapes the habits of thought in which our native propensities grow...[The] use of money 'exerts a distinct and independent influence of its own'... We wish to seem well-to-do... In practice we make goods in order to make money. ([1937] p. 304)

Economic instabilities are the side-effect of this dichotomy. The

> recurrent crises and depressions which ever and again reduce that flow of goods to consumers are due to business, not to industry. There is no technological reason why every few years we should have idle factories and unemployed men walking the streets, while thousands lack the goods employers and men would like to supply. ([1937] p. 307)

Classical theory, with its emphasis on equilibrium, addressed such problems only by denying them.

[7] *The Backward Art of Spending Money*, [1937], p. 292.

3.2.2. Mitchell and the Quantity Theory

Mitchell's writings on monetary economics have two distinct strands. The first is a critical appraisal of the Quantity Theory in the form proposed by Irving Fisher; the second, the idea that the workings of the 'money economy' are the source of the instabilities leading both to 'business cycles' and financial crises. Mitchell initially disliked, but later found much common ground with, the quantity theorists, as is shown by his comments on *The Purchasing Power of Money.*[8] He accepted Fisher's 'inductive verification' of the equation of exchange from a statistical study of prices for the years 1896-1909 in the United States. As might be expected, Mitchell was more interested in the process of dynamic adjustment, described by Fisher in Chapter IV for 'abnormal' periods, than in the steady state. This redeemed the theory for Mitchell; it was the lags in adjustment that were important, not the resultant position after the adjustments had worked through. The 'transitional' adjustment process was 'real', whereas the steady state was merely 'notional'. Fisher's work might thus be improved by addressing lags, not only 'of interest rates to changes in the price level', but also in all parts of the 'system of prices', as well as in raw materials, etc.[9]

This view that the distress caused by short-run adjustment-lags was more important than long-run relations led Mitchell to search for a description of the underlying institutional conditions that generated lags: the fundamental idea behind the 'business-cycle' hypothesis. He did not, however, entirely break with Fisher over the relation between money supply, the price level and aggregate output: the discussion of Quantity Theory in BCPS, analysing the conditions under which the equation is valid, follows Fisher's argument very closely. Like most contemporary economists Mitchell held that business cycles were driven by changes in expectations of business profits and, by

[8]*Political Science Quarterly*, 1912.

[9][1912] p. 164.

implication, prices; but that both short- and long-run behaviour were compatible with Fisher's framework.

> In terms of the equation of exchange [the lags] mean that of the payments (MV+M′V′) made today, the bulk are payments for goods transferred (T) some time ago, at prices (P) most of which were agreed upon still earlier... [The] day-by-day relations between MV+M′V′ and PT are indeterminate – the payments made to-day are most unlikely to equal the prices quoted to-day multiplied by the goods exchanged to-day...

However, in the 'long-run' the relation was valid:

> Quite different is the position when we test the equation of exchange as summarizing the transactions of a large community for some interval such as a year... On that basis, we can say both that the payments, prices and transfers represented all refer to approximately the same period of time, and that the two sides of the equation are nearly equal in fact. ([1927] p. 131)

In Mitchell's model, 'revival' emerges out of 'depression' because the quantity of money in circulation 'exceeds current requirements'. Velocity is depressed and 'idle money' accumulates in banks, with a consequent reduction in interest rates. The money supply adjusts itself to conditions in the real sector. With higher expected profits, loans increase unchecked by monetary constraints, promoting 'an ascending spiral' in trade volumes and prices. Lags between contracts, deliveries and payments allow businesses to arrange finance.

> In Professor Fisher's terms...an increase of P, which swells the value of inventories, becomes the basis for an increase in M′... An increase in T...plays the same rôle, unless it is offset by a decline of prices... Thus most of the time P and T are the 'active' factors in the equation of exchange...([1927] pp. 134-137)

Elasticities of all factors affecting payments are such that occasionally, but not invariably, *PT* reaches the limits of money supply, bringing a liquidity crisis. The key to reconciling the 'contradictory statements' lies in an understanding of the lag mechanisms governing Fisher's variables:

> Relations which hold in long periods do not hold in short ones... I do not think that anything said here is incompatible with Professor Fisher's exposition...provided his term 'normally' is not taken in the sense of usually. Nor is the present discussion inconsistent with the celebrated theorem: 'Other things being equal, prices vary directly as the quantity of money in circulation.'...[or] 'Other things being equal, prices vary directly as the physical volume of trade.' ([1927] p. 138)

Mitchell was consistently hostile to the classical idea of equilibrium, and its complementary notion of a 'normal' economic state, because of their static nature. The true dynamics of economic adjustment are lagged and frictional, a view which for its time was surprisingly modern. As an alternative to the steady-state he proposed the concept of a dynamic equilibrium based on balance sheet and profit-and-loss accounting identities, since 'business enterprises cannot "carry on" unless in the long run their incomes exceed their outlays by a satisfactory margin of profits'.[10]

3.2.3. Mitchell's Theory of Business Cycles: a Problem of the 'Money Economy'

Quantity-theoretic ideas were accepted at least until the outbreak of war in 1939. The contentious issues continued to be Fisher's 'antecedent causes' and the nature of lags in price and profit adjustments. Mitchell's theory of business cycles can be described as Institutionalist philosophy superimposed on the framework of equations of exchange. In the letter to Clark cited above, he commented that his interest in the 'ebb and flow' of economic activity originated during Ph.D. research on price fluctuations during the suspension of the gold standard in the US from 1862 to 1879: the period of the 'greenbacks'. He soon became as dissatisfied with the purely quantitative approach as he

[10] [1927] pp. 187-8.

was with classical abstractions. The ideas crystallized into a monetary theory, and this in turn required him to work out the subject of 'business cycles as a *Vorarbeit* to the [more general study of the] Money Economy'.[11] The result of his investigations was the 1913 monograph, BC, regarded as 'an introduction to economic theory'.

Mitchell saw three institutions as critical to the propagation of economic fluctuations: (1) the money economy; (2) business enterprise (based largely on Veblen's book of 1904, *The Theory of Business Enterprise*); and (3) the 'system of prices'.[12] The goods/money dichotomy had been neglected by the classical economists, who rather sought the essence of behaviour beneath 'the money surface of things'. Thus the hypothesis that periodic financial collapse in the business world precipitated physical 'gluts' — theories of 'overproduction' — wrongly inverted the chain of causality, and financial institutions were by this reckoning of real rather than merely superficial importance. The third 'institution' was of paramount importance. Prosperity depended upon profits, which in turn reacted with changes prices through a process of feedback, thus constituting a system. It served 'as a social mechanism for carrying on the process of providing goods'. It was also the source of family incomes on which the economy depends, thereby permitting 'rational direction' of activity by providing an accounting basis. Most important, the margins obtaining 'hold out that hope of pecuniary profit, which is the motive power of the business world'.

Price behaviour together with the cumulative nature of fluctuations were the key to the Institutionalist view of business cycles. Each phase grows out of the conditions of the previous phase and gives rise to the next phase. Prosperity 'breeds' crisis because increased costs and over-stretched financial markets 'undermine the conditions upon which prosperity rests'.[13] The 'cause' of each new development is the preceding economic

[11]Letter to Lucy Sprague Mitchell, published in *Wesley Clair Mitchell*, pp. 62–6.

[12][1913] Chapter II.

[13]*Ibid.*, Chapter XIV.

state rather than some underlying factor, so that when considering the nature of business cycles, the object must be to try to determine 'how they run their course' rather than what 'causes' them. This was, of course, an interpretation of Fisher's equation: the p's drive the cycle in real volumes (the Q's) through lag structures, with the role of the circulation MV being determined secondarily through changes in expected profits (and presumably, though not explicitly, through investment). The point which distinguished Mitchell's theory from, say, that of Fisher and Marshall was the refusal to interpret cyclical behaviour as adjustment towards a 'normal' or equilibrium state. This is the significance of seeing 'causes' as preceding states, i.e., as a cumulative and evolutionary process.

These themes were elaborated, rather than developed, in BCPS. Mitchell framed an explanation of business cycles based on lagged relations among Fisher's variables. The motivation for the phenomena was 'certain technical exigencies of profit-making' by businessmen.[14] Prosperity is initially an effect of rising prices caused by such exogenous factors as an increase in the gold supply or government expenditures. This stimulates investment by a few sectors, but soon extends to the 'remoter branches of trade' as profit expectations rise.

> The sequence of growing demand, rising prices, increasing expectations of profit, swelling capitalization...and expanding credit keeps repeating itself on an ever growing scale... But eventually the process undermines its basis. ([1927] pp. 42–3)

Increased labour costs bring increased production costs which in turn lead to increased commodity prices. In the end 'these costs gain so much upon prospective selling prices as to narrow the anticipated margins of profit'. The 'confident tone of business expectations' gives way to apprehensiveness. Mitchell agreed with Veblen that a depression, once begun, persists, rather than giving way naturally to recovery.

Mitchell's final statement of the theory of business cycles was given in Chapter

[14]Specifically, as discussed in *The Theory of Business Enterprise*, Chapter vii.

V of BCPS, pp. 451-455. In the nineteenth century the debate had 'centered in the validity of rival doctrines' with only 'occasional citations of evidence'. It was a scholastic debate, the character of which was 'dialectical' rather then empirical. Crises, which were conceived of as discrete events, and consequently appeared 'abnormal', lying 'at the edge, rather than in the center, of the theoreticians's domain', hidden 'under the blanket [*ceteris paribus*] assumption'.[15] Although the work of Wade on the commercial cycle was evidence of the beginnings of a broader conception, Mitchell considered Juglar's study of credit cycles to be the 'turning point'. As well as being empirical, it attempted to show that the phases of 'prosperity, crisis and liquidation..."always follow one another in the same order"'. Juglar subsumed a theory of cycles in his theory of commercial crises, rather than the other way round, and later work was of much the same character, but carried out 'on a larger scale and more intensively'. Thus, many ideas 'which seem fresh to us' can be read 'between the lines of Juglar's book, if not in the text itself'. However, Mitchell criticized Juglar for failing to address the problem of a more general cycle. The intervals between his crises 'frequently contain two, and sometimes three, alternations of prosperity and depression', which must be identified from time-series data and annals. Furthermore, Juglar's use of the term 'periodic' was misleading, as it implied a regular cyclical frequency to later researchers.[16] Both Juglar and Wade appeared to see the cycle as a sequence of unitary swings between prosperity and depression, and 'a ghost of this dim notion still haunts' the minds of those looking for single 'causes'.

Despite the elegance and symmetry of these quantity-theoretic/institutionalist formulations, Mitchell had discovered, as had others before him, how complex and divergent economic behaviour could be. Inductive verification thus presented an almost intractable problem, for most observers had come to accept the existence

[15][1927], p. 451.

[16]It is unclear from this discussion whether Mitchell had read the 1900 pamphlet.

not of one phenomenon, but a congeries of interrelated phenomena. Increasing emphasis upon the diversities in respect to amplitude and timing found among the cyclical fluctuations of different processes is highly characteristic of recent work. ([1927] p. 454)

Mitchell argued for a 'general term to designate the whole', yet antiphonally, and characteristically, cautioned against generalization:

> the words we use set a trap for us. Starting with a vague conception of a group of seemingly interrelated phenomena we wish to study, we name it... If the name is a compound of words familiar in other uses, we may take their implications for facts. Assuming tacitly that we know what we have named, we may begin contriving explanations, when we should be trying to find out what our words mean. ([1927], p. 454)

This passage implies that Mitchell thought the business-cycle hypothesis itself to be premature and that taking 'implications for facts' might itself prejudice the necessary empirical enquiry. In a footnote, he argued for using the term in the plural rather than the alternative 'the business cycle' because he wanted to indicate that each sequence was a unique episode. The whole discussion indicates how shaky he judged the historical evidence to be. He nevertheless attempted a 'definition' of business cycles which would summarize its salient features for empirical testing.[17]

> Business cycles are a species of fluctuations in the economic activities of organized communities. The adjective 'business' restricts the concept to fluctuations in activities which are systematically conducted on a commercial basis. The noun 'cycles' bars out fluctuations which do not recur with a measure of regularity. ([1927] p. 468)

This ruled out (1) changes in conditions between the dates of crises (2) fluctuations not widely diffused throughout the economy (3) seasonal fluctuations and (4) 'long waves'. Hence

[17]Although he used the term 'definition' consistently, Mitchell really meant 'hypothesis'. This caused confusion later.

> the generic features and the distinguishing characteristics of business cycles...are recurrences of rise and decline in activity, affecting most of the economic processes of communities with well-developed business organization, not divisible into waves of amplitudes nearly equal to their own, and averaging...from about three to about six or seven years in duration. ([1927] p. 468)

This definition had its own problems: there were always 'doubtful cases'; and the empirical evaluation of the above 'definition' revealed 'some fluctuations which are difficult to classify on any scheme'.

3.3. THE SEARCH FOR EMPIRICAL BUSINESS CYCLES

Mitchell's theoretical ideas are all but forgotten, eclipsed by a contribution to statistical studies which was both more direct and more radical for the time; yet as indicated above he saw the two lines of enquiry as complementary rather than as mutually exclusive. The impetus for the development of theories, however, must come from observation, and so in the later stages of his work the empirical came to dominate the theoretical, if only as a means to an end. Having rejected pure theory as 'qualitative analysis', he argued that the new analysis must attack the problem through 'broad observation upon average behavior' of agents in the 'money economy', defending the approach by analogy to statistical mechanics with its idea of predictable motion of aggregate gases rather than of individual molecules.[18]

3.3.1. Early Statistical Work on Price Behaviour

Mitchell practised what he preached from the outset. In his Ph.D. dissertation, published as the *History of the Greenbacks*, the main emphasis was on the effect of a paper currency on prices and the distribution of wealth. In the chapter on profits he gave evidence that real wages and incomes fell during the greenback regime, and hence that

[18][1937] p. 34.

'as a rule profits must have increased more rapidly than prices'. The theme was further developed in the 'frightful second book', and this shows and early dissatisfaction with the orthodoxy of 'normal', or equilibrium, behaviour and an interest in the 'frictional' properties of short-run adjustment.[19] The greenback period featured 'two revolutions' in prices: 'first a great rise, then a great fall'. In both swings, fluctuations of 'extraordinary...violence, rapidity, and inclusiveness' were experienced, a consequence chiefly of instabilities in monetary policy.

Although Mitchell intended the 1913 volume as a theoretical discussion, it also contained substantial empirical data on prices. That its publication established him as a public figure was largely a result of the theoretical content, and the research programme set out there remained substantially unaltered for the rest of his life. Neither the theoretical nor the empirical enquiry was ever completed. War and depression intervened, and the second volume was delayed until 1927. Nevertheless, three important landmarks in his intellectual development were passed in the intervening period: (1) *The Making and Using of Index Numbers* was published in 1915; (2) the National Bureau of Economic Research was established in 1920; and (3) the classification of economic conditions from contemporary records *(Business Annals)* was published in 1926.

Index Numbers became the standard work upon the subject in the interwar period. By 1915, there were numerous published series, both commercial and official, although they were 'both difficult and troublesome to compile' when attempting to measure price changes. The need for accurate measurement was now compelling because of the price inflation since 1896. Mitchell was already aware of divergences in behaviour, as shown in the paths of 230 wholesale commodity prices for the years 1890–1913. In fact,

> (1)...no two of the commodities quoted underwent the same changes in price...
>
> (2) In every year a considerable proportion of the commodities rose in price, a considerable proportion fell, and a somewhat smaller proportion remained

[19]*Gold, Prices, and Wages under the Greenback Standard* [1908].

unchanged. (3) The range covered even by the fluctuations from one year to the next was very wide... ([1915] p.13).

The techniques of indexation — weighting, averaging and aggregating — and the rudiments of a 'leading indicator' forecasting approach were also explored (pp. 21-2). All methods were seen to have drawbacks, an example of his constitutional ambivalence: careful and detailed explanation of a novel technique, followed immediately by a discussion of its inapplicability to the issue in question. He concluded that while index numbers are a 'convenient concentrated extract' of variations, they did not competently represent 'all the facts' which they summarize, and that hence both the index and its constituents should be published.

3.3.2. Empirical Methods at the National Bureau of Economic Research to 1929: 'Business Cycles' Crystallized

During the war years, Mitchell served as head of the Price Section of the Division of Planning and Statistics of the War Industries Board, and so had direct experience of the 'social possibilities'. As Director of Research after 1920, he collaborated in the NBER's first project, *Income in the United States: Its Amount and Distribution, 1909-1919.*[20] However, he always intended the NBER to implement a business-cycle research programme as soon as possible. The problem became urgent with the severe decline in output in 1920 (the postwar 'return to normalcy', as President Harding expressed it in 1921). Two works which emerged in this period, *Business Annals* and BCPS, became the foundations of the concept of 'business cycles' as it has been known ever since.

Business Annals represents a logical discontinuity in Mitchell's thinking. It was an attempt to characterize 'general business conditions' in the economies of the US, the UK, France and Germany in every year from 1790, as deduced from contemporary press

[20]See Dorfman in *Wesley Clair Mitchell*, pp. 134-5.

reports. Mitchell was by this time increasingly conscious of the particularity rather than the generality of time-series behaviour, and it is not at all clear what purpose he thought these characterizations might serve. Although the nominal authority of *Business Annals* was that of Willard Thorp, the research strategy and direction were Mitchell's. A four-phase cycle was assumed *a priori* — 'prosperity', 'recession', 'depression' and 'revival' — but no empirical basis for this schema was given. 'Recession' was proposed as an alternative to 'crisis', which Mitchell considered ambiguous. In a long introductory chapter he questioned the 'trustworthiness' of the press reports on which the work was based. Correspondences with the 'truth' were conceded to be only 'approximate', but results could be tested 'objectively' by comparison with time-series evidence. Such analyses were shown for the US economy against the Persons, Snyder and AT&T indices of trade volumes, and for the UK against Dr. Thomas's Index of British Business Cycles, 1855-1914.[21] The correspondences among the American series and the annals were very close; but less so in the case of the British index.

Thorp was less confident about synoptic summaries of the 'phases' of the cycle:

> Both industrially and geographically, quite different business conditions may exist within one country at the same time...[so the] phase-summary...gives the central tendency from which reviewers state exceptions. 'Prosperity except in textiles' is obviously not complete prosperity... [For example] although observers generally agree that 1925 was a prosperous year for the United States, they also note that although construction activity reached new records, the textile industries were depressed, and that although the Florida boom created great activity in the South, parts of New England were virtually in a state of depression. ([1926] p. 103)

[21]These were known generically as 'business barometers', and were meant as coincident, leading and lagging indicators of the 'trade cycle'. Their chief proponents in the US were Warren Persons, Thomas Babson and Alvin Hanson. The usual procedure was to 'detrend' and then smooth the data either by moving average or by eye to remove noise. Although widely accepted at the time, they are of questionable value because of biases inherent in the data transformations. Barometers are discussed at greater length in Chapter 4 below. See also Morgan [1990], Chapter 2.

Mitchell apparently hoped that evidence of 'general business conditions' would support his institutionalist ideas, yet true to form he was sceptical of any evidence he actually thought that he had found. The suspicion is that *Business Annals* was judged by its authors to have been a failure.

BCPS is also characteristically full of antiphonal statements, as the opening passage shows:

> In trying to prove their divergent explanations correct, successive theorists did prove that business cycles were more intricate phenomena than any of them had surmised... This lesson from experience...has been confirmed in recent years by work with statistics... But we have no statistical evidence of business cycles as wholes. What the data show us are the fluctuations of particular processes... In certain cases we...average the data...and say that we have 'indexes' of cyclical fluctuations in wholesale prices, physical production, the volume of trade, or even 'general business conditions'. Yet even the most inclusive indexes we can make fall far short of showing all that we mean by business cycles. The more intensively we work the more we realize that this term is a synthetic product of our imagination — a product whose history is characteristic of our ways of learning. Overtaken by a series of strange experiences, our predecessors leaped to a broad conception, gave it a name, and began to invent explanations, as if they knew what their words meant. But in the process of explaining they demonstrated how inadequate their knowledge was.... An enquiry into business cycles, then, cannot wisely begin by defining the general concept... It should begin rather with the individual processes which can be studied objectively, seeking to find what these processes are, how they affect each other, and what sort of whole they make up. ([1927] p 1)

Mitchell's emphasis changed over the three volumes. The 1913 work was largely theoretical: only four of the 14 chapters contain any statistics. By 1927 the theoretical content had shrunk to under one-half, and by 1946 the emphasis had become entirely empirical. In BCPS he commented that most business-cycle theory was still governed by intellectual processes 'which would have seemed familiar to Sismondi and Ricardo'. It was hoped that theoretical and empirical sources would each contribute to the advancement of the other. Such a programme would ensure that 'rational hypotheses' would focus the attention of the empirical investigator, who would in turn 'determine the relations among and the relative importance of the numerous factors stressed by business-cycle theories'.

The potential circularity of such a process was not lost on Mitchell. Although basing his hopes on correlation and filtering techniques for the advancement of the 'knowledge' of business cycles, he felt that the data were inadequate to the purpose because the behaviour 'from case to case' was so diverse that generalization about individual processes was impossible. In addition, both the length of available time series and the comparability among those of sufficient length were inadequate. He plotted several time series to illustrate the point, reproduced here as Figure 3.3.1. This chart indicates how the secular tendency dominated real-sector series in contrast to price, monetary and labour market indicators. Because the latter appeared stationary, Mitchell speculated that if the 'trend' were 'removed' from the former it might then be possible to identify business cycles across a wide spectrum of activity. He proposed the standard scheme of four unobserved components: trend, cycle, seasonal and irregular, yet on the other hand was fully aware of the disadvantages of moving average filtering and curve fitting. For example, Chart 2, p. 217, demonstrates how different linear trend lines produced different residuals, and how difficult it was to find an optimal fit. The main difficulty was that of interpretation. A linear 'trend', for example, must be interpreted as a 'future increase without limit'. Thus, if a 'straight line fitted a given series well', were such 'fits of mathematical curves discoveries in economics?' Alternatively, could not the investigator 'choose curves whose mathematical implications correspond to [his] causal hypotheses', which could then be modified in the light of empirical evidence?[22] Mitchell thought it likely that growth dominated the industrial economies, that the 'great

[22][1927] p. 221.

commercial nations' were showing a tendency 'to produce an ever larger supply of goods':

> On this basis, cyclical fluctuations appear as alternating accelerations and retardations in the pace of a more fundamental process. ([1927] p. 224)

The main difficulty with time-series data was, however, seen at the time as the estimation of the several unobserved components, and especially the separation of cyclicals from noise. The problem was that the latter came not from zero-mean, serially independent random shocks as, for example, Edgeworth had hoped, but from a diversity of both random and persistent disturbances — wars, floods famines, strikes, technological changes, etc. — and from a superposition of measurement errors on these. The remedy, an attempt to isolate 'true' noise from persistent disturbances, had its own problems. It was hence impossible satisfactorily to identify the components of interest: they were all to an extent contaminated by each other and each component was effectively stochastic.

Rather than abandoning time-series analysis in the face of such difficulties, Mitchell sought to make it 'more rigorous'. He failed to specify the exact methods for achieving this, but seemed to be saying that each component, together with noise, could be estimated in some way and then smoothed by moving average or other means. The irregularities still there would make the transformed series inappropriate for forecasting, but would give the theorist some information. Even so, past changes in the cyclical components had been 'so sudden, so frequent and so considerable [that they made] the notion of a "normal cycle" inappropriate'. He seemed to be saying that a theory of business cycles would propose some kernel of common behaviour, while at the same time arguing that the idea of a 'typical' cycle was meaningless.

Mitchell was also sceptical about the crude filtering methods used in the construction of business barometers. For example, Babson claimed that he employed 'Newton's Second Law' in fitting linear time-trends, by which he meant that as much of the series was 'above the line' as 'below the line' (see Chart 8, shown here as Figure

3.3.2). Apart from distrusting the smoothing technique, Mitchell was also not very hopeful that empirical business cycles could ever be tracked with any reliability. It was

> highly improbable that all the curves will reach the crests and troughs of their successive cycles in the same months. As a rule the crests and troughs of the various curves are distributed over periods of several months — often over periods of more than a year. ([1927] p. 280)

Inspection of leads and lags in the timing of the various peaks and troughs presented 'that mixture of uniformity and differences with which economic statistics commonly confront us'. The lead-lag relationships were found to be in general as variable as any other measure and therefore of doubtful value. None of the existing business barometers

> give, or are meant to give, an adequate picture of business cycles. For, as has been said several times, business cycles are congeries of cyclical fluctuations in a large number of economic activities, fluctuations which differ widely in amplitude and considerably in timing... A real chart of one business cycle would be a hopelessly complex tangle of hundreds of curves. ([1927] p. 309)

(See Figure 3.3.3.)

Mitchell had thus found, even at this comparatively early stage, evidence against a cycle in 'general conditions' capable of being captured by a single index. Unfortunately, having already invested so much 'human capital' in these ideas he felt obliged to persist, bravely, groping in the dark, as it were, for 'typical' business-cycle behaviour. Although barometers might be of some use in comparing the amplitude, duration and timing of successive cycles, they were of little value 'for intensive theoretical work'. The problems of summarizing data as 'general conditions' were both technical and interpretative. Aggregates obtained merely by averaging index numbers of such diverse series as prices, interest rates and output volumes had no direct interpretation because the constituent series were 'incommensurable'; yet such indices as Dr. Thomas's and the AT&T were constructed in just such a manner.[23] No index could be 'general' unless it included all activities. Those constructed merely from data conveniently to hand would be misleading for the study of business cycles. Nor was there any obvious method of finding appropriate weights for the constituents empirically.

The application of statistical distributions to turning points in the business barometers was not much more promising.[24] Histograms were plotted for each in respect of monthly percentage changes, durations and amplitudes of cycles. Identification of historical cycles was attempted from turning points in five such series: the AT&T, Frickey's Clearings, the two Snyder indices and Persons's trade index. The inferences from these were obscure: some of the 'lesser' fluctuations were noisy, and there sometimes occurred 'double or triple' peaks or troughs.[25] The 'irregularities of contour...causes considerable difficulty when one tries to count the number of business cycles in a given period'. Nevertheless, by comparing turning points, in this case monthly, Mitchell managed to find 13 cycles for the United States for the years 1878-1924. For many of these the span was greater than 12 months, but Mitchell thought that smoothing by means of 'free-hand' curves would improve the agreement among the several series. Those 'cycles' culminating in 1882, 1893, 1907, 1917 and 1920 were clearly visible in the curves; but in all periods 'there are stretches when the cyclical fluctuations are less easy to identify', especially the later 1880s, the mid-1890s, the early 1900s, 1910-1913 and 1923-1924. It 'would be hard [from these curves] to lay down rules for determining...what movements...shall be counted a business cycle'. Also, multiple peaks and troughs 'frequently' occurred.

Mitchell concluded from this extensive statistical analysis that

[23]Morgan has described Persons's attempts at such inappropriate composites as 'a strange collection' of variables. See Chapter 4 below.

[24]*Ibid.*, pp. 337-54.

[25]In fact, an examination of Table 14 shows 27 turning points between 1878 and 1924 in the AT&T index, of which ten were multiples.

> As one analyzes successive business cycles in various ways, one find evidence, even in the bleak statistical records here used, that each cycle has special characteristics of its own, or rather a special combination of characteristics. More intensive study carried over a wider range in time and space would strengthen this impression. *Strictly speaking, every business cycle is a unique historical episode, differing in significant ways from all its predecessors, and never to be repeated in the future* [emphasis mine]. ([1927] p. 354)

He hoped that the study of particularities would enable the investigator to 'see that a given rule does apply to cases which at first sight seems to form exceptions', although it is not specified exactly how such a contradictory process could operate. A synthesis of historical, quantitative and theoretical studies should elucidate the problem, beginning with a synopsis of prevailing conditions from contemporary sources:

> ...some of the statisticians...have supplemented their time series on occasion by preparing annual summaries of business conditions. But the histories have dealt largely with what was common in the episodes treated, and the summaries have been confined to rather brief periods in a few countries. *For theoretical uses* [emphasis mine], there is needed a systematic record of cyclical alternations of prosperity and depression, covering all countries in which the phenomena have appeared, and designed to make clear the recurrent features of the alternations...
> In the present book, we can use the volume of *Business Annals* much as we use the coördinate statistics. ([1927], pp. 361-2)

It might be possible thus to see common elements in episodes which at first seemed unconnected, recognizing at the same time the methodological pitfalls of subjectivity and circularity. The disadvantage was in the subjectivity both of the compiler and the sources, and so business indices must be used as corroboration; yet these, because of trend adjustments and smoothing were also not wholly satisfactory. Comparisons were seen as tests as much of one source as another. The 'broadest conclusion' of the *Annals* was that

> there is no 'normal state of trade.' The phrase is common both in treatises upon economic theory and in the talk of business men. Yet the historical record shows

no reality corresponding to this figment of the imagination... [The] annals show that the only normal condition is a state of change... ([1927] p. 376)

Mitchell justified the use of the term 'cycle' by an appeal to the evidence of the annals that different phases recur, and that these recurrences 'lend themselves to measurement'.[26] However, 'the term "periodicity" we should not use with reference to business cycles' because of the irregularity of 'time intervals', and moreover 'no two [of them] in all the array seem precisely alike'.[27]

Business cycles differ in their duration as wholes and in the relative duration of their component phases; they differ in industrial and geographical scope; they differ in intensity; they differ in the features which attain prominence; they differ in the quickness and the uniformity with which they sweep from one country to another. ([1927] p. 383)

The answer to such behavioural divergences lay in frequency distributions, 'variations around a central tendency, a technique which reveals the existence of formerly unsuspected uniformities among variations themselves'.

Objections to existing theories lay not in their level of abstraction but in their reliance upon the idea of equilibrium, the assumption of which would preempt the investigation by focusing it on how the equilibrium 'is overcome at times and how it presently reasserts itself'.[28] Mitchell, in rejecting such a 'point of departure' in favour of the four phases of *Business Annals*, was himself making a strong and preemptive theoretical assumption:

The best framework for a discussion of how business cycles run their course is that provided by the phases of these cycles — prosperity, recession, depression and revival. Our collection of business annals and of theories lend themselves

[26]A 1922 conference at the Carnegie Institution had attempted to find a definition of 'cycle' applicable across all the sciences. Mitchell quotes that of Dr. F.E. Clements as the most satisfactory: 'a recurrence of different phases of plus and minus departures, which are often susceptible of exact measurement'. (BCPS, p. 377)

[27]*Ibid.*, pp. 377–8.

[28]*Ibid.*, p. 462.

> readily to this plan; for the annals mark changes in the tides of activity, and most current theories explain crises by what happens in prosperity and revivals by what happens in depressions. ([1927] p. 472)

Time series would be segmented 'for use in a discussion which treats first all periods of prosperity as a unit, then all periods of recession, and so on'. These units were taken from *Business Annals*, and purported to mark off, for each country, the dates 'not only of successive cycles, but also the successive phases of each cycle'. The data would be therefore analysed by comparison with a 'standard pattern derived from the annals' of successive phases rather than by identifying specific phases in each individual time series. The expectation was that specific and 'reference' phases would 'correspond fairly well', but that also there would be cases of 'divergence in timing — cases which our plan will throw into high relief, and from which we will learn much of interest'.

3.3.3. Empirical Investigations 1929-1939: 'Measurement without Theory'

'Factual testing' of the business cycle concept by the NBER continued and expanded. In 'Testing Business Cycles' published in 1929, and attributed to Mitchell, the technique of finding 'reference dates' by comparing time-series data with the annals was further developed.[29] Although foreshadowed in BCPS, the term 'reference cycle' appeared explicitly for the first time in this article as a set of dates which 'show the month and year when economic revivals and recessions occurred', and which 'mark, as nearly as may be, the beginning and ending of successive cycles in business at large'.[30] This was an important departure, for it marked a formal break with the cautious process of simultaneous theoretical and empirical enquiries, and seems to imply the failure of that research strategy. Previously, Mitchell had avoided such generalizations because of the insufficiency of the data. 'Finding out more about the facts' must precede the definition

[29] NBER *News-Bulletin No. 31*, March 1, 1929, pp. 1-8.

[30] [1929a] p. 2.

of a 'general concept'; and this had consisted in the objective study of individual processes and their interactions.[31] The revised techniques were claimed as an attempt objectively to compare individual processes against some general reference in order to arrive at 'central tendencies' in, and measures of variability of, behaviour over time. Individual series were broken into 'reference cycle segments': that is, the series was divided into chunks conforming to the reference dates, rather than to the dates of its own maxima and minima. Each reference cycle was itself decomposed into eight segments, three of expansion, three of contraction, and two periods of three months each centred on the reference maximum ('recession') and minimum ('revival'). For specific series, the average for each reference cycle segment was taken in index form and plotted as a curve (see Figure 3.3.4 for an example).

The analysis was intended as a systematization of

> comparisons between the cyclical fluctuations of the same series in different cycles and of different series in the same cycle. It also eliminates the greater part of the secular trends. ([1929a] p. 2)

Specific cycles, i.e., the trough-peak-trough sequence in each series, were also plotted without reference to the general dates. Mitchell conceded that these turning points

> ...are no more likely to coincide precisely with the reference cycles in timing than the price fluctuations of a single commodity are likely to agree precisely in timing with the changes in an index number. A few series show no cycles at all; others show cycles which diverge widely from the standard pattern. These 'non-conforming' cases are especially interesting theoretically. But most series conform more or less closely...
>
> When applied to a large number of time series, the scheme provides a searching test of the notion that business cycles are a genuine species of economic phenomena. If these cycles were the figment of a stereotyping imagination, little similarity could be expected either in the behaviour of any one

[31]This approach has been characterized by Morgan [1990] as 'Mitchell's empirical (but non-operational) ideas' (p.66).

series in different reference cycles, or in the behaviour of different series in any one reference cycle. ([1929a] pp. 2-3)

In the same year Mitchell published an article in *Recent Economic Changes*, the report of the Conference on Unemployment set up by President Harding in 1921. The emphasis of this document was on 'structural' changes rather than on 'cycles': i.e., population growth, technological innovations and the comparative postwar ascendency of the United States over the European economies. Improvements in industrial and agricultural productivity during the 1920s were seen as bringing hardships in the form of the failure of uncompetitive businesses, the redundancy of certain trades and the decline both of older industries and of geographical regions. Price declines, especially during the severe fall in output from 1919 to 1921, were thought to be a destabilizing factor because of the direct effect on profits. Mitchell believed that the argument, then current, that business cycles had been 'ironed out' was premature and saw current conditions as part of an evolving state, more or less severe, of economic distress.

In 1935, NBER *Research Bulletin No. 57* contained 'The National Bureau's Measures of Cyclical Behavior', by Mitchell and Arthur Burns, an expansion of the 1929 article. The emphasis was beginning to shift from the monetary to the real sector. The specimen series was now industrial: monthly coke production in the United States, 1914-1933. The analysis was wider. Business cycles were seen as 'interrelated fluctuations in many economic processes' but which 'differ from one another in timing, amplitude and relationship to the expansions and contractions of general business'. Knowledge of these differences had been 'piecemeal and vague for the most part', and thus

> economists have had to construct their theories of business cycles without adequate knowledge of the phenomena they were trying to explain, and without adequate means of testing the relations between their hypotheses and what actually happens. ([1935] p. 1)

Filtering techniques were still unsatisfactory. 'Reference' dating was as in the 1929 paper, but extended to the UK, France and pre-1933 Germany. Series were analyzed both for reference and their specific cycles to determine if there were systematic timing relations between the former and latter. The object was to 'lay a satisfactory basis for a theory of business cycles'. The technique of indexing individual series' average behaviour, first discussed in 1929 (the so-called 'cycle relatives'), 'eliminates from the original data what we call the "inter-cycle" portion of the trend'.[32] The analysis was extended by the use of index numbers to such activities as US exports, factory payrolls, bank clearings, US and UK wholesale prices AT&T index of industrial activity and indices of motor vehicle production and capital goods in the United States. The AT&T index was the only series adjusted for 'trend'. An examination of the pattern charts shows a wide variation in profiles both across series and across cycles.

An analysis of the recent 'great depression' by Burns and Mitchell appeared in *NBER Research Bulletin No. 61*, November 9, 1936, under the title 'Production during the American Business Cycle of 1927-1933'. The authors found that the depression of the 1930s was not propagated by the stock market crash. Although there was 'a definite concentration of peaks [in time series] around June 1929', the 'building boom reached its highest point in February 1928...wholesale prices in September [1928]...and production of consumer goods in November [1928]'. In all, 40 series were analyzed, with the greater number peaking within five months of June 1929. The trough of 1933 was more difficult to date: 17 series reached bottom in March, and seven more within a two month interval either side; but they found a 'secondary concentration' around the summer of 1932, thus establishing a 'double bottom', thought to be a common occurrence. (This identification problem for the 'cycles' of the 1930s was never satisfactorily solved.) The 40 series included a number of 'trend-adjusted' indices of business conditions (e.g., the AT&T, Persons, Ayres and Axe-Houghton); indices of production (Babson's, the FRB and Standard); indices of industrial groups; and single series indicators such as pig iron production, freight car loadings, wholesale prices and banking and money market

[32][1935] p. 1.

statistics. The pattern charts again showed diverse behaviour. In their discussion of the expansion and contraction 'phases' the authors expanded the number of series to 73, which gave an even more diffused picture of the 1928–9 peak.

> [Total] construction contracts declined after February 1928... Orders for passenger-train cars and apple shipments followed within six months. Later that year contracts for industrial buildings and the production of oak flooring, inner tubes and solid tires turned downward. Portland cement began to decline in January 1929, and five series in February, including passenger automobiles. Thereafter the recession spread rapidly...([1936] p. 18)

A few of the food and construction series turned round in the summer of 1930 as the government attempted to reverse the tide of unemployment.

'Statistical Indicators of Cyclical Revivals' by Burns and Mitchell, published in May 1938 in *Research Bulletin No. 69*, was a discussion of the problems of applying leading indicators as a potential forecasting tool. The authors were trying to predict an upturn in the economy (by which they meant the reference dates) based on the analysis of '487 statistical series in monthly or quarterly form'.

> But one of the clearest teachings of experience is that every business cycle has features that are peculiar to it. Accordingly, no one who knows the past expects that what happened during any earlier business revival will repeat itself during the next revival. Even average experience over several revivals establishes no more than a presumption concerning the general character of the developments that may be anticipated when the next business tide begins to rise. ([1938] p. 1)

The proposed list of indicator variables was not seen as a '"forecasting machine", but rather [as] a registering device...' to assist in the interpretation of the drift of historical fluctuations. After removing seasonal variations, turning points in each series were found. There were series that lead, coincide with, and lag the reference dates

> with some regularity. As this statement implies, a business-cycle revival is not an event that happens in a single month, but a complicated series of changes that

> occur cumulatively in various economic processes during a period that may last a year or more. ([1938] p. 2)

Thus by a process largely of judgment, the most 'convenient' point to use for a revival was determined as the month 'around which cluster the cyclical upturns in different types of production, construction...commodity prices, merchandising, employment...security prices...interest rates...and other economic variables of which we have statistical records'. Unfortunately, from the reference dates it appeared that the

> durations of business cycles...differ so much and so irregularly that they give little help in judging when the next cyclical turn may occur. And the durations of cyclical contractions appear to be even more variable than those of cyclical expansions. ([1938] p. 3)

The specification of an ideal leading indicator was given: it would cover at least 50 years; it would lead the turning point regularly by about six months; it would have no erratic movements; its cyclical movement would be pronounced; and finally, it would conform so well to the general movement that its future behaviour is predictable. Not surprisingly the authors found no such series, and so it was 'unsafe to base judgements...upon the behavior of any one series, or of a few series'. They gave a list of some 75 series which were the least untrustworthy. Difficulties in forecasting upturns were given as (1) monthly records not being up to date; (2) changes in seasonals from year to year; (3) the erratic nature of time series making it difficult to recognize an upturn; and (4) the general unpredictability of economic behaviour.

3.3.4. The 'Definitive' Volume of 1946

Measuring Business Cycles was intended as an interim volume, a summary of the work of the NBER up to the entry of the United States into the World War II. The 'definition' by now had been amplified.

> Business cycles are a type of organization found in the aggregate economic activity of nations that organize their work mainly in business enterprises: a cycle

> consists of expansions occurring at about the same time in many economic activities, followed by similarly general recessions, contractions, and revivals which merge into the expansion phase of the next cycle; this sequence of changes is recurrent but not periodic; in duration business cycles vary from more than one year to ten or twelve years; they are not divisible into shorter cycles of similar character with amplitudes approaching their own. ([1946] p. 3)

The 'definition' was immediately qualified by restatement as an hypothesis:

> Considerable evidence can now be cited to support [the above definition's] every clause. But an intensive study of the best available records is necessary if we are to ascertain conclusively whether many economic activities really fluctuate in unison as the definition states, and how different activities behave with respect to the alleged cycles. ([1946] p. 5)

Existing theories were again rejected, though this time on empirical grounds:

> To say that business cycles are departures from and returns toward a normal state of trade or a position of equilibrium, or that they are movements resulting from discrepancies between market and natural rates of interest, will not help, because we cannot observe normal states of trade, equilibrium positions, or natural interest rates. [1946] p. 5)

So many problems seemed to be raised by the 'definition' that the authors were constrained to give a phrase-by-phrase critique. The same difficulties noted in 1927, 1935, 1936 and 1938 presented themselves. For example, what is meant by 'economic activities'? If 'recurrent but not periodic', can business cycles be distinguished from seasonal fluctuations, secular movements and noise? Are there 'several sets of general cycles running concurrently, each set perhaps periodic but combining with cycles of other periods to produce the variability our definition admits'?[33] To what extent should empirical work rely upon aggregate measures, and to what extent upon the component parts? Are business cycles really continuous? How general are these movements? Are turning points in time-series data clustered, or are there so many leads and lags that

[33]See Schumpeter [1939] for a discussion of this hypothesis.

'...the divergences in timing of these [specific] cycles produce an unchanging total'? The timing issue posed such potential difficulties that the authors were forced to admit that if '...there were no bunching of cyclical turns, there would be no business cycles answering to our definition'. Even this catalogue of questions was not seen as exhaustive: anyone attempting to explain business cycles must examine at some stage the historical record in order to 'know what he is explaining'. Defects in previous theories were largely the result of 'a sadly incomplete, sometimes badly twisted, knowledge of the facts'. Nevertheless, the process of enquiry could not be separated into 'inductive' and 'deductive' stages, because no research had ever been able to 'devise a hypothesis concerning business cycles entirely apart from the facts to be explained'.

> Hence, an investigator who seeks earnestly to discover the cause or causes of business cycles should not restrict himself to testing any single hypothesis. If he concludes that the facts of experience are consistent with one hypothesis, he should make sure that they are not equally consistent with other hypotheses. ([1946] p. 9)

The observational problems were acknowledged. On pp. 12-14 are found the plaintive statements

> business cycles...can be seen through a cloud of witnesses only by the eye of the mind...
>
> [When] we speak of 'observing' business cycles we use figurative language... What we literally observe is not a congeries of economic activities rising and falling in unison, but changes in the readings taken from many recording instruments of varying reliability.

To deal with these difficulties, the approach (following the 1930s research bulletins) was to identify the specific cycles in more than 800 seasonally adjusted time-series and then, in order to avoid an unwieldy number of bivariate comparisons, to relate them to a 'common unit' of measurement, the 'reference dates'. These had now become the backbone of the analysis. They purport to mark off the troughs and peaks of successive business cycles, and to measure the leads and lags of specific-cycle troughs and peaks from these benchmarks. This step is the crux of the investigation; it involves passing from the specific cycles of individual time series, which readers not embarrassed by experience are likely to think of as objective 'facts', to business cycles... ([1946] p. 12)

Two assumptions were made about the nature of business cycles:

First, we shall assume that [they] run a continuous round... Second, we shall assume that it is sufficient to mark off the dates when expansions and contractions in general business activity culminate [implying] that business activity as a whole does not linger [at troughs or peaks] but commences rather promptly to decline [or] to rise. ([1946] p. 71)

These dates were justified as a 'common denominator' against which to compare 'cyclical' movements in specific time series, but their derivation raised yet more difficult questions. How could aggregate activity be measured? How could noise be filtered out, both in aggregated and disaggregated measures, and, indeed, should it be? In any case the authors were quick to point out that the reference dates themselves were only 'a tool of analysis'. Otherwise

[an] investigator whose main concern was to establish a chronology of business cycles might wish to spend years in studying specific cycles before venturing to date the peaks and troughs of business cycles. But our interest in a reference scale is largely incidental to theoretical ends. Early in the investigation it seemed wise to determine whether our working definition of business cycles was a promising guide to further inquiry [and if so] to organize the work in such fashion that we could learn quickly...how the cyclical fluctuations of leading economic activities are related to one another. ([1946] p. 72)

Thus, although apparently contradicting the multivariate nature of the business-cycle hypothesis, the reference cycle was actually quite consistent with it. The objective of finding theoretical explanations, rather than of predicting future episodes, required that central tendencies be extracted from the time-series data. To meet the objective, the reference dates should ideally have been a set of turning points in an aggregate measure '...given a definite meaning and made conceptually measurable by identifying it with gross national product...'[34] The possibility of calculating central tendencies in 'each cluster of turning points in individual economic activities' was discussed as a proxy for aggregate activity. Even if an aggregate existed, could it be given operational significance for the purpose of business cycle analysis? Could it then be checked against cycles in specific series, and in that case, what series would be appropriate? Furthermore, it

> will also be necessary to determine whether cycles in aggregate activity are sufficiently diffused through the economy to rate as business cycles. What series or processes should be examined to determine this issue?... Should not a fluctuation that satisfies the criterion of diffusion meet also certain criteria of duration and amplitude, phase by phase as well as for the entire fluctuation, before it is admitted to the family of business cycles? Finally, how should we distinguish between cyclical and erratic in aggregate activity...? ([1946] pp.71-2)

At the time, however, only annual series were available[35]. 'Monthly index numbers of the physical volume of production [failed] to meet our needs', since they were compiled from indices whose constituents changed over time, and which could thus shift the dates of turning points. Indices with unchanging constituents which were of sufficient length, such as prices, financial data and 'business indexes' were considered to be 'inadequate gauges of business cycles' because their coverage was not sufficiently diffused. By the now standard NBER technique of comparing annals with time-series data, a set of monthly dates had been found. This was viewed with suspicion owing to the uneven range and quality of the statistical records, and to the large range of behavioural variation

[34]As defined by Kuznets in *National Income and Capital Formation, 1919-1935*, NBER [1937].

[35]A quarterly US GNP series was not published until 1947.

in the data. Correspondences were then sought between reference turns and 'specific-cycle' turns in each time series. In many cases this correspondence

> is imperfect; sometimes it is negligible. Thus if two specific cycles occupy approximately the same period as a business cycle, the trough between the two specific cycles and at least one peak will not correspond to any reference turn. ([1946] p. 117)

For such occurrences, the matching operation became difficult, requiring 'objectivity' and 'self-restraint'. The rule was that if two turns in different months be designated as 'corresponding', there must be no extra turns, either reference or specific, in the interval between the two. The object was to 'help to weed out "extra" cycles, that is, specific-cycle movements unrelated to business cycles'. If specific turns appeared either side of a reference turn, each meeting the above criteria, the first would be treated as corresponding only if it was within three months of the reference point. Otherwise, the specific turn would be treated as non-corresponding. The difficulty in applying the technique was that '...the rules may reduce unduly the number of timing comparisons in series that conform well to business cycles' thus biasing the calculations of average behaviour. The rules were therefore relaxed for 'well-conforming' series so that any specific turn that deviates less than a full phase from the reference turn was taken to be 'corresponding'. Furthermore, any sufficiently 'mild' specific turn in the neighbourhood of a reference date was ignored, though no criterion for mildness was given. In practice, non-corresponding turns (as for example in the case of structural steel orders during 1923–6) were discarded if there were any other possible turns which improved the correspondence *even though the rules were violated.* These criteria applied only in the case of series which were judged closely to conform to the reference dates. There was no quantitative test for closeness, although measures of conformity and 'other evidence' were taken into account.[36]

[36] [1946] pp. 116–128.

These highly flexible rules seem to have been honoured more in the breach than in the observance. The results were given for specific-cycle turning points, series by series, together with leads and lags.[37] The average deviations of turning points from reference dates ranged from four to about six months; but the decisive feature of these tables was the apparent unpredictability of 'leads', 'lags' and 'correspondences' with reference turns, even after substantial filtering out of inconvenient specific turns. The authors had strong reservations about the accuracy of these leads and lags, because 'some business cycle turns are more difficult to date than others'. An alternative to finding specific cycles, following the 1929 paper, was to take the value of each time series at the reference date as the peak or trough for that specific cycle. These were denoted, confusingly enough, as 'reference cycle patterns' and were seen as a way of dealing with the variability of individual time series. The result was, nevertheless, that however timing was measured, the lead and lag relations were unique for each reference episode in the sample.

Considerable variation was encountered in the specific cycles in 828 monthly and quarterly American series. All but three percent of these were found to have sequences of peaks and troughs. From a very small sample of these series — employment data from ten US manufacturing industries — a 'tendency toward a common rhythm' was detected. However, six of the ten had either 'extra' or 'missing' cycles; and furthermore, where corresponding turns were found in all ten series, they were dispersed, suggesting that 'turning zone' was a more appropriate term than 'turning point' for business cycles. For example

> every series turned up in 1932-33, but the trough spans 10 months. The 'turning zone' is still longer at the upturn in 1921. On account of the divergencies in the timing of cyclical turns, all industries experience specific-cycle expansion or all

[37] E.g., Table S1, p. 26.

experience specific-cycle contraction in only 69 of the 239 months covered, less than a third of the full period.

> Let the reader imagine a thousand time series arranged on [this] plan, and he will begin to face in their full complexity the timing relations among the cyclical movements of actual life. ([1946] p. 70)

(Burns and Mitchell never found a satisfactory method of fixing reference dates, and this, in the spirit of objectivity, they acknowledged.)

The pattern of each of these hundreds of time series was calculated, for both 'specific cycles' and the reference peaks and troughs. An example of the basic analysis was given in Chart 2 on p. 35, reproduced here as Figure 3.3.5. Some additional detail on deviations from average values of the pattern at stages of the cycle was given; but since the basic approach was that of the 1929 paper, it can almost certainly be attributed to Mitchell (although he credits the technique to Burns). The NBER reference cycle was checked against Frickey's 'standard pattern of short-term fluctuations' and found to have the same number of cycles. Frickey's series was an index of bank clearings, railroad earnings, imports, exports, immigration, commodity prices, wholesale prices, industrial and railroad stock prices, bond prices and commercial paper rates, the construction of which involved both 'detrending' and smoothing.

As noted above, the MBC study of specific cycles was largely a search for statistical distributions to support a theoretical structure; so, for example, the patterns for bituminous coal production were shown for eight cycles beginning in 1907 and for the average of these cycles. Apart from the considerable inter-cycle variation, statistical analysis of this sort ignored the problem of 'historical succession', that is, the possibility of 'structural' change over time. When average patterns were shown for different series, as in Chart 16, p. 156, the 'consilience' in cyclical timing and character sought by the authors seemed ever more elusive (see Figure 3.3.6). Moreover, when reference cycle patterns were analyzed, as in Chart 19, p. 165, the variations were even more striking (Figure 3.3.7). Most series in the sample had 15 cycles or less, with great variations in both duration and amplitude. An example of such variations was given in Chart 53, pp. 373–4, for seven American series: railroad bond yields, bank clearings, shares traded, call money rates, pig iron production, railroad stock prices and freight car orders. The number of specific cycles in these series varied from 15 to 23; the duration of cycles from 38 to 50 months; and the amplitude by as much as 18 times.[38]

The idea was to 'expose the typical characteristics' of cyclical behaviour in different activities and of 'business as a whole' in order to 'establish a base from which the wide variations...observable in actual life may be...explored'. Average deviations from some norm of cyclical behaviour had 'positive value', for they helped to differentiate processes which 'can be counted upon with assurance to behave in a standard fashion' from those which 'vary in an unpredictable fashion from one cycle to the next'. Antiphonally, the use of averages was questioned.

> Yet we must face the difficulty that results are apt to become less trustworthy as the range of variation becomes larger and the number of observations fewer. This difficulty is especially acute in handling temporal sequences subject to cumulative changes. ([1946] p. 371)

> An average covering a dozen cycles in a series takes no account of the historical sequence in which they occurred... If secular, discontinuous, or cyclical changes of formidable scope occur...the repetition that justifies the use of averages becomes a repetition in name only... [Their] historical value may be slight and their value as bases of future expectations slighter still. ([1946] pp. 381-2)

Secular and discontinuous changes could not be taken for granted: for example, Marx held that commercial crises became progressively more severe, while Veblen argued that economies would subside into a state of 'mild depression'. Others hoped that the business cycle had been 'ironed out'. A further complicating factor was the possibility of 'long cycles' as 'identified' by Kondratieff, Kitchin and Kuznets. Indeed, Burns found '"trend

[38]From Table 140, p. 375.

cycles" of about 15 to 20 years in production and other business activities'. Thus it might be that

> the position that an individual business cycle occupies in a 'long cycle' determines...whether its expansion develops into a 'boom', whether its recession becomes a 'crisis', and whether its contraction turns into a drastic 'depression'. ([1946] p. 382)

Tests for structural change, owing to small sample sizes and other uncertainties, were judged as inconclusive. Chapter 10 dealt with the problem of 'secular changes in cyclical behaviour'. Linear time trends were fitted by regression methods to the seven American series alluded to above. F-tests with a null hypothesis of no secular change in amplitudes or durations of specific cycles were significant only for amplitudes in call money rates. Results for the reference-cycle patterns were less clear. The authors' impression was that secular changes in cyclical behaviour had not taken place over the sample period; but that there was a wide variation among series. Thus, although there might be long-run changes in behaviour, these were not strong enough to negate the validity of averaging.

The 'long cycle' hypothesis was examined in Chapter 11. Burns and Mitchell found that the building industry had been 'characterized by long cycles of remarkably regular duration' of 15 to 20 years. They were 'clear cut in outline, attain enormous amplitudes, and are paralleled by long cycles in other real estate processes'. Because they seemed to have occurred in the above seven series, the authors speculated that long building cycles might induce business cycles, although they then immediately rejected this argument. They also found long swings in prices for the United States, Britain, Germany and France. However, Schumpeter's assertion that 120-month 'Juglar' cycles, for which he claimed there was historical evidence, could each be subdivided into three 40-month cycles was unsupported:

> only about 28 per cent of [American] cycles since 1854 fall between 37 and 43 months. No arrangement of our monthly measures in groups of three consecutive

cycles will produce an approximation of 'Juglar cycles' of from nine to ten years. ([1946] p. 442)

In general, different investigators had reached 'widely divergent results' apparently from the same data, and hence 'it seems reasonable to infer that [longer] cycles are probably far from being a clear or pronounced feature of economic development'. The decisive inference was of no clear evidence that short cycles evolve or change systematically with time, but rather that such changes are pronounced but unpredictable.

The final chapter of MBC dealt with questions of stable and irregular behaviour, following from the conclusion that the latter type of change was 'far larger in scope than secular or cyclical changes' (in specific cycles).

> This tentative finding will not be questioned by students who believe that it is vain to strive after a general theory of crises, or depressions, or business cycles. Their argument is that each of these episodes must be explained by the peculiar combination of conditions prevailing at the time, and that these combinations differ endlessly from one another. [Such students] may see little value in statistical averages... If the episodic features...are the thing of real importance, averages that conceal the episodic movements are futile if not mischievous...as futile and mischievous as general theories of business cycles. ([1946] p. 466)

The chapter is a discussion of these criticisms. Although each 'business cycle is an individual...differing in countless ways from every other', and 'business activity at any time is influenced by countless "random" factors', the authors argued that without a 'norm', the extent of such particularities could not be assessed. Without, therefore, the use of averages, notions of what is 'usual' or 'unusual' about each cycle 'cannot be made more definite'. A chart showing 'average' patterns and patterns for individual cycles the in the seven series was given as an example of the value of such analysis. The chief merit of averages was that they 'indicate roughly what cyclical behaviour is characteristic of different activities'. However, they may also mislead the observer into finding regularities where there are none, and vice versa. Even more important was to 'recognize that averages, if used judiciously, enable us to describe what cyclical behavior has been characteristic "in the long run"' (i.e., 'typically').

Averages were calculated for amplitudes and durations of cycles in the seven series. The general conclusions were that in

> the first place our tests, as far as they go, bear out the concept of business cycles as units of roughly concurrent fluctuations in many activities. In the second place, they demonstrate that although cyclical measures of individual series usually vary greatly from one cycle to the next, there is a pronounced tendency towards repetition in the relations among the movements of different activities in successive business cycles... These facts have a vital bearing on the value of averages *as a tool of theoretical analysis* [emphasis mine]. ([1946] pp.490-1)

Exogenous disturbances and secular influences were also discussed:

> ...we may find significant differences between cycles that come in times of war and those that come in times of peace, between cycles occurring in times of agricultural prosperity and in times of agricultural depression...between short and long business cycles, mild and violent cycles, and so on. ([1946] p. 506)

The permutations were numerous and required the study of distributions ('averages and average deviations') of the relevant variables. The tests on seven time series were, however, inconclusive. A greater number was required, both aggregated and disaggregated. The authors argued that business cycles had been 'sufficiently stable' in the 'long run' to justify a 'primary interest' in average or typical behaviour. This in turn was seen merely as a 'first approximation' to an ultimate explanation, in a definitive theoretical volume, of the 'business cycles of actual life'. Such an explanation would encompass such divers structural factors as '...the increasing scale of business enterprise, the spread of absentee ownership, the building up of colonial empires, the disappearance of our frontier...the declining rate of population growth, the development of instalment selling, the increased role of government in economic affairs, and many others...' The cycle of development and decline of specific industries, such as the American railroads,

'...penetrate[s] the world of business...' in ways which affect cyclical behaviour, although the individual case may be masked by the aggregates. The study of this effect, presumably a combination of long swings and structural change, 'is one of the most important problems to be tackled in the theoretical volume of this series'.

These ideas are confusing given Mitchell's rejection of 'normal' behaviour and indeed his views of business cycles as distinct historical episodes. However, reliance upon the use of empirical averages was seen as an alternative to objectionable *a priori* neo-classical assumptions: instead of relying upon a fictitious normality and the 'dreamland of equilibrium', the study of average behaviour would permit a theory of business cycles to be drawn, as it were, directly from life. This would be valid only if business cycles have many 'repetitive features other than the cyclical movement in "total output" or "total employment"...' The discussion of theoretical objectives did not state whether or not a general theory of business cycles is possible when only the aggregates exhibit the required behaviour; but it did emphasize the heavy reliance of any such construction on 'cyclical averages'. The projected volume would 'seek to explain how business cycles come about and why they differ from one another', using at every stage 'the array of averages'. The authors seemed to be arguing the Schumpeterian hypothesis that business cycles originate in structural changes, but this point is not entirely clear.

3.3.5. The Valedictory Volume and Burns's Annotations

Mitchell died in 1948. An incomplete final volume was published posthumously as *What Happens during Business Cycles, a Progress Report*, in 1951, in which Burns summarized the 'broad conclusions' of Mitchell's life-work. These were (1) that although there is much diversity of disaggregated behaviour, 'peaks tend to come in bunches and likewise the troughs... The business cycle of experience is an alternating succession of these sustained majorities...' (2) although there is 'turmoil' within the cycle, it has a 'systematic core'; and (3) there is thus a 'typical' sequence during cycles. The account gave further detail of the timing sequence of the several variables; yet 'this recital delineates characteristic movements during business cycles, not invariant sequences'. Divergences, however, 'are not so numerous as to destroy the validity of a generalized sketch'. In any case, there was some asymmetry between expansions and contractions.

> The arrays of individual turning points at business-cycle troughs 'are more dispersed and skewed toward leads' than are the arrays at peaks. Expansions of aggregate activity are longer than contractions. They are also more vigorous...
> ([1951] pp. xviii-xix)

At the end Mitchell was acutely aware of the contradictions in his empirical evidence. He had hoped that the use of averages as pointers to the exceptions, the 'deviations', would 'lead to the discovery of unsuspected regularities among what had seemed annoying exceptions to orderly relations'. These could then be incorporated into working hypotheses, leading to further testing and further generalizations. His conclusion was that the statistical evidence available 'considerably understate[s] the extent to which business cycles dominate the American economy'.

> 89 percent of our series should be thought of as typically swayed by the cyclical tides throughout reference cycles... [But objection] may well be raised to this blunt statement. It suggests argument in a circle. First we define business cycles as a congeries of roughly synchronous expansions in many activities followed by similarly general contractions; then we attribute the cyclical movements in a large majority of individual series to the cyclical tides, which are merely a summation of movements in individual series. ([1951] p. 77)

The interdependence of aggregate and individual measures of 'cyclical tides' was justified by an appeal to the interrelationships within the economy, so that 'every part is influenced by changes in every other part'.

Burns updated the work in the essay 'New Facts on Business Cycles' in *Business Cycle Indicators* [1961] Volume I. The section 'The Need for Scientific Work' put business-cycle research squarely into the Cold War firing-line:

> Despite the relatively good business conditions of recent years, the business cycle continues to haunt the thinking of the American people. The reason is not only a wish to obliterate human miseries and material wastes [but] also political necessity. The old Marxist dogma that capitalism is doomed has become a weapon of propaganda, used adroitly and energetically to confuse the uninformed and to stir discontent the world over. Our government and other democracies have met the challenge by building a variety of defences against depression. How well the defences have been built, no one yet knows... For the present, obituaries of the business cycle are romantic expressions of human impatience, not records of solid achievement. They serve neither the nation nor economics, and may prove seriously harmful if they lead to any relaxation of the scientific work on business fluctuations now going forward... ([1961] p. 13)

By 'scientific' Burns apparently meant quantitative, statistical. His analysis, based primarily on US prewar data, of specific cycles in 'over 600' indicators (reproduced as Figure 3.3.8), was remarkable to the extent that it showed how dispersed rather than concentrated were the turning points. The spikes in either peaks or troughs represented at most about 17 per cent of the series. Burns found that the lower line on this chart was slightly more reassuring.

> Rising series [are] only a thin majority at the beginning of a business cycle expansion. Their number swells as aggregate activity increases, though expansion reaches its widest scope not when aggregate activity is at a peak, but perhaps six months or a year earlier. In the neighborhood of a peak, cross currents are the outstanding feature of the business situation. Once the economy is on the downgrade, the number of expanding activities becomes smaller and smaller, though the scope of expansion does not shrink indefinitely. Perhaps six months or a year before the aggregate activity reaches a trough, the proportion of contracting activities is already at a maximum; thereafter the majority of contracting activity dwindles, while the minority of expanding activities becomes ever stronger and before long becomes the ruling majority.

Thus a continual transformation of the economic system occurs beneath the surface phenomena of aggregate expansion and contraction. ([1961] pp. 10-11)

This use of specific turning points gave a rather different picture of business cycles: one of swelling and receding momenta, measured by cumulatively increasing and decreasing changes of direction in disaggregated activities. Indeed, Burns saw this as evidence of two cycles: the 'seen' cycles of changes in direction in the 'aggregates or averages' (these terms appear to be interchangeable) of activities; and the second '"unseen" cycle in the relative distribution of expansions and contractions of specific activities'. However, even the latter distribution showed a maximum clustering of only 15-20 percent of turning points in any one month. Despite these attempts at general descriptions of business cycles, Burns continued to stress the diversity of behaviour. In the section on forecasting, he wrote:

> I take it as a matter of course that it is vital, both theoretically and practically, to recognize the changes in economic organization and the episodic and random factors that make each business cycle a unique configuration of events. ([1961] pp. 10-11)

He then showed a leading indicator based on the seven series which appeared to anticipate all the reference dates in the period 1919-1939, and a coincident indicator for the same period — a seeming contradiction of the position of each episode as unique. This is another example of the circularity implicit in confusing reference dates with business cycles. How indicative it was of contemporary thinking is not clear. If indeed representative, it partly explains how the confusion of aggregate with 'typical' behaviour in econometric research has developed.

The unease evident in Mitchell's work is reflected by all writers of the period. As early as 1923, Irving Fisher referred to the 'so-called business cycle' in a paper in which he argued that for the period 1916–1922, such fluctuations could be fully 'explained' by changes in (distributed lags of) a wholesale price index. Explanation in this case meant correlation or regression, based on the prior theoretical view 'long recognized [that] a rising price level temporarily stimulates trade and that a falling price level depresses trade'.[39] Kendall's 1946 study of oscillatory AR(2) models, although specifically criticising Beveridge's index of British industrial production, 1785–1938, applies to all contemporary research.[40] Kendall demonstrated that Beveridge apparently ignored 'minor' turning points in his data in order to find a cycle of eight-years' duration, which, had these been included, would have produced a duration of only four years.

3.4.1. The Theory vs. Empiricism Debate

The best-remembered critique of Mitchell and the NBER was Koopmans's 1947 review of MBC, entitled 'Measurement without Theory'. This was part of a wider debate between the NBER and the Cowles Commission, the course of which can be traced in the correspondence between Koopmans and Rutledge Vining in the *Review of Economic Studies* from August 1947 to May 1949.[41] Koopmans's title gave the impression that Mitchell and the NBER rejected theory, a clear misunderstanding of the significance of the work and its results. Koopmans held that the stages of the development of 'economic science' were analogous to what he called the 'Kepler' and 'Newton' stages in the theory of celestial mechanics. The former was supposedly the stage of data-accumulation and

[39]'Our Unstable Dollar and the So-Called Business Cycle', *Journal of the American Statistical Association*, **17**, pp. 179-202. Further discussed in Chapter 4 below.

[40]*Contributions to the Study of Oscillatory Time-Series*, discussed fully in Chapter 4 below. The index in question appeared in *Full Employment in a Free Society* [1944].

[41]Reprinted in Gordon and Klein (eds.) *Readings in Business Cycles* [1966].

measurement, and the latter the deduction of general empirical 'laws of nature' from these observations. MBC was supposed to be a contribution to the Kepler stage. Koopmans thought this inadequate: a 'shorter road' to the explanation of such phenomena would use 'the concepts and hypotheses of economic theory [by which he meant general equilibrium and other micro-foundations] as part of the process of observation and measurement'. His three specific objections to MBC were: (1) that for the analysis of 'such a many-sided phenomenon, theoretical preconceptions about its nature cannot be dispensed with' and that the argument is weakened by so doing; (2) that 'the prediction, within the narrowest attainable limits of error, of the [policy] effects of stated hypothetical measures' is the most important objective of the Burns and Mitchell analysis, and that without theory, the relevant conclusions about policy 'cannot be drawn'; and (3) that hypothesis testing, in the technical sense, cannot be carried out without distributional assumptions, which are missing from MBC.

Vining's objection to the Koopmans position was that the core of its criticism relied on the Burns and Mitchell choice of a macroeconomic unit of analysis — the 'business cycle' — as fundamental. The Cowles Commission favoured a microeconomic unit — the preference-ordering, optimizing and representative agent within a competitive equilibrium model. Although the NBER approach had limitations, Vining thought that of Koopmans to be even less promising, and offered a three-point defence of Burns and Mitchell. First, economics, as a 'science of variation' had perhaps not developed to the point where a general mathematical model had any chance of success. Second, Vining interpreted Koopmans's view as an argument for carrying out the two stages of enquiry together, and criticised it for being equivalent merely to a form 'in the main available from works no later than those of Walras'. Third,

> modern theories of statistical estimation and of tests of hypotheses with their emphasis upon distributional hypotheses, upon the extraction of maximum information, upon the power of a test, and the like, are almost beside the point

in attempts to derive hypotheses, the exploratory stage that characterizes a great part of the work in all developing fields of knowledge. ([1966] p. 207)

Koopmans, in 'A Reply', argued that

> if a theory formulates precisely...the determination of the choices and actions of each individual in a group...in response to the choices and actions of other individuals...then the set of these individual behavior characteristics is *logically* equivalent to the behavior characteristics of the group [emphasis in original]. ([1966] p. 219)

This does not imply, however, that aggregating from collections of individuals is easy or obvious. The problem lies in the lack of micro-foundations, and Koopmans refers to Arrow's view that aggregate equations are not even necessary for macroeconomic analysis. His specific criticism of MBC is that its statistical measures are 'inefficient tools' because the application of theory can narrow down the choice of alternative hypotheses *a priori*.[42] Vining's 'Rejoinder' criticised Koopmans's argument that 'all actions...associated with human beings are subject ultimately to individual determination'. Koopmans's view of aggregation was seen as 'excessively formal'. The distinction was, for example, analogous to that between aggregates of electrons hitting a diffraction grating and the behaviour of an aggregate biological population: in the former case, behaviour could be analysed by relative frequencies, whereas in the latter case the population itself was the indivisible unit. Moreover, according to Vining, Koopmans misrepresented MBC as an attempt at conclusive results rather than a tentative exploration of economic variation, which is how its authors saw it. In such primary research, specific distributional hypotheses are counter-productive. (There is evidence that Koopmans later softened his opposition to NBER methods. In *Three Essays on the State of Economic Science* [1957] he argued that in some cases the selection of measures such as turning points on 'intuitive

[42]He did not apparently realize that this is exactly what Mitchell did.

grounds' can give 'reasonably efficient summaries of...the relevant information in the series'.[43])

Koopmans's specific criticisms of Burns and Mitchell have been largely forgotten. What is remembered is 'Measurement without Theory': i.e., MBC is seen as the definitive purely statistical work because its methods of data-collection and evaluation appear to readers unfamiliar with the issues to be conclusive. It is considered to be the last word on the subject, and all subsequent business-cycle analysis has taken its 'results' as a justification both for the formulation of theories and for empirical modelling and forecasting. This interpretation was reinforced by Marschak [1951], who in response to Burns's introduction to *What Happens during Business Cycles,* attempted to rewrite the 'definition'. Although this might have clarified the position, it had the opposite effect. Marschak argued that empirical economics was at 'its very beginnings', and it was thus necessary to 'translate' the definition into 'terms...that might yield themselves to...empirical tests'.[44] The MBC definition, which was a mere 'hypothesis that such cycles exist', should thus be modified as follows:

> In a society consisting mainly of business enterprises, and apart from seasonal and small random influences, certain economic variables are periodic functions of time; the period length is the same for all these variables; for each variable, each period contains exactly one peak and one trough. (It follows that for any two variables the distance in time [the 'lag'] between the corresponding peaks is constant. ([1951] p. 15)

This rendering implied that 'apart from random influences', each 'observable economic variable' is a lagged function of a zero-mean 'stochastic process' which is known as 'general business activity'; and Marschak argued that such an hypothesis was 'implicitly accepted' by Mitchell as a 'drawing...from a random universe'.

[43][1957] p. 216.

[44]Universities-National Bureau *Conference on Business Cycles*, pp. 14-24. Jacob Marschak succeeded Frisch as Research Director of the Cowles Commission, which by this time had moved to the University of Chicago.

Marschak was making a serious point. The real difference between the NBER and the 'Chicago Interpretation' was the importance of micro-foundations, not the acceptance or rejection of the role of government in economic management and in promoting welfare. Institutional attitudes towards stabilization policy needed to be looked at, not because of 'free market' considerations, but because

> a wave pattern that can describe American business cycles at a time when public expenditures equal a tenth of national income is unlikely to have much predictive value when public expenditures equal a fifth or a fourth of national income...
> ([1951] p. 24)

The difference between interpretations was perhaps a 'difference between generations'. Mitchell's approach was in the 'grand manner' of nineteenth-century thought, where 'economic laws' were seen to be 'inexorable, with little place...for deliberate changes'. By the early 1950s, however, the belief was that far from being inexorable, these laws 'depend on habits, institutions and policies' (an echo of the Institutionalist position?); and the economist's task is to predict the effect of 'exogenous' stimuli on the 'parameters' of the system.[45] 'Deliberate changes' meant government intervention as in wartime to optimize both resource allocations and welfare — the 'social possibilities'. The force of 'inexorable laws' would thus be modulated by some of the prescriptions of command economics, and in this view the positions of the parties were not that far apart.

So far so good; yet for obscure reasons, Marschak's words have been taken as a 'correct' interpretation of the Burns and Mitchell results by later generations. Burns tried his best to deflect Marschak's judgments. Stung by the Chicago Interpretation, he replied in the same volume that this 'sterile exercise in translation' misrepresented Mitchell's position. Mitchell 'saw the essence of the business cycle in a systematic *divergence* [emphasis in original]' of the various sectors of the economy, and not in its relation to

[45]Operationally this meant the estimation of 'systems-of-equations' structural parameters, as advocated by the Cowles Commission. This type of econometric analysis was implicit in Koopmans's remarks, and is discussed in the context of 'calibrating' RBC models in Section 3.5.3 below.

an unobserved index of 'general business conditions'.[46] There was 'never any such design or...statistical construction' at the NBER; and the reference scale was *not* a proxy for 'an index of general business'.

> Mitchell's findings would not be significantly altered if his reference scale, which is derived from numerous economic series, were replaced by a scale...derived from a single comprehensive series such as national income... ([1951] p. 28)

Nevertheless, the Chicago Interpretation over the years seems to have become the preferred one in applied work, perhaps because it has the merit of simplicity. Lucas took it as a point of departure for his own version of business cycles, declaring in a 1977 paper that 'business cycles are all alike', and again in *Studies in Trade Cycle Theory* [1981]:

> Mitchell sought an empirical definition of business cycles through the systematic exclusion of those movements in economic time series that appeared likely to be explicable by then existing theory: the general level and pattern of growth in economic activity, and movements in individual series that seemed to arise from supply and demand conditions specific to individual markets...
>
> [It] is easy to forget the remarkable character of the regularities that Mitchell succeeded in discovering and documenting... The central finding, of course, was the similarity of all peacetime cycles with one another, once variation in duration was controlled for, in the sense that each cycle exhibits about the same pattern of co-movements among variables as do the others. ([1981] p. 274)[47]

3.4.2. Defences of Mitchell's Theoretical Views

Mitchell's objective of formulating an empirically-based theory was perhaps overlooked by Koopmans and Vining, but not by Mitchell's NBER research associates, Burns and Milton Friedman. In an essay in the memorial volume in 1952, Friedman attempted to construct a general theory from Mitchell's work in the 'modern language

[46][1951] p. 26.

[47]See Section 3.5.3 below.

of propensities, multipliers and accelerators'. He interpreted Mitchell's overall position as an attempt to improve the quantity theory by, in effect, including time subscripts in the formulation.

> So in the analysis of problems of position, the quantity theory can be used without paying special attention to the dating of the variables on which it centers attention. On the other hand, in the analysis of process rather than position, the dating of the variables is likely to be critically important; it is precisely the lags reflected in differences in dating that must be the central elements of a dynamic theory. ([1952] p. 252)

Friedman argued that of the three interpretations of cycles then current — (1) reactions to exogenous shocks, (2) fundamental instability giving rise to fluctuations which are limited by some reflecting barrier, and (3) a self-generating process of fluctuations through some adaptive mechanism which encompasses (1) and (2) — Mitchell accepted the third view in *Business Cycles* and 'never found any reason to change this conclusion'.[48] However, although Mitchell was 'deeply interested' in 'money' in both the broad and narrow, technical, senses, he believed that prices and quantities were the 'active' factors in business-cycle movements, rather than money supply.[49] This apparent contradiction between 'the dominance of the quantity of money in the long-period movement [and] its...passiveness in short-period movements' was resolved by interpolating lag structures into the formulation, and this was considered by Friedman to be 'a theoretical contribution of the first importance to the quantity theory'.[50] Mitchell, as a theorist, was thus

> almost exclusively concerned with a part of economic theory that was largely outside the main stream...and that even today is least developed and least satisfactory — the dynamic adjustment of the economic system as a whole.

[48][1952] p. 253.

[49]*Ibid.*, p. 251.

[50]*Ibid.*, pp. 251-2.

> Because we know so little about this part of economic theory, we tend to neglect it... This circumstance, I think, partly accounts for the widespread illusion that Mitchell was antitheoretical... [For] Mitchell's work was consistently and almost exclusively devoted to the development of a theory of economic change. [Lags] in response are the central elements in theories of this type. Further...the lags in response must be pervasive, they must operate on a broad range of activities, these activities must be linked to one another and to the remainder of the system, and the whole must display consistent, though not identical, responses in successive cycles. ([1952] pp. 237-282)

Schumpeter, in the same volume, concurred with Friedman but went even further, suggesting that MBC was 'an exercise in the dynamic theory of equilibrium'.[51] Burns, in the introductory essay, concurred with these views.

3.5. LATTER DAY BUSINESS CYCLE RESEARCH

Burns's 1961 paper was the last to rely on prewar data. During the 1950s and 1960s the use of aggregates became standard with the development of national income accounting procedures. This was a period of rapid economic growth in all sectors, to the extent that 'cyclical' contractions in GNP and industrial production were rare and shallow. As a consequence the concept of business cycles was modified to that of 'growth cycles'. Moore and Zarnowitz [1986] define these as 'a fluctuation around the long-run growth trend of a nation's economy, that is, a trend-adjusted business cycle'. Lucas [1977] proposed a similar 'definition'.[52]

[51] *Ibid.*, pp. 259-60.

[52] This model requires the 'trend' to be 'removed' from the data. In the 1960s a moving average technique known as 'phase averaging' was widely applied to this end. It was used at the NBER by Mintz, by Moore and Zamowitz at Columbia University, and by Friedman and Schwartz in studies of the long-run. It is currently employed by the UK Central Statistical Office to 'identify' the UK 'business cycle' from nonstationary time-series data.

3.5.1. An Algorithmic Approach to the Identification of Turning Points

Although specific-cycle dating has always relied upon subjective assessments, attempts have been made from time to time to find algorithms for its determination. The most ambitious of these was an NBER project in the 1960s, directed by Gerhard Bry and Charlotte Boschan, which tested computer-based procedures for finding turning points in monthly data, identification of which was seen as an 'essential element' in finding 'cyclical turns in general business conditions'. The programmed approach, as discussed in Bry and Boschan's *Cyclical Analysis of Time Series* [1971] (CATS), differed somewhat from previous methods, which in the authors' opinion 'rely heavily on impressionistic judgments'. The authors took a strong *a priori* view of what actually constituted a specific cycle. Following NBER practice, duration for the most part was set in a range of two to seven years, with a minimum of 15 months. Problems had always arisen with anomalous behaviour in time-series data producing what may be loosely categorized as 'false signals'. Examples of seven of these types are discussed, the most obvious being the occurrence of non-alternating sequences of peaks or troughs (see Figure 3.5.1).

The CATS solution was to apply a number of filtering operations to the data to eliminate both noise and outliers. Peaks and troughs were placed 'at the highest and lowest points of cyclical fluctuations', and were forced to alternate, so that a peak always followed a trough, and vice versa. Where 'multiple' peaks or troughs appeared (with equal values) the latest was chosen. If 'plateaux' or 'valleys' occurred, that is, 'step' patterns, the search for turning points might be inappropriate. Since every such addition to the rules produced more problems, the approach was considered as highly provisional. The forced alternation of peaks and troughs could produce arbitrary results, as for example for the bituminous coal series (Chart 3, reproduced here as Figure 3.5.2). The two 'turning points' found by the program in this series for 1934 (indicated by arrows) do not look like true specific-cycle turns as defined by the NBER; and very likely there were other instances of such anomalies.

3.5.2. The Postwar 'Stabilization' Literature

Some attempts have been made to re-examine Mitchell's reference chronology, both for its general accuracy and for what it implies about postwar stabilization policy. Watson [1992] finds evidence that 'apparent stabilization is largely spurious, and is caused by differences in the way that prewar and postwar business cycle reference dates were chosen by the NBER', owing mainly to the available prewar data having greater volatility than the 'aggregate economy'. Romer [1992] offers a criticism that is perhaps more serious. She argues that, far from using all the series then available, the pre-1927 chronology is based almost entirely on two indices of 'general business conditions': the Snyder and the AT&T, both of which were adjusted for some estimate of 'trend'. The latest of the absolute troughs in series is generally the reference trough, with the reference peak taken as the absolute peak of the two. She maintains that although the correspondences are not exact, they are close enough to permit such an inference to be drawn.

> That the early NBER reference dates appear to have been set largely on the basis of just a few detrended series may seem to contradict the numerous references in *Measuring Business Cycles* to the hundreds of series analyzed by the NBER. The resolution of this seeming contradiction is that the reference dates were an input to the analysis of many other series, not the final result of this analysis. ([1992] p. 10)

Watson [1994] continues the theme of spurious stability. He finds that prewar (i.e., pre-1929) and postwar cycle durations are not significantly different, although the 'volatility' is greater in the former period. He questions whether it is possible to determine from the historical record exactly how Mitchell arrived at the reference dates, and whether the Snyder and AT&T indices were given as much weight as Romer believes. Watson finds the Burns and Mitchell definition of expansions and contractions 'too vague' to be of assistance in dating specific cycles, and favours instead the Bry-Boschan algorithm. However, when applied to the monthly pig iron production series in MBC, the latter technique finds turning points that are little different from those of the former.[53] Watson also argues that certain sectors used by Burns and Mitchell for reference dating were 'systematically more volatile than the aggregate economy', which biases the chronology. Neither Romer nor Watson, however, question the idea of a cycle in general business conditions.

3.5.3. 'Real Business Cycles': the New Classical Interpretation

A substantial theoretical literature, known as 'real business cycles' (RBC), has developed since the 1970s. Its origins lie in the 1930s' transmission mechanisms for random shocks proposed by Slutsky and Frisch, as developed in postwar neoclassical growth theory by Solow [1956, 1957], who postulated exogenous productivity shocks as an impulse mechanism for short-run fluctuations.[54] Output growth from production in this model is given by $g_y = \alpha g_l + (1-\alpha) g_k + z$, where g is a growth rate, the subscripts y, l and k denoting total output, labour and capital respectively, while z represents the growth unaccounted for by labour or capital — the 'Solow residual'. This equation implies, by rearrangement, that per capita output growth depends on a linear combination of growth in the capital-output ratio and in the residual, the latter, according to RBC theory, being the source of 'business-cycle' fluctuations.

The opposing, neo-Keynesian, view is that demand and monetary shocks are more important, and this debate has itself generated an extensive empirical literature. In one of the earlier of these assessments of productivity shocks, Shapiro [1987] estimated the Solow residuals for a number of US industries in order to determine whether they represent an exclusive shift in the production function or whether there is a demand component as well. Comovements ('procyclical fluctuation') were measured in some detail

[53] [1994] pp. 25-27.

[54] Blanchard and Fischer [1989] have argued that Lucas's approach, 'originally... known as..."rational expectations" was modified to an 'equilibrium business cycles approach' when it was realized that such behaviour 'came more from the assumption that markets cleared continuously' than from expectations ([1989] p. 311, note 9).

by computing cross correlation coefficients between sectoral Solow residuals and residuals for aggregate GNP, prices and wages. Shapiro's data set contained 14 US aggregates and 25 of their constituent variables, sampled at annual frequencies for the period 1950–1985. Correlations with prices residuals were all negative, implying that price shocks are contra-cyclical. On the other hand, correlations between industrial output and aggregate GNP residuals were mixed. Examples of these the coefficients are: total manufacturing, 0.77; total transportation, 0.68; wholesale trade, 0.36, finance, 0.27; construction, 0.06; and agriculture, -0.31. Table 3.5.1 shows the proportion of both aggregates and sectoral constituents with at least reasonably strong positive correlations — that is, with coefficients greater than, say, +0.5.

Table 3.5.1. Correlations between Solow Residuals and GNP Changes for US Annual Output and Real Wage Variables, 1950–1985

Variables	Percentage of output variables per sector with coefficients >+0.5	Percentage of real wage variables per sector with coefficients >+0.5
All output aggregates	29	43
Manufacturing disaggregates:		
Nondurables	10	70
Durables	36	36
Transportation disaggregates	60	100

(Source: Shapiro [1987], Table 1, p. 121)

Wage residuals are correlated more strongly than those for output. Shapiro finds evidence that American economic fluctuations are generated principally by productivity shocks, although there are exceptions in some industries. Also, because the data are annual, there may be relevant effects that do not appear at that frequency.[55] Other, more recent, studies of Solow residuals, such as Caporale [1993] and Costello [1993], indicate that for other national economies, both types of shocks play a role in short-run fluctuations, and that output growth is more highly correlated across national economies than productivity growth. King *et al.* [1991] model permanent productivity shocks as innovations to a

[55] [1987] Table 2, p. 123.

common stochastic trend in output, investment and consumption. They 'conclude that the U.S. data are not consistent with the view that a single real permanent shock is the dominant source of business-cycle fluctuations', because it does not explain the majority of the volatility in either output or investment.[56] Such shocks do Furthermore, permanent changes in real interest rates are better than productivity shocks at explaining short-run fluctuations in output.

The original inspiration for RBC models came from Lucas, as in his interpretation of MBC results in the 1981 book quoted above, but most strongly expressed in the 1977 paper.

> Though there is absolutely no theoretical reason to anticipate it, one is led by the facts to conclude that, with respect to the qualitative behaviour of comovements among series, *business cycles are all alike*. To theoretically inclined economists, this conclusion...suggests the possibility of a unified explanation of business cycles, grounded in the *general* laws governing market economies, rather than in political or institutional characteristics specific to particular countries or periods [emphasis in original]. ([1977] p. 10)

These views mean introducing microeconomic 'foundations' into the analysis, i.e., preference-ordering agents and explicit production technologies, with 'solutions' involving recursive, Pareto-optimal general equilibria. Lucas defined the business cycle as deviations of aggregate real output from 'trend', with certain 'empirical regularities' in the 'comovements' of deviations from trend with other aggregates: investment and durable production tend to have fluctuations of greater amplitude that GNP, whereas consumption and capital stock are much less variable.

Kydland and Prescott [1990] have argued that Lucas's understanding of business cycles 'differs importantly from that of Mitchell', especially in the latter's emphasis on 'sequences of expansions and contractions...emphasizing turning points' and the four

[56][1991] p. 838.

phases of depression, revival, prosperity and recession.[57] They quote the 1922 Clements definition of 'cycle' from BCPS, p. 377 as accurately representing Mitchell's position, and appear to be arguing that Mitchell's hypothesis was one of 'deterministic cyclical laws of motion' which fail to account for business cycles from a theoretically sound basis

> because cyclical laws of motion do not arise as equilibrium behaviour for economies with empirically reasonable preferences and technologies... ([1990] p. 5)

(Their own definition is 'consistent with' Clements's, but substitutes 'deviations' for 'departures'.[58]) Kydland and Prescott propose an alternative 'identification' of the 'cyclical component' using what has become known as the 'Hodrick-Prescott filter' (see below). There is again a significant degree of sectoral disaggregation in this study, with ten input and 20 product and income variables.[59] Their results are presented as 'business cycle facts', i.e., the 'cyclical behaviour of aggregates'; i.e., the degree of cross correlation of the cyclical component ('deviations from trend') with real GNP, the phase shifts relative to the 'overall business cycle', and the amplitudes of input and output variables. Hence 'comovement' here means cross correlations with aggregate output rather than with each other. The H-P approach has been criticized as statistically unsound (see, for example, the discussion of Stadler [1994] and Pagan [1994], following, and of Harvey and Jaeger [1993] in Chapter 4 below.)

RBC models have been more recently seen as a method of carrying out quantitative experiments — effectively simulations — whose objective is to answer specific comparative questions, rather than to reproduce the salient features of time-series data. Methodological issues are discussed most recently in Kydland and Prescott [1995]. Building on the 'neoclassical growth framework', these authors argue for simulations, or

[57] [1990] p. 4.

[58] See footnote 26 above.

[59] [1990] Tables 1 and 2, pp. 10, 11.

'computational experiments' as 'econometric tool[s] used [in] deriving the quantitative implications' of such theories.[60] RBC analysis addresses such questions as the consequences of changing welfare policy, the relative importance of a given factor in propagating particular behaviour, and the effect of introducing a new factor on the 'deviation from standard theory'.[61] However, because all models are abstractions, they

> can be judged only relative to some given question. To criticize or reject a model because it is an abstraction is foolish: all models are necessarily abstractions and therefore false. ([1995] p. 6)

Furthermore,

> searching within some parametric class of economies for the one which best fits a set of aggregate time series makes little sense... A model economy is obviously an abstraction and, by definition, false. With enough data, statistical hypothesis-testing almost surely will reject any model along some dimension...
>
> Using probabilistic theories...to compare a given scientific theory with all others that might fit the same data is a futile effort... All historically significant theories have agreed with the facts, but only to a degree. No precise answer can be found to the question how well an individual theory fits the facts. ([1995] p. 6)[62]

The authors argue that although RBC theory has evolved from the original Lucas [1977] definition, it currently departs from the original formulation in important respects. In the 1970s, when it was believed that 'one set of factors was...behind the cyclical component and that an entirely different set of factors accounted for the movement of the growth component', the preferred method for isolating 'cycle' from 'trend' was to 'fit a smooth curve through the time series'. The H-P filter was intended to isolate the cyclical component and, by the 'calibration' of a parameter, to make 'the fitted curve

[60] [1995] Abstract and p. 4.

[61] *Ibid.*, p. 3.

[62] The authors are here agreeing with Kuhn [1962] that 'few philosophers of science still seek absolute criteria for the verification of scientific theories'.

mimic...one that business cycle analysts would draw'.[63] Those features of the data designated 'business cycle fluctuations' are 'nothing more than well-defined statistics' and 'given the way the theory has developed, these statistics measure nothing'.[64] The 1970s view has changed. Nowadays

> business cycle theory treats growth and cycles as being integrated, not as a sum of two components driven by different factors. ([1995] p. 9)

The H-P filter merely 'summarizes in a reasonable way what happens at business cycle frequencies...as a means of presenting the findings and of judging the reliability of the answer' in the abstract context of the RBC model; hence, to complain about the generation of 'spurious cycles makes no sense'.[65] In contrast to the 'systems-of-equations' approach of the Cowles Commission, the preferred model is not necessarily the one 'that better fits the data'; and furthermore, because 'any model of a national economy is an abstraction and therefore false...statistical hypothesis-testing is not a useful tool for testing theory'.[66]

The problems of calibration are considered explicitly in Pagan [1994]. Given the 'model' $y_t = g(z_t;\theta) + u_t = y_t^* + u_t$, where z is exogenous, u an error term and y^* the 'estimate' of $g(z_t;\theta)$, the RBC approach is to derive the functional form of g from theory and to remain relatively indifferent to the precise nature of the error term.[67] This approach assumes that 'the calibrated model is the DGP [data generation process] and then proceed[s] to do inference under the condition that $y_t = y_t^*$'; but

[63][1995] p. 9. The term 'calibration' has somewhat divergent meanings, depending on context, but essentially means imposing a parameter value on *a priori* criteria.

[64]*Ibid.*, p. 10.

[65]This is a reference to Harvey and Jaeger [1993]. See Chapter 4 below.

[66][1995] p. 18.

[67][1994] pp. 2–4.

lurking in the background is the concern that it does not seem plausible that $y_t = y_t^*$. Indeed one feels that this theme is always present in calibration exercises... ([1994] p. 19)

In other words, all calibrated model economies are 'false' in the specific sense that, unlike the systems-of-equations approach[68], they do not investigate the 'unknown' properties of the error term u_t. In particular, no attempt is made to investigate its magnitude or correlation with y_t^*; yet both these values are of great importance in assessing whether the 'restrictions implied by the model are rejected by the data'.[69]

This seeming implausibility is also criticized in Canova, Finn and Pagan [1991]. These authors argue that much of the underlying theory, e.g., Cobb-Douglas production and CRAA utility functions, are 'ad-hoc assumptions [and hence] can scarcely be defended on the ground that they are "theory"'.[70] No rigorous statistical criteria exist for evaluating whether model behaviour is acceptably 'close' to actual behaviour. Moreover, in

> what purport to be business cycle models, which classically are interpreted as giving rise to a **peak** in the spectrum for output, evaluation is performed with respect to variances, which represent the **area** under the spectrum [emphasis in original]. It is not surprising therefore that these models generally don't have a cycle at all, exhibiting real rather than complex eigenvalues [e.g. King, Plosser and Rebelo [1988], the Kydland-Prescott 'time-to-build' models]... At the moment RBC models only possess cycles if some rigidity such as wage contracts are [*sic*] built into them... ([1991] p. 14)

Kydland and Prescott [1991] criticized the systems-of-equations approach partly because of a record of predictive failure, but mostly because of 'advances in neoclassical theory that permitted the application of the [competitive equilibrium] paradigm in

[68] That is, the estimation of the structural parameters of a simultaneous-equations model.

[69] *Ibid.*, p. 20.

[70] [1991] p. 13.

dynamic stochastic settings'.[71] They argued for a 'recursive' stochastic general equilibrium approach in which the effects of shocks on the 'willingness of agents to substitute commodities' could be assessed. The 'precise question [was] how much variation in aggregate economic activity would have remained if technology shocks were the only source of variation'. Such comparatives are characteristic of RBC research: within an abstract, neoclassical framework with a restricted class of technologies and preferences, what is the relative contribution of each behavioural category to overall fluctuations. RBC models do not, according to Kydland and Prescott, attempt to match the behaviour of national income accounts data (up to an error term with known properties).

> Model-economy selection depends on the question being asked [rather than] on the answer provided... Unlike the systems-of-equations approach, no attempt is made to determine the true model. All model economies are abstractions and by definition false. ([1991] p. 170)

They do not, therefore, directly address empirical issues such as the identification of business-cycle components from time-series data; nor is their success measured by an ability to capture 'real-world' behaviour. They are essentially asking *ceteris paribus* questions about the composition of short-run macroeconomic fluctuations within a recursively-determined general equilibrium framework.

Earlier RBC applications are discussed in Blanchard and Fischer [1989], Chapter 7. At this time interest centred on the explanation of fluctuations as the propagation of productivity shocks through an economy without market imperfections. The authors demonstrated how such behaviour could be generated in the Ramsey and Diamond models via the stochastic evolution of capital stock and output growth following an AR(1) process, with and without unit roots to allow for permanent and transitory effects. A multi-sector version of these dynamics is discussed in Long and Plosser [1983], who obtain serially and cross correlated shocks from serially independent impulses. However,

[71][1991] p. 167.

the order of these serial and cross correlations are only 0.3 and 0.2 respectively. King and Plosser [1988] applied the Bry–Boschan algorithm to data from the Adelmans [1959] simulation of the US economy (see Chapter 4 below) to determine whether an RBC model could reproduce the durations of 'business–cycle' expansions and contractions.

Kim and Pagan [1993] have demonstrated that such models, in order to capture the 'stylized facts' of durations, must be driven by processes 'close to I(1)...with drift', i.e., stochastic trends.[72] These authors criticize RBC models in general because they assume a 'deterministic steady state growth path' which can 'be easily removed from the data' — the strategy behind H–P filtering. It 'involves a serious inconsistency', because if the 'forcing processes are not integrated, then they cannot produce a stochastic trend, yet there is one in the data'. If the filtered data still possess a stochastic trend, and 'hence the variance of the detrended data does not exist', comparisons between moments of H–P filtered data and those of RBC models will be invalidated. Such inconsistencies are sufficient grounds 'for doubting the validity of the calibrations' on many RBC models; but there is a further objection to filtering.

> If the stochastic trend has no impact on the features of interest such an action [H–P filtering] seems acceptable. However, for the purpose of assessing the nature and causes of business cycles there is ample evidence that the stochastic trend is a major contributor to these, in the sense that the 'permanent' impulses explain a large percentage of the variation in output at the business cycle horizon... ([1993] p. 14)

The most recent RBC applications are contained in the collection of essays *Frontiers of Business Cycle Research* [1995], edited by Cooley and Prescott. According to these authors

> modern business cycle theory starts with the view that *growth* and *fluctuations* are not distinct phenomena to be studied with separate data and different analytical tools [emphasis in original]. ([1995] p. 4)

[72] [1993] p. 16.

This view is attributed originally to Hicks, Goodwin and others in the 1950s, but in the neoclassical context requires modifications of the growth model to produce the desired short-run effects. There is no single or 'correct way...to produce business-cycle type fluctuations', and the book is an exploration of various models by a number of authors.[73]

The Cooley and Prescott paper in *Frontiers* proposes a 'stochastic growth economy with labor-leisure choice'. A 'law of motion', specified as AR(1), is introduced for both a random productivity shock and for aggregate capital stock. Model parameters are calibrated so that behaviour 'mimics the actual economy on the dimensions associated with long term growth'.[74] The types of restrictions imposed on these parameters 'depend...on the kind of questions being asked of these artificial economies'. The model is 'very abstract', containing 'no government sector, no household production sector, no foreign sector and no explicit treatment of inventories'. Calibration is difficult because of definitional inconsistencies in the national income and product accounts. The assumed 'business cycle facts' are the

> regular...way the variables move together. It is these comovements of variables that Burns and Mitchell worked so hard to document and that Robert Lucas emphasized as the defining features of the business cycle. These are the features of fluctuations that we would like an artificial economy — a business cycle model — to replicate. ([1995] p. 26)

A real US GNP series is decomposed via the H-P filter following the Lucas trend-stationary approach, with MBC duration criteria.

> We normally think of the business cycle as fluctuations about the growth path that occur with a frequency of three to five years. That is what Burns and Mitchell [1946] characterized as the usual business cycle frequency. ([1995] p. 28)

The authors find that although their model 'does display a business cycle', its behaviour differs in several respects from actual US experience. None of the models discussed in

[73] [1995] p. 12.

[74] *Ibid.*, pp. 12, 14.

Frontiers is intended to be 'comprehensive' because of continuing developments in the field. The assumption of a Burns-and-Mitchell/modified-Lucas cycle in the 'real-world' economy is accepted and 'mimicked' within the general equilibrium framework of optimizing agents under a number of theoretical specifications. These include non-Pareto-optimal equilibria, heterogeneous agents, monetary shocks, and non-Walrasian and imperfectly-competitive economies. The Canova *et al.* comments apply: productivity shocks and the evolution of capital generally follow first order autoregressive processes in these models.

Although RBC studies demonstrate that fluctuations mimicking 'business cycles' can arise in abstract models of competitive equilibrium (i.e., the 'empirical regularities' specified by Lucas in 1977 and later expanded), they leave most empirical issues unresolved. A general critique is given in Stadler [1994], who argues that the 'true purpose' of RBC models is to

> provide the important insight that the existence of fluctuations in output does not imply any failure of markets to clear. Even economies with complete and efficient markets will display business cycles if technical change is stochastic. Although government intervention may be welfare-improving if equilibrium is not Pareto-optimal to begin with...the existence of cycles per se is not sufficient to justify stabilization policies. ([1994] p. 1778)

These models are in effect designed to demonstrate these propositions.[75] Stadler argues that nevertheless (or perhaps consequently?) the RBC approach has 'difficulty in accommodating a number of empirical facts': it does not 'adequately account for the dynamics of output' or for agent heterogeneity; and although it 'can generate cycles, these...are not like the cycles observed'. The introduction of additional shocks to tastes and preferences, or to government spending or to the money supply, still fails adequately to explain such fluctuations as the pattern of household investment, or volatility in the

[75] This paper pre-dates *Frontiers* and perhaps does not give sufficient weight to the increasing catholicity of the latter's model specifications.

terms of trade. Furthermore 'there is no microeconomic evidence for the large real shocks that drive these models'.[76] The predictions of general pro-cyclicality in prices and productivity, and contra-cyclicality in real wages, are not confirmed by US experience.

Stadler also maintains that none of the RBC formulations are able to provide a convincing explanation of the mechanism whereby serially-independent impulses are transformed into serially correlated output dynamics. Comparisons of correlation coefficients, as in Shapiro [1987], are open to several criticisms. Solow residuals reflect 'shifts in the production function' only in the presence of perfect competition and constant returns to scale, and 'will capture changes in total factor productivity only if there are no measurement errors' in the input series. These errors will otherwise

> show up as variations in the estimated...residual... [Also] Solow residuals...do not behave like an exogenous impulse, but are Granger-caused by money, interest rates and government spending... [Moreover] between one quarter and one half of the variation...is attributable to variations in aggregate demand [suggesting] that Solow residuals...capture a variety of other factors at work in the economy...and reflect both supply-side and demand-side impulses. ([1994] pp. 1775-6)

Stadler further cautions against the use of correlation coefficients 'because these coefficients are not necessarily informative about the true cyclical comovements of variables'.[77] He thus concludes that while 'there is no persuasive evidence against RBCs here, there is no clear evidence in their favor either'.[78]

[76] [1994] p. 1772.

[77] *Ibid.*, pp. 1778-9.

[78] *Ibid.*, p. 1775.

3.5.4. Current Reference-Dating Practice

The idea of a general cycle persists and continues to be tracked by governments and international organizations. Their approach has largely consisted in attempts to find proxies for these hypothetical 'comovements' within the methodological framework of MBC. For example, in the UK the dating of domestic 'cycles' is based on a close paraphrase: 'recurrent' sequences of expansions and contractions in 'many economic activities and sectors', the comovements extended in this case to include growth rates.[79] The CSO implementation relies on the fixing of 'reference dates' as a proxy for generalized fluctuations, i.e., 'the set of turning points representing the cyclical movements of a hypothetical variable which may be thought of as "aggregate economic activity"', and which can be tracked by leading, coincident and lagging indicators.[80]

Since 1980, the US reference chronology has been determined by the NBER Business Cycle Dating Committee, still using the Burns and Mitchell 1946 definition of expansions and contractions in 'aggregate' activity, but having abandoned the classification of cycles according to Mitchell's four 'phases'. Romer argues that in fact the procedures are as 'laid down in more or less final form in *Measuring Business Cycles*'.[81] The Committee fixes turning points in aggregate activity between expansions and contractions ('recession') by assessing movements in several coincident indicators. This process is described in an article by the NBER Director of Public Information in the *NBER Reporter*, Spring 1989:

> Historically, as now, determining when there was a turning point (peak or trough) in the economy was a two-step process. First, has there been a turning point? Here the NBER's historical series are useful for comparing current economic activity with earlier business cycles in terms of: duration, depth of the

[79]'Cyclical Indicators for the United Kingdom Economy' in *Economic Trends*, March 1975, pp. 95-98.

[80]*Ibid*., p. 95.

[81][1992] p. 4.

decline in aggregate activity, and diffusion among different economic activities and in different industries, sectors, and region [known as the '3-D' criteria].

> To identify the peak, for example, total business sales, the industrial production index, real GNP, the unemployment rate, non-agricultural employment, man-hours of nonfarm employment, and personal income must all be considered. Composite indexes of these series, and the components of each series, are also useful. As Moore points out, one of the advantages of basing the decision on such as wide variety of evidence 'is that it reduces the possibility of error and the need for subsequent revision'.[82] (pp. 2-3)

An NBER press release of 25th April, 1991, gives a flavour of the committee's deliberations and shows how the emphasis has shifted from Mitchell's original intentions to the tracking of the reference cycle as an end in itself.[83]

> The Business Cycle Dating Committee of the National Bureau of Economic Research reached the judgment today that the peak of U.S. economic activity occurred in July 1990. The current recession began in July 1990, in the committee's view. ([1991] p. 1)

The tendency of turning points to be spread over a number of months is still evident.

> Each of the indicators peaked in a different month... Nonfarm payroll employment reached a peak in June. Real personal income peaked in July. Real manufacturing and trade sales peaked in August. The index of industrial production peaked in September.
>
> The committee does not use a fixed formula to reach its conclusion about the date of a business cycle peak. It reaches a judgment based on a variety of monthly indicators. ([1991] pp. 1-2)

A further discussion of the dating procedure, written by Robert E. Hall, the committee chairman, appeared in the *Reporter*, Winter 1991/2. He argued that the 'basic problem of dating a business cycle' is that 'different cyclical indicators have different

[82] G.H.Moore, *Business Cycles, Inflation and Forecasting*, Second edition, NBER Studies in Business Cycles No. 24.

[83] The evidence that NBER reference dates track turning points in US aggregate output is given in Table 5.3.2 below.

turning points'. Hall expresses further misgivings about the '1990-1 recession' and about the procedure in general.

> All recessions are hard to forecast, and the most recent is no exception. But this recession is particularly remarkable for the breakdown of the one apparently reliable principle of recession forecasting that had held previously. Financial market stress rather systematically preceded almost all previous recessions. Many different indicators of stress have been shown to have some forecasting power, including the stock market, monetary aggregates, and interest rates. ([1991/2], p. 2)

In 1989, however, these leading indicators failed. As incorporated, for example, in the Stock and Watson 1989 probability model, 'based on historical statistical relationships', they yielded 'extraordinarily low probabilities of impending recession in early 1990, even when conventional forecasters were gloomy'.[84]

> Apparently the current recession is a very different animal from others summarized in the data Stock and Watson used. In particular, this recession began outside of the financially sensitive sector of the economy. ([1991/2] p. 3)

In any case, reference dates have never been entirely free from controversy (as is evident in the stabilization literature). In an article appearing in the Summer 1991 *Reporter*, Hall lists the recessions beginning in 1967, 1973 and 1980 as examples of 'previous episodes [that] have challenged the Bureau's dating process'. Because the dating criteria are informal, there is always the danger of differences of opinion within the ranks of professional economists.

The latest word from the NBER is that of Zarnowitz [1992]. This author accepts, with misgivings, the MBC reference chronology and empirical methodology, and uses it *inter alia* to discuss differences between pre- and post-war 'cycle' durations. He sees business cycles as fluctuations in aggregates — employment, output, income and expenditure — and also in 'co-movements and interactions of many variables'. The Burns

[84] *Ibid.* The models of Stock and Watson are considered in Chapter 4, below.

and Mitchell 'definition' encompassed a 'broad range of durations' (one to twelve years) deliberately to 'admit...both short and long cycles...vigorous and weak expansions, severe and mild contractions'.[85] Business cycles are thus 'a class of varied, complex and evolving phenomena of both history and economic dynamics', intractable to single-cause models or theories; but are '*not* the fluctuation of any single aggregate [emphasis in original]'.[86] Zarnowitz also distinguishes 'growth cycles', described in a manner similar to Lucas as 'deviations from trend', from 'business cycles', which must be fluctuations in levels.[87] However, the 'trend' in this case is obtained as a separate component by the application of moving-average filters, a method which has been much criticized in the time-series literature for inducing distortions in the component estimates.[88]

3.6. DISCUSSION

'Business cycle' research in recent years, both theoretical and empirical, is largely based on the assumption that the phenomenon is 'proved', contrary to the inconclusiveness of the MBC findings. Burns and Mitchell stressed again and again that each historical episode is unique.[89] Burns [1951, 1952] considered the results as merely establishing the hypothesis and not as conclusive. Those closest to the problem in the NBER have always been circumspect about the nature of short-run fluctuations. After Mitchell's death the impetus for further technical development was lost, and even now the reference-dating procedure (the '3-D' criteria) is as prescribed in MBC. It is not entirely clear why the authors' warnings about the reliability of their results have been so

[85] *Ibid.*, p. 9.

[86] [1992] p. 9.

[87] *Ibid.*, Section 3.4.

[88] This issue is discussed fully in Chapter 4 below.

[89] E.g., in BCPS, Chapters II and III, in the 1938 paper and in the remark in MBC, p. 467, that 'each specific and business cycle is...an individual differing in countless ways from every other'.

persistently ignored; but it is likely that unresolved problems in the work have themselves contributed to the judgments of later generations of economists.

For example, it is not apparent why Mitchell, who understood the inductive method as well as anyone, should fall into the elementary error of mislabelling his hypothesis as a 'definition'. This point may seem somewhat pedantic, but an important methodological distinction does exist which, if not observed, can lead to confusion. The 'definition' is used in mathematics as the basis for the construction of a formal argument leading to a general result about a class of objects. The 'hypothesis', on the other hand, is the starting point for an empirical enquiry leading to statements about the properties of observed phenomena. It is not clear whether either the 'proof' of an hypothesis by deduction or the 'validation' of a theorem by observation are statements that have any meaning.[90]

There are other significant methodological lapses. Having inadvertently misrepresented their own case by mislabelling the central hypothesis, Burns and Mitchell then failed rigorously to define the (mathematical) metric, the turning point, by which they meant to measure it. By arbitrarily fixing a reference cycle which strictly should have been found empirically from turning point data, they introduced both circularity and bias into the analysis. Their response was to attempt to adduce 'annals' and a subjective chronology of specific cycles as evidence. Since both these sources are of questionable reliability, the error may have been further compounded. It is only possible to speculate as to the causes of these failures of method. Beveridge, of course, made a similar mistake at about the same time (1944), suggesting that something in the intellectual climate might account for such lapses. Perhaps Mitchell's contemporaneous concern with the establishment of the national income accounting procedures, where definitions are appropriate, might have inadvertently confused his approach to business cycles. Perhaps he felt that time was short, tempting him to cut corners in the search for a definitive

[90] Professor Richard Feinman pointed out some years ago that 'all [scientific] theories are wrong' and should be treated as mere approximations to be discarded or modified as more information becomes available.

statement. None of these explanations is really persuasive: Mitchell was perhaps cleverer than his own arguments, shown by the antiphonal self-doubt running through all his writing.

There is, however, another possible explanation, which owes as much to the misperceptions of the present day as to Mitchell's failures. It is important to remember that MBC was written during the War, and that its empirical results were based on a sample period ending in 1939, during the latter twenty years of which there was no overall growth. The authors had no aggregate series, hardly any output statistics and no experience of the persistent growth patterns which have become so potent a feature of the postwar economic landscape. Before World War II there was an effective theoretical consensus that the engine of macroeconomic fluctuations was price behaviour and its influence on business profits — ideas which in the postwar climate became discredited. As late as Burns [1961] these ideas still had some force. This may partly account for the generation gap pointed out by Marschak.[91] Unfortunately, the historical context of Mitchell's results has not been given proper weight in recent business-cycle research.

On the principle that economists tend to shoot at moving targets, the problems of growth were relegated to the background by most writers of the inter-war period because of stagnant conditions. The Balke and Gordon estimates show that all net real growth in US GNP from 1918 to the date of publication of MBC took place in the war period 1938-45; but this was certainly overshadowed by three episodes of quite severe falls (see Figures 1.1 and 1.2 above). In fact, real output in the US economy fell by about 22 percent from the first quarter of 1945 to the first quarter of 1947, roughly the magnitude and duration of the 'depression' of 1919-1921.[92] There is thus no way in which the postwar acceleration in growth rates could have been anticipated at the time, and the

[91]The debate was not about which variables to include, on which a consensus existed, but rather, as Wicksell had put it, about 'chickens and eggs'.

[92]So while MBC was in galley proof, it looked as though history might be about to repeat itself. See estimates in the Appendix to R.J. Gordon (ed.) [1986], pp. 789-99. The index numbers are 595.55 for 1945:1 and 466.05 for 1947:1.

probability of a structural 'boom' appeared less than that of a return to chronically 'depressed' conditions. The latter were indeed were being confidently predicted in the second half of 1945.

At the same time, versions of the command economy in the war years had succeeded in a spectacular fashion in mobilizing resources in the UK and the US (as indeed in the Axis powers and the Soviet Union). These 'social possibilities' were seen as a model for postwar reconstruction, for example by Beveridge and later by Keynesians such as Rostow. There is even an echo of such *dirigisme* in Marschak [1951]. Economists thus generally felt it imperative to continue the study of 'cycles' in an attempt to understand and perhaps help to forestall a new slump by the action of government. Moreover, quite soon after the end of the War 'anti-communist' incentives were thought to be required to prevent the masses of the West being beguiled by Marxist ideology, as so carefully expressed in Burns [1961]. The emphasis thus changed from the quantity-theoretic to the domino-theoretic. It was this confluence of command experience during the World Wars and the exigencies of the Cold War that gave the 1950s and 1960s a distinctly centralist flavour, with demand management, 'fine-tuning' and high marginal rates of tax the favoured policies.

The revisions of new classical theory were similarly motivated by observed instabilities: the post-Smithsonian shocks of the 1970s. By this time, however, growth was again dominant, leading Lucas to advocate trend-stationarity — processes distinct from Mitchell's four-phase cycle, as argued by Kydland and Prescott. However, because the RBC literature embraces the MBC concept of comovements as one of its foundations, it can be argued that its view of empirical behaviour is not that far removed from Mitchell's, even though its causal mechanisms are distinct. As Romer has argued, the NBER dating committee also follows the methods of Burns and Mitchell. The four-phase hypothesis was quickly discarded after Mitchell's death, so that in their pure form 'Mitchell cycles' have only been of historical interest in the whole of the postwar period.

Nevertheless because the comovement hypothesis is supported in most conceptions, the most widely held view of business cycles is not all that far from the diffusion criterion proposed in MBC. The phenomena are thus still regarded as fluctuations in general conditions, although most authorities now believe them to be a species of behaviour related to growth.

The RBC approach consists principally in simulating 'business-cycle' behaviour. This follows Lucas's understanding of Burns and Mitchell's empirical descriptions, as modified by Kydland, Prescott, Cooley and others. The interpretation is of stochastic general equilibria with Pareto-optimal allocations, implying the rejection of Phillips Curve trade-offs as a means of improving welfare. All such models are considered 'false' by their authors, though perhaps this is too harsh a value-judgment. What Kydland and Prescott actually mean is that the models are not designed to be representations of the DGPs of any empirical time series, and that correspondences with empirical data are incidental to the main purpose. They are more in the nature of tests of the models' plausibility than of empirical verification, because MBC 'conclusions' are assumed to be 'correct' *a priori*. Commentators on these 'quantitative' theories such as Pagan argue strongly against drawing inferences about empirical phenomena from the outputs of RBC models — a caution shared by the model-makers themselves. Stadler points out many of the problems still facing RBC models, and argues that the current lack of conformity to actual behaviour restricts their explanatory power.

Mitchell's great prestige, aided by the Chicago Interpretation and by Lucas, meant that the MBC results are understood as definitive rather than as exemplary, whatever might have been the unconscious motivation of economists after the 1945. Taken with the confusions of method, the apparently conclusive nature of MBC results seems to have created the impression that the results were 'scientifically' reliable and universally applicable. It is clear however from Burns [1951] that Mitchell rejected the idea of 'general business conditions' out of hand; and that even the existence of a GNP series would not have changed this view. Given the postwar experience of growth, perceptions of 'business cycles' have moved closer to the 1927 interpretation: fluctuations 'in the pace of a more fundamental process' — the exception being Zarnowitz, who makes a clear distinction between the two types of behaviour.[93] Recently expressed views of Cooley and Prescott concur with the unitary interpretation. The implications of structural change for the 'business-cycle' hypothesis have also been recognized. Zarnowitz argues that the idea of business cycles must be continually reexamined because conditions 'evolve' over time; and that the 'many interacting processes...are certainly not the same at all times, and in all developed...economies'.[94]

An important source of confusion for economists of later generations has been the role of the 'reference' dates. These were originally intended as an aid to theoretical analysis, but soon metamorphosed into a distinct phenomenon: a 'reference cycle' in aggregate output, as discussed by Burns in 1961. Even in that paper, however, the author commented that each episode was 'a unique configuration of events'. Unless it is remembered that the object of Mitchell's programme was to find an overarching theory the significance of the reference dates cannot be properly understood. They were only ever seen by Burns and Mitchell as a preliminary attempt to capture the average behaviour on which the theory would be based, and not as definitive 'business cycles'. Mitchell always insisted that business cycles are multivariate phenomena, and can be validated only by the clustering or 'bunching' of turning points and, according to Burns, recognized the difficulties that empirical 'divergences' presented for his hypothesis.

Theorists acknowledge the importance of 'comovements' for business-cycle behaviour, and rely on the Burns and Mitchell definition. Disagreements, especially in recent papers, appears to be about the sources of fluctuations and their relation to growth rather than about their generality, the evidence of which is generally understood to have

[93]The inferences of Chapter 12 of MBC that noise was more important than growth were based on atypical time-series evidence from the 1920s and 1930s.

[94][1992] p. 14.

been established in MBC. Although the RBC interpretation does not directly consider turning-point evidence, it can be argued that the business-cycle 'component' in these models shows a clear alternations of expansions and contractions at predetermined (i.e., H-P filtered) frequencies, which implies clearly identified peaks and troughs. The comovements assumed by the theory, together with filtered components, are highly suggestive of Mitchell's 'many activities' criterion as endorsed by the NBER and other tracking agencies. However, the emphasis on comovements in the theoretical literature places it perhaps closer to the spirit of Burns and Mitchell than current NBER dating procedures with their emphasis on the 'reference cycle'.

APPENDIX 3

CHAPTER 3 GRAPHICS

CONTENTS

Figure 3.3.1. BCPS Chart 1

CHART 1.

Sample Time Series Plotted on a Logarithmic Vertical Scale.

Only the Relative Slope, Not the Vertical Position, of These Curves Is Significant.

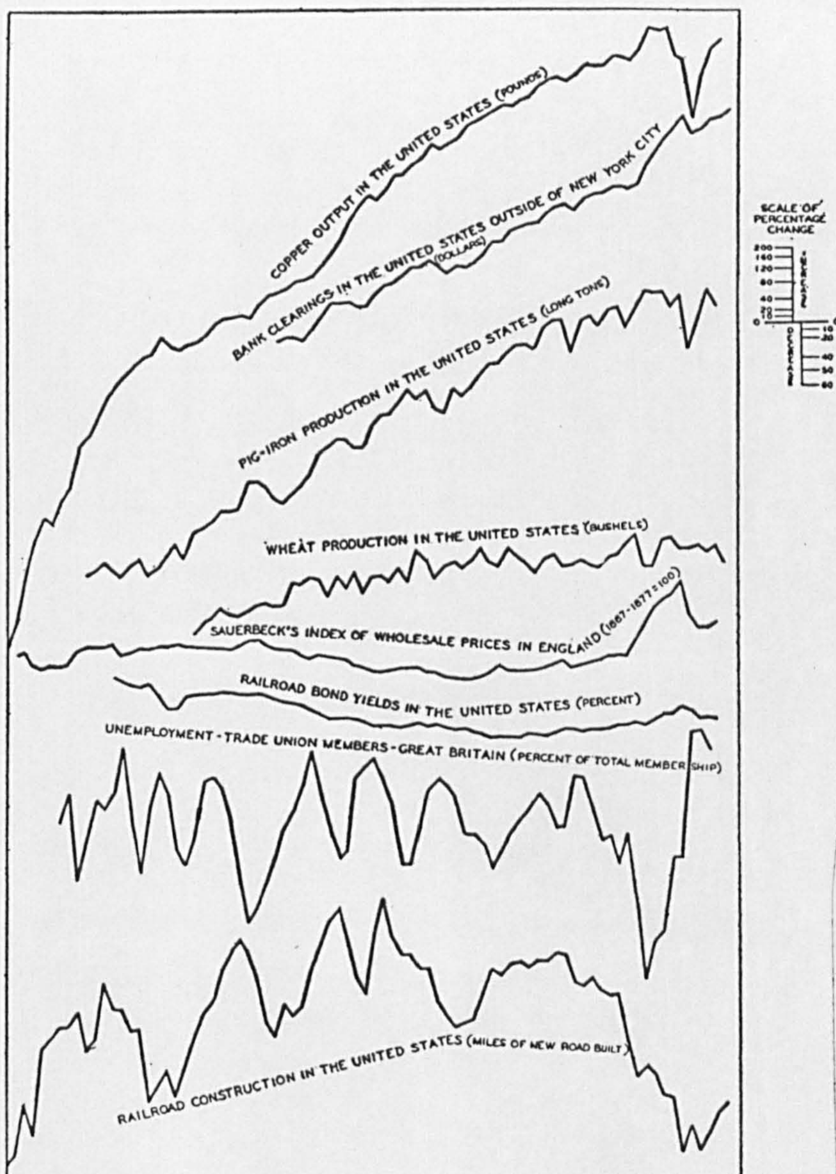


Figure 3.3.2. BCPS Chart 8

CHART 8.

RESIDUAL FLUCTUATIONS OF TIME SERIES AFTER ELIMINATION OF SECULAR TRENDS AND SEASONAL VARIATIONS.

Percentage deviations of original items from secular trend corrected for seasonal variations.

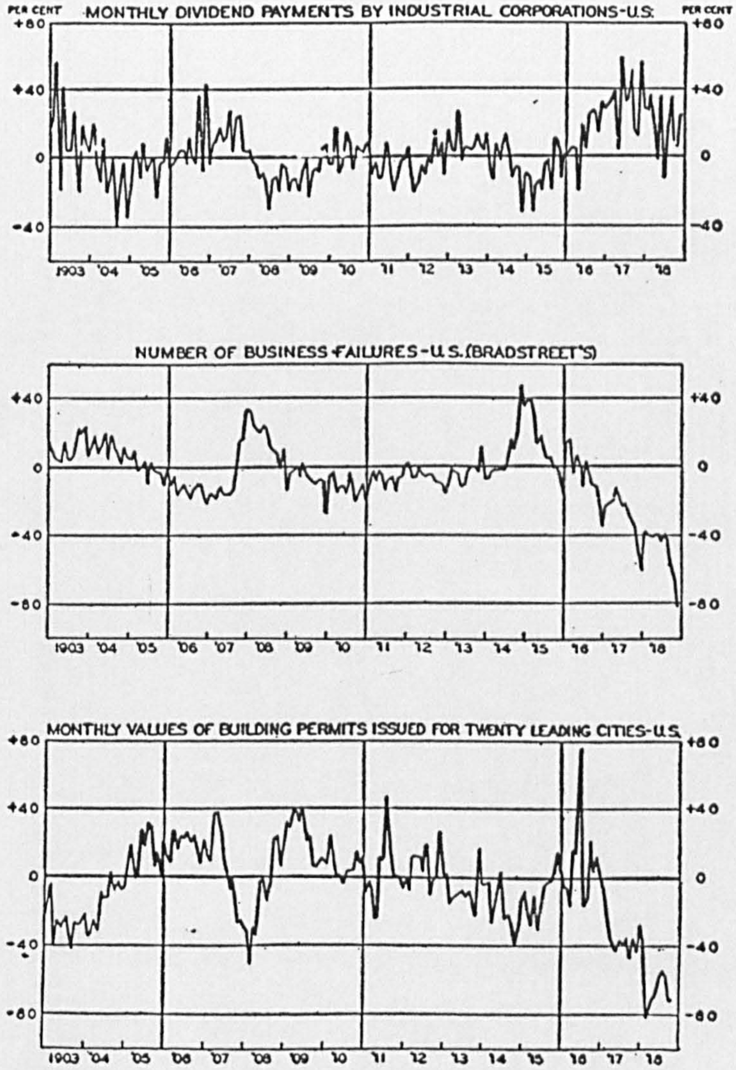


Figure 3.3.3. BCPS Chart 18

CHART 18.

ONE BUSINESS CYCLE, UNITED STATES, 1919-21, SHOWN BY A PLOT OF 27 OF ITS COMPONENTS.

Twelve of the Components Used in Snyder's Volume-of-Trade Index to Represent *Productive Activity* and *Primary Distribution.*

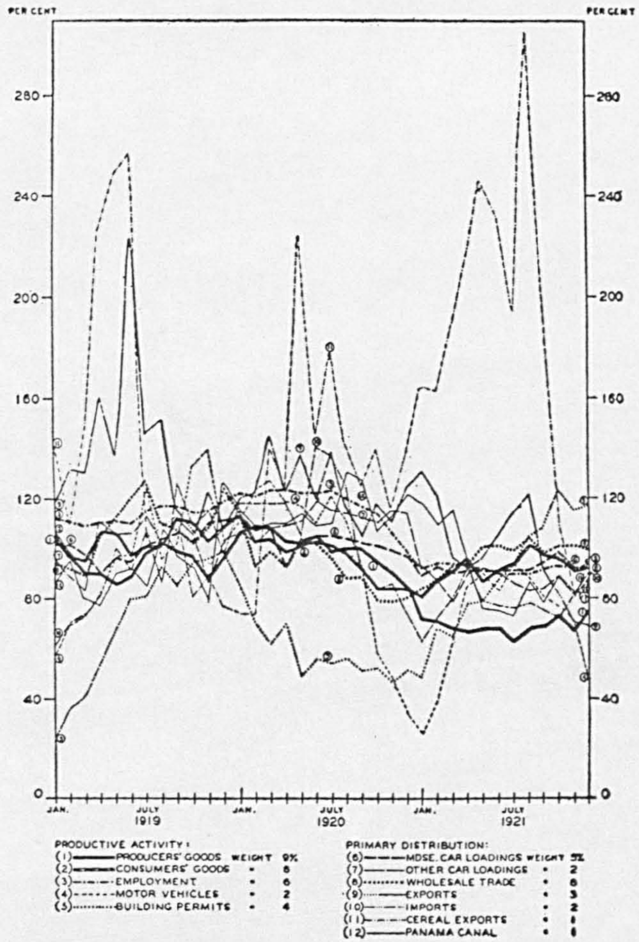


CHART 18 (*Continued*).

ONE BUSINESS CYCLE, UNITED STATES, 1919-1921, SHOWN BY A PLOT OF 27 OF ITS COMPONENTS.

Fifteen of the Components Used in Snyder's Volume-of-Trade Index to Represent *Distribution to Consumers, Finance,* and *General Business.*

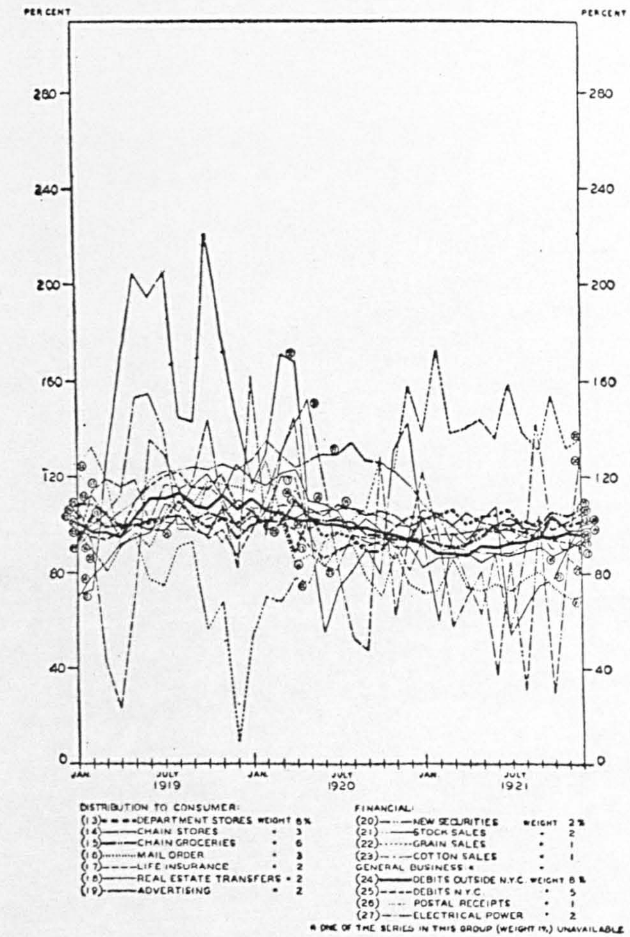


Figure 3.3.4. Example of Pattern Charts from 1929

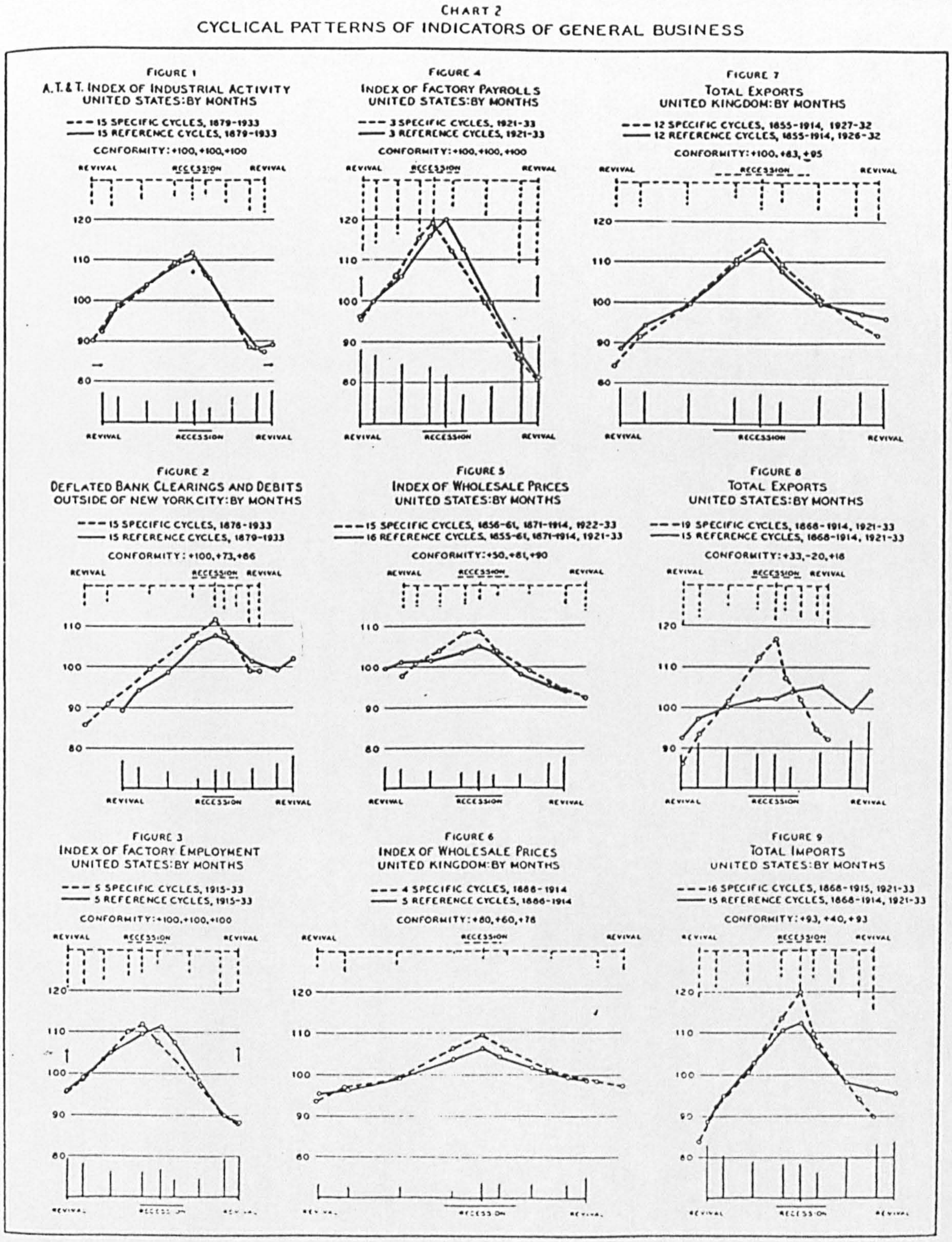
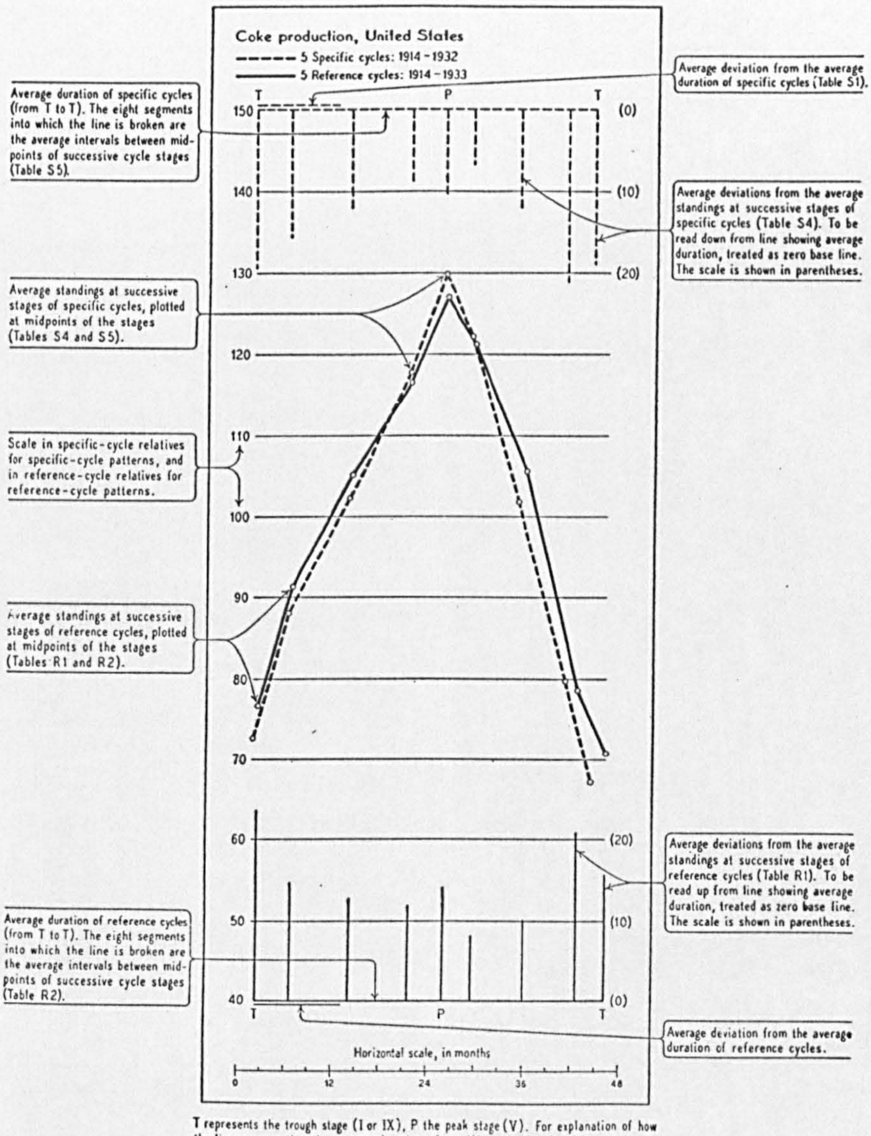


Figure 3.3.5. MBC Generic Pattern Chart

CHART 2

Sample Chart of Cyclical Patterns

(Drawn to twice the standard scales)



T represents the trough stage (I or IX), P the peak stage (V). For explanation of how the line representing the average duration of specific cycles is placed in relation to the line representing the average duration of reference cycles, see Ch. 5, Sec. VIII.

Figure 3.3.6. MBC Chart 16: Divergences in Specific–Cycle Patterns

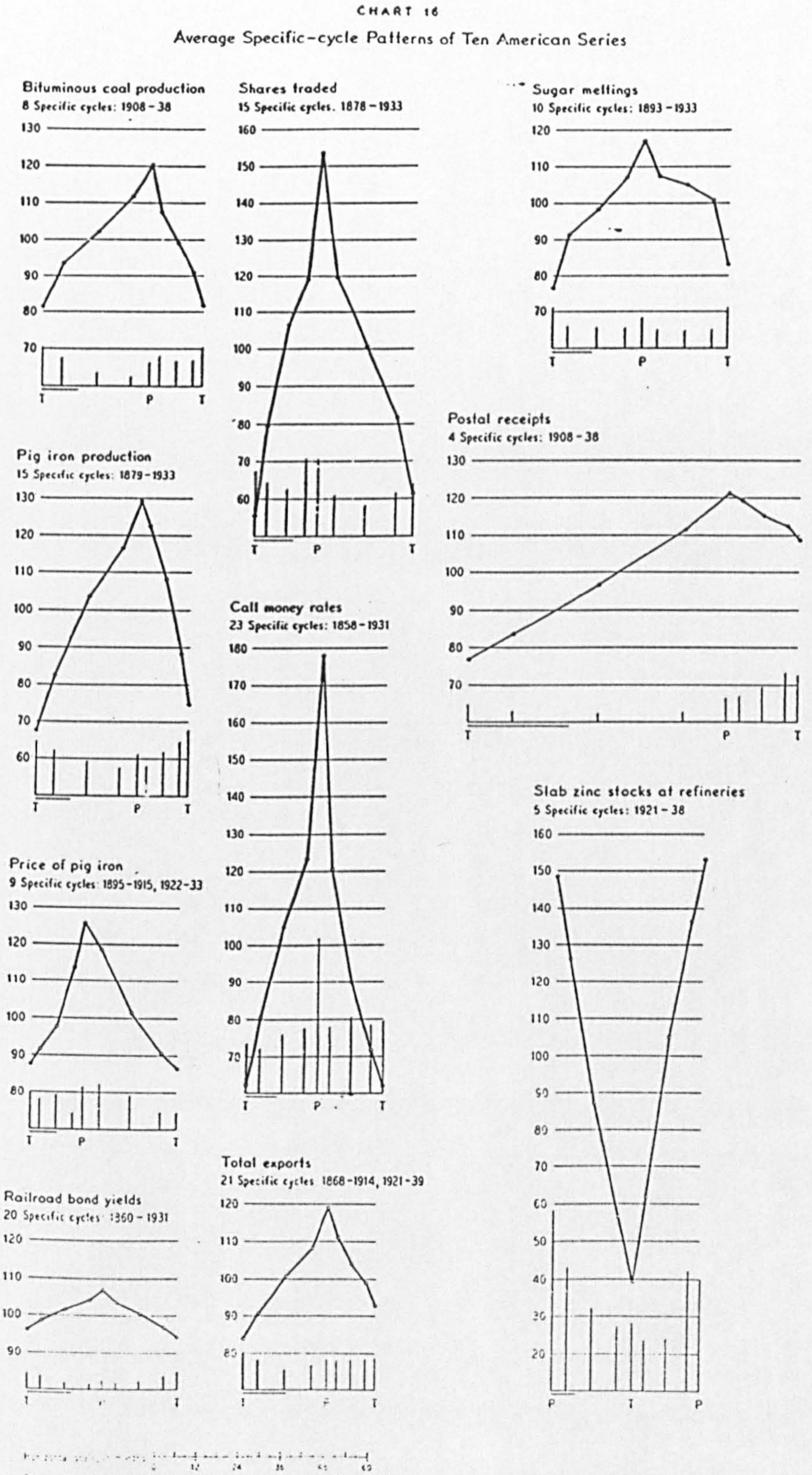


Figure 3.3.7. MBC Chart 19: Divergences in Reference–Cycle Timing

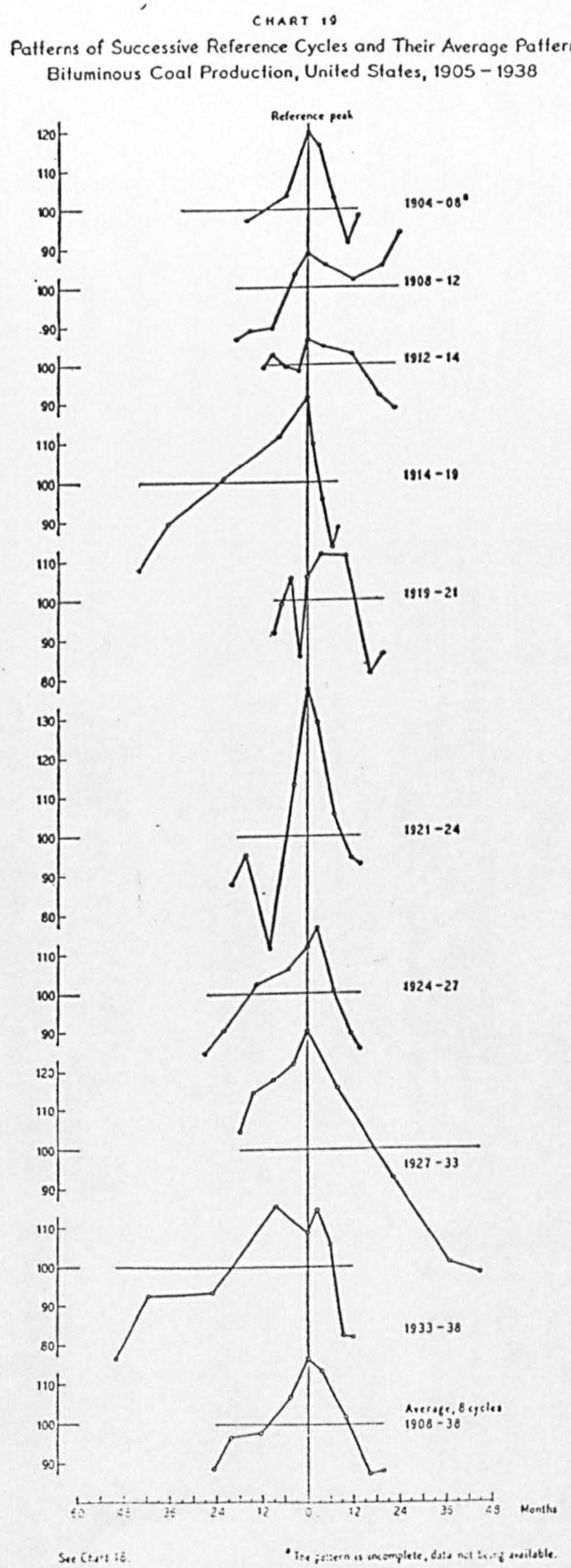
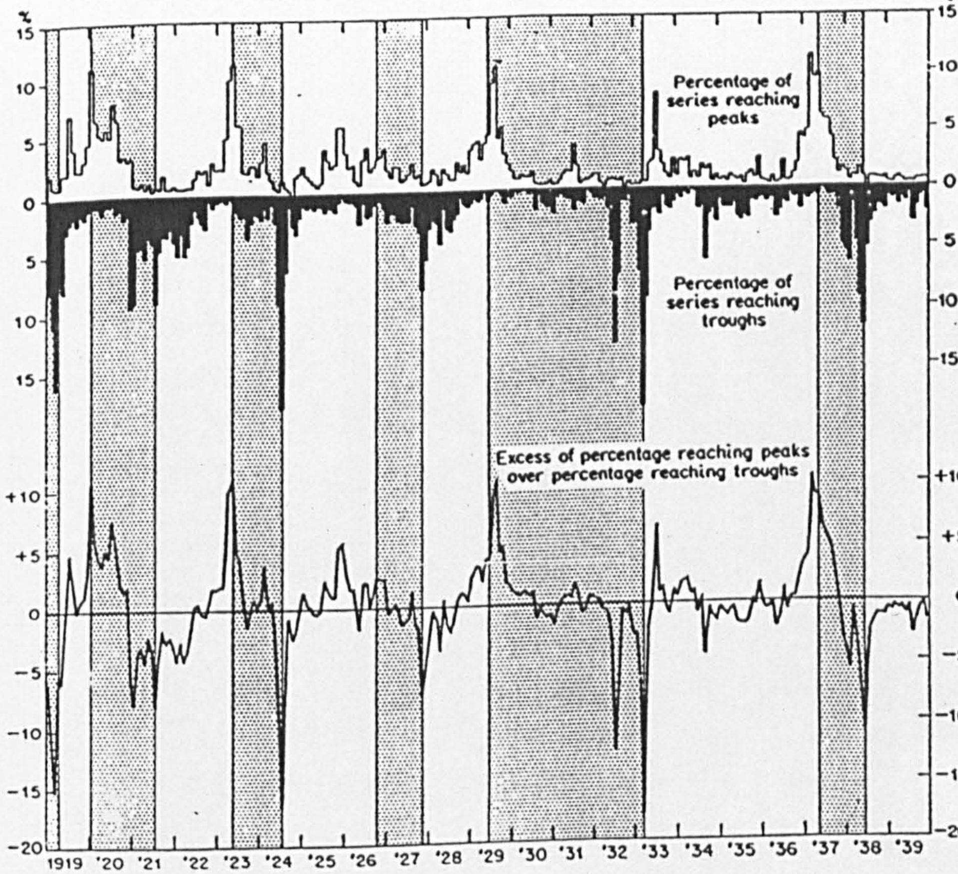


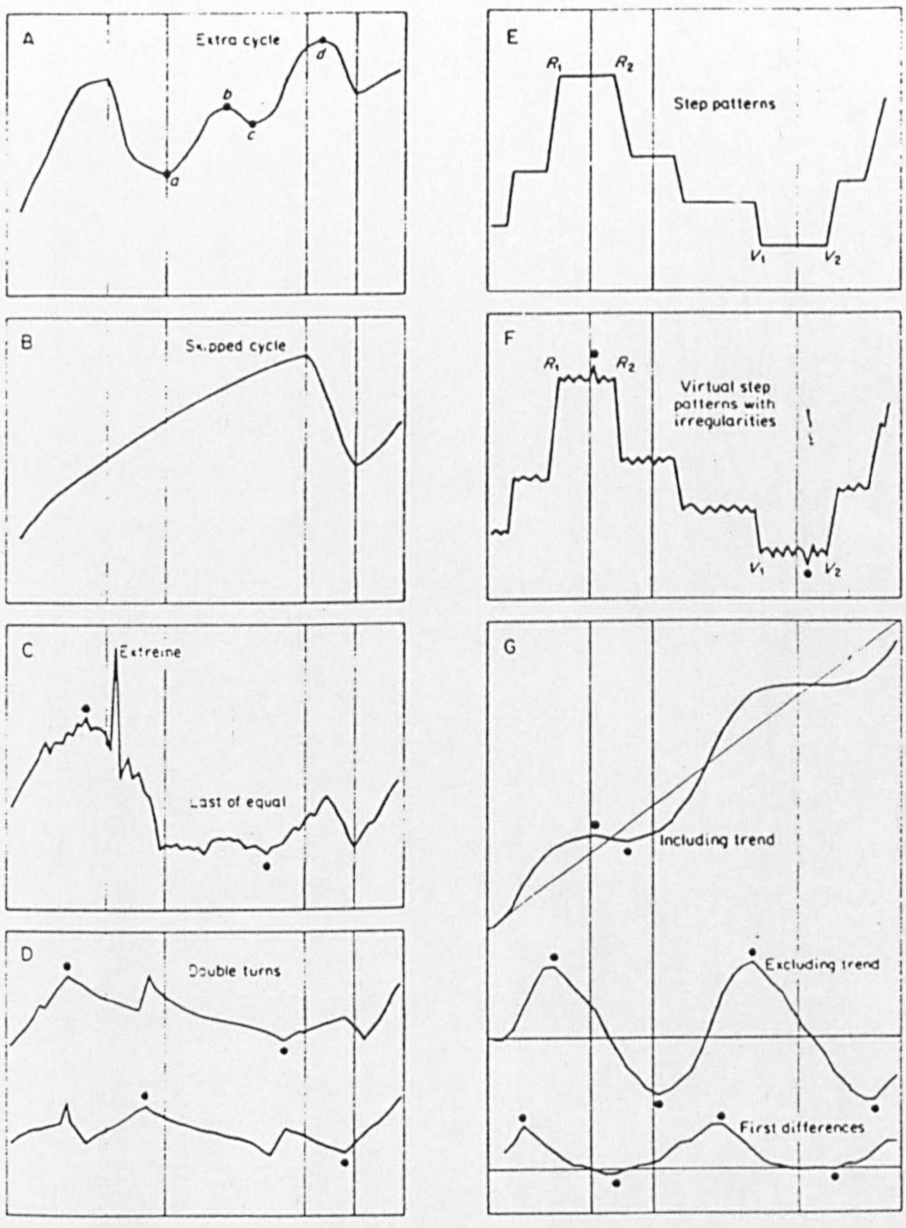
Figure 3.3.8. Burns's Seen and Unseen Cycles

Distribution of Turning Points of Specific Cycles in a Sample of over 600 Economic Time Series, 1919–39



Shaded areas represent business contractions.

Figure 3.5.1. CATS 'Problems of Turning Point Determination'

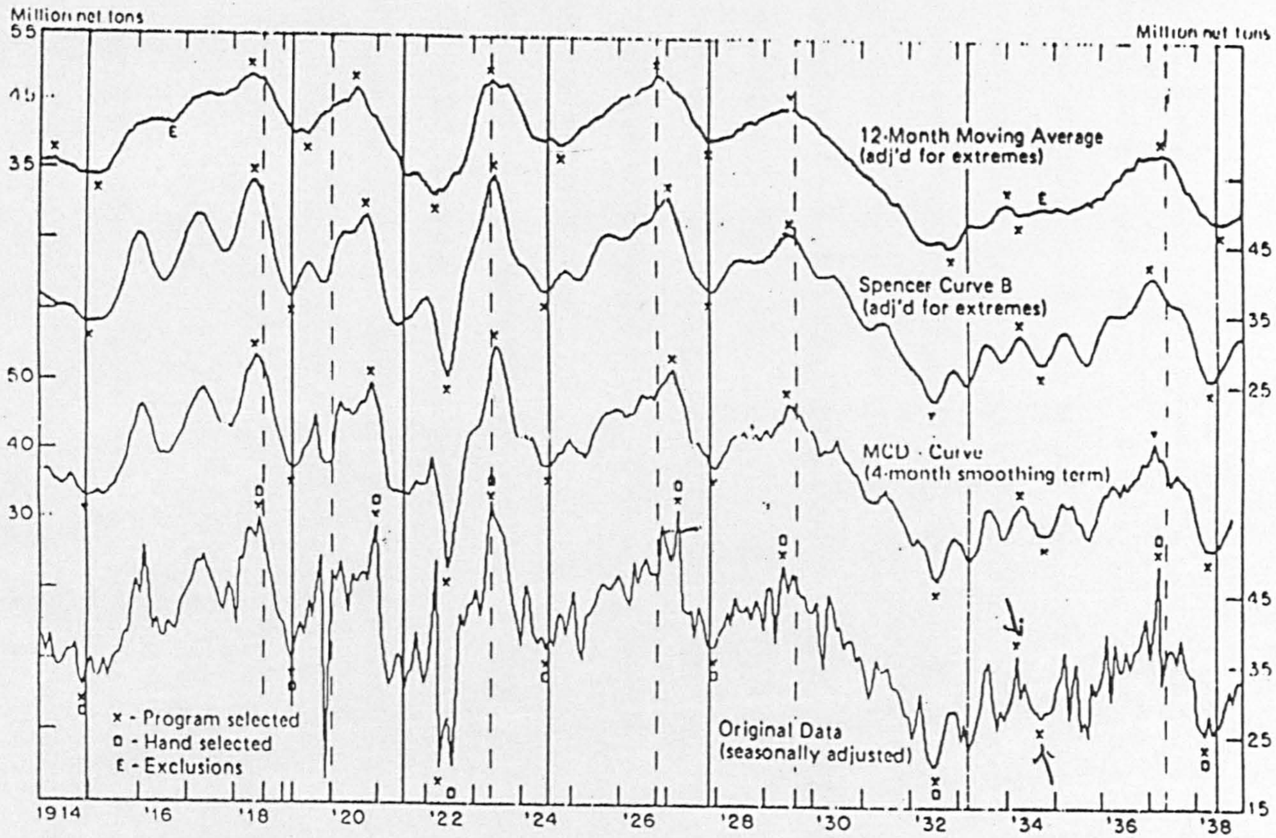


Note: Circles denote specific cycle turning points. Vertical lines stand for alternating peaks and troughs in general business activity.

Figure 3.5.2. Operation of the Bry–Boschan Algorithm.

CHART 3

BITUMINOUS COAL PRODUCTION AND MOVING AVERAGES, 1914–38



Note: Broken vertical lines denote business cycle peaks; solid vertical lines denote business cycle troughs.

CHAPTER 4

REPRESENTATIONS OF 'BUSINESS CYCLES': EVIDENCE FROM TIME-SERIES DECOMPOSITIONS

4.1. INTRODUCTION

> It has been customary to analyse an economic time series by extracting from it a...trend...and then scrutinizing the residual portion for short term oscillatory movements and random fluctuations. The assumption...is that the long-term and short-term movements are due to separate causal influences and therefore that the mathematical...analysis corresponds...to a real distinction of type in the generative system. When it began to appear that better accounts of such movements were given by autoregressive schemes in which the disturbance element played an integral part, it was an easy step...to enquire whether the so-called trend in a series was in fact separable from the short term movements, or whether it should be regarded as generated by a set of forces which gave rise to the short-term movements.

This passage appears in a paper given some years ago before the Royal Statistical Society by Sir Maurice Kendall, and expresses the author's scepticism about standard methods of empirical business-cycle analysis.[1] The 'distinction of type in the generative system' was questioned at the time by a number of authors: for example, both Orcutt [1948] and Goodwin [1953] argued that macroeconomic time series did not have separate data generating processes (DGPs) for 'trend' and 'cycle'. Decompositions of this type had become popular before the First World War as it became evident that 'secular' tendencies were present and that they generally dominated other 'components'. Serious attempts to track cycles hence required that they be separately identified, normally by transforming the data to achieve stationarity. Partly through custom and partly because it is plausible, a four-component meta-model of economic behaviour — i.e., trend, cycle,

[1] [1953] p. 11.

seasonal and irregular — has come to be taken for granted in time-series analysis; yet as Kendall and others indicated, there is no obvious reason why there should be four components, or indeed more or less. For present purposes the issue is not whether this 'classical' decomposition is 'correct', but rather how its wide application over three-quarters of a century has influenced empirical research, and what inferences can be made from it about business cycles. As will presently be demonstrated, the evidence is inconclusive, and inferences depend largely upon whether the hypothesis is believed *a priori*. Section 4.2 discusses the early modelling literature, and how this led to the interpretation of business cycles as the cumulative effects of random shocks. Section 4.3 considers the use of large-scale models to represent business cycles as endogenous phenomena, and shows how this view later changed. Section 4.4 surveys models of asymmetry, a feature of short-run fluctuations in economies where growth predominates. Section 4.5 discusses the recent modelling literature and critiques of the classical decomposition. Section 4.6 presents the results of a Monte Carlo study of measures of asymmetry for nonstationary oscillatory AR(2) processes.

4.2. EMPIRICAL AND EXPERIMENTAL ANALYSIS OF 'BUSINESS CYCLES' IN THE 1920S

In the 1920s social scientists still believed that 'solutions' to the problems of poverty, disease and social unrest could be found rationally. A prerequisite to this search for stability was the understanding of fluctuations in prosperity — the 'business cycle' — which had proved so damaging to economic welfare in the previous hundred years. The great hope of economists was that science and mathematics could be pressed into service, and that the unfavourable conditions could thereby be predicted and stabilized. The question became more urgent when the decade began inauspiciously with a violent postwar 'depression' whose concurrent price instabilities to some extent masked the real effects. Contemporary observers, bemused by these complexities, sought an intuitive and convenient method of visualizing these fluctuations 'in the eye of the mind', and of forecasting them.

4.2.1. Business Barometers: a 'Plausible' Representation

In the 1920s, 'business barometers' were a popular method of synthesizing fluctuations in 'general business conditions' from time-series data by the construction of indices. Their best-known advocate was Professor Warren Persons of Harvard University, who according to Morgan was responsible for giving the technique both popularity and academic respectability.[2] Persons, in a 1919 article, noted that the large volumes of statistical indices then becoming available in the financial press gave 'little or no information' about either the scope of the data or the methods of analysis. Because such results were not 'reproducible' and so violated a primary requirement of experimental science, he set out to make his analyses transparent by publishing both data and methodology. This turned out, however, to be a complicated process. A 'preliminary survey of the graphs' of widely differing data types ('bank clearings, iron production, commodity prices, and new building permits') — Morgan's 'strange collection' — led to the 'working hypothesis...that each series is a composite consisting of four types of fluctuations' (as above). No empirical reasons were given for such a decomposition; Persons was simply following the consensus-view of the time, which thus rendered the 'working hypothesis' little better than *a priori* opinion.

Wide variations were found in all the components across all series, especially in the cyclical:

> each [series] appears to have cycles, but these cycles are neither synchronous nor of the same intensity... General methods...must always take into account the special characteristics of each series. ([1919] p. 8)

[2]See Morgan [1990] Section 2.3.

Persons nevertheless compiled composite indices because, in a manner similar to Mitchell, he argued that the individual series 'convey a meaning only in case we are able to compare [them] with others of a similar nature, or with a standard...' In order to render the 'strange collection' comparable, he tried to isolate cyclical from secular and seasonal components. He experimented with moving averages and linear and polynomial time trends, but none were entirely satisfactory: moving averages did not achieve stationarity, and trend lines varied considerably in slope with the sample period. Persons had hoped that the observations for each series would fall into 'homogeneous' clusters of years with similar behaviour and without noticeable structural breaks. If such were the case the trend found for each cluster would be justified as representing a smooth transition. If, on the other hand, conditions changed abruptly or unpredictably from time to time, the interpretation of trend lines would be tenuous — and in the event the latter seemed to be the case. As a result, Persons judged that each series should have two trend lines, one for the period 1879–1896 and a second for 1896–1913, and that these should be interpreted as 'normal' conditions. Each series was then expressed as deviations from the 'normal', and the barometers were constructed from weighted averages of these transformed series in deviations.

Persons and his assistants grouped together those detrended series having approximately the same distances between peaks and troughs, even if they were not necessarily in phase. Using a light box (as described by Morgan, p. 61) 'correlations' among groups of variables were worked out by eye. From these groups, three curves composed of weighted averages of detrended series with similar timing patterns emerged, known as the Harvard A-, B-, and C-curves. In a further study published in 1925, Persons discussed an index of trading conditions constructed in a similar manner, which he interpreted as representing a coincident indicator of US 'general conditions' from 1903 to 1925. This index appears stationary, although with the fluctuations increasing in amplitude between 1916 and 1921. The curve seems to behave plausibly: along the plot are noted various crises: the 'rich man's panic' of 1903, the panic of 1907, the San

Francisco earthquake, the War, etc., and the index showed plausible increases and decreases. Persons argued that the 'timing of advance and recession' was roughly the same as in the constituent variables — the 'strange collection' again — a contrast with his earlier assertion of idiosyncratic movements in each. The effect is presumably the result of the data-transformation process, although this is not clear from the discussion. However, a clue to the behaviour of Persons's model is given in the penultimate paragraph of the paper:

> For all...sections covered...the choice of series and weights is determined by the object in mind, that is, the construction of an Index responsive to variations in the general physical volume of trade. ([1925] p. 78)

The importance of business barometers lies not so much in what they demonstrated about the nature of 'business cycles' as in how they conditioned those who took notice of them. That they were popular in the 1920s is not in dispute: Professor Irving Fisher mentions nearly 'fourscore forecasting agencies' engaged in tracking an '*alternation* of booms and depressions [emphasis in original]'.[3] Morgan talks of an 'explosion' of interest in statistical business-cycle research in all industrial countries, albeit with varying intensity; and despite some claimed differences of emphasis all research followed similar programmes. Wagemann, in Germany, for example, claimed not to employ the 'classical' decomposition (as above), but Morgan has shown how close to Persons's views this work really was. Slutsky at the Moscow *Konjunctur* Institute, and Yule in the UK, both favoured an experimental approach; but whatever the method, most researchers would have had 'in the eye of the mind' some form of 'coincident' indicator, stationary and with fluctuations of irregular period, summarizing 'general' business activity.

Fisher was the chief dissenter from this line of research, arguing that business cycles were illusory: a nominal rather than a real phenomenon. He believed that the

[3] [1925] p. 180.

alternations so assiduously sought by the constructors of indicators ignored qualitative differences between price and other variables. Prices were subject to longer swings than output variables, and hence attempts to detrend them were 'useless' because they 'could not properly be said to have any secular trend'. More importantly, the extreme price fluctuations of the 1914-1918 period were omitted although these could have explained other fluctuations. Furthermore, 'trend' and 'cycle' were not separable components: the elimination of a 'supposed trend' discarded 'a part of the rise and fall which...is so vital a factor'.[4]

4.2.2. Experimental Work in the 1920s

Early business-cycle research looked for 'causes' of what appeared to many as a phenomenon of irregular periodicity. First Jevons and later H.L. Moore believed that the superposition of waves could link cosmological phenomena with economic cycles. By the 1920s frequency domain analysis of time series data was being actively pursued.[5] The time-domain analogue of this approach was a form of multiple 'correlation' analysis, as practised by Persons and the forecasting agencies. Among statisticians, however, there were many critics of both approaches, and their disillusionment produced some landmark literature. The strategy favoured by the latter was to generate data that resembled economic time series by Monte Carlo methods and then to investigate their time-series properties. Yule's 1926 paper considered moving averages of uniformly-distributed shocks whose first differences were either serially independent or serially correlated. Both types, because adjacent terms tended to be close in value, showed long nonstationary sequences. The analysis demonstrated that for such time series, small samples would have high

[4] The 1925 paper. Fisher argued (p. 181) that the rate of change, rather than the level, was the critical factor, and that inflation and deflation were largely responsible for variations in trade: a quantity-theoretic view, as might be expected.

[5] H.L. Moore, who believed that business cycles were 'caused' by crop cycles, which were in turn influenced by the rotation of the planet Venus. See Morgan [1990] pp. 26-34.

cross-correlations, whereas for the whole series both the theoretical and experimental cross-correlations tended to zero.

Yule [1927] was an attempt to find DGPs for stationary series which would simulate the 'disturbed periodicities', e.g. as observed in Wolfer's sunspot numbers. He found that if the irregularities were the result merely of 'superposed fluctuations' — for example measurement errors — then the underlying periodicities would only be masked and would still be detectable by periodogram analysis. If, however, there occurred 'true' disturbances whose effects persist, the graph of the process would remain smooth but with wide variations in amplitude and with continuous phase shifts, a process which Yule described by analogy to boys pelting a pendulum with peas. Frequency domain analysis was not applicable in the latter case.[6] Instead, Yule experimented with various autoregressive schemes and found the 'best' representation of the data to be damped harmonic oscillation, i.e., an AR(2) process with complex eigenvalues of modulus less than unity. This model, however, required external stimuli to maintain the oscillations, which implied the stochastic form $y_t = \phi_1 y_{t-1} + \phi_2 y_{t-2} + \epsilon_t$, where the ϵ_t are a sequence of serially independent shocks. Yule thought that the damped harmonic formula represented 'some physical reality', and that economic variables were probably subject to this type of disturbance, because economic systems responded to exogenous influences.

The stochastic representation had also occurred to Eugen Slutsky in the course of research in the Soviet Union in the early 1920s. His 1927 paper looked explicitly at economic fluctuations for the first time in the context of time-series models.[7] He observed that all economic phenomena 'occur in sequences of rising and falling movements, like [irregular] waves', and asked whether it were possible that these irregularities could be approximated by Fourier series, i.e., by 'the summation of regular

[6]True disturbances most closely approximated the case of sunspot series. Morgan argues that the powerful association between sunspot and business cycles suggests implied criticism of frequency domain methods in the analysis of the latter. See [1990], Section 3.1.1.

[7]The English-language translation appeared in *Econometrica* in 1937.

sinusoidal fluctuations'. The periodogram method had for Slutsky one great disadvantage: it assumed that the observations are statistically independent, whereas in economic time series they are nearly always serially correlated, as implied by Yule's research. Slutsky was scornful of theories of regular periodicity in economic behaviour, such as Moore's eight-year period for the business cycle: for what force could explain the regularity of a 'sinusoidal wave which rises and falls on the surface of the social ocean'? If applied uncritically, the wave hypothesis of economic behaviour was little better than superstition; to account for such regularities it would be necessary to appeal to the almighty — i.e., for 'the eyes of the investigators [to be] raised to the celestial luminaries...even now, as centuries ago'.[8] Although such 'bold hypotheses' were admissible, the alternatives must also be considered. (In a footnote to this passage, Slutsky acknowledged that Yule [1926] 'approaches our theme rather closely'.)

Slutsky found, as had Yule, that moving sums of serially independent shocks (the 'summation of random causes') could produce 'a system of more or less regular waves'. A ten-period moving sum was obtained from numbers drawn from the state lottery, a uniform distribution (Model I). Further moving sums were taken of Model I: a ten-period moving average of Model I (Model II); a succession of ten two-period averages (Model IVa), etc. The effect of applying successive filters was to make the autocorrelation functions approximate a Gaussian distribution up to a scale factor. When plotted, these autocorrelated series bore strong resemblances to business barometers (see Figure 4.2.1). Additionally, by taking frequency distributions of peak-to-peak and peak-to-trough distances in his artificial series, Slutsky found a correspondence between these distances and those in the turning points of Thorp's *Annals* for 12 countries (see

[8] [1937] pp. 106-107.

Figure 4.2.2).[9] This similarity in frequency, together with the tendency for first differences in artificial 'coherent' series to be small, implied not only

> *that the summation of random causes may be the source of cyclic...processes...*[but that] it must be so inevitably [emphasis in original]. ([1937] p. 114)

Moreover, both abrupt and gradual changes in periodicity could be simulated by taking the deviations of the experimental series from their Fourier approximations.

Slutsky argued that Yule's model of disturbances was equivalent to his moving average formulation. A stochastic process subject to periodic damped oscillations has a movement in two parts: first the impulse, determined by initial conditions, which damps out, and second, the 'summation of random causes', i.e., a moving average of shocks, as the propagating mechanism which maintains the oscillations. In the limit, if the disturbances are small enough, the process will converge to a sinusoidal form and can even explain, for example, planetary trajectories. Nevertheless, as Morgan has pointed out, Slutsky claimed only that his artificially constructed series could mimic business barometers, not that an MA process was the actual DGP for business cycles.[10]

Kuznets investigated the MA process further in a 1929 paper. He showed that even 'random' series will have either 'big clusters of deviations above or below the average, or single exceptional deviations', implying that cumulations or moving averages of such series will produce patterns resembling business cycles. 'Random causes' sampled from distributions which are non-uniform and skewed tend to produce the most clear-cut cyclical behaviour; and centrally-weighted moving averages the smoothest curves. Unlike Slutsky, however, Kuznets asked whether a 'reverse inference' could be drawn from these experiments:

> It has been shown that the summation of random causes yields cycles... [By inverting the proposition] can one...say that, therefore, cyclical oscillations may

[9]However, Slutsky 'edited' his turning points by discarding the high frequency fluctuations (which he called 'ripples') in his experimental series.

[10][1990] p. 82.

be conceived primarily as results of summation of random causes? ([1929], pp. 273-4)

He was cautious about such an inference, preferring to frame it as a 'significant but not exclusive' hypothesis, which if supported, would imply that the need for a single, recurrent 'cause' of business cycles 'becomes supererogation'. If the responses to economic stimuli are in some way cumulative, then the Institutionalist interpretation of business cycles, favoured by Mitchell, could be justified. Because the shocks persist in their effects owing to the habituated behaviour of agents, the cumulation hypothesis would predict an inevitable 'cyclical' swing in the opposite direction sooner or later, given random impulses. Kuznets also observed that the smoothest such oscillations occur in index series, where repeated averaging of the components becomes the source of the smooth behaviour. On the other hand, volatile disaggregated series — New York Stock Exchange trading volume for example — do not have cumulative and persistent effects owing to their homogeneity and short sampling intervals.

The experimental methods of the 1920s were, as Morgan has characterized them, 'theory-free'. They were not designed to support or refute any business-cycle hypothesis, but rather were designed to show that a comparatively simple underlying process could produce the same behaviour as the cyclical indicators of the day. In the case of Yule's serially correlated first differences, such summations could also produce long sequences of increasing or decreasing levels in the series, so that for those naïve enough to confuse artificial DGPs with real processes, it seemed that 'business cycles' could be explained in these elementary terms. Such explanations, of course, only held if the existence of the phenomenon was already accepted. Business barometers similarly relied on prior assumptions, and hence on heavily processed data whose turning point properties probably differed substantially from the original series. Furthermore, their function was not empirically to verify the existence of a cycle in general business conditions, which was 'known' to exist, but rather to construct indices which moved in concert with the 'known' fluctuations. Such constructions therefore amounted to little more than simulations with transformed empirical data. Although both lines of research were thus circular, their 'results' were unfortunately highly suggestive. Their plausibility reinforced the existing opinion that fluctuations existed at business-cycle frequencies through 'separate causal influences' (that is, separable from long-run effects and noise); and this opinion was further reinforced by the crisis of 1929-1932, even though the barometers failed to predict it.[11]

4.3. MACRO-DYNAMIC MODELS OF BUSINESS CYCLES

The term 'macro-dynamic' was used by Frisch in a 1933 paper to describe simultaneous-equation models of national economies in which the responses of variables to various stimuli are both contemporaneous and lagged, thereby creating endogenous cyclical behaviour.[12] Frisch believed, as did Yule and Slutsky, that the irregular appearance of 'cycles' in time-series data suggested that they were driven by a combination of damped deterministic cycles and random innovations with persistent effects. Unlike them, he had attempted, without success, a more elaborate decomposition of economic series into waves of various frequencies, all of which were labelled as 'trends'.[13] His scheme for modelling fluctuations was second-order and stochastic: $y(t) = P(\tau).y_0 + Q(\tau)\dot{y}_0 + \sum_{k=1}^{n} Q(\tau_k)\, e_k$, where τ is a function of the distance of t from the origin of the series. Frisch claimed that this was a synthesis of Yule's and Slutsky's formulations, the latter having not fully appreciated the 'accumulation of erratic

[11]Schumpeter [1939] has suggested that Persons's curves did point to a crash, but that he did not have the courage of his convictions.

[12]Ragnar Frisch, founding editor of *Econometrica* and for a time research director at the Cowles Commission. The article appeared in the Cassel memorial volume of essays.

[13]Frisch's terminology was non-standard. See Morgan [1990] Section 3.3. A similar decomposition was also proposed by Schumpeter.

influences'.[14] In any case, once the initial impulse had damped out, the linear operator $Q(\tau)$ gave the further values of the series, with weights determined by the lag coefficients.

Frisch strongly influenced Jan Tinbergen, the great pioneer of large-scale macroeconomic models. Tinbergen built and estimated simultaneous equation business-cycle models of the Dutch, American and British economies between 1936 and 1951, each of them on the assumption that oscillations are damped harmonic and endogenously generated, but subject to exogenous shocks. Within this general dynamic framework the models were designed to test the business-cycle hypotheses then current (as discussed, for example, by Haberler). Each model was a just-identified system with both linear and nonlinear relations. The data for the Dutch and US studies were transformed by a non-standard form of indexing. For example, the Dutch price indices for the years 1923-1935 were expressed relative to an average of the years 1923-1933; and indices of physical volume were expressed in units whose value 'averaged 1923/33 cost Hfl 1,754=100'.[15] All variables in the US model were expressed as deviations from the 'mean' of the whole sample period, 1919-1932, while UK data for the period 1870-1914 were expressed as deviations from nine-year moving averages.

Apart from tests of theory Tinbergen wanted to use the dynamics of his models as a basis for evaluating stabilization measures in the context of the depressed conditions of the 1930s. This required the isolation of the pure damped harmonic oscillations from the stochastic component to see how stable the economy would be under the impulses of contra-cyclical intervention. The idea was to find an 'autonomous' path, as Frisch had expressed it, whose dynamics would not be affected by structural changes, and which would describe the 'general characteristics of the business cycle'.[16] These 'indirect relations' were deduced by a process of simplification, elimination and substitution from

[14]Frisch [1933] p. 199. It is likely that this is a misinterpretation of Slutsky.

[15][1959] p. 46, Table I.

[16][1939], Volume II, p. 129. The analysis is described in the Introduction and in Chapter VI of that volume.

the reduced form of the model, yielding a single, k-th order linear difference relation: the 'final equation'. Tinbergen interpreted the systematic oscillations as perturbations about an equilibrium path, but according to Morgan admitted that it was difficult to find the economic meaning of such a fine distillation.[17] The Dutch final equation was second order with complex eigenvalues: $Z_t = 0.15\,Z_{t-1} + 0.26\,Z_{t-2} - 34.7$, where Z represented non-labour income. The US final equation was fourth order: $Z_t^C = 0.398\,Z_{t-1}^C - 0.22\,Z_{t-2}^C + 0.013\,Z_{t-3}^C + Z_{t-4}$, with Z^C the path of corporate profits. Goldberger [1959] has shown that the characteristic roots of this equation are a real pair, 0.386 and -0.240, and a complex conjugate pair, $0.126 \pm 0.511i$. The first root contributes a monotonic component to the motion of the system $(0.386)^t$, the second a 'sawtooth' component $(-0.240)^t$, and the complex pair an harmonic oscillation with a period of five years and a damping factor $(0.521)^t$. The UK model had second-order equations for the several cases considered.

Tinbergen justified these elaborate procedures by arguing that the final equation 'contains a theory of the cycle, i.e. of the cumulative process as well as of the turning point'. Without harmonic oscillation, the coefficients produce monotonic behaviour, and a 'separate theory of the turning point' would be necessary; but since the equations are all second order, the 'literary' theorists (whose number included Keynes) could not distinguish the two modes of behaviour by purely 'verbal treatment'. In the example, with an equation in Z_t, cyclical behaviour gives $Z_2 < Z_3 > Z_4$, etc. He then asks, 'with Mr. Keynes: how does the reversal come in?' It does not arise from exogenous factors, or reflecting barriers, or changes in structure, but rather from a change in the relative strengths of the positive and negative forces.[18] Tinbergen considered the weakness of non-mathematical theory to be that it cannot represent purely endogenous fluctuations,

[17][1990] p. 106. The approach became known as 'night train analysis'.

[18][1940] pp. 84-5.

but must appeal to outside forces. (Nevertheless, because his own equations were damped they required exogenous forcing functions to propagate the fluctuations.)

Tinbergen later modified his position on single-equation dynamics, as shown by his treatment of the UK model after the war. In the Universities/NBER 1951 *Conference on the Business Cycle* volume he argued for the need to reformulate theories to take account of the complexities of 'cyclical' behaviour.[19] Econometric models were by then becoming much larger, and for that reason 'unintelligible'. He proposed a system of sectoral models with an 'inner-circle' of aggregate variables and an 'outer-circle' of 'supplementary relations meant to specify and analyse the inner-circle relations'.[20] This seems to indicate that different sectors have different dynamics, making the interpretation of a general path difficult; but also that in some way the aggregate relations can synthesize the cyclical behaviour for the whole economy. Johnston [1955], commenting on this passage, hoped that the inner circle of relations would not 'impose just as severe restrictions on the variety of behaviour...as [did] the single aggregative model'.[21]

Tinbergen's work presents other difficulties to those trying to assess empirical evidence for business cycles as a distinct class of phenomena. For a start, the sample periods on which the Dutch and US models were estimated were so short that little confidence could be placed in the parameter estimates. This was pointed out by Orcutt [1948], who found from examining autocorrelations that all of Tinbergen's endogenous variables can be well represented by the same DGP: $Y_t = 1.3\,Y_{t-1} + 0.3\,Y_{t-2}$, an AR(2) with a unit root.[22] Even so, an examination of these series shows a dispersion of turning points even at the 'peak' of the 1920s boom, with less than half having local maxima in

[19] The volume containing the exchange between Burns and Marschak, discussed in Chapter 3 above.

[20] [1951] p. 140.

[21] [1955] p. 227.

[22] Kendall [1953] described this as a 'remarkable result'. Wallis [1977] has shown how it can be obtained analytically from a decomposition of the coefficient matrix of predetermined variables.

1929 (33 out of 69).[23] The same representation does not necessarily give the same turning points because of differences in innovations across the variables. In the case of the pre-1914 data used in estimating the UK model, moving average filtering can produce spurious peaks and phase shifts.[24] Furthermore, Hoffmann's indices, on which the UK model relies, have subsequently been shown to be unreliable.[25]

Kendall [1946] followed Yule in deriving periodic measures in models of damped harmonic oscillation as functions of the autocorrelations ρ_1 and ρ_2, and hence of the lag coefficients. These were, specifically, mean distances between peaks and 'upcrosses', and the autoregressive period. ('Upcrosses' refers to the point as which the series crosses its mean in an increasing sequence.) A fourth measure, the mean length of upward monotonic sequences ('upruns'), was not found to have an analytical formulation. He also stochastically simulated four (stationary) AR(2) models and found empirical values for each of the above. In the case of m.d.(peaks), although the observed values were very close to the theoretical ones, he considered the use of this measure potentially misleading for two reasons. First, there was an unfortunate tendency for investigators to 'edit' the series, discarding peaks which appeared in inconvenient places on the basis of 'judgment' rather than on quantitative criteria. The leading example of this, as already mentioned in Chapter 3 above, was Beveridge's turning-point analysis of his industrial production series.[26]

> [Sir William] deduces from his main series a period of about 8 years by counting the occurrence of what he regards as the principal peaks; but to reach this result he has to ignore a number of minor ones. Had he included them all he would have found a 'period' of 4 years... Now is it legitimate to exercise an individual judgment in the rejection of minor peaks[?]... Sir William has rejected exactly

[23]The data set is given in Tinbergen [1939] Appendix C, p. 205.

[24]See the discussion of this problem in Section 4.5 below, especially Osborn [1993].

[25]See Chapter 5 below.

[26]From *Full Employment* [1944].

half the peaks (and hence doubled the mean-distance between peaks). The line of division is far from clear for Sir William has accepted one peak in the range 100 but rejected seven with greater values. ([1946] p. 57)

The difficulties associated with subjective selection become

> intensified if we allow ourselves to be influenced by other factors such as the existence of neighbouring peaks which are close together as corresponding to a single oscillatory maximum and hence to reject one of them. There is no justification for such a course, so far as I can see; and if it is pursued to any extent there will result too few short intervals and an excessively long m.d.(peaks), which is just the kind of thing we do observe in many inquiries. ([1946] p. 58)

Secondly, Kendall thought the m.d.(peaks) measure to be 'extraordinarily insensitive' to variations in the AR coefficients. Thus, for a sample of first-lag coefficients ranging from -0.8 to -1.2, and second lag coefficients from 0.4 to 0.8, m.d.(peaks) were found to be clustered in the range 4.96—5.13 time periods. For a 'wide range of values we shall find a m.d.(peaks) of between 4 and 6 units'. The measure could thus be misleading, as when applied, for example, to a study by Davis of the distribution of 'cycles' in 17 countries, where the m.d.(peaks) is 5.2 years.[27]

> In my mind, this does not by any means imply that there is any kind of rhythmic influence at work generating business oscillations with a mean period of about five years. It would be quite consistent...to suppose that [these economic structures were] capable of representation by [widely differing] autoregressive models... ([1946] p.57)

Other such studies were undertaken in the 1950s. The Klein-Goldberger [1955] model of the US economy was estimated from annual data for the period 1929-1952. Its dynamic properties were analysed in Goldberger [1959], with distinct final equations derived for the real and monetary sectors. The characteristic equation of the latter sector

[27]H.T. Davis, *The Analysis of Economic Time Series* [1941]. This work is discussed further by Morgan.

is given by $\lambda^2 - 1.393\,\lambda + 0.3942 = 0$ with two real roots, whereas the real sector follows the process $\lambda^4 - 2.0886\,\lambda^3 + 0.5774\,\lambda^2 - 0.4696\,\lambda + 0.0567 = 0$ with two real roots and a pair of complex roots. The dynamics are thus mixed monotonic and damped cyclical. The Klein, Ball, Hazlewood and Vandome [1961] study of the UK economy, 1946–1956, produced a quarterly model with a final price equation in seventh order differences, of which the characteristic equation is $\lambda^7 - 0.097\,(\lambda^6 + \lambda^5 + \lambda^4) + 0.097\,(\lambda^2 + \lambda + 1) = 0$. Reflecting strong postwar growth in the British economy, the KBHV final equation has a both a positive and a negative unit root, as well as a real root, and two pairs of complex roots, one on and one inside the unit circle, giving a time path of the trend–cycle type with a period of one year. However, because this equation was derived specifically to study the dynamics of inflation it was not intended to be a unique final equation.

The K–G model had 'decomposable' dynamics, which implies the divergences in cyclical behaviour noted by Johnston [1955]. In 1959, Irma and Frank Adelman simulated this model, the object being to determine whether, and under what conditions, it could produce business-cycle-like behaviour. The Adelmans first ran a computer simulation of the systematic part of the model and found that the extrapolations were virtually monotonic. They next imposed occasional random errors on the values of the exogenous variables ('Type I' shocks) and these, although the paths departed from the linear, did not give fluctuations of sufficient magnitude. Finally, error terms were added to each equation ('Type II' shocks) and the model was stochastically simulated. The behaviour was found to be cyclical with 'reasonably realistic' properties. A 'reference cycle' was derived by finding the 'years' in which peaks and troughs were 'bunched': specifically those years with a modal number of specific-cycle turns, provided that at least ten like turns were found in a two-year period including the modal year.[28] The average reference-cycle length was found to be about four years, conforming to NBER experience for the US economy in the period. However, an examination of these results shows that

[28][1959] p. 612, fn.

only about half the specific-cycle turns occurred in the year of the reference peak or trough, with about 30 percent spread equally one year on either side, and the final 20 percent further dispersed.

The Adelmans's results were analysed by Goldberger [1959]. He thought that the behaviour of the model might be a Slutskian 'summation of random causes', this being, as in Tinbergen's models, effectively the consequence of the MA-equivalent form. Goldberger also concluded that because of the dichotomy between the real and monetary sectors in the K-G model, Kendall's 'remarkable result...must be qualified'. Thus, where the relevant matrix is 'decomposable, so that the variables are not really *bound* together [emphasis in original], then the dynamic properties need not be common to all the endogenous variables'.[29] The implication of all research on large-scale models is that, both for artificial and empirical data, a dynamic path common to all variables is exceptional, and that such behaviour can only arise in the presence of very strong restrictions. Johnston [1955], for example, found divergences in the average duration of cycles in NBER data across five sectors of the US economy, which led him to argue that sectoral models which would capture the 'varying characteristics' of each sector are more appropriate.[30]

4.4. MODELS OF ASYMMETRY

An important feature noted by all business-cycle researchers is that amplitudes and durations of expansions appear asymmetrical with those of contractions in time-series data. Such behaviour can occur quite naturally in a model with a strong secular tendency. Baumol [1955], among others, demonstrated that a damped harmonic process with a positively sloped deterministic time-trend component has downswings shorter than

[29]Goldberger [1959], p. 134. See also Wallis [1977], p. 1482.

[30][1955] pp. 222-227.

upswings. Over the years, however, there has been interest in isolating asymmetry characteristics from other behaviour in empirical data with unknown DGPs, and this has involved either stationary nonlinear models, or, where appropriate, the filtering of data to achieve stationarity, or both. An early example was the Radice [1936] model of the UK trade cycle which proposed an explanation of asymmetry as differential lags in investment between upswings and downswings — an hypothesis not supported by the data.

In recent years Markov models with explicit regime switching have been fitted to US time-series data. In Neftçi [1984] it is argued that correlation properties are effectively asymmetric across various phases of the 'cycle' and that this might be incorporated into the probability structure as a function of regime switching. He examines postwar US employment data and proposes a test of the asymmetry hypothesis employing the theory of finite-state Markov processes. Thus, let $\{X_t(\omega), t=0,1,2,\ldots; \omega \in \Omega\}$ be a stochastic process, and suppose $\{X_t\}$ is pro-cyclical, linear and stationary with zero mean. Define $\{I_t\}$ to be $I_t=+1$, if $\Delta X_t>0$, and $I_t=-1$, if $\Delta X_t \leq 0$. The process $\{I_t\}$ will generally be positive during upswings and negative during downswings in the data. If the series appears to show sharp drops during contractions and more gradual rises during expansions, then $\{I_t\}$ would be expected to remain in the state +1 longer than in the state -1, implying that the transition probability for moves from +1 to +1 is greater than that from -1 to -1. The test is implemented empirically with $\{I_t\}$ restricted to being stationary and second order Markov. The parameters of interest are the transition probabilities associated with consecutive declines and consecutive increases, denoted λ_{00} and λ_{11} respectively. The technique has been applied to three US employment series in levels: total unemployment, unemployment for insured workers and unemployment over 15 weeks' duration. Neftçi's reasons for using these series were, first, that he thought they were a good proxy for 'the business cycle' and second, that they were 'trend free', making filtering unnecessary. The null hypothesis was $\lambda_{00} = \lambda_{11}$, and some evidence against this was found for all three series. Sichel [1989] found, however, that Neftçi's confidence ellipsoids were wrongly calculated, reversing the latter's findings on asymmetry; but by restricting the model to be first-order Markov he confirmed Neftçi's results.

Hamilton [1989] applied an extension of this model to US GNP growth rates (i.e., first differences of logs). It is assumed here that the general 'state' of the system is unobserved at any time t, and must thus be extracted as a signal from the observations, and that the series follows a stationary, nonlinear, autoregressive process. The nonlinearities in this case

> arise if the process is subject to discrete shifts in regime — episodes across which the dynamic behaviour of the series is markedly different. ([1989] p. 358)

The author argued that the advantage of his method is that it explicitly models nonlinearity in contrast to the current approaches — TS models and cointegration — both of which assume that growth rates for US GNP are stationary and linear. The 'Hamilton Filter' assumes that the process y_t has the AR representation

$$y_t - \mu(S_t) = \phi_1 (y_{t-1} - \mu(S_{t-1})) + \dots + \phi_m (y_{t-m} - \mu(S_{t-m})) + \sigma(S_t)\, u_t \,,$$

which implies that the first and second moments (μ and σ^2) can vary with the operative regime, denoted as S_t. The regime is modeled as an unobserved, two state, first order Markov process with S_t having the value either 0 or 1. Thus S_t depends only upon S_{t-1}; and μ and σ can be parameterized as $\mu(S_t) = \alpha_0 + \alpha_1 S_1$, $\sigma(S_t) = w_0 + w_1 S_t$. The filter is a five-step maximum likelihood algorithm for estimating the probability that the system is in state 0 or 1 at time t.

Hamilton originally thought that his technique could formalize the interpretation of turning-points as events signifying structural changes in the economy. 'Long-term trends' might thus have been identified as differing regimes of fast or slow growth. The results, however, coincide quite closely with the NBER reference dates (see Table 5.3.2, Chapter 5 below). Hamilton also argued that the Markov representation 'encompasses' the ARIMA model in the sense of Hendry and Richard, but with growth rates and the heteroscedasticity of residuals at odds with those of the linear models. The Markov model thus provides an alternative perspective on the effect of shocks on the business cycle. Linear models, for example, predict that a shock of about one percent to GNP will have a permanent effect between 0.8 and 1.7 percent; whereas the Markov formulation predicts an asymmetric effect of +0.66 percent during an expansion but of -3.0 percent during a contraction, an example of a 'deepness' asymmetry.

Other recent work involves the explicit modelling of these asymmetries. Unlike Hamilton, Falk [1986] found little evidence of asymmetry within the Markov framework when applied to US series other than employment. Luukkonen and Teräsvirta [1991] carried out LM tests of linear versus ARCH representations for three nonlinear AR models: the self-exciting threshold autoregressive (SETAR), the smooth transition autoregressive (STAR) and the bilinear (BL); but found that the tests did not give results independent of the model specifications. For the US and Japan, unemployment series appear to be symmetric. The two countries with seeming nonlinear behaviour of the STAR type are France and West Germany. McQueen and Thorley [1993] tested US unemployment and industrial production series for 'steepness' (falls steeper than rises) and 'deepness' (troughs deeper than peaks) asymmetries and found the change in growth rates around NBER reference troughs to be significantly greater than around peaks. Changes from contraction to expansion are typically abrupt, whereas changes at peaks are more gradual. A three-state Markov chain is also applied, and it is shown that the transition probability of jumping from a contraction into an expansion is significantly greater than from an expansion to a contraction. The series are broken down into their constituent parts; and these show divergent behaviour, both from the aggregates and from each other. Additional tests suggest that asymmetries have been more pronounced in the postwar period. Sichel [1993] computes skewness coefficients for distributions of the cyclical 'components' of US time series. Evidence of deepness is strong in employment and industrial production data, but weaker for GNP. Steepness is found only for employment. Sichel suggests that the presence of these features in time series data implies that linear structural models with symmetric disturbances 'cannot represent the observed stylized facts for these variables', and calls for further investigation of theoretical nonlinear DGPs.

None of these results is particularly helpful in evaluating the basic business-cycle hypothesis of comovements among many economic variables. When series from several sectors are subject to the same tests, as in Sichel [1993], the results are mixed; nor is it obvious how the number of states in a switching model can be identified except on *a priori* grounds. Otherwise, what emerges is a case for further investigation of nonlinear representations of growth rates in specific macroeconomic variables.

4.5. RECENT LINEAR REPRESENTATIONS OF SHORT-RUN FLUCTUATIONS

As the postwar period unfolded and secular tendencies became the dominant macroeconomic feature, economists and econometricians became concerned more with the long-run empirical properties of the data than with the search for short-run statistical regularities. The question whether the data contain enough information to permit the decomposition of the two remains problematical. Increasingly, time series models of the unobserved ARIMA(p,d,q) form are being employed which allow both for structural, time-varying trends and for time-varying parameters. The main linear formulation is given by $\phi(L)\Delta(L)y_t = \theta(L)\epsilon_t$, where $\phi(L)$, $\Delta(L)$ and $\theta(L)$ are the AR, difference and MA lag operator polynomials respectively. Two subclasses are the 'trend stationary' (TS) and 'difference stationary' (DS) (see next page). An extension of this is the state space form, which is applicable in the case of unobserved components and time-varying parameters. If, for example, $y_t = \pi_t + \epsilon_t$, where only y_t is observed, or if $y_t = x_t\beta_t + \eta_t$, then these models can be rewritten in a two-equation form which permits ML estimation via the Kalman Filter: $y_t = X_t\beta_t + \epsilon_t$, the 'measurement' equation, and $\beta_t = T_t\beta_{t-1} + R_t\eta_t$, the 'transition' equation. The ϵ_t are error terms and the η_t are disturbances, both with zero mean.

4.5.1. Modelling the Nature and Persistence of Shocks

With the renewed instabilities of the 1970s, attention was again drawn to the questions whether and how cumulative shocks propagate fluctuations. Blanchard and Fisher [1989] and others have suggested a decomposition into permanent and transitory shocks, the relative importance of which has become central to the debate between RBC and neo-Keynesian theory. The two most common models are thus ARIMA specifications with exogenous trends: the (TS) class with deterministic trend, and the unit root or (DS) class. The TS model has the form $y_t = \alpha + \beta t + c_t$, $\phi(L) c_t = \theta(L) u_t$, $u_t \sim i.i.d(0,\sigma^2)$; where c_t represents deviations from trend, α and β are fixed parameters and $\phi(L)$ and $\theta(L)$ are lag operator polynomials satisfying stationarity and invertibility conditions. The DS process is given by $\Delta y_t = \beta + d_t$, $\delta(L) d_t = \lambda(L) u_t$, $u_t \sim i.i.d(0,\sigma^2)$. If the changes d_t are serially uncorrelated, then the DS reduces to a random walk with drift. Both models treat low frequency components as deterministic (the time trend $\alpha + \beta t$ in the TS case, and the drift term β in the DS case).

Like the Orcutt [1948] findings, most research has found that postwar macroeconomic time-series data have an 'integrated' representation, i.e., stationary in differences. It has been shown *inter alia* in Nelson and Plosser [1982] and Harvey [1985] that up to 1945, macroeconomic aggregates generally had stationary representations, but that subsequently the unit root hypothesis has been impossible to reject for most such series. Blanchard and Fisher (p. 10) argue that the dynamic response of the TS model to shocks indicates that the induced short-term fluctuations are transitory, whereas shocks to the DS model induce permanent effects. In Campbell and Mankiw [1987] it is shown that the quarterly US GNP series is well represented by the ARIMA(1,1,2) form, which implies that the rate of growth of the secular component is ARMA(1,2). The authors also examine the decomposition issue and find that both TS and DS representations give approximately the same fit. In Blanchard and Quah [1987] prior restrictions are placed on responses to the two types of disturbance, which gives the interpretation of permanent shocks affecting the supply side, and transitory shocks relating to the demand side. These authors find also that when supply innovations are set to zero, output demand fluctuations closely follow NBER reference turns for the period 1950–1987. In Perron [1989], applying piecewise linear regression to the Nelson and Plosser data set, the null hypothesis of a DS model with possible non-zero drift term is tested against the alternative TS model. Both hypotheses allow for a one time structural break in the level or slope of the trend function. It is shown that the standard tests tend not to reject the unit root hypothesis if the true DGP is that of stationary fluctuations around a trend with a one time break. For the quarterly US GNP series, the break occurred as a consequence of the 1929 crash and brought a reduction in the level of the series, whereas for postwar real GNP, a break during the 1973 oil shock led to a change in slope. The author concludes that if the process is not modelled as time-invariant, then these two breaks can be exogenous and are the only shocks that persist. The TS representation is thus accepted. On the other hand, in Sims [1989] it is argued that persistence can be modelled by

> ARMA processes with unit roots, ARMA processes with explosive roots, processes with deterministic polynomial or exponential trends, fractionally integrated processes and covariance stationary ARMA processes with high power at low frequencies, among other possibilities. ([1989] pp. 3–4)

Sims shows that models with exponentially explosive deterministic components can be approximated entirely within the class of stationary AR processes, but that because they lack predictive power they have been abandoned when data show trend-like behaviour. He concludes that the models should reflect the long run uncertainty of these processes rather than attempting to hide it.

The King *et al.* 1987 paper extends the persistence-of-shocks analysis in a multivariate framework. Using a neoclassical model with permanent technology shocks, they test empirically the effect of such disturbances. This is implemented, as in the 1991 paper discussed above, via a variant of the 'common trends' model, in which an n-dimensional vector of variables $\mathbf{X}_t$ is regressed on a k-dimensional vector of random walks with drift. The model may be decomposed into a stationary component, which represents transitory fluctuations, and a nonstationary component representing the trend. In this case the restriction $k<n$ is imposed, so that some of the elements of X_t move together. There is thus some implied cointegration, with $n-k$ cointegrating vectors, which gives the vector error correction representation $\Delta X_t = a + B(L)\Delta X_{t-1} - d(\alpha' X_t) + \epsilon_t$, where a is $n\times 1$, d is $n\times(n-k)$, B(L) is an $n\times n$ lag matrix, and α is an $n\times(n-k)$ matrix of cointegrating vectors. When applied to a model of consumption, investment and income, the unit root hypothesis is not rejected. The overall results suggest that transitory shocks account for two-thirds of postwar variation in US GNP.

Blanchard and Watson [1986] and Blanchard [1989] investigate comovements in five macroeconomic variables — output, unemployment, prices, wages and nominal money — through the medium of a VAR with both contemporaneous and lagged relationships. By imposing a set of just-identifying restrictions to determine a set of five uncorrelated structural innovations from the reduced form disturbances, a 'Keynesian' model is obtained which includes aggregate demand, aggregate supply, price setting, money supply and fiscal relations. In the 1989 paper, the author finds that demand shocks account for most of the short run fluctuations, with positive shocks giving increases in nominal prices and wages. Supply shocks, on the other hand, dominate in the long run and lead to decreases in wages and prices. Additionally, correlations of GNP with the other variables at various leads and lags are measured for seven postwar cycles, the result showing considerable variation across episodes.

Apart from identification problems there is also the question of bias in the estimates of low frequency components when filters are applied. For example, Chan, Haya and Ord [1977] concluded that fitting a linear time trend leads to '...the low frequency portion of the spectrum [being] exaggerated and the high frequency portion attenuated' as compared to a first differencing transformation. Extending this line of enquiry, Nelson and Kang [1981] found evidence that such 'pseudo-periodicity' is a function of sample size. Recently, Osborn [1993] has argued that spectral bias extends to DS processes when filtered by moving-average methods. Two famous examples of low frequency bias are 'evidence' of fifty-year economic cycles — the 'Kondratieff wave' — and the 'long swing' hypothesis investigated by Kuznets [1961].

4.5.2. Index Models

The common factor models implemented by King *et al.* are special cases of the unobserved-components type defined above. Such models in general fall into observable and unobservable sub-classes. The first is discussed in Sargent and Sims [1977]; Goldberger [1972] surveys applications of the second in the social sciences, giving the example of the Permanent Income Hypothesis, where the unobservable 'permanent income' is modelled in terms of two observable indicators, total income and consumption. The latter model is of interest owing to the recent work of Stock and Watson [1988, 1989, 1991, 1992], which attempts to formalize the NBER approach to business cycle tracking using a linear single index. Their formulation implicitly defines a variable that is thought of as the overall 'state of the economy', based on an interpretation very close to Marschak [1951], that a single unobserved process drives 'cyclical' fluctuations in many economic time series. The authors, however, claim their point of departure as the 1946 Burns and Mitchell 'definition' (quoting from MBC, p. 3). The S-W interpretation of the derived single index is as a monthly proxy for GNP which, it is claimed, provides a formal rationalization for the heuristic dating methods of the NBER. Estimates of the unobserved index, constructed as a weighted sum of variables that move contemporaneously with 'overall activity', provide the basis for indices of coincident and leading indicators (XCI and XLI), and for an index which attempts to forecast recessions (XRI).[31] Both are indicators of the NBER reference cycle, which is identified with 'the broad-based swings in economic activity known as the business cycle'.[32] However, the

[31]These are now published monthly in the US.

authors concede that such a concept is not in keeping with the spirit of Burns and Mitchell, and so modify their formulation to take account of comovements. They propose a single-index model with observable vector of variables X_t composed of two unobserved components: C_t, a scalar representing the 'state of the economy', and an n-dimensional component u_t which captures the 'idiosyncratic' movements of each variable. Because the structures are linear and stochastic, the nonlinearities arise 'extrinsically' from the time series properties of the indicator variables rather than from 'intrinsic macroeconomic shifts'.[33]

The main identifying assumption is that the comovements of the multiple time series arise from the single source C_t. The authors test several constituent series from the US Department of Commerce coincident indicator for the presence of cointegrating vectors, but find that the null hypothesis of no cointegration is not strongly rejected. This requires them, like Hamilton, to work in growth rates for each variable. The mean of the single index C_t is calculated as an endogenously weighted average of the growth rates of four constituent series: total industrial production, personal income, total manufacturing and trade sales and non-agricultural hours worked. Residual series are found to have widely different AR representations. The single index is then converted to a levels series and compared to the DOC coincident indicator. The authors report that their index has a contemporaneous correlation of 0.936 with the DOC series, and that coherence with growth rates in the latter is high at low frequencies. The plot of the two indicators is shown in Figure 4.5.1.

The recession index XRI, constructed as a VAR using the index ΔC_t, gives estimates of the probability of being in 'recession' k months hence, for various k. The authors define recession as a sequence of values of ΔC_t falling below a specified cutoff level $b_{r,t}$ either for six consecutive months or for at least seven months in a nine-month

[32][1989] p. 352.

[33][1989] p. 357. In this characterization the authors are following Slutsky rather than, for example, Hamilton or Neftçi.

period. 'Expansions' are defined symmetrically, with boundary $b_{e,t}$. Both phases are thus modelled as 'fuzzy' sets with a view to capturing the 'judgmental' approach of the NBER. Recession and expansion events are denoted R_t, E_t, respectively. The XRI is thus the c.d.f. $P_{\tau|t}$ of the economy being in recession in month τ, given information available at month t. Computation of $P_{\tau|t}$ is by Monte Carlo methods, and is the proportion of occurrences of R_t to total occurrences: i.e., the statistic $\# R_t / (\# R_t + \# E_t)$, where # is the number of occurrences of the event.

These efforts have met with limited success. The XLI and XRI failed to predict the sharp downturn in the fourth quarter of 1990. The authors consider that the key difficulty lies in the choice of leading indicator variables, but argue that any changes in these variables could only have been made with hindsight. Wallis [1991] and others, in their discussion of the 1991 paper, have argued that the model itself may be inappropriate. Sims [1989] and Zarnowitz and Braun [1989] have both questioned the dependence of the S-W leading index on financial rather than real variables. Wallis suggests that forecasts of recession could have been improved by including traditional indicator variables, and also expresses reservations about the linearity of the model. Sims, on the other hand, believes that time-varying parameters should have been tried.

4.5.3. Trend/Cycle Decompositions in Structural Models

Harvey [1985, 1989] and Harvey and Jaeger [1993] have proposed a 'structural' version of the unobserved components model as an alternative to the Box-Jenkins approach. As described in Harvey [1985], this is the 'trend plus cycle' model $y_t = \mu_t + \psi_t + \epsilon_t$, where $\mu_t = \mu_{t-1} + \beta_{t-1} + \eta_t$ and $\beta_t = \beta_{t-1} + \zeta_t$ (in state-space form). The trend is modelled as stochastic and linear, whereas the cycle is represented either by the explicit sinusoidal form

$$\begin{bmatrix} \psi_t \\ \psi_t^* \end{bmatrix} = \rho \begin{bmatrix} \cos\lambda & \sin\lambda \\ -\sin\lambda & \cos\lambda \end{bmatrix} + \begin{bmatrix} \omega_t \\ \omega_t^* \end{bmatrix}$$

or by damped harmonic oscillation: an AR(2) process with complex eigenvalues. When $\sigma_{\omega}^{2}=0$, however, the former is equivalent to the latter. As is demonstrated in Harvey [1985, 1989], all structural models have an ARIMA reduced form, but one on which 'restrictions [are] imposed by *a priori* considerations'. For example, in the above case, when the variance $\sigma_{\zeta}^{2}=0$, the reduced form is ARMA(2,1).[34] It is claimed that the unobserved components of a structural model have a 'direct interpretation' — trend, cyclical and seasonal. The traditional Box-Jenkins strategy of selecting 'a parsimonious model...on the basis of the data themselves' is not followed. The purpose in estimating structural models is twofold: the first is to 'make forecasts'; and 'the second is to provide a way of presenting the "stylized facts" associated with time-series data: i.e., the trend, cycle, seasonal and irregular components'. It is not 'intended to represent the underlying data generating process'. These components 'are of interest to economists in themselves' and are further claimed to follow the 'encompassing principle' of Hendry and Richard.[35] Harvey finds that the 'cyclical trend' model, in which the cycle is modelled as part of the stochastic trend rather than independently, fits US GNP better than the trend plus cycle version. For industrial production a 'random walk with drift...may be the best one can do'.[36]

Harvey and Jaeger [1993] have followed this philosophy in fitting structural models to US and Austrian GNP series. They criticize the Hodrick-Prescott approach because (as also argued by Kim and Pagan [1993]) it does not take account of the stochastic properties of the data and therefore induces 'spurious cycles' in the filtered components. However, they find that the US series, when the above trend-cycle model is fitted, 'yields a detrended series which is difficult to distinguish' from the output of the

[34] See [1985] p. 220.

[35] *Ibid.*, p. 225.

[36] *Ibid.*, p. 222.

H-P filter. The latter is thus 'tailor-made for extracting the business cycle component from US GNP'.[37] This similarity does not obtain for the Austrian series. The authors also examine certain time series for evidence of cross correlations in the cyclical component, but find that these are spurious.[38] The conclusion, as in Kim and Pagan, is that the H-P filter may bias estimates of the 'cyclical component' and of correlations in this component across time series. However, the structural model itself is subject to identifying restrictions: its reduced form is ARMA(2,1) with complex eigenvalues inside the unit circle.[39]

4.6. A MONTE CARLO STUDY OF MEASURES OF ASYMMETRY

Stock and Watson [1989] distinguish between models which generate fluctuations 'intrinsically', through harmonic oscillation, and those whose swings are generated 'extrinsically' by linear combinations of random influences. Empirical evidence favours the latter generating process — for example Orcutt's findings for Tinbergen's US series and the Adelmans's simulation of the K-G equations. What is certain is that all macroeconomic aggregates, as well as most of their constituents (with the exception of the interwar period in the United States) have exhibited strong secular tendencies since the beginnings of industrialization. As Baumol has shown, the superposition of a trend line on a known stationary DGP will noticeably alter its 'periodic' characteristics e.g., the distance between turning points, the symmetries of lengths of upswings with downswings, etc.

There has been little specific investigation of the effects of trend upon the latter measures in time series models. The periodic properties of stationary damped harmonic

[37] Harvey and Jaeger [1993] p. 236.

[38] *Ibid.*, p. 245.

[39] *Ibid.*, p. 232 and Harvey [1985] p. 219.

processes were studied analytically by Yule and later by Kendall [1944, 1946], Johnston [1955] and Howrey [1968]. The advantages of this model have been shown by the above three authors: oscillations are generated endogenously, and expected values of periodic measures can be derived analytically from the autocorrelation coefficients. Since Kendall found the derivation of a theoretical measure for m.l.(upruns) intractable, this section attempts to bridge this gap in the literature by determining experimentally the effects of trend on such AR(2) models, expressed as, say,

$$y_t = a\,T + b y_{t-1} + c y_{t-2} + u_t$$

(or, in homogeneous form $y_t - b\,y_{t-1} - c\,y_{t-2} = 0$) where u_t is a general error term, usually distributed as $N(0,1)$, and T is a time trend.

The basic experimental design is the Monte Carlo simulation of a group of AR(2) models with coefficients in the region bounded by the parabola $b^2=4c$. Coverage of this space is effected by a 'grid' of models, as shown in Figure 4.6.1. First lag coefficients b are separated in the grid by 0.1, and second lag coefficients c by 0.2. The simulations test whether the position of the model in the space (i.e., the values of the coefficients) affects the behaviour of m.l.(upruns), m.l.(downruns) and their ratio as a deterministic trend is added to the model. In order to compare the effect of trend across models, it is necessary to adjust the slope coefficient a. Figure 4.6.2 shows the effect of adding a trend with $a = 1.0$ to realizations of the four Kendall [1946] models. The effects are disproportionate, with each resultant slope being different. In order to permit direct comparisons across models, the slopes are adjusted by subtracting the 'mean' of the process, i.e., the trend. Suppose the original model is, say, $y_t = a\,t + \phi_1\,y_{t-1} + \phi_2\,y_{t-2}$. Subtracting the 'mean' $\mu=bt$ gives

$$\begin{aligned} &y_t - \mu_t = \phi_1 (y_{t-1} - \mu_{t-1}) + \phi_2 (y_{t-2} - \mu_{t-2}) \\ &\Rightarrow y_t = b\,t - \phi_1 (b(t-1)) - \phi_2 (b(t-2)) + \phi_1 y_{t-1} + \phi_2 y_{t-2} \,. \end{aligned}$$

Then, by comparing coefficients, $a = b(1-\phi_1-\phi_2)$, which gives a scaled 'trend-in-mean' $b = \frac{a}{1-\phi_1-\phi_2}$.[40] The effect of this can be seen in Figure 4.6.3 showing the same Kendall models with equal 'trends-in-mean' rather than 'trends-in-intercept'.

The simulation routines are implemented using a FORTRAN program. For this exercise, each of the models was iterated 550 times, with the first 50 observations discarded to allow for non-random effects. Each simulation included 500 cycles of the above, giving a run of 250,000 observations for each model. For each cycle, the program calculates each of the above measures. The algorithm for finding peaks is based in this case on a 'window length' of three observations as in Kendall [1946]: thus y_t is a peak *iff* $y_{t-1}<y_t>y_{t+1}$. 'Upruns' and 'downruns' are found by the program as monotonic sequences of increasing and decreasing values respectively. In addition, the mean ratio of uprun- to downrun-length was computed over the 500 cycles of the program for each model. Originally, the plan was to compute the above measures at window lengths of three to 13. However, it was found in practice that because of the asymmetries induced by the introduction of a trend, turning points tended to vanish at higher window lengths so quickly that the scope for cross-model comparisons became severely restricted. An example of the effects of a steep slope on turning points is given in Figure 4.6.4, where the series becomes monotonic. The m.l.(upruns)/m.l.(downruns) ratio is unity for all simulations of stationary processes and hence serves as a reference for assessing asymmetries. Because an increase in the trend-in-mean also produces rapidly vanishing turning points, the trend coefficient is set at unity and mean adjustments are made for each model in the study.

As the measures being studied vary considerably across the grid of models, changes associated with the imposition of the trend are calculated as percentages. The results are summarized as 'response surfaces' in Figures 4.6.5 — 4.6.7. These are in the same orientation as the coefficient-space diagrams above, but with the addition of a

[40]The author is indebted to Professor Ken Wallis for suggesting this metric.

vertical axis showing rates of change. An hypothesis of this study is that the asymmetries induced by trend vary as the autocorrelations ρ_1 and ρ_2 or with the output variance of the series. However, the graphs show clearly that for m.l.(upruns), m.l.(upruns)/m.l.(downruns) and their mean variances, the sensitivity of response is directly proportional to the values of the coefficients rather than to some intermediate function. The degree of change is also directly proportional to (a) the value of the mean-adjusted slope and (b) to the degree of positive autocorrelation in the stationary process. These measures vary in the same way: they increase in magnitude (shown as percentages on the vertical axes) as the values both of b and c increase, with the rate of increase in the standard deviations approximately double that of the means. The position of the boundary of cyclical behaviour depends upon the value of the slope coefficient a, moving to the left as a increases. As the boundary beyond which behaviour is monotonic is approached, the rate of increase is explosive, making the analysis of rates of change by a specific functional form difficult.

The experimental results suggest that, apart from changes in the sensitivity of these 'periodic' measures to changes in coefficients of the artificial DGP, the asymmetries between upward and downward sequences are functions of the strength of the trend. An obvious question is whether the results of such an experiment can be extended to empirical data. The ratio m.l.(upruns)/m.l.(downruns) can be calculated, but because of the comparatively short 'sample periods' in empirical series, inferences from the results of such an exercise must be made with some caution. For example, UK macroeconomic series are recorded quarterly from 1948 at the earliest, and US data from 1947, giving at most only 45 years of observations – less than 10 percent of the length of the Monte Carlo runs. Furthermore, macroeconomic series, as noted by Perron and others, are subject to structural breaks which would be equivalent to changes in both trend slope and AR coefficients in the experimental models. The real US GNP and UK GDP series, for example, exhibit frequent changes in rates of growth as well as in the frequencies of turning points and, during the 'long boom' of the 1950s and 1960s were essentially monotonic in levels. Such variations within a series are typical.

Nevertheless it is possible to extract some information from the application of the Kendall [1946] measures to such data, even though the possible structural heterogeneity renders the relationship between periodic measures and 'trend' slopes in empirical series less tractable than for artificially-generated data. However, the application of these measures permits at least tentative cross-series comparisons to be made, and may reveal correlations between asymmetric behaviour and growth. Additionally, differences in asymmetries across series can give some indication whether the 'divergences' in economic behaviour among the various sectors noted by Burns and Mitchell actually exist. Such analysis is mainly of historical value, since structural changes are largely unpredictable. Also, because of the small sample size it is not certain that differences in the empirical magnitudes of these measures will be statistically significant.

For exemplary purposes 15 quarterly macroeconomic series have been examined: eight series for the UK from 1955 to 1993; and seven for the US from 1947 to 1983. They are:

US GNP	UK GDP
US residential construction	UK engineering and related production
US producers durable equipment production	UK chemical and man-made fibre production
US nondurable goods production	UK textile and related production
US nonresidential construction	UK mining and quarrying
US durable goods production	UK food and drink processing
US government purchases	UK total manufacturing
	UK fuel processing

These have been chosen for the diversity of their behaviour rather than as representative of the macroeconomy as a whole, and are shown in Figures 4.6.8 and 4.6.9. US residential construction shows very little in the way of net growth during the period, but very large short-run fluctuations. UK textiles shows a similar lack of overall growth, but in this case there is a 'head and shoulders' pattern in the middle period, spanning the period of the two Labour governments, and possibly representing two different policy regimes. On the other hand, US nondurable goods production is virtually monotonic and shows strong growth. The two GNP series are less noisy in general than their constituents.[41]

Periodic measures are calculated for each series using a window length of five. (That is, a peak y_t is greater in value than the two previous and two subsequent observations. The reasons for this choice are discussed fully in the next chapter in the context of an analysis of historical macroeconomic data.) Table 4.6.1 shows the periodic measures for each series. The m.l.(uprun)/m.l.(downrun) ratio appears loosely correlated with average growth rates over the sample period, as shown in Table 4.6.2 and plotted in Figures 4.6.10 and 4.6.11. It is possible that, were more series of longer duration available, a linear regression line with positive slope — or possibly several lines — could be fitted to such data. This particular type of asymmetry seems to be present in empirical time series, its degree related to secular tendencies in the data just as in the experimental series, but with larger variances. It can also be argued that average growth rates are problematical measures for economic series because of their sensitivity to the choice of sample period and because of variations within series; but the present exercise is not intended as definitive, and no doubt more precise measures of empirical trend can be found. Nevertheless, the character of the time-series plots indicates considerable divergences across the sectors of both economies; and despite the high variances, the ratio m.l.(upruns)/m.l.(downruns) appears asymmetrical and to vary with secular strengths. Investigations of a larger number of series might reveal stronger regression relations.

[41]These historical variations are analysed in Chapter 5 below.

Table 4.6.1(a). Summary of Periodic Measures: Eight Quarterly UK Time Series					
Series	M.L.(upruns)	Standard Deviation M.L.(upruns)	M.L.(downruns)	Standard Deviations M.L.(downruns)	M.L.(upruns)/ M.L.(downruns)
GDP	10.0	9.6	2.2	1.9	4.5
Engineering industries	5.5	5.0	3.3	1.7	4.2
Chemicals and man-made fibres	16.0	16.4	3.8	1.7	1.7
Textiles, etc.	4.4	3.4	4.7	2.3	0.9
Mining and Quarrying	4.6	2.4	5.7	4.4	0.8
Total manufacturing	6.5	5.0	4.1	2.9	1.6
Food & drink	4.9	4.5	4.0	4.5	1.2
Fuel processing	5.0	3.6	5.7	3.6	0.9

Table 4.6.1(b). Summary of Periodic Measures: Seven Quarterly US Time Series					
Series	M.L.(upruns)	Standard Deviation M.L.(upruns)	M.L.(downruns)	Standard Deviation M.L.(downruns)	M.L.(upruns)/ M.L.(downruns)
GNP	16.0	9.0	2.7	1.3	5.9
Residential structures	6.3	3.5	5.3	2.9	1.2
Producers durable equipment	9.9	7.1	3.3	1.4	3.0
Nondurable goods	33.3	N/A	7.7	N/A	4.3
Government purchases	8.2	5.1	4.7	3.9	1.7
Nonresidential structures	8.2	5.1	4.7	3.9	1.7
Durable goods	5.4	3.9	5.2	4.4	1.0

Table 4.6.2. Asymmetries and Growth		
SERIES	AVERAGE ANNUAL GROWTH RATE (%)	M.L.(UPRUNS) / M.L.(DOWNRUNS)
US residential structures	3.1	1.2
US nondurable goods production	3.1	4.3
US GNP	3.3	5.9
US producers durable equipment production	3.9	3.0
US government purchases	3.7	2.2
US nonresidential structures	2.7	1.7
US durable goods	4.8	1.0
UK textiles	0.2	1.1
UK engineering	1.6	1.7
UK GDP	2.3	4.5
UK chemicals and man-made fibres	4.6	4.2
UK mining & quarrying	-0.9	0.8
UK total manufacturing	1.8	1.6
UK food & drink	1.8	2.3
UK fuel processing	3.5	0.9

4.7. SUMMARY

Empirical business cycle research originated in early twentieth century forecasting methods — the 'business barometers' — whose popularity was eclipsed by the 1929 stock market crash. The approach assumed that the forces driving short- and long-run behaviour were separable, which, because the latter was nonstationary, required filtering to 'remove' unwanted components. This in turn required the identifying assumptions of 'trend', 'cyclical', 'seasonal' and 'irregular' components. The approach has changed since the 1920s only to the extent that the techniques employed in the construction of the business barometers do not yield MMSE estimates of the components of interest and have therefore been rejected in favour of explicit probability models. However, all unobserved ARIMA models and their equivalent representations still rely on identifying assumptions, e.g., those of Stock and Watson, and Harvey. In the latter case the author does not even attempt to represent the underlying DGP, but assumes *a priori* that business cycles are separable from other components.

It can be argued that S-W and Hamilton are actually modelling long-run rather than short-run behaviour, since both work in growth-rate transformations of levels series. In Hamilton [1989] the objective of analysing growth patterns is explicit. As Sargent and Sims argue, the index model is a method of summarizing the covariance among the component variables; and when the latter are, as in the present case, growth-rate series, the model is actually picking up growth fluctuations rather than cycles in levels. The S-W plot of their exponentiated coincident index, $e^{\Delta C_t}$ against that of the DOC index also shows an emphasis on secular tendencies: both consist in a strong upward movement over time of very much greater magnitude than the 'business-cycle' type fluctuations (see Figure 4.5.1). This suggests that time-series models may reveal more about long-run tendencies than about short-run fluctuations.

Nor do linear models address the asymmetry issue. As has been demonstrated above, this property can be simulated for a linear process only by the superposition of a nonstationary component. The Harvey structural model normally represents cyclical components as AR(2) with complex roots whose residuals must be symmetrical, and which hence will require the trend component to induce upswings greater than downswings. Asymmeties in the S-W coincident index also appear to be a function of growth, that is, in the exponentiated form. Evidence from nonlinear models is mixed and does not address the comovement issue because it largely relates to the behaviour of aggregates.[42] Hamilton suggests that 'permanent' effects are the major contributor to short-run growth fluctuations, but in aggregate US GNP rather than in 'many variables'. Kim and Pagan argue that models need to take account of such effects in levels, that these can only be captured by a stochastic trend, and that estimates of a separate cyclical component will be distorted if such a trend is actually a feature of the data.

Empirical evidence has been mixed. Tinbergen and others found that the dynamics of large-scale models tended to be decomposable, suggesting that the sectors represented were only loosely correlated. Harvey [1985] finds that the 'cyclical trend' model is on balance favoured over the trend plus cycle variant for certain UK data, so that optimal representations would not separate trend from cycle. Stock and Watson failed to predict the 1990 downturn in the US economy, which implies that index models do not capture structural changes in behaviour — in this case of the financial markets. The strongest evidence of asymmetries from nonlinear models is from those in growth rates, *per* Hamilton [1989]. The difficulty in assessing such evidence has been the same since business barometers were first published: the 'cycle' is identified *a priori* so any inferences about its behaviour are circular.

Both experimental and empirical evidence indicates functional relations between asymmetries and the 'slopes' of 'secular components'. Of course, in empirical data the DGPs are not known, and secular tendencies are subject to structural changes, so confirmation of any such relations can only be tentative. The evidence, suggests, however,

[42]None of the papers discussed above analyse comovements in the degree of detail of some RBC studies as, for example, in Shapiro [1987].

that linear models cannot capture such behaviour independently of the secular tendency in the data, which casts further doubt on the traditional decomposition into 'trend' and 'cycle'.

APPENDIX 4

CHAPTER 4 GRAPHICS

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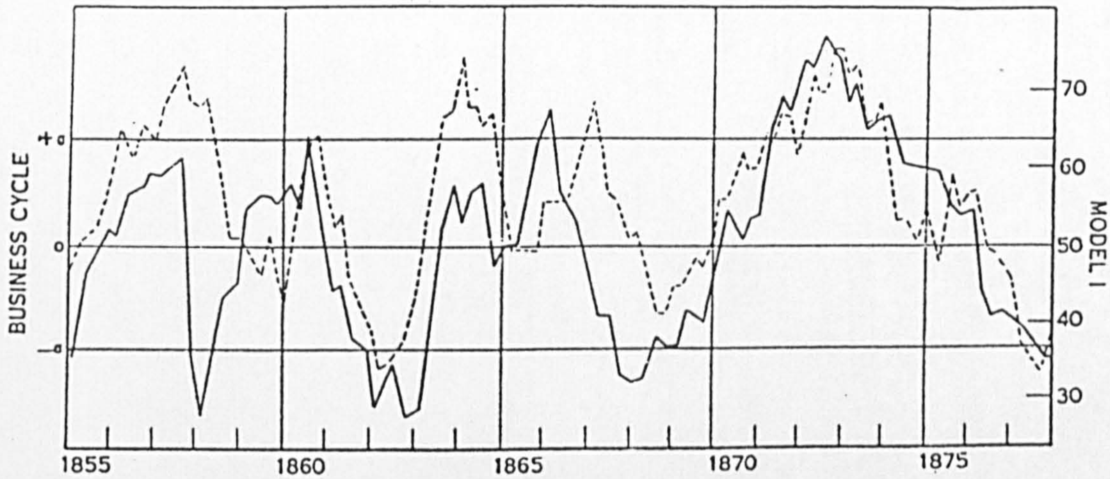
Figure 4.2.1. Dr. Thomas's UK Business Cycle Index (left-hand scale) and Terms 20–145 of Slutsky's Model I (right-hand scale)



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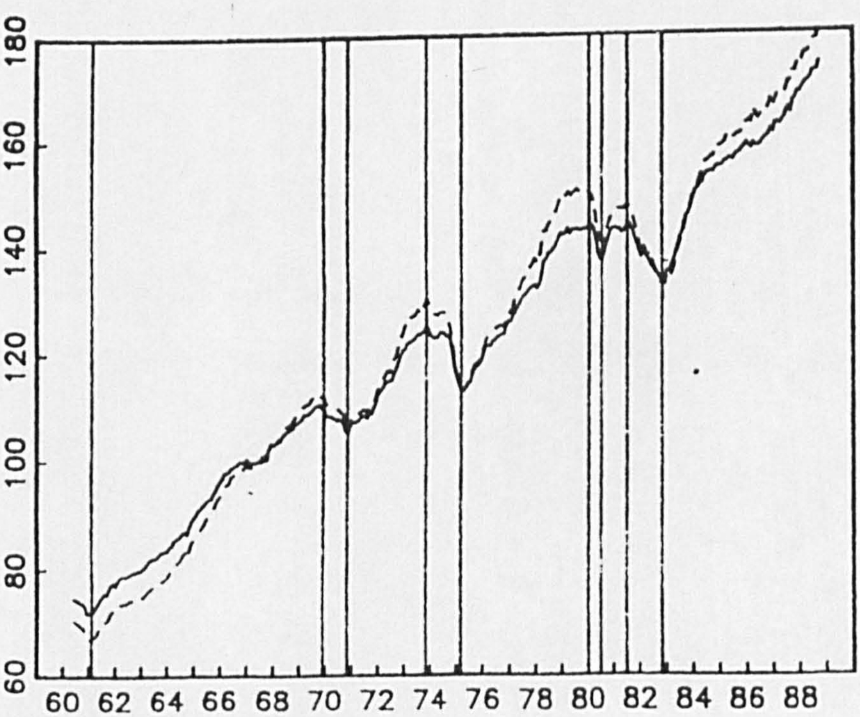

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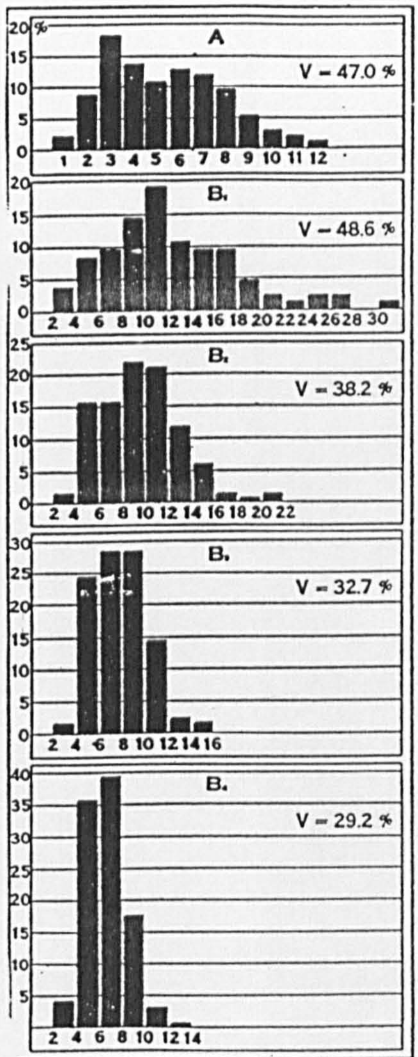
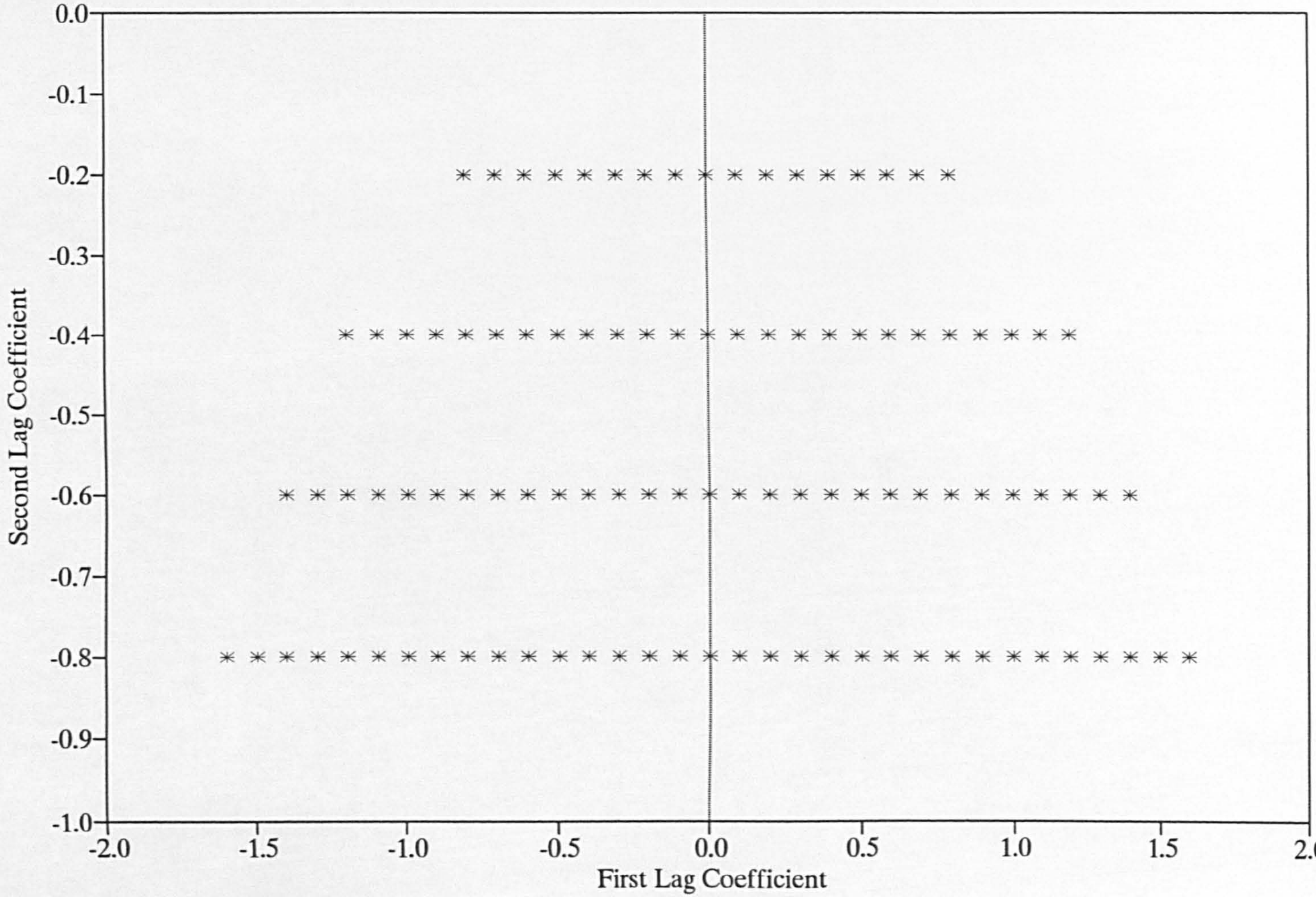


Figure 4.6.1. Grid Models: Position in Coefficient Space

Model: y(t)=by(t-1)+cy(t-2)



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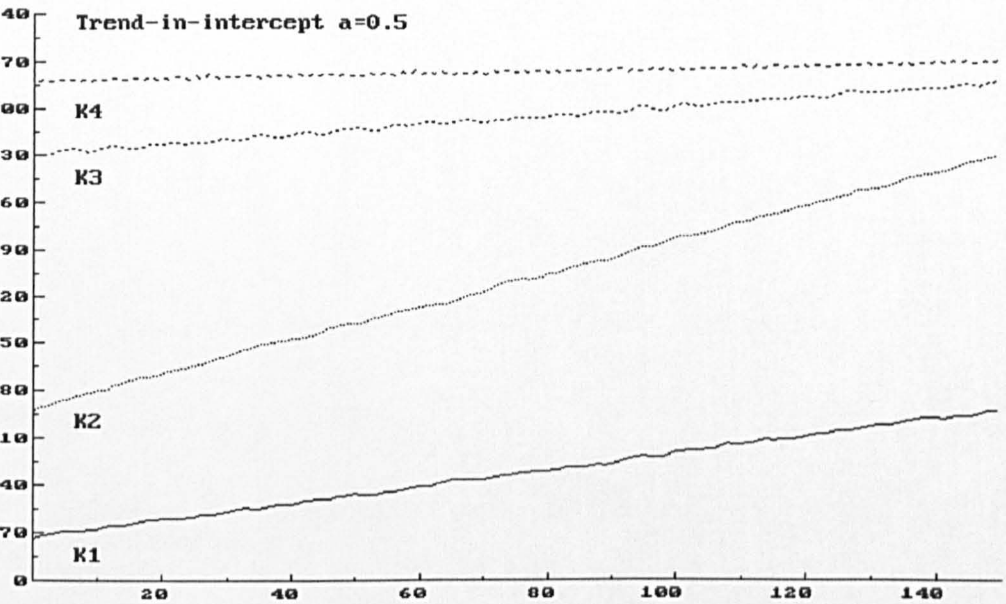

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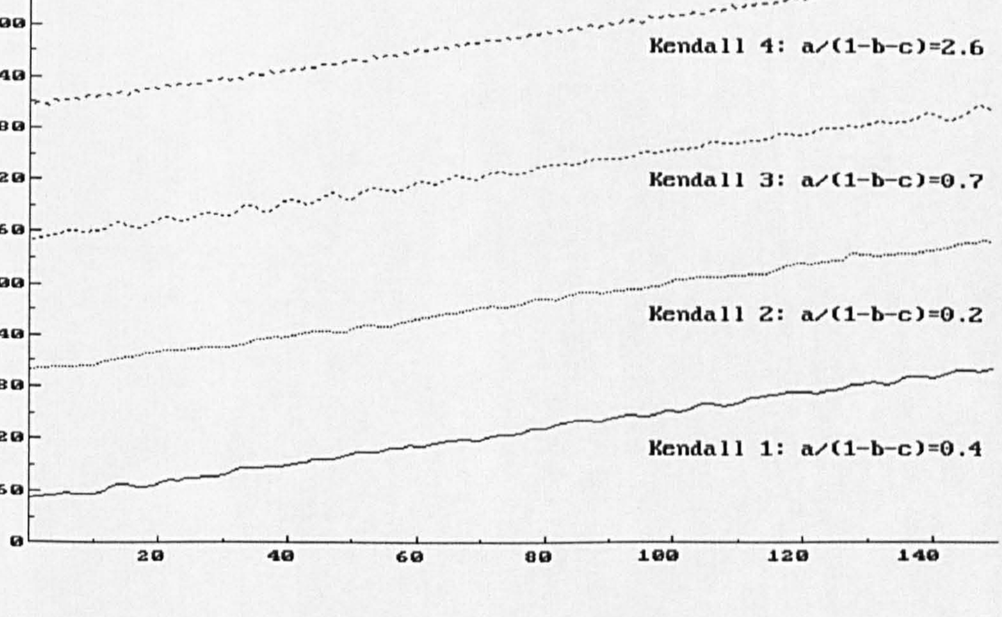

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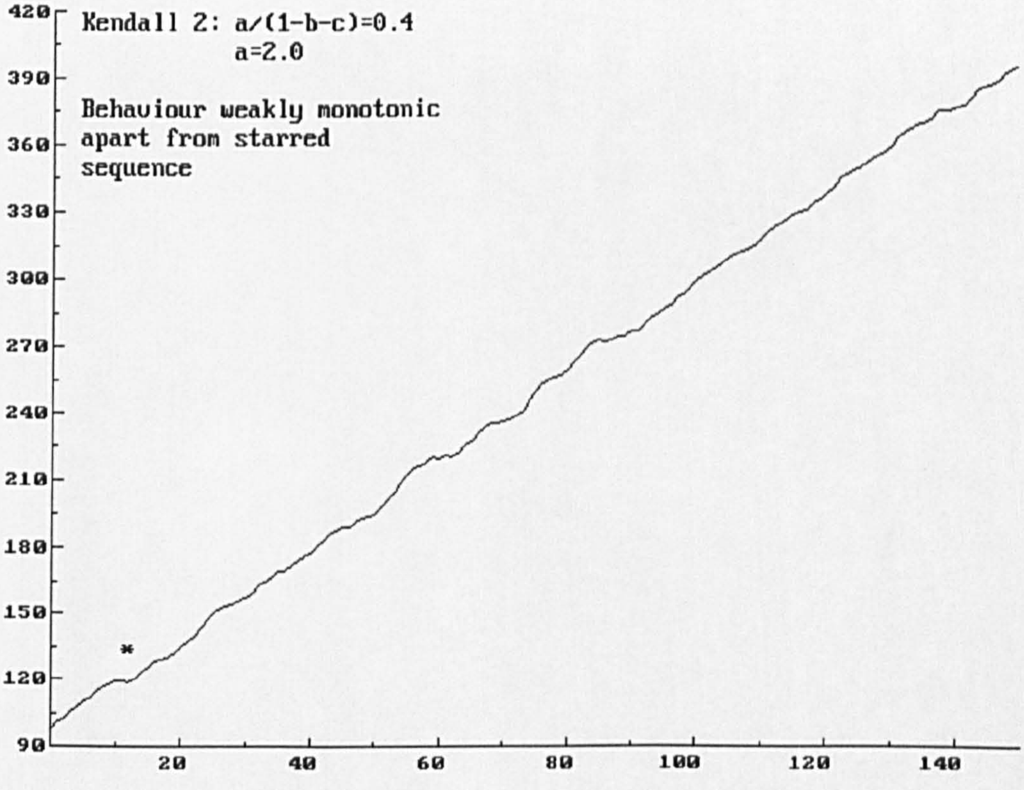
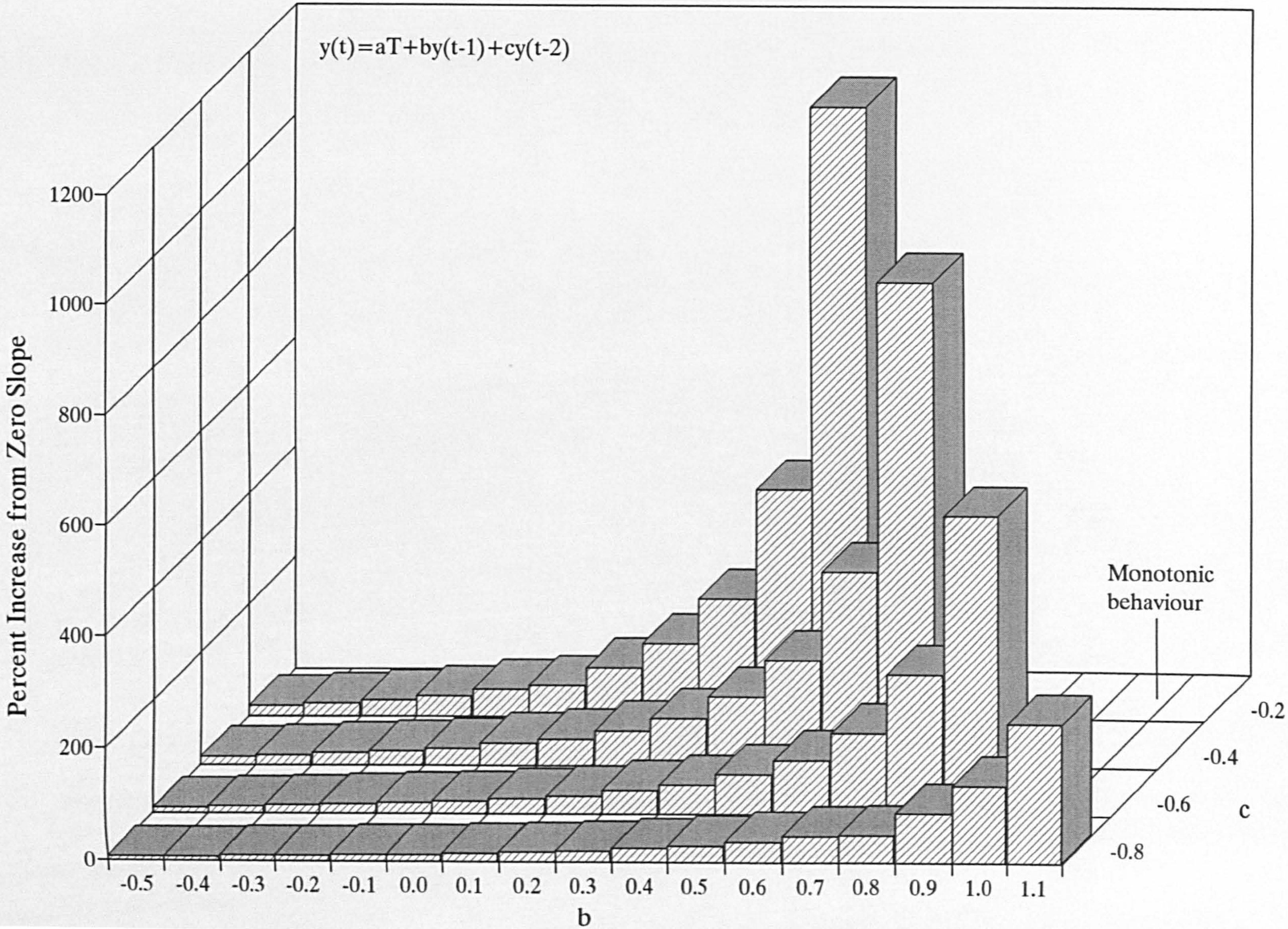


Figure 4.6.5. M.L.(U pruns): Sensitivity of Coefficient to Change in Slope



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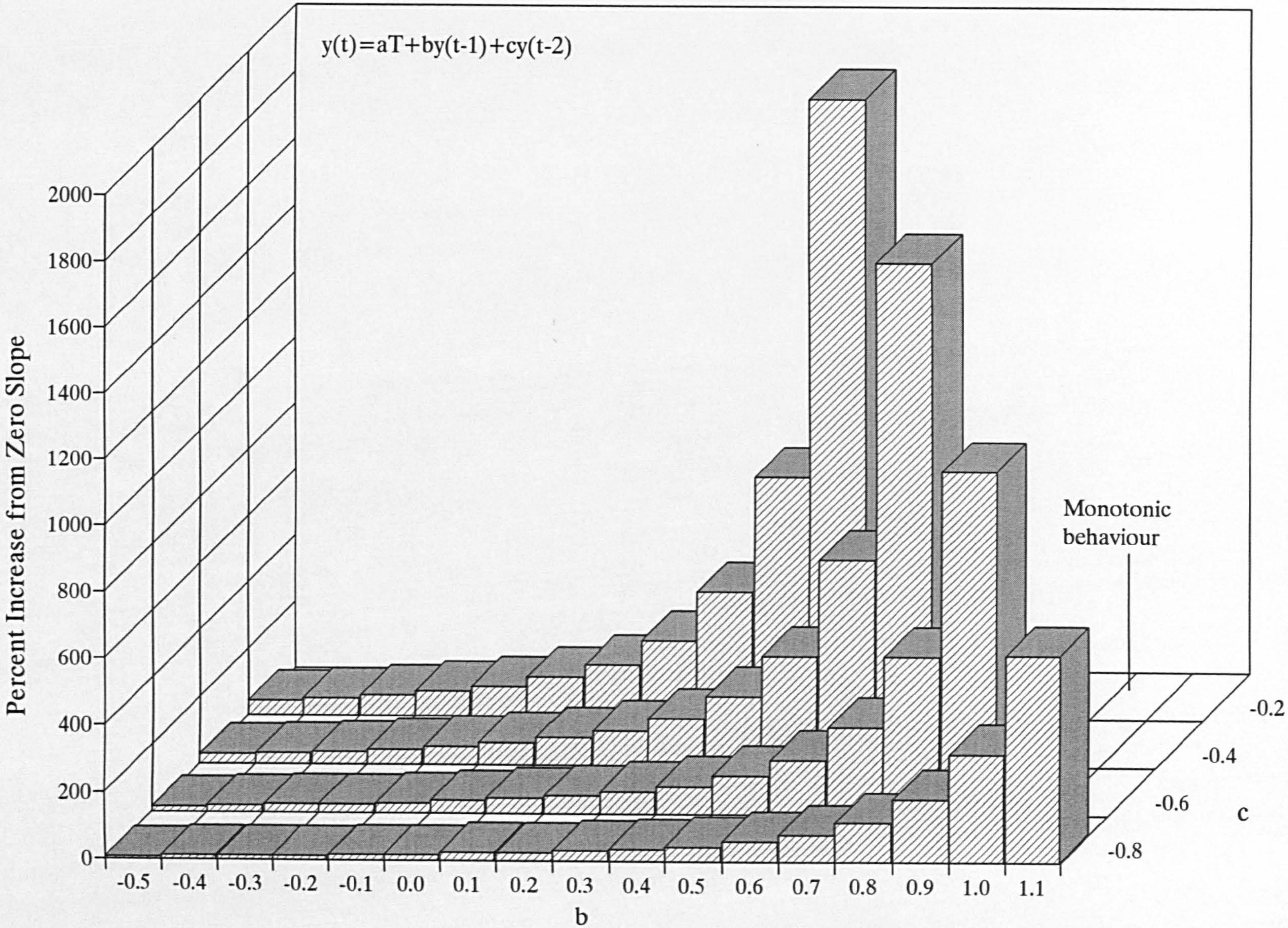

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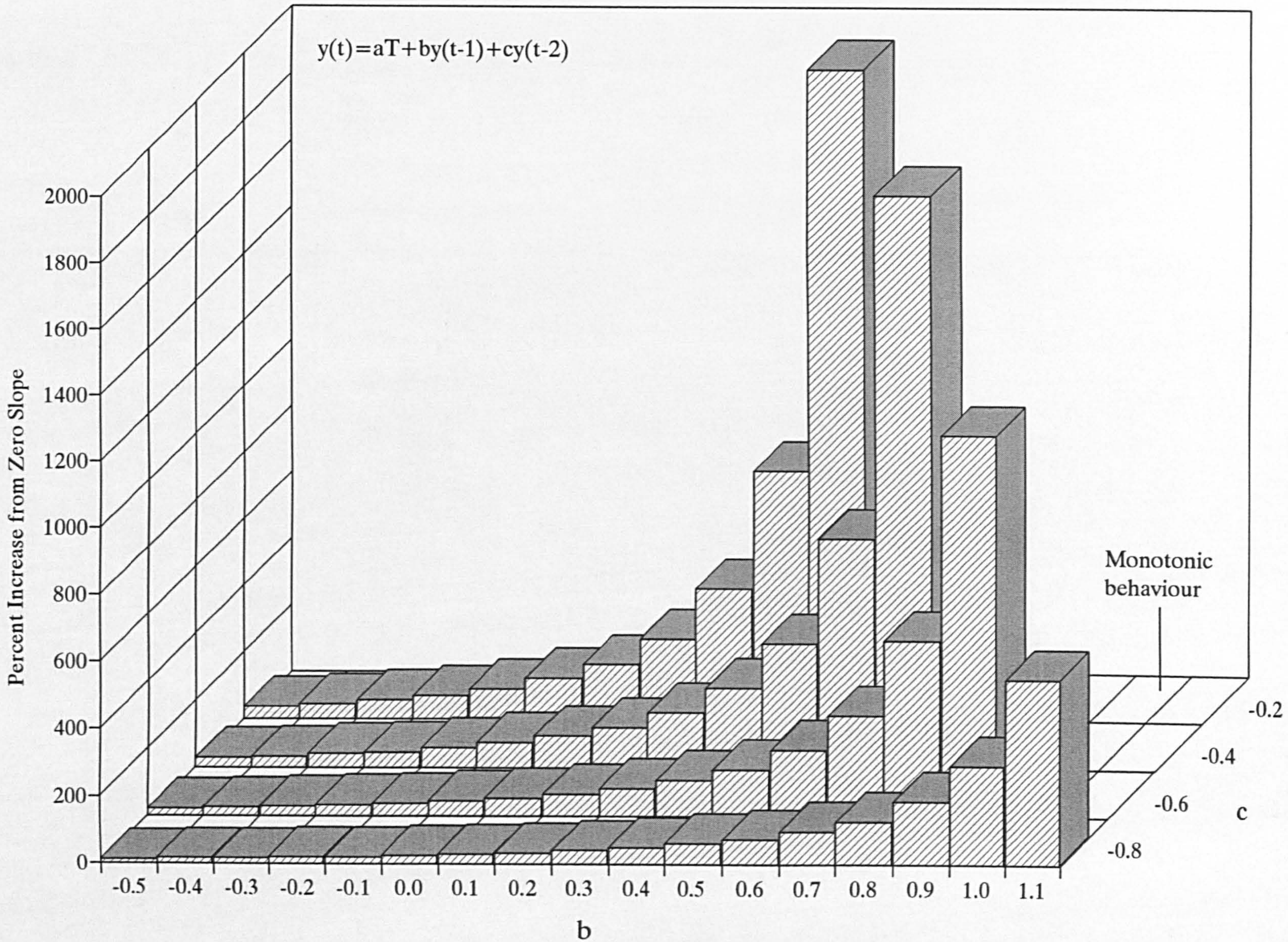

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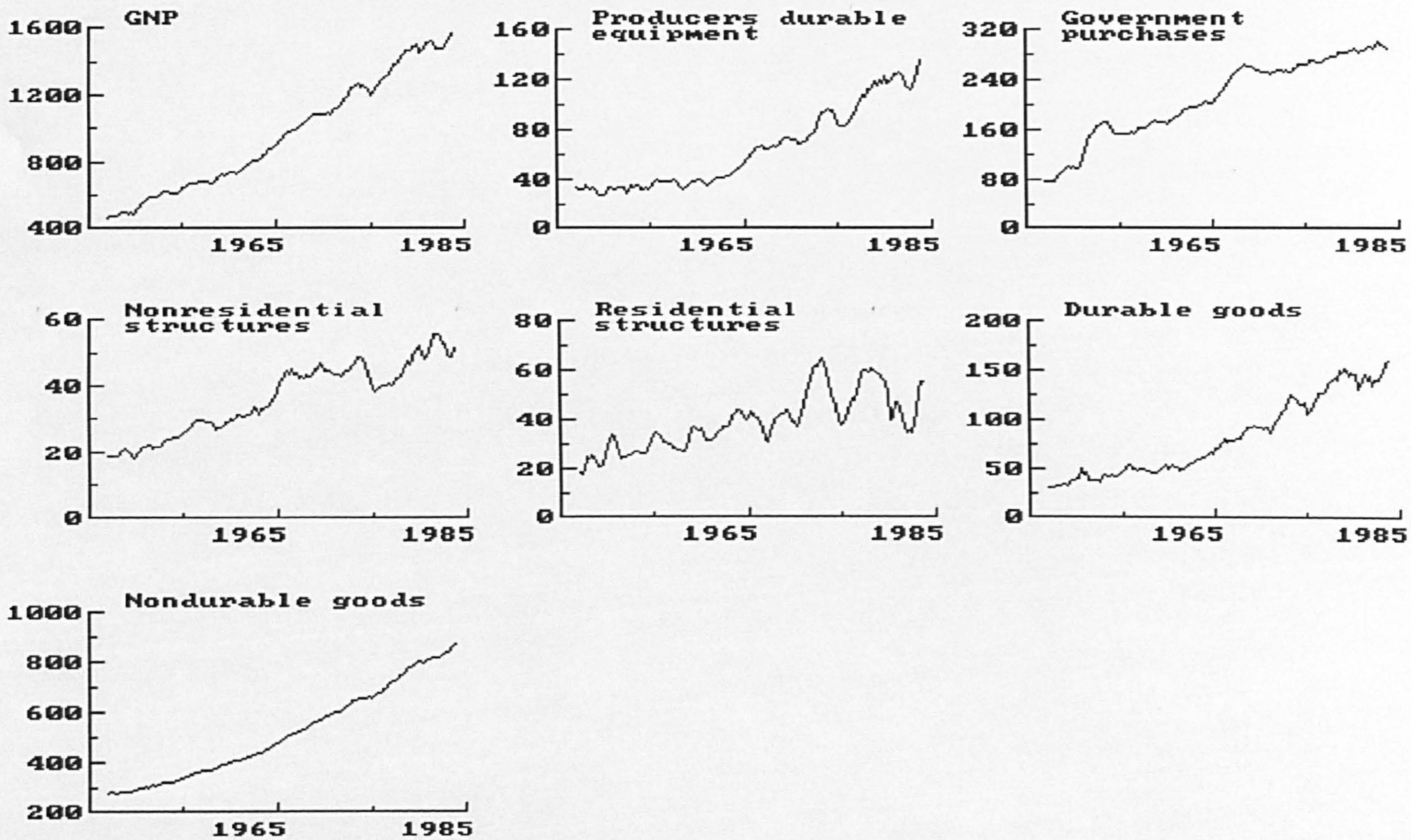

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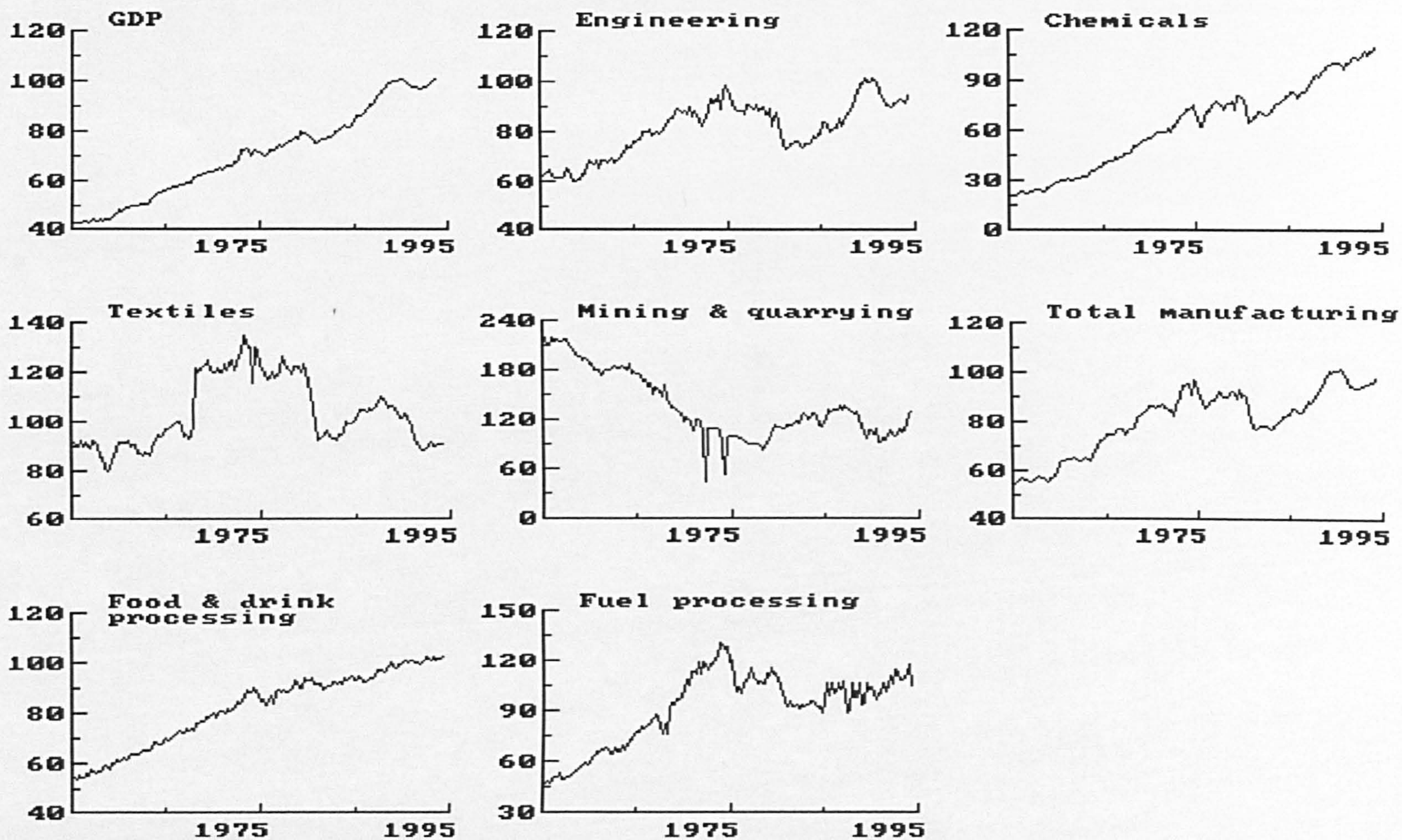

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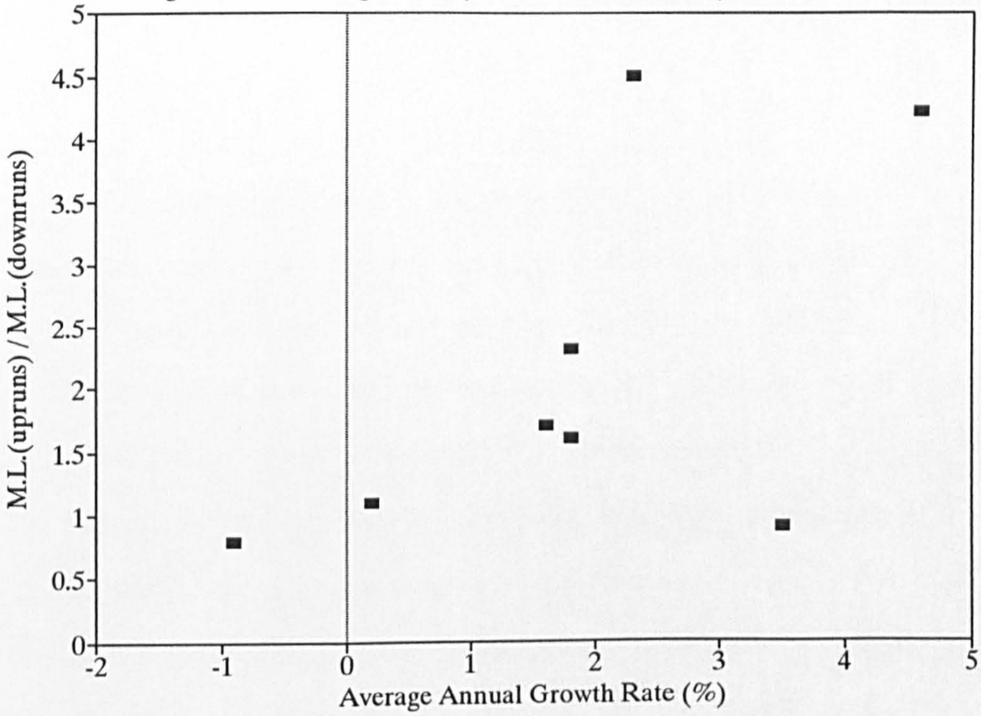

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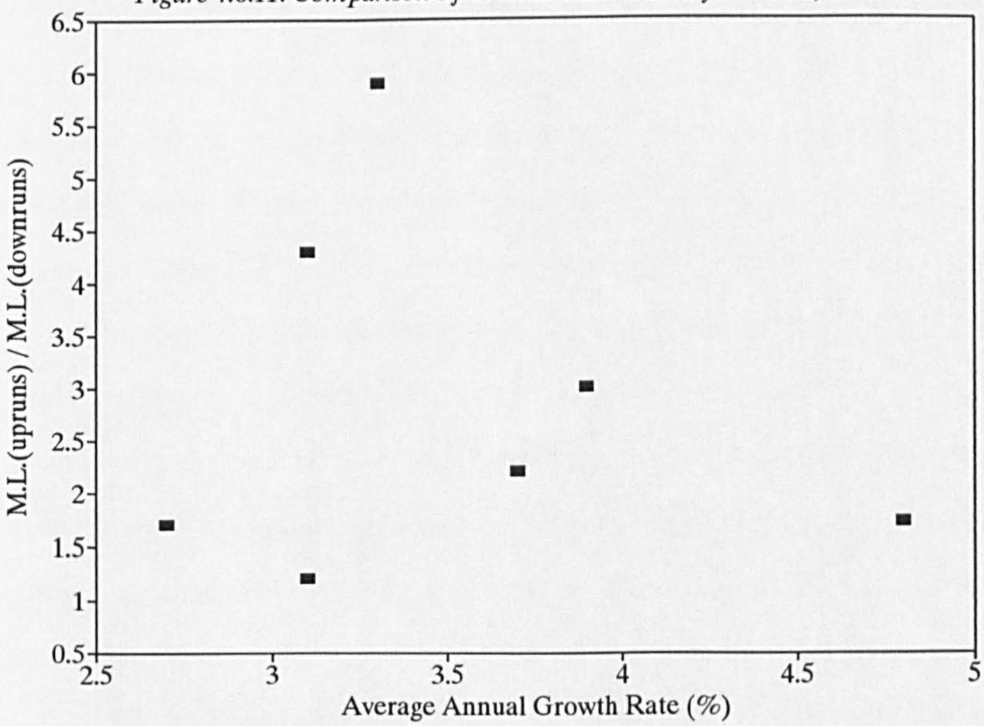


CHAPTER 5

HISTORICAL BUSINESS CYCLES: THE TIME-SERIES EVIDENCE

5.1. INTRODUCTION

The historiography of business cycles emerged from philosophical roots in the nineteenth century and became accepted before any serious attempt was made to measure output variables. When finally such estimates were made, beginning in the early 1900s, the idea of cycles had become so intuitively plausible that the statistics were tailored to produce the desired cyclical effect, Beveridge's 1909 'Pulse of the Nation' and the business barometers being cases in point. The usual decomposition of such data is quatra-partite: trend, seasonal, cyclical and irregular. In interwar and early postwar research, because of 'depressed' conditions, the emphasis was on 'cycle' rather than on 'trend'. When full employment returned in the late 1940s attention shifted to the investigation of fluctuations and structural breaks in growth paths: the 'growth cycles' and 'climacterics' thought to have occurred at various times since the beginnings of industrialization. The doubts expressed by Mitchell as early as 1927 about the pervasiveness of 'general business conditions' have therefore not been addressed, although much of the published work comes close to discarding the traditional assumption of separate DGPs for each component. In the particular case of cycles, the scientific method would require evidence of a kernel of behaviour common to all historical episodes, rather than merely an *a priori* assumption. This implies direct observation, if possible, from time-series data rather than 'indirect' inference from statistical decompositions requiring identifying assumptions. It moreover requires that the underlying structure of the economy remain sufficiently similar to permit comparisons across episodes.

The purpose of this chapter is to investigate these issues by evaluating objectively the time-series evidence for short-period fluctuations in general business conditions as 'real' and not merely superficially recurrent phenomena in the era since industrialization.

Section 5.2 discusses the postwar historical literature and shows how its authors have come increasingly to question the pervasiveness of 'cycles'. Section 5.3 proposes an unrestricted metric (the turning point) for tracking them. Section 5.4 presents empirical evidence of 'turning points' and timing relations in economic time series for the pre-1914 and post-1945 periods. Section 5.5 assesses the magnitude of structural change during the two eras in the UK and compares time-series data to nineteenth century 'annals'.

5.2. DATA-HANDLING AND INTERPRETATION BY ECONOMIC HISTORIANS SINCE 1944

The main concern of economists in the closing stages of World War II was to avoid a return to mass unemployment. Such an outcome was not implausible. It had happened after 1918 and was perceived as a political threat when contrasted with the success of command economics during the war. The business-cycle literature of the time initially assumed that conditions of the 1930s were 'normal' if deplorable, but changed its emphasis as it became apparent that 'Keynesian' expedients — a combination of reconstruction, mass consumerism and Cold War rearmament — could forestall a return to depressed conditions and actually promote strong economic growth. The filtering and interpretation of time-series data reflected these concerns and the change of emphasis as the postwar reconstruction progressed. Beveridge's *Full Employment in a Free Society* [1944], published at the close of a period of peacetime economic collapse and wartime economic boom, emphasized the 'cyclical' concerns of the early postwar transition. The theoretical framework was still quantity-theoretic, a 'regular feature of the trade cycle' being the comovement of prices, nominal output and employment.[1] The cyclical character of conditions was reflected in the 'new index of industrial activity from 1785 to 1938' published in Appendix A, pp. 275-314. These estimates, both for the aggregate and its constituents, were presented as percentages of a 'trend ordinate', where the latter was

[1] [1944] p. 287.

either a straight line or polynomial curve fitted by OLS. The findings were artifacts of the statistical methods common at the time: the practice of 'detrending' and the subjective approach to turning point identification. Although these techniques have from time to time been questioned they have until comparatively recently been accepted as correct, thus reinforcing the accepted view of 'business cycles'.

Experience of the 'social possibilities of war' led to a shift of political orientation in the 1940s from *laissez-faire* to command economics. Rostow [1948] is the earliest of the 'Keynesian' revisionists to re-interpret nineteenth century 'business cycles' against this novel background of full employment, scarce resources and rising prices in peacetime.[2] Rostow maintained the assumption of a decomposition into 'trend' and 'cycle': Chapters I and II discuss these components separately. Chapter I identified five periods of differing 'trends' (i.e., growth rates) ending in the years 1815, 1847, 1873, 1900 and 1912. Each of these epochs was chosen to conform to price trends over the whole period — an unconscious echo of the Quantity Theory, perhaps. The composition and weighting of Beveridge's index were criticized. Chapter II proposed an alternative set of cyclical turning points; but Beveridge's central hypothesis of 'the existence of a persistent trade cycle in Great Britain from the close of the eighteenth century' seems to have been accepted.[3]

Rostow found 24 cycles from 1790 to 1913, an average duration of 5.25 years. He distinguished, however, 14 'major' and ten minor cycles, with expansion phases in the former type distinguished by full employment and by growth in long-term investment. Although the average duration was thus more nearly in line with Kendall's 1946 estimate than with Beveridge's own findings, such analytical details are important only for what they reveal about the circular interaction of statistical evidence with the theoretical priors of the researchers. The 'existence of a persistent trade cycle' was not so much an

[2][1948] p. 2.

[3]*Ibid.*, p. 34 fn.

empirical inference as a starting point for the manipulation of the data into a form which would permit the desired inference to be drawn. In the late 1940s there was still little doubt about the nature of 'business cycles'.

Rostow collaborated with Anna Schwartz and Professor Gayer on a two-volume statistical study of the British economy (GRS) which applied the analytical techniques of Burns and Mitchell (the only such example outside the NBER). This work also distinguished between 'major' and 'minor' cycles, but found that the 'precise timing and intensity of minor cycle expansions and contractions cannot be easily generalized'. Output may actually increase during such a contraction; but in any case, general recovery, depending upon investment, arises out of a variety of conditions, no one of which is 'both necessary and sufficient', and any of which may or may not be present at any particular moment. The analysis of 'typical' behaviour over the several cycles, given in Chapter III of Volume II, showed large numbers of irregularities, especially in the war years up to 1815. Specific cycle peaks were usually spread over several years, but the authors considered them to be merely 'random in character' and not 'violating' the basic structure.[4]

The theoretical structure of GRS was classically Keynesian, with lags in price-adjustment and investment, and with the statistical evidence designed to test this hypothesis. Matthews, in a 1954 critique of GRS in the *OEP*, raised strong objections to the idea of 'general trade conditions', especially but not exclusively in the war period. Different sectors 'often enjoyed very different fortunes at the same time'; so that for the war period at least the idea of a 'general level of business activity' had to be employed 'with very great caution'.[5] The problems of interpreting 'uneven' fluctuations were further discussed in Matthews's *Study in Trade Cycle History* [1954b], an account of the British economy from 1833 to 1842. Although the period could be divided into five 'phases',

[4]Volume II, p. 614.

[5][1954a] p. 14.

there was no general agreement about their timing: for example, Beveridge and Rostow found a double 'peak' in 1836 and 1839/40, whereas Thorp classified the years 1838–1842 as all depressed.[6] Matthews argued that the period 1837–42 can be interpreted 'as forming a single cyclical contraction phase', albeit one which appears 'long, confused and heterogeneous'. There were a number of 'complicating factors': (1) the revulsion from the mania of 1836, (2) business fluctuations in the US, (3) long lead times for investment projects which came on stream well after the boom, and (4) exogenous shocks to the supply of grain. The boom itself also appeared heterogeneous. Although it could be explained to an extent by 'a cumulative interaction...between investment and income', there also seem to have been a number of 'circumstances of an extraneous or even fortuitous character'.[7] The abiding impression was that the upswing was more complicated than the downswing, but that the latter was also 'interrupted and confused by a variety of complicating factors.'[8]

In the final section Matthews discussed the 'pervasiveness of the cycle', concluding that 'especially in the years 1837–40' there was 'a good deal of divergence between the timing of fluctuations in different industries'.[9] He found, for example, that the peak for textiles occurred in 1836, for coal in 1838, and for shipbuilding in 1839–40. The regional discrepancies were 'more striking', for example in the statistics for brick production. Underlying these industrial fluctuations were movements in food supplies and prices, in these years largely exogenous. The only instances of 'pervasiveness' were at the temporal extremes, that is the end of the 'mania' in 1836 and the 'depression' of 1842; yet even in the latter instance the matter was not that clear cut.

[6] [1954b] p. 1 and fn.

[7] *Ibid.*, p. 218.

[8] *Ibid.*, p. 218.

[9] *Ibid.*, p. 224.

Hughes's 1960 study dealt with the period 1848–1860. 1848 was seen as a year of 'depression', but 'mixed in its effect, with not all sectors...equally hard hit'.[10] The harvest failure in Ireland in 1846 led to 'large-scale unemployment and falling income', a marked decline in exports and a 40 percent decrease in industrial prices. At the same time cotton exports were increasing, which 'may have mitigated some of the adverse effects of the depression in home demand'. A 'recovery and boom' took place from 1849 to 1853, and was 'the most pervasive economic expansion of the 1850s'; but even here the evidence was conflicting. A rise in business failures, falling prices and 'scattered trade complaints' during 1851 were seen as less persuasive an indicator than the evidence of continuing investment in textile plant, a strong expansion of bills of exchange and evidence of new investment in basic industries. The period of the Crimean War was on the whole prosperous; yet textiles were 'depressed' from the fourth quarter of 1853. 1854 and 1855 were years of wartime boom; yet iron and coal prices had begun to fall by the end of the period. 1856 saw a revival of exports, but unaccompanied by any revival in domestic investment. The shipbuilding and construction industries were depressed by 1856, and this finally became 'general' by the autumn of 1857. Although 1858 was 'judged as one of the worst depressions of the nineteenth century', some industries, textiles primarily, were in 'full production' by the year-end. The recovery of 1859–60 was 'uneven'.

Dornbusch and Frenkel [1984] analyse the credit crisis of 1847 in the context of the operation of the classical gold standard. The origins of the crisis are seen as diverse: partly 'real'; partly 'autonomous', as the effects of the Irish famine put pressure on the domestic bullion supply; and partly 'financial'. The latter was a consequence of the former: a collapse of short-term credit resulting from a large external payments deficit, and from the railway speculation of 1845. The 1847 crisis differed both from that of 1857 and that of 1836–41, being actually two crises in the same year. The first, in April, arose from a tightening of credit by the Bank of England; while the second, in October, ensued

[10][1960] p. 28.

from a loss of confidence in the convertibility of bank deposits owing to the operation of the 1844 Act.[11]

Rostow [1972] extends the GRS analysis of the dating of nineteenth century British cycles to the period 1790-1914. Again, doubts are raised about the pervasiveness of cycles, about long-term comparability of conditions and, for the first time, about the turning-point dates themselves. In fact, the discussion suggests that this author had encountered the same problems of identifying reference dates as had Burns and Mitchell in the 1930s.

> No two cycles, of course, are quite the same; and one can trace as well, certain long-period changes in the character of the cycles... It should be emphasized that the year designated [as a turning point] is, in many cases, a matter for judgment. The processes involved in the cyclical turning-points are complex; and they are woven in each case into unique historical circumstances... ([1972] p. 74)

Rostow also questioned the amplitude criteria for 'the marking off of a formal trade cycle'. On some occasions, as in 1823 for example, only exports declined, whereas in 1801–2 some indicators expanded while others contracted. In order to find a cycle chronology the analyst is hence constrained to make

> arbitrary judgements, no matter how large or superficially comforting the mass of statistics with which he is able to surround himself. ([1972] p. 78)

The GRS chronology thus left 'untidy points of demarcation' because of ambiguities in the data. The average duration was 'not particularly meaningful' since 'these cycles differed significantly in character among themselves'.

There was also a strong sense of structural change. In the earlier period fluctuations of an 'inventory cycle' type were in evidence which could be traced 'back even to medieval times'. From the 1780s, on the other hand, longer swings in investment became pronounced; yet it is only from the 1860s that the 'short cycle' could not only be

[11] [1984] p. 236.

'detected...but...had sufficient power to produce distinguishable general movements...[12] The 'shift in the...structure' of all economies from agriculture to industry changed the 'character of cyclical fluctuations' between 1790 and 1910. By the end of this period a far greater percentage of the population 'felt the impact of the trade cycle' than at the beginning.[13]

Price and output movements were complex, especially during the 'Great Depression'. For example, in the supposedly depressed years 1873–1879 average unemployment was 3.1 percent, compared with 4.7 percent for the whole period 1850–1914.[14] Prices, interest rates and profits fell in the 1870s, but house and ship building and 'other domestic enterprise of low expected yield' flourished. In Rostow's view, commonly held in the 1970s, the main feature of this depression was the lack of investment opportunities at high rates of return, so that while the speculative instabilities of earlier periods were conspicuous by their absence, industry continued to grow.[15]

Statistical estimates of nineteenth century British economic output developed in parallel with national income accounts in the postwar period. Beveridge's industrial production series were the earliest of these; all were subject to 'detrending' and smoothing. The Hoffmann [1955] index of industrial production was subsequently found to be unsatisfactory in some of its weightings. Aldcroft and Fearon [1972] applied a nine-year moving average to the Hoffmann index and also expressed the resultant series as deviations from 'trend'. In any case, by the 1960s, in response to the long postwar reconstruction boom, interest had shifted from short-period fluctuations to the sources of growth. Deane and Cole [1962] gave estimates for the British economy from 1688; but

[12] [1972] p. 83.

[13] *Ibid.*, p. 85.

[14] *Ibid.*, p. 89.

[15] *Ibid.*, p. 290.

it has been the work of Feinstein and Lewis in the late 1960s and early 1970s to which most attention has subsequently been paid.

Feinstein's UK estimates for the period 1855-1980, which appeared in 1972, were the first designed to conform to national income accounting definitions. They were, however, subject to certain unfortunate 'adjustments' which reduce their reliability for present purposes (see below, Section 5.3.1). Lewis's *Growth and Fluctuations 1870-1913* appeared in 1978, but the statistical appendix on which it is based was completed in 1967 and circulated in manuscript. Its objective was initially to reconcile conflicts in average growth rates in various other series, e.g., Hoffmann's and Prest's, the result being a new aggregate UK industrial production index with fourteen constituents. The raw estimates contained 'unlikely annual fluctuations', and so the iron and steel, iron and steel products, building, printing and chemicals series were 'smoothed' by means of three-year and nine-year moving averages.[16] Lewis also produced current and constant price estimates for 15 UK GDP constituents, as well as some price indices. He was principally interested in the 'speed and regularity' of growth in four 'core' economies: the US, the UK, France and Germany.[17] On the other hand, the existence of a 'trade cycle' driven by separate forces was viewed with scepticism:

> [Trade-cycle] models, while they explain the past satisfactorily, always fail to predict the future with reasonably accuracy. If the term 'cycle' is to be confined to a movement whose future can be predicted from its own past, then the movements of industrial production, though wave-like, are not cycles; and the models which can explain them backwards but not predict them forwards have to be viewed with suspicion. ([1978] p.18)

Ford [1981] also applied nine-year moving averages to data on income, investment and unemployment at current prices and calculates peaks and troughs from

[16] [1978] pp. 253-258.

[17] *Ibid.*, p. 15.

the residuals after subtracting the 'trend'. He found conformity in the turning points in these residuals, a result different in character from most other studies of the nineteenth century UK 'trade cycle' in which the data are in constant prices. Nevertheless Ford, in common with other investigators, found it 'difficult to specify a typical British fluctuation or cycle'; each episode had 'unique institutional and geographical features...apart from varied behaviour patterns'.[18]

This sceptical tone is evident in the recent literature. Eichengreen [1983], who considers cycles and their 'causes' in Victorian Britain, distinguishes the 'real' theories, such as those of Lewis and Rostow, from the 'monetary' ones, as proposed by Bordo, Friedman and Schwartz. Nevertheless, the

> proponents of these various explanations for the trade cycle face a common challenge. To build a convincing case, they must do more than merely demonstrate that fluctuations in certain real or monetary variables coincided with fluctuations in British national income. Such coincidences could have resulted from the impact of other factors on both national income and the supposed explanatory variable... ([1983] p. 146)

Eichengreen questions the use of structural models originating from economic theory, as estimated e.g. by Tinbergen, as a 'basis for convincing tests of the various theories' because of the circularities inherent in such strategies. It is not empirically obvious which variables are autonomous, nor how their 'impulses are transmitted'; yet when the model is supposed to 'identify the channels through which the cyclical impulses are transmitted' such a 'procedure is tantamount to assuming one's conclusions'.[19]

Eichengreen's model is a five-dimensional VAR, the dependent variables being output, prices, exports, residential construction and money stock, and with a sample period 1833–1913. The evidence of the model is of structural change: up to 1869 prices are found to be correlated negatively with changes in output, and neither exports nor the

[18] [1981] p. 134.

[19] [1983] p. 153.

monetary base are related in their movements to the 'subsequent paths of real incomes or prices'.[20] In the later period the results are 'strikingly different'. Export shocks have a significant effect on domestic prices, as does residential construction; yet the latter has no effect on real incomes, and its movements are no longer a leading indicator of price level changes. Explanations for this somewhat confusing picture are sought in the possible emergence of a 'law of one price' in the world trading economy and in rigidities imposed by the general adoption of the gold standard.

Solomou [1987] deals exclusively with variations in growth patterns, but nevertheless identifies many of the problems of generality encountered in conventional business-cycle research. This paper assesses the evidence for 'Kuznets' long swings (14 to 22 years) and 'Kondratieff' waves (about 50 years) in British economic growth from 1850 to 1973. The Kuznets swings are thought to be 'generated by a more complicated historical process than has been recognized'.[21] For example, during much of the pre-1914 period there was an 'inverse relationship between investment levels and the rate of growth of output in the British economy'. Despite high levels of investment in the 1870s and 1890s, the economic processes in these two periods were very different; in the first growth was declining while in the second an 'upward adjustment was taking place'.[22] In general the evidence suggests that 'the swings were episodic rather than endogenous'; nor were they 'the result of a systematic exogenous variable'. For example, although climatic variations 'are important in explaining the long swings' in investment, the effects of these are 'filtered out' by structural change. In any case, the sectors whose fluctuations accounted for the swings 'were very different over time'.[23]

[20] *Ibid.*, p. 159.

[21] [1987] p. 11.

[22] *Ibid.*, p. 104.

[23] *Ibid.*, p. 130.

Solomou [1994] further assesses evidence for long swings and discusses the problems of modelling unobserved components in British historical data.

> Much of the existing historical research on business cycles makes the assumption that an economic variable...can be decomposed into three parts: trend, cycle and a residual of random influences. If such a simple perspective were wholly valid all one would have to do to identify the cycle would be to fit a long-run trend to the data and then take the pattern of absolute or relative deviations about the trend. But the procedure makes strong econometric assumptions, which may be misleading... ([1994] p. 248)

An even greater problem is inaccuracies in the data arising from interpolation, from the use of proxies, from imprecise adjustments and in the case of Lewis, from the imposition of a nine-year cycle on the data.

Just how complex can be the problem of identification of unobserved components is demonstrated in Crafts, Leybourne and Mills [1989] (CLM). The authors construct a revised index of industrial production for the UK for the years 1700-1913. Using state-space methods they fit various structural models to this series as well as to the original Hoffmann index from which it is derived and also to a third variant, concluding that 'there is no strong evidence against analysing the three series as though they are DS processes...'[24] The 'underlying trends are stochastic rather than linearly deterministic'. However, 'conventional Box-Jenkins identification' of the implied reduced form yields an ARIMA(0,1,1) specification for all three series with constant trend growth rate and no cyclical component.[25] This is, in fact, 'at odds with the received historiography of the sample period and the generally accepted behaviour of series for industrial output'. ARIMA(2,1,3) and (2,2,4) specifications are also fitted. The first improves the fit but still imposes a constant trend component. The second, allowing both stochastic trend and

[24][1989] p. 49.

[25]*Ibid.*, p. 55.

cycle components, has a worse fit but more favourable Q^* statistics for a higher number of residual autocorrelations. The authors argue, following Harvey [1985], that

> such...models are not intended as parsimonious representations of the underlying data generation process but aim to present the historiography of the series in terms of a decomposition into trend, cycle and irregular components. ([1989] p. 56)

Although these decompositions perhaps present a 'clear and intuitively sensible description of the evolution of industrial output during the 18th and 19th centuries', it is not possible to reject, from the evidence presented, alternative decompositions that may or may not have a cyclical component.

Crafts and Harley [1992] pursue the problem of trend rates of growth in the nineteenth century. A revised estimate of industrial production shows a steadily rising growth rate to a maximum of about 3.5 percent in the 1840s, and thereafter a steady decline in growth to about 1.5 percent by the 1880s.[26] Crafts and Mills [1995] consider the problem of structural breaks in the DGP for industrial production over the course of the eighteenth and nineteenth centuries, and the implications of these for calibrating trends. Tests for structural breaks 'strongly suggest that both the difference stationary and segmented linear trend stationary models are inadequate processes for explaining the behaviour of industrial production'.[27] A more satisfactory representation is via a 'segmented quadratic trend plus a shifting cyclical component'. The latter reflects *inter alia* the view that the

> growth process was disturbed by technological surprises which could change the trend rate of growth. ([1995] p. 20)

Further comments on the diversity of 'cyclical' behaviour are given in Friedman [1986] and Dimsdale [1990]. Friedman, in a study of the financial markets and their relationship to business cycles in the United States, finds that structural changes in US

[26]This paper addresses the question of variations in growth rates in the UK since 1700, and the effect of revised estimates of these on the historiography of the industrial revolution.

[27][1995] p. 2.

financial markets render the search for recurrent quantitative relationships between real and monetary variables obscure and problematical. 'Qualitative' regularities — the 'procyclical behavior of money, credit and interest rates' and evidence of the 'tendency of money and credit growth to "lead" real economic growth at major business cycle turning points' — have 'not corresponded to persistent regularities in the *quantitative* [emphasis in original] relationships that constitute the main focus of modern business cycle analysis'.[28] Dimsdale, in an analysis of cycles in Britain since 1830, has similar problems of interpretation. The

> pre-1870 cycles differed from those which followed later in the century... During the early cycles the recovery culminated typically in a domestic financial crisis as in 1839, 1847, 1857 and 1866. In the later cycles domestic financial disturbances played a much less important role on account of the increasing stability of the monetary system as indicated by the absence of major internal banking crises after 1870. ([1990] p. 154)[29]

Shocks figure prominently in this author's perception of the cycle-phenomenon; e.g., from overseas financial crises in 1873 and 1907 and from the 'massive gold inflow' of the 1890s.[30] The general conclusion is that, like Friedman, the evidence points to a 'complex variety of shocks generating the British cycle'.[31]

[28][1986] p. 396.

[29]This is certainly an overstatement. There were at least two more great bank failures in the UK: the City of Glasgow in 1882 and Barings in 1890.

[30][1990] p. 176. Dimsdale also investigates the diversity of postwar 'cycles'. These are divided into four of the 'stop-go' variety and the two associated with oil-price shocks, 1971-75 and 1979-81. A further 'distorted' cycle is thought to have occurred from 1961 to 1967. (See p. 180.)

[31]*Ibid.*, p. 188.

5.3. SHORT–PERIOD FLUCTUATIONS: AN ATTEMPT AT OBJECTIVE ASSESSMENT

Evidence for and against the existence of a general business–cycle phenomenon appears, at best, evenly balanced. The literature emphasizes the difficulties of identification, in both the technical and general senses, and it appears that short–period fluctuations may be dominated by non–recurring, episodic elements. Circularity is apparent in all analytical treatments up to Lewis's: the authors assume the existence of the business or trade cycle *a priori* and then make the evidence fit that view. The bias has been compounded over the years by Mitchell's hypothesis being taken as a 'definition'; and the analysis is further obscured by the use of 'judgment' in measuring the length of each episode, especially in the use of the 'reference cycle' as a base metric against which specific time–series evidence is evaluated. Even the 'optimal' estimating techniques associated with the fitting of structural models require identifying assumptions and restrictions on parameter spaces as, for example, in Harvey [1985], CLM and Harvey and Jaeger [1994]. As Harvey [1985] indeed argues, such exercises are intended to model 'stylized facts' rather than underlying DGPs.[32]

Mitchell argued that the concentration of turning points is a necessary condition for the existence of business cycles (though not, perhaps, a sufficient one); yet the admission of 'leading' and 'lagging' indicators implies some dispersion. This is the effect of Burns's 'unseen' cycle: stable leads and lags building to a concentration at the peaks and troughs of a 'general cycle'. Indeed, it is possible to have a species of 'cycle' whose turning points are completely dispersed: all that is required is regular frequencies for all variables, and with phase–shifts such that an equal number of variables reach peaks or troughs in each time period. The juxtaposition of the 'concentration' hypothesis with possible stable lead/lag relations is potentially contradictory for any empirical verification of business–cycle behaviour. Zarnowitz's correlation criteria are nowadays substituted for

[32][1985] p. 225.

bare 'clustering', but as suggested by Stadler [1994], these may be misleading when applied to macroeconomic time series.

Model-based methods may be more methodologically consistent than NBER procedure, for example, but are unfortunately not free of specification problems, especially given the comparatively low power of diagnostic tests associated with the Box-Jenkins selection technique.[33] Structural models do not address this problem in the case of 'business cycles' because of the required identifying assumptions and parameter restrictions. Decompositions of the ARIMA type are perhaps more appropriate for identifying long-run variations in the data than they are for picking out short-period fluctuations, as the recent work of Crafts *et al.* seems to indicate. Whatever the formal technique, all model-based studies of business cycles rely on turning points in filtered or decomposed time series data to identify the empirical regularities of interest, e.g., duration, conformity, etc. The purpose of this section is to investigate short-period behaviour of turning points in time series that have been subject only to deflation and in some cases seasonal adjustment, and which thus have not had *a priori* restrictions or filters imposed on them.

5.3.1. A Proposed Metric

The obvious questions raised by all these doubts are (a) whether a more objective metric, not relying on specific priors, would validate the hypothesis of generalized comovements and (b) how such a metric might be defined. Would such an approach, when applied without restrictions, produce the relative concentration of 'peaks' and 'troughs' in time series seen as a necessary condition? Alternatively, would it produce 'unseen' cycles with stable timing relations? The Bry-Boschan algorithm filters out 'false signals' which, as has been shown in Chapter 4 above, gives rise to other anomalies. The weakness of existing technique is its circularity: specifically (a) in the imposition of a

[33] For a discussion of this point, see *inter alia* Harvey [1990] pp. 212-13 and Harvey [1981] Chapter 6, Section 2.

single set of reference dates on a whole macroeconomy and (b) in forcing both reference and specific peaks and troughs to alternate. An unrestricted version of Bry-Boschan would rather define turning points strictly as local time-series maxima or minima, as follows:

> a local maximum in a time series X_t occurs *iff* $(X_t > \max\{X_{t-k}, ..., X_{t-1}\})$ *and* $(X_t > \max\{X_{t+1}, ..., X_{t+k}\})$; a local minimum occurs as above, but with $>\max\{...\}$ replaced by $<\min\{...\}$.

Multiple and non-alternating turning points need also to be defined for these purposes, as these cannot now be filtered out. For annual data, multiples occur when, for some t, $X_t = X_{t+1}$; and for quarterly data, when $X_t = X_{t+1}$ and $X_t = X_{t+k}$, for some k. Plateaux and valleys in quarterly data would be captured by allowing $k>t$ a range of values, i.e., $X_t = X_{t+1} = ... = X_{t+k}$. The sampling interval t is necessarily annual for pre-1914 data, but a matter of choice for the post-1945 period. Mitchell thought that the interval should be monthly, and this frequency has been observed with some difficulty by the NBER for US reference-cycle dating purposes. In the UK the CSO have recently abandoned monthly for quarterly reference tracking (although they are now reported to be developing a monthly GDP series).

Macroeconomic data are subject to error, as acknowledged by every investigator who has ever tried to estimate them, whether contemporaneously or retrospectively. For example, the CSO post-1955 error estimates range from ±2½ percent in the case of aggregates such as GDP to as much as ±7½ percent for some other series.[34] The problem is still more serious for pre-1914 data: not only have moving averages and other 'adjustments' been imposed, but according to Feinstein [1972], serious additional types of error are inherent. In Section 1.4 of Chapter 1 he identifies at least five sources of error: (1) inaccuracies in the basic data; (2) mistakes in allocating constituents across more than one variable; (3) adapting data to serve an unsuitable purpose for which it was

[34]See *CSO United Kingdom National Accounts, Sources and Methods*, HMSO, third edition.

never intended; (4) inaccurate estimates when no source data are available; and (5) inaccuracies introduced by the investigator.[35] His 'best guess' at margins of error is given by the following table:

Table 5.3.1. Feinstein's Reliability Estimates, 1855–1913 (± percent)			
Series	1890-1913	1870-1889	pre-1870
Income from employment	5-15	15-25	15-25
Income from self-employment	15-25	15-25	>25
Gross trading profits of companies	5-15	15-25	>25
Rent	5-15	5-15	5-15
Net property income from abroad	5-15	15-25	15-25
Consumers' expenditure	5-15	15-25	–
Public authorities' current expenditure	5-15	5-15	–
GDFCF	15-25	15-25	15-25
Stocks and work in progress	15-25	15-15	15-25
Exports	5-15	5-15	15-25
Imports	5-15	5-15	15-25
Taxes on expenditure	<5	5-15	–

Source: Feinstein [1972] Table 1.9, p. 21

There is, however, yet a further source of error in these estimates: in assessing their reliability Feinstein compares his GDP turning points with the NBER and Thorp [1926] chronologies and then 'adjusts' them to conform to the 'reference' dates. In practice this involves increasing or decreasing levels when the data clashes with descriptions in the *Annals*, the amounts varying from +£143 to -£78 million (±8 percent on average), while leaving average levels unchanged. Given the reliability of the *Annals* as discussed above, it would have been better to use the quantitative estimates to adjust the reference dates – in other words, to work the other way round. Crafts and Harley [1992] also call their new UK industrial production index a 'best guess', acknowledging the difficulty of making

[35][1972] pp. 19-20.

accurate estimates. Any inferences are thus subject to some uncertainty, and these will be greater at higher sampling frequencies because of possible contamination by noise.

Quarterly frequencies have been chosen for the postwar period. If the expected concentration of turning points does not occur at quarterly frequencies it will certainly not be found in monthly series (and if indeed found at the lower sampling frequencies further analyses can then be carried out). The proposed algorithm does not force peaks and troughs to alternate, as does Bry-Boschan. No *a priori* reference dates are set. The value of k is unity for annual data, while for quarterly data, the value chosen should ensure that no two maxima or minima should be any closer than Mitchell's hypothetical minimum of about one year, i.e., $k \geq 2$. Any shorter window length would pick up noise as well, whereas a longer length could exclude fluctuations of a single year's duration. Otherwise, calibrating k for quarterly data must have an empirical basis, an issue considered in Section 5.3.3 below.

5.3.2. Description of the Available Data Sets

In the interests of testing the comovement hypothesis, constituents of each aggregate should be available, as well as the aggregate itself. In order to determine how pervasive the movements are, price, real expenditure and output variables should be used, rather than relying, as for example, do the CSO, on such 'confidence' indicators as CBI survey data or other such proxies. Nineteenth century price data are available from UK Board of Trade statistics for the period 1871-1913, in both aggregate and constituent form. (The latest industrial production estimates of CLM and Crafts-Harley are not suitable because they are only published as aggregates.) Other sets for the pre-1914 UK economy are Feinstein's estimates of 13 industrial production, ten investment and six GNP (expenditure) series; Lewis's 15 industrial production, 15 GDP constituent, and eight commodity price series; and Beveridge's four industrial production series. Postwar data are available from official sources for the US and the UK. The ten US series, 1947-1983, are given in the appendix to R.J Gordon (ed.) [1986]; while the 23 series that have been selected from the CSO Database cover the longest available time period for the postwar quarterly UK GDP series — 1955 to 1993. Details of these data sets are given in Appendix 5A.

Feinstein's industrial production series are plotted in Figure 5.3.1; the Feinstein and Pollard investment series in Figure 5.3.2.[36] Expenditure estimates of GNP and constituents are shown in Figure 5.3.3. Plots of Lewis's industrial production series are given in Figure 5.3.4; and his GDP components in Figure 5.3.5. The Board of Trade prices series are shown in Chapter 2 above and the UK and US postwar series in Chapter 4. Lewis's price data for the same period are given in Figure 5.3.6. Inspection of the plots reveals diverse behaviour of the constituents, both in relation to each other and to the aggregate. A number of the Lewis GDP sectors have monotonic or virtually monotonic behaviour over the period 1852–1913. The Feinstein and Pollard investment series are volatile, as are Lewis's building and construction series. Feinstein's series for agricultural output shows fluctuations which at times appear random, and seems to reflect the effects of exogenous climatic factors. The one series showing 'business-cycle' behaviour (apparent stationarity, with short-period fluctuations dominating) is unemployment; but in this case, because it includes only statistics for trades union members, it is incomplete. (Railway investment looks stationary but perhaps heteroscedastic.) Price data show general falls to the mid-1890s, with rises thereafter; but prices are in general volatile. 'Coherence' across series at low frequencies, associated with a 'trend' component, appears weak.

5.3.3. Calibrating the Programmed Approach

Because of the large number of data sets, computer programs have been written to find (unrestricted) turning points. Unfortunately, no such approach can be entirely free of assumptions. In this case, the shortest elapsed time between unlike turning points (i.e.,

[36]All graphics for this chapter are shown in Appendix 5B.

local maximum or minimum values) for annual data has been set at one year, and for like turning points, two years. This follows most of the existing research, but does not reject any turning points on non-mathematical criteria, and does not distinguish between 'major' and 'minor' turning points. Multiples are also catered for, and are signalled in the output files. For quarterly data, the minimum window length is assumed to be four quarters (i.e., the program searches ± two quarters from time t) following NBER practice and, if multiples, plateaux or valleys are encountered, they are signalled. A multiple peak or trough occurs in $\{X_t\}$ when $X_t=X_{t+1}$ or $X_t=X_{t+2}$ and a plateaux or valley occurs when $X_t=X_{t+1}=X_{t+2}$. The idea is to approach as close as possible the accepted methods of finding these local extrema while avoiding subjective judgments or the Burns and Mitchell practice of 'weeding out'; and this, as will be shown in the results section, produces multiple turns in most of the quarterly series.

The above values conform to the MBC 'definition' and could thus be criticized for circularity. However, turning points for the US GNP series 1947-1983 found by the program, employing the minimum window length of ± two quarters (shown in Table 5.3.2) are remarkably similar to the NBER reference dates and those found by Hamilton [1989] and Romer [1992]. The number of turning points is the same with all methods for equivalent periods: differences in timing are minor, peaks and troughs naturally alternate, and no multiples are found by the program. The discrepancies in dates can probably be explained by the differing nature of the sources: the NBER dating committee do not have GNP data available when making their assessments, as described above in Chapter 3. Romer uses an industrial production index and Hamilton works in GNP growth rates. The exercise shows that, at least for the US GNP, the turning points found directly by the program are nearly the same as those found by 'indirect' methods. The window length is hence justified empirically, and has been used for all series in the study for comparative purposes. This result also quite strongly suggests that the 'indirect' dating procedures are tracking US aggregate fluctuations in levels (and perhaps growth-rate regimes as well) rather than comovements.

Table 5.3.2. *Turning Points in the US 'Reference Cycle', GNP and Other Measures, 1947–1983*							
NBER (Year: Quarter)		Romer (Year: Quarter)		Hamilton (Year: Quarter)		GNP (Program) (Year: Quarter)	
Peaks	Troughs	Peaks	Troughs	Peaks	Troughs	Peaks	Troughs
1948:4	1949:4	1948:4	1949:4	NA	NA	1948:4	1949:2
1953:3	1954:2	1953:3	1954:3	1953:3	1954:2	1953:2	1954:4
1957:3	1958:2	1957:3	1958:2	1957:1	1958:1	1957:3	1958:1
1960:2	1961:1	1960:2	1961:1	1960:2	1960:4	1960:1	1960:4
1969:4	1970:4	1969:4	1970:4	1969:3	1970:4	1969:3	1970:1
1973:4	1975:1	1973:4	1975:3	1974:1	1975:1	1973:4	1975:1
1980:1	1980:3	1980:1	1980:3	1979:2	1980:3	1980:1	1980:2
1981:3	1982:4	1981:3	1983:2	1981:2	1982:4	1981:3	1982:3

Sources:

NBER and Romer dates: Romer [1992] Table 2.

Hamilton dates: Hamilton [1989] Table II.

GNP: program output.

Some idea of how the algorithm performs can be inferred from its findings of average 'cycle' length for the various series, or, in this case, the mean distance between peaks and troughs. For the main UK aggregates of Feinstein and Lewis, m.d.(peaks) and m.d.(troughs) are in the range four to six years, but with large sample standard deviations from, in all cases, very small samples. The GDP and industrial production specific-cycle lengths differ by about 33 percent. This may not be significant; if it is, however, it would indicate that specific fluctuations are not generally in phase. These results accord well with Kendall's findings and those in Rostow's later work, both of which show m.d.(peaks) of the order of five to 5¼ years. Details are presented in Table 5.3.3.

Although emphasis on the 'clustering' of turning points is complicated by the need to address the issue of leads and lags it nevertheless remains central. Clustering or 'comovement' implies that the majority of series, both aggregates and constituents (apart from 'inverted' series such as unemployment), must have local maxima and minima in the same years. If such behaviour were observed in the actual data, the distribution of the number of years in the sample period for each annual turning point frequency would show a large number of years with no turning points, and some years in which all turning points are concentrated. Frequency distributions for annual data with perfect clustering are shown as histograms in Figure 5.3.7(a). The assumption is made of ten annual series with regular cycles over a 60-year sample period. All series reach peaks and trough in the same years; and distributions for four-, six- and eight-year cycles are shown. Thus, for example, for the four-year cycle the graph shows half the years (30) with zero turning points and half with ten — the maximum number. (Numbers of years for each frequency are shown on or above the bars.) To give effect to the possibility of lead/lag relations, a similar analysis for 16 series with eight-years cycles, showing clustered, dispersed and 'unseen' cycle behaviour, is given in Figure 5.3.7(b). The assumption here is that in each half-cycle of four years the unseen cycle has one, two, five and eight turning points respectively, thus building to concentrations of half the total number of series at four-year intervals. (A perfectly uniform distribution of leads and lags over time would also produce the 'spiked' histogram indistinguishable from that of randomly-dispersed tuning points.)

Table 5.3.3. UK Macroeconomic Series 1852-1913

Distance between Peak and Troughs

Series	M.D.(peaks) (Standard Deviation)	M.D.(troughs) (standard Deviation)
GDP (Lewis)	6.40	6.25
	(5.20)	(3.23)
Industrial Production (Lewis)	4.64	4.42
	(4.29)	(2.90)
Industrial Production (Feinstein)	4.20	5.56
	(3.60)	(3.72)
Manufacturing (Feinstein)	4.27	4.17
	(3.89)	(2.61)

For quarterly data the distribution is slightly more complicated because assumptions must be made about the incidence of turning points within the year. An example of this is shown in Figure 5.3.8 for 16 series over a 120-quarter sample period with a four-year cycle. Distributions are shown for the following assumed behaviour: (1) all series turn in the third quarter of years in which turns occur; (2) half the series turn in the second and third quarters respectively; and (3) five series turn in quarters II and III, and three in quarters I and IV. In this case the graph also shows the frequency with no clustering (i.e., with an equal number of turns in all quarters of the sample period).

5.4. EMPIRICAL RESULTS

This section presents the results of the analysis of unrestricted turning-point incidences for the data sets listed in Appendix 5A. Section 5.4.1 shows the frequency distributions of turning points per period, while Section 5.4.2 analyses lead/lag relations within data sets and across aggregates.

5.4.1. Turning-Point Frequency Distributions

The summaries of the turning point frequencies for nine annual UK data sets for the pre-1914 period are shown in Figures 5.4.1 to 5.4.10. For purposes of comparison, distributions for systematic behaviour — clustered and dispersed — for the same number of series over the same sample period are also given by way of comparison, with the assumption of a four-year cycle.[37] Although these data sets are diverse in character, most have comparatively flat distributions, truncated at the upper end. These distributions indicate the most important feature of empirical behaviour: that the numbers of (unrestricted) turning points varies markedly in series from the same data set and across data sets: the 'extra' or 'missing' turns commonly 'weeded out'. Some pre-1914 series have instances of multiple peaks and troughs, though not occurring frequently enough to alter the general results, as shown in Figure 5.4.11 for the Feinstein GNP expenditure series. The latter are also analysed for the period 1948-1980, and show a similar pattern of turning points (Figure 5.4.12), but no extra turns are found. With the exception of US GNP, all the UK and US postwar quarterly series have non-alternating turning points and multiple peaks and troughs. Distributions are shown both with and without these anomalies in Figures 5.4.13 to 5.4.16, with systematic behaviour shown for a four-year cycle. As above, assumptions are made about the pattern of turning points in the quarters of the year. For the eleven US series, all turns are assumed to cluster in quarter III of the given year; whereas for the 23 UK series, double the number, turns are assumed to be spread equally over quarters II and III. Again, the number of multiple turns in each sample is not sufficient to change the result: both sets appear to show behaviour similar to the annual series for the earlier period.

[37]Where the length of the cycle does not divide the number of series the dispersion of turning points is imperfect, since the number of turning points is an integer function.

5.4.2. Analysis of Leads and Lags

Ever since the Burns and Mitchell 1938 study of leading indicators, the identification of stable timing relations among variables, leading, coincident or lagging, has been among the principal concerns of empirical business-cycle research. The necessary condition for such lead/lag behaviour, that all series have equal numbers of turning points so that direct comparisons can be made, is not found empirically in any of the data sets in the present study, as can be seen from the first columns of Tables 5.4.2(a) to 5.4.8(a). As the above frequency distributions reveal, different series have 'extra', 'missing' or multiple peaks and troughs — the problem earlier identified by Bry and Boschan — and this applies both across aggregates and between aggregates and their constituent series. It is a familiar result in historical business-cycle research: for example, Aldcroft and Fearon found 22 turning points in UK GNP against 25 in industrial production for the period 1836-1913.[38]

This section analyses pre-1914 and post-1945 time-series data for evidence of stable leads and lags between aggregates and their supporting series. As these are defined only when both series have the same number of peaks and troughs, and as such behaviour is seldom found in the 'real world', two approaches are employed here. The first evaluates pairwise conformity of turns when none are excluded; the second analyses timing relations when the 'extra', non-equivalent, turns in the noisier series are edited out. ('Conformity' is here defined in a narrow sense: those turning points that either coincide or else occupy the same positions in their respective sequences.)[39] The latter approach is unsatisfactory for two reasons: first, because non-conformity is the rule rather than the exception empirically; and second, because the search for conforming turns requires some degree of subjective 'judgment' and is therefore likely to induce 'observer bias'. Thus the 'editing' of turning points must be treated with caution: it was the

[38] The series in question were taken from Feinstein [1972], pp. 9, 12.

[39] The series with the smallest number of turning points is taken as a reference, and the turns in the other series which most closely coincide with these reference turns are retained. Coincident turns in the two series are thus emphasized.

approach employed by both Mitchell and Beveridge with disappointing and ambiguous consequences.

Results for nineteenth century (annual) data are shown in Tables 5.4.1 to 5.4.5. The wide variation in numbers of unedited turning points make them poor contemporaneous predictors of peaks and troughs in the aggregates. Turning points for Feinstein's GNP expenditure series and its consumers expenditure constituent (whose value is approximately 80 percent of the aggregate) are given for the period 1830-1913 in Table 5.4.1. Consumption is somewhat smoother than GNP over this period (see plot in Figure 5.4.17). With the first (unedited) approach the proportion of coincident turning points is 0.5, or approximately that which would occur randomly (because in this case there are no instances of lags); and the proportion of leads is 0.14.[40] Because of the different number of turns, the 'odds' on the turns being equivalent are about 0.6 (i.e., 22/36). Differences in 'smoothness' can be indicated by the average amplitude and duration of downturns (i.e., from peaks to troughs). The average decline is approximately 4½ times greater in GNP than in consumption; and the average duration of downturns is about 30 percent greater.

This result suggests a lack of system in timing relations which is reflected in other data sets. Examples of these are Feinstein's aggregates, industrial production and investment series, 1855-1913 (Tables 5.4.2(a) to 5.4.4(a)); and the Board of Trade commodity price series, 1871-1913 (Table 5.4.5(a)). With their turning points unedited few show a score sufficiently high to classify them as indicators of the aggregate. When the second approach is applied, peaks and troughs in GNP (expenditure) and consumption coincide in about 80 percent of occurrences; but this is exceptional. For other nineteenth century data, shown in tables 5.4.2(b) to 5.4.5(b), there is no strong evidence of consistent timing relations across the variables. The exceptions are manufacturing and transport and communications (Table 5.4.4(b)), both of which coincide

[40] The proportions are calculated by scoring +1 for a coincidence, lead or lag, and 0 otherwise. 'Extra' turning points are scored as 0's. The proportion is then the arithmetic mean of the sequence of zeros and units.

with the industrial production aggregate in about 80 percent of cases; and food and drink prices (Table 5.4.5(b)), with a 'hit rate' of 74 percent. In the latter case, because the series has the same number of (unedited) turns as the aggregate, it is the only adequate natural coincident indicator of all the pre-1914 series tested.

The postwar data sets, shown in Tables 5.4.6, 5.4.7 and 5.4.8, reveal similar non-systematic behaviour. US variables tend to have as few as two-thirds and as many as 2½ times the number of turning points as GNP, which makes them even less effective contemporaneous predictors than UK series. When restricted to equivalent peaks and troughs, turns tend to be evenly spread among coincidences, leads and lags. Only exports, whose lags are 71 percent of the total, and durable goods, with 64 percent leads, approach consistency. UK variables are similar, but with the proportions of leads, lags and coincidences even more evenly spread for equivalent turns (Table 5.4.7(b)). UK industrial production series show a reasonably close coincidence between total manufacturing and the aggregate, and a leading relationship between the aggregate and transport and communication (Table 5.4.8(b)). However, even though these two series have the same number of unedited peaks and troughs, these are not all equivalent: for example, the peaks in manufacturing in 1961, 1965 and 1978, the peak in production in 1973, and trough in manufacturing 1979, and in production in 1985, are not matched in the other series (shown in bold type in Table 5.4.9).

As a rule, behaviour changes over time, as can be seen from tables 5.4.10, 5.4.11 and 5.4.12. Both US and UK variables (in both periods) tend to show unpredictable movements from leads to coincidences to lags. The length of leads and lags can be volatile, as shown in the examples of pairwise timing relations for postwar UK and US quarterly time series (Figures 5.4.18 to 5.4.30). Most leads and lags fall within ±4 quarters for the US data, but in four of the comparisons rise to as much as ±9 quarters. The UK is more volatile, with leads or lags as great as 15 quarters. Both economies have unstable timing relations, moving from leads to coincidences to lags. When common turning points among more than two variables are considered, the analysis breaks down.

In the UK, for example, only one conforming turning point (in 1979) was found for five industrial production series: the aggregate, total manufacturing, construction, distribution and transport and communication.

Table 5.4.1. Real GNP (Expenditure) and Consumption Turning Points, 1830–1913			
GNP		Consumption	
Peaks	Troughs	Peaks	Troughs
1831	1832	–	–
1836	1837	1836	1837
1839	1842	1839	1842
1846	1847	1846	1847
1849	1850	1849	1850
1857	1858	1854	1855
1859	1860	1857	1858
1871	1872	–	–
1874	1877	–	–
1878	1879	1878	1879
1880	1881	1880	1881
1884	1885	–	–
1891	1893	1891	1892
1896	1897	–	–
1899	1900	1899	1900
1901	1903	–	–
1907	1908	1907	1908
1911	1912	–	–
Total Turning Points: 36		Total Turning Points: 22	

Table 5.4.2. Timing Relations, Feinstein's UK Aggregates 1855-1913[41]							
(a): Unedited					(b): Edited		
		Proportion to Aggregate of:			Proportion to Aggregate of:		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
GNP (expenditure)	27						
Consumers expenditure	14	0.35	0.19	0.00	0.64	0.36	0.00
Industrial production	14	0.31	0.15	0.08	0.57	0.29	0.14
Investment	15	0.00	0.27	0.31	0.00	0.50	0.50
Unemployment	19	0.19	0.19	0.31	0.28	0.28	0.44

[41]Source: Feinstein [1972].

Table 5.4.3. Timing Relations, Feinstein and Pollard Investment Series 1855–1913

(a): Unedited					(b): Edited		
		Proportion to Aggregate			Proportion to Aggregate		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
Manufacturing investment	15	0.40	0.13	0.47	0.40	0.13	0.47
Railway investment	17	0.21	0.26	0.21	0.31	0.38	0.31
Agricultural investment	13	0.16	0.26	0.11	0.30	0.50	0.20
Investment in dwellings	19	0.32	0.14	0.09	0.58	0.25	0.17
Mining and quarrying investment	18	0.09	0.18	0.23	0.18	0.36	0.45
Distributive investment	8	0.18	0.18	0.00	0.51	0.50	0.00
Transport and Communication investment	25	0.20	0.40	0.00	0.33	0.67	0.00

Table 5.4.4. *Timing Relations, Feinstein's Industrial Production Series 1855–1913*							
(a): Unedited					(b): Edited		
		Proportion to Aggregate of:			Proportion to Aggregate of:		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
All Industries	14						
Manufacturing	22	0.59	0.00	0.05	0.85	0.07	0.07
Chemicals	18	0.20	0.35	0.05	0.33	0.33	0.33
Metals	26	0.19	0.19	0.15	0.36	0.36	0.28
Engineering	22	0.35	0.22	0.43	0.57	0.36	0.07
Textiles	25	0.24	0.16	0.16	0.42	0.29	0.29
Food	22	0.32	0.09	0.23	0.50	0.14	0.36
Other Manufacturing	12	0.50	0.06	0.06	0.80	0.10	0.10
Building	20	0.40	0.20	0.10	0.57	0.29	0.14
Transport and Communications	10	0.57	0.14	0.00	0.80	0.20	0.00

Table 5.4.5. *Timing Relations, Board of Trade Price Series 1871–1913*							
(a): Unedited					(b): Edited		
		Proportion to Aggregate of:			Proportion to Aggregate of:		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
Aggregate	20						
Textiles	20	0.40	0.50	0.10	0.40	0.50	0.10
Coal	14	0.35	0.10	0.24	0.50	0.14	0.36
Food and Drink	19	0.70	0.05	0.20	0.74	0.05	0.21
Miscellaneous Materials	15	0.50	0.20	0.05	0.79	0.21	0.00
Corn	17	0.38	0.29	0.05	0.50	0.38	0.06

Table 5.4.6. Timing Relations, US Macroeconomic Time Series 1947–1983							
(a): Unedited					(b): Edited		
		Proportion to Aggregate of:			Proportion to Aggregate of:		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
GNP	14						
Producers durable equipment	26	0.19	0.00	0.35	0.36	0.00	0.64
Residential structures	24	0.13	0.29	0.17	0.21	0.50	0.29
Nonresidential structures	25	0.15	0.04	0.26	0.33	0.08	0.58
Durable goods	36	0.03	0.25	0.11	0.07	0.64	0.29
Nondurable goods	9	0.21	0.29	0.07	0.38	0.50	0.13
Government expenditure	21	0.00	0.33	0.33	0.00	0.50	0.50
Exports	30	0.07	0.07	0.33	0.14	0.14	0.71
Imports	33	0.09	0.18	0.15	0.21	0.43	0.36

Table 5.4.7. *Timing Relations, UK Macroeconomic Time Series 1955–1993*							
(a): Unedited					(b): Edited		
		Proportion to Aggregate of:			Proportion to Aggregate of:		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
GDP	23						
Retail sales	30	0.17	0.14	0.20	0.33	0.28	0.39
Industrial production	31	0.26	0.18	0.15	0.45	0.30	0.25
Government final consumption	36	0.04	0.11	0.16	0.14	0.36	0.50
Total manufacturing	31	0.31	0.14	0.09	0.58	0.26	0.16
Vacancies at job centres	49	0.02	0.12	0.25	0.05	0.30	0.65

Table 5.4.8. Timing Relations, UK Industrial Production Series 1955–1993							
(a): Unedited					(b): Edited		
		Proportion to Aggregate of:			Proportion to Aggregate of:		
Series	Number of Turning Points	Coincidences	Leads	Lags	Coincidences	Leads	Lags
Aggregate	31						
Total manufacturing	31	0.54	0.06	0.20	0.70	0.07	0.22
Engineering	40	0.37	0.05	0.23	0.57	0.07	0.36
Chemicals	23	0.22	0.11	0.17	0.44	0.22	0.33
Textiles	40	0.24	0.37	0.12	0.33	0.50	0.17
Construction	32	0.08	0.26	0.32	0.12	0.40	0.48
Distribution	23	0.11	0.26	0.17	0.21	0.47	0.32
Transport and communication	21	0.13	0.38	0.13	0.20	0.60	0.20

Table 5.4.9. UK Industrial Production and Manufacturing: Turning Points 1955–1992

Industrial Production		Total Manufacturing	
Peaks	Troughs	Peaks	Troughs
	1958Q3		1958Q3
		1961Q2	
	1961Q1		
1961Q4	1962Q1		1962Q1
1962Q3		1962Q3	
...	...	...	...
		1965Q1	
1966Q3		1966Q1	
	1966Q4		1966Q4
...	...	...	...
	1972Q1		1972Q1
1973Q3			
	1974Q1		1974Q1
...	...	...	...
	1977Q3		1977Q3
		1978Q3	**1979Q1**
1979Q1		1979Q2	
...	...	...	...
1985Q2	**1985Q4**	1989Q2	
1990Q2		1990Q2	

(Not all turning points are shown; gaps are indicated by ellipses. Non-equivalent turning points are shown in bold type.)

Table 5.4.10. Feinstein's Aggregates: Changes in Timing against GNP, 1855–1913		
Variable	Period	Relation
Consumers Expenditure	1855–1860	Leading
	(1860–1878	No turning points in either series)
	1878–1891	Coincident
	1891–1898	No turns
	1898–1913	Coincident
Industrial Production	1855–1858	Coincident
	1858–1878	Leading
	1878–1902	Alternate leading and lagging
	1902–1913	Coincident
Investment	1855–1877	Leading
	1877–1883	Lagging
	1883–1886	Leading
	1886–1913	Lagging
Unemployment	1855–1865	Alternate coincident, leading and lagging
	1865–1872	Leading
	1872–1886	Lagging
	1886–1900	Alternate coincident, leading and lagging
	1900–1906	lagging
	1906–1913	Coincident

Table 5.4.11. US Data: Changes in Timing against GNP, 1947–1983		
Variable	Period	Relation
Producers Durable Equipment	1948–1958	Alternating leads, lags and coincidences
	1958–1969, 1970–1975	Lagging
	1969, 1980	Coincident
Nonresidential structures	1948–1960	Lagging
	1960–1992	Alternating
Durable Goods	1948–1958	Leading
	1958–1961	Lagging
	1961–1982	Alternating
Nondurable Goods	1947–1983	Alternating
Government Expenditure	1948–1957	Lagging
	1957–1970	Coincident
	1970–1982	Alternating (lead/lag)
Exports	1947–1954	Lagging
	1954–1960	Alternating (coincident/lead)
	1960–1983	Lagging
Imports	1947–1983	Alternating

Table 5.4.12. UK Data: Changes in Timing against GDP, 1955–1992		
Variable	Period	Type
Retail Sales	1955–1992	Alternating
Industrial Production	1955–1957	Leading
	1957–1962	Lagging
	1962–1992	Alternating (coincident/lead)
Government Expenditure	1956–1974	Alternating
	1974–1992	Lagging
Total Mfg.	1955–1992	Alternating
Vacancies at Job Centres	1956–1962	Alternating
	1962–1985	Lagging
	1985–1992	Alternating (lead/lag)

5.5. SHOCKS, STRUCTURAL CHANGES AND SUBJECTIVE ASSESSMENTS

This section attempts to assess the issue of the recurrence of fluctuations (a) by showing evidence of structural change in the US and UK economies over time (which raises questions of comparability across episodes), and (b) by comparing time-series data with Thorp's annals for the period 1855-1913.

5.5.1. Structural Changes over the Sample Periods

The industrial economies were growing at a rapid, albeit uneven, rate during the half-century up to 1914. Structural changes which occurred can be thought of in three main categories: changes in regime, qualitative and quantitative changes. Regime changes in the UK are evident e.g. in agriculture, with the repeal of the Corn Laws, in trade with the general lowering of tariffs and in the increasing stability of the banking system after 1870. Quantitatively, sectoral shares in total output varied considerably, and such changes give an indication of the evolution of economic structure. As discussed by Crafts and Mills [1995], there were also structural instabilities in nineteenth century UK growth rates which are 'unlikely to be characterized by a single shift in a linear trend function'. Qualitative changes occurred in technology, e.g., in faster and more productive land and sea transport and in the mechanization of manufacturing industry. This process has been described in the above paper as the effect of 'technological surprises which could change the trend rate of growth' but which are not 'fully endogenous'. The revolutionary 'macroinventions', those which emerged 'ab nihilo', produced initial productivity shocks; but the development process for a given technology, the 'microinventions', was subject to diminishing returns, which meant that the effects of growth innovations would eventually damp out.

For quantitative assessments, sectoral shares of total UK output are available for the pre-1914 period (Lewis), and for the post-1950 period in the annual *Blue Book* estimates. Additionally, shares of total investment in the UK have been calculated by Feinstein and Pollard for the period 1855-1913. Most of these data are in current prices, and so comparisons over time are shown in percentage terms. Table 5.5.1 gives the proportions of GDP for the period 1852–1912 at decennial intervals from Lewis [1978]. The largest fall was in agriculture, from about 20 percent in 1852 to less than seven percent in 1913, the importance of which on employment and geographical consumption patterns cannot be over-estimated. The share of the distributive industries grew by about one-quarter over the period. Manufacturing, which already accounted for 26 percent in 1852, grew by nearly 30 percent in share of total output (from 26 to 34 percent). Income from rents, the mainstay of the middle class according to Keynes, fell by three-eights in share from a base level of about 13 percent in 1852. Such shifts in the proportions of output, together with changes in the size and longevity of the population, and in technology and infrastructure make comparisons over more than a few years problematical; and certainly suggest that to treat the entire period as a sample from a single population is stretching a point if not testing it to destruction.[42]

Table 5.5.1. Lewis's UK GDP Constituent Series

Percent of GDP at Decennial Intervals 1852–1912

Series	1852	1862	1872	1882	1892	1902	1912
Manufacturing and mining	26.0	27.4	31.0	32.6	31.3	32.5	33.8
Distribution	12.8	13.6	14.7	15.2	15.6	15.7	16.1
Rent	13.0	11.8	10.6	9.9	9.4	8.5	8.1
Construction	4.2	4.4	4.4	4.1	3.7	5.1	3.1
Transport of Goods	3.7	3.9	4.3	4.5	4.5	4.6	4.7
Professions	3.5	3.9	4.0	4.7	5.3	5.4	5.9
Agriculture	19.7	17.0	13.4	10.7	10.1	7.9	6.7

Table 5.5.2 shows the changes in sectoral shares of total UK investment to 1913. The figures confirm the impression of rapid change, but here the shares of the total are volatile, with only railway and agricultural investment showing persistent declines. In the period since 1945, shown in Table 5.5.3, agriculture has continued to decline, but so has

[42]Cf. the Rostow [1972] assessment of structural changes in the UK economy up to 1914.

manufacturing, by about 35 percent in share. The public and financial services sectors have grown by 175 and 600 percent respectively, while the service sectors appear to have maintained relatively stable shares.

Table 5.5.2. Feinstein and Pollard UK GDFCF Series

Percent of Total at Decennial Intervals 1853–1913

Series	1853	1863	1873	1883	1893	1903	1913
Manufacturing	21.5	17.3	16.8	14.6	18.4	25.3	25.2
Transport and Communication	11.2	14.5	14.3	23.2	16.7	17.5	26.9
Railway Investment	16.8	23.3	14.4	15.3	12.9	9.0	7.0
Dwellings	22.9	15.1	20.3	19.6	19.5	19.1	8.1
Agriculture	13.0	13.6	10.2	9.8	7.5	4.3	5.7
Public Service	3.6	5.2	7.0	6.5	10.4	8.1	7.1
Distribution	4.0	5.6	9.0	6.0	8.6	6.9	9.4
Gas, Water and Electricity	4.6	2.8	2.9	3.6	5.3	7.9	5.1
Mining	2.4	2.6	5.4	1.5	0.9	2.0	5.6

Table 5.5.3. UK GDP Constituent Estimates

Percent of Total at Decennial Intervals 1952–1992

Series	1952	1962	1972	1982	1992
Manufacturing	34.5	34.2	31.2	25.8	22.3
Public Sector	9.9	10.1	13.0	16.0	17.3
Financial Services	2.6	3.4	7.9	11.7	16.4
Mining and Quarrying	3.7	3.0	1.4	7.4	1.9
Agriculture	5.6	3.9	2.8	2.3	1.8
Distribution	12.6	11.9	11.1	12.9	14.1
Transport and Communication	8.7	8.4	8.4	7.5	8.1
Construction	5.5	6.4	7.8	6.0	6.2

5.5.2. Shocks and Annals

Mitchell's reference dates, were largely derived from Thorp's Annals and do not correspond all that well with the historical data sets. Figure 5.5.1 shows Lewis's GDP series for the UK with the years of acknowledged crisis and Thorp's years of 'depression'.

In general, a crisis was followed by a depressed sequence wherein the level of GDP tended to decline for a few years thereafter, but this was not universal. 1873 was a crisis year and one of strong growth, and after 1857 growth continued with a one-year interruption of small magnitude. Volatility increases in magnitude over time, but some of the 'depressed' years have strong growth. Figure 5.5.2 shows the same analysis for the Crafts-Harley [1992] industrial production series from 1811 to 1913. Again, volatility increases in the latter period, but here growth is strong throughout, and increasing. The depressed years show less conformity with industrial production, but it must be remembered that up to 1900, this represented less than half of total output.

The conformity with constituent series is even less persuasive than with the aggregates. For Feinstein's GDP series, his aggregate industrial production and its constituents from 1855 to 1913, only about half conform to Thorp's years of economic stress, i.e., of 'recession' or 'depression'. ('Conformity' here is defined by the series having an annual change in levels of +1.0 percent or less in those years.) However, 87 percent conform in prosperous years. This is either an indication of the known asymmetries associated with persistent economic growth, as analysed in Chapter 4 above, or perhaps of the 'adjustments' made by Feinstein to conform to Thorp. Conditions in a little less than half (42 percent) of these years were stressful, which is another indication of asymmetry but not a very strong one. Significantly, all the distressed years had rising unemployment according to the Feinstein index (i.e., rising among trades union members). As previously discussed, prices and profits fell until about the mid-1890s with the same persistence with which real activity tended to rise, and contemporary opinion about conditions seems to have been decisively influenced by these nominal factors.

5.6 SUMMARY

This chapter has attempted to assess whether directly-observable quantitative evidence supports the comovement hypothesis; or alternatively, whether such fluctuations are unique historical episodes without a kernel of empirical regularities. The postwar historical literature, which deals in the main with the nineteenth century UK economy, initially assumed that the nature of short-run fluctuations was as described by Burns and Mitchell and by Beveridge. Matthews and Hughes, in the 1950s and early 1960s, questioned their 'pervasiveness'; Aldcroft and Fearon found different numbers of turning points in two UK aggregates, suggesting a lack of conformity in short-run dynamics. Rostow, who originally accepted the traditional historiography, by the 1970s also had second thoughts about the generality of such behaviour, finding, for example, contradictory conditions during the 'Great Depression' of the late nineteenth century. He concluded that fixing a business-cycle chronology requires considerable exercise of 'judgment' because the 'points of demarcation' are 'untidy'; and that furthermore, the marked structural changes over the period 1790-1914 made comparisons across episodes difficult.

Eichengreen in the 1980s pointed out the circularity of argument in all discussions of 'business cycles' and presented a model which also indicated structural change in the late nineteenth century. Such scepticism is also evident in later research by *inter alia* Friedman, Solomou and Dimsdale. The work of Crafts *et al.* is as much about the (considerable) structural changes in growth rates over the course of the nineteenth century as about short-run 'cycles'. The CLM analysis of cycles is limited to three estimates of aggregate industrial production. It is not clear whether the same structural model fitted to a different aggregate — GDP, for example — would produce a conforming turning-point chronology.

The main results given in Section 5.4 are in accord with much that is at least implicit in the historical literature. There is a wide variation in numbers of turning points across many economic time series when these are found objectively. This is the essential difference between 'real-world' behaviour and the behaviour of unobserved-components models. It applies both to nominal and to real series and accounts for the comparatively flat histograms shown in Figures 5.4.1 to 5.4.16. The subjective assessment of business conditions, Thorp's *Annals,* which was the basis of the NBER 'reference' chronology, is not supported by recent time-series estimates. Even the presence of deliberate bias towards conformity with existing reference dates — as in Lewis's and Feinstein's data — does not yield strong evidence of comovements. Timing relations are volatile, with most constituents showing a mixture of lags, coincidences and leads, and with the lengths of leads and lags also showing wide variation; this result despite the editing of turning points which emphasizes conformity. Evidence of structural change reinforces the view of an episodic rather than recurrent nature of short-run fluctuations.

The difficulty with all such inferences is that the data are inaccurately estimated in the sense discussed by Feinstein [1972]. Apart from prices, they may even be too inaccurate to be of any use in finding turning points, owing to the magnitude of the published margins of error. This, however, represents a counsel of perfection; in the current instance the similarity of turning-point behaviour in a large number of data sets gives at least an indication of divergence which cannot be completely ignored. Likewise the general instabilities in timing relations must be tentatively acknowledged. Whether more accurate data would confirm or deny such irregularities is moot. However, the similarity of results across data sets suggests, subject to the above 'health warning', that the comovement hypothesis, as proposed on page 3 of MBC, and on which subsequent business-cycle research has largely been based, is not supported by time-series data.

Appendix 5A

Details of Data Sets

Details of each empirical data set are as follows:

Table 5A.1. Feinstein's UK Constituent Series 1855–1913	
Macroeconomic[43]	Investment (GDFCF)[44]
Aggregate industrial production	Agriculture
Agricultural output	Mining and quarrying
Construction	Manufacturing
Chemical industries	Gas, water and electricity
Distributive trades	Distribution and other services
Engineering industries	Railways
Food processing	Other transport and communications
Gas generation	Public and social services
Metal industries	Dwellings
Total manufacturing	Aggregate GDFCF
Mining and quarrying	
Other industries	
Transport and communication	
Textiles	
Unemployment	
Gross domestic product	
GNP 1830-1913 and 1948–1980[45]	
Real GNP (expenditure) at market prices	
Consumers expenditure	
Public authorities current expenditure	
Value of physical increase in stocks	
Net exports	
GDFCF	

[43]Source: Feinstein [1972].

[44]Source: Feinstein and Pollard [1988], Appendix Table IX.

[45]Source: Mitchell [1988] 'National Accounts 6', pp. 837–844.

Table 5A.2. Lewis's UK Macroeconomic Series 1852–1913[46]	
GDP Series	Industrial Production Series
Aggregate	Aggregate
Agriculture	Iron and steel
Manufacturing and mining	Iron and steel products
Construction	Shipbuilding
Shipping	Building materials and construction
Transport of goods	Houses
Transport of people	Commercial building
Distribution	Other construction
Finance	Clothing
Professions	Textile finishing
Catering	Printing and materials
Domestic service	Chemicals
Government	Electricity
Defence	Food manufacture
Rent	Gas
Miscellaneous services	

Table 5A.3. Price Series 1871–1913[47]	
Lewis [1978]	Board of Trade
Manufactured goods	Wholesale aggregate
Wheat	Food and drink
Cotton	Textiles
Wool	Coal
Coffee	Miscellaneous materials
Tropical crops	Corn
Cereals	Animal products
Freights	Sugar
	Wine

[46]Source: Lewis [1978], Appendix I, Tables A.1 and A.3.

[47]Source: Mitchell [1988] Chapter XIV, Table 5.

Table 5A.4. Central Statistical Office Real Quarterly Macroeconomic Series 1957–1993[48]	
Series	CSO Identifier
GDP	DKHJ
Index of output: agriculture	CKAP
Index of output: distribution, hotels and catering	CKAQ
Index of output: transport and communication	CKAR
Index of output: other services	CKAS
Index of output: total service industries	CKCE
Index of output: construction	DVJO
Total production	DVZI
Mining and quarrying	DVZJ
Total manufacturing	DVZK
Food, drink and tobacco	DVZL
Textiles, clothing and footwear	DVZM
Fuels	DVZN
Chemicals and man-made fibres	DVZO
Engineering industries	DVZQ
Other manufacturing	DVZR
Gas, electricity and water supply	DVZS
New car registrations	DKBY
General government final consumption	DIAT
Level of manufacturing stocks	DKCI
Manufacturing investment	DKCK
Retail sales volume	FAAM
Vacancies at job centres	BCOM

[48]Source: CSO Databank, July 1994.

Table 5A.5. Official US Macroeconomic Series 1947–1983[49]
GNP
Changes in business inventories
Durable goods consumption
Non-durable goods consumption
Residential structures
Non-residential structures
Producers durable equipment
Exports
Imports
Government purchases

[49]Source: R.J. Gordon (ed.) [1986] Appendix B.

Appendix 5B

Chapter 5 Graphics

Figure	Page

Figure 5.3.1. Feinstein's UK Industrial Production Series 1855-1913

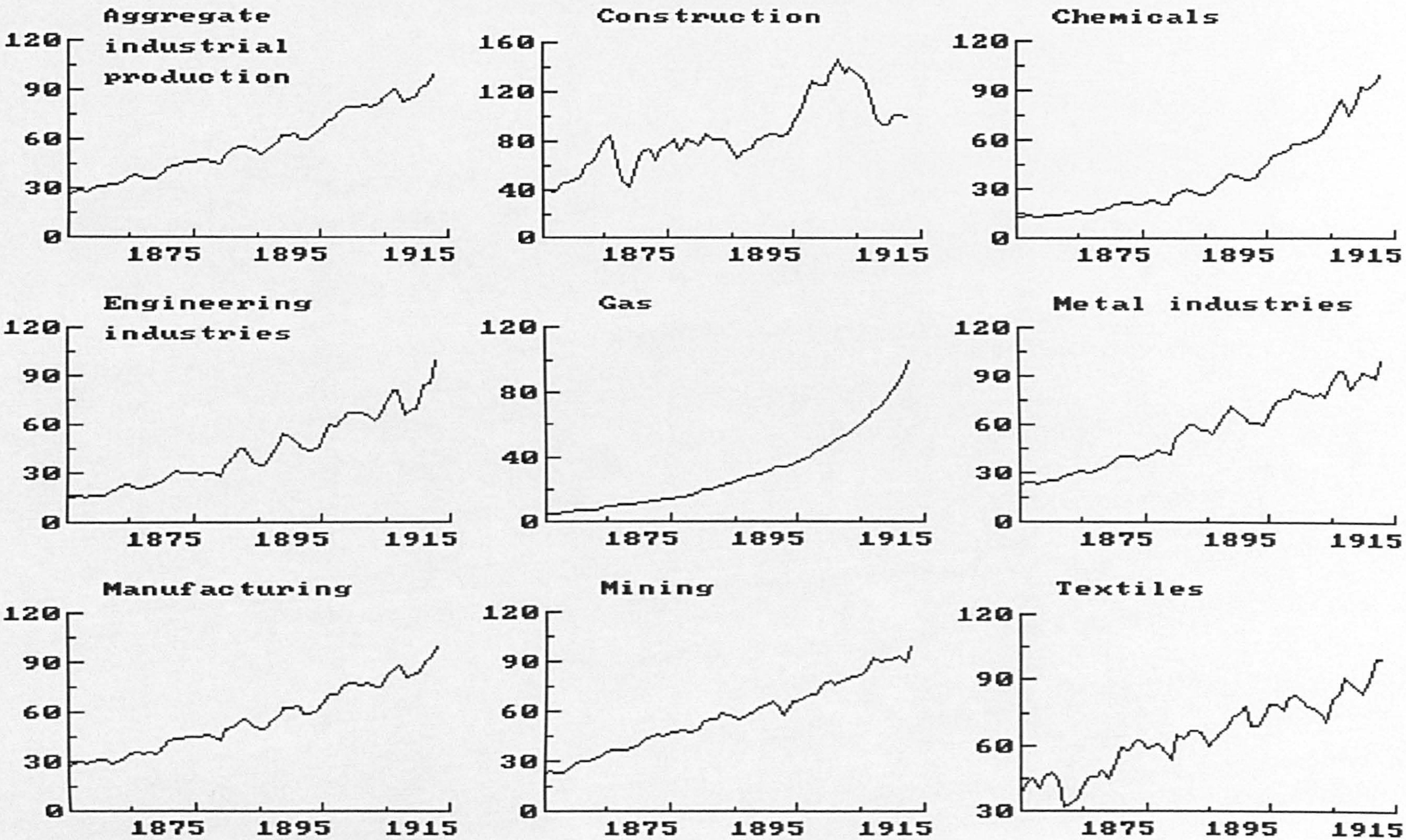


Figure 5.3.1 (Continued)

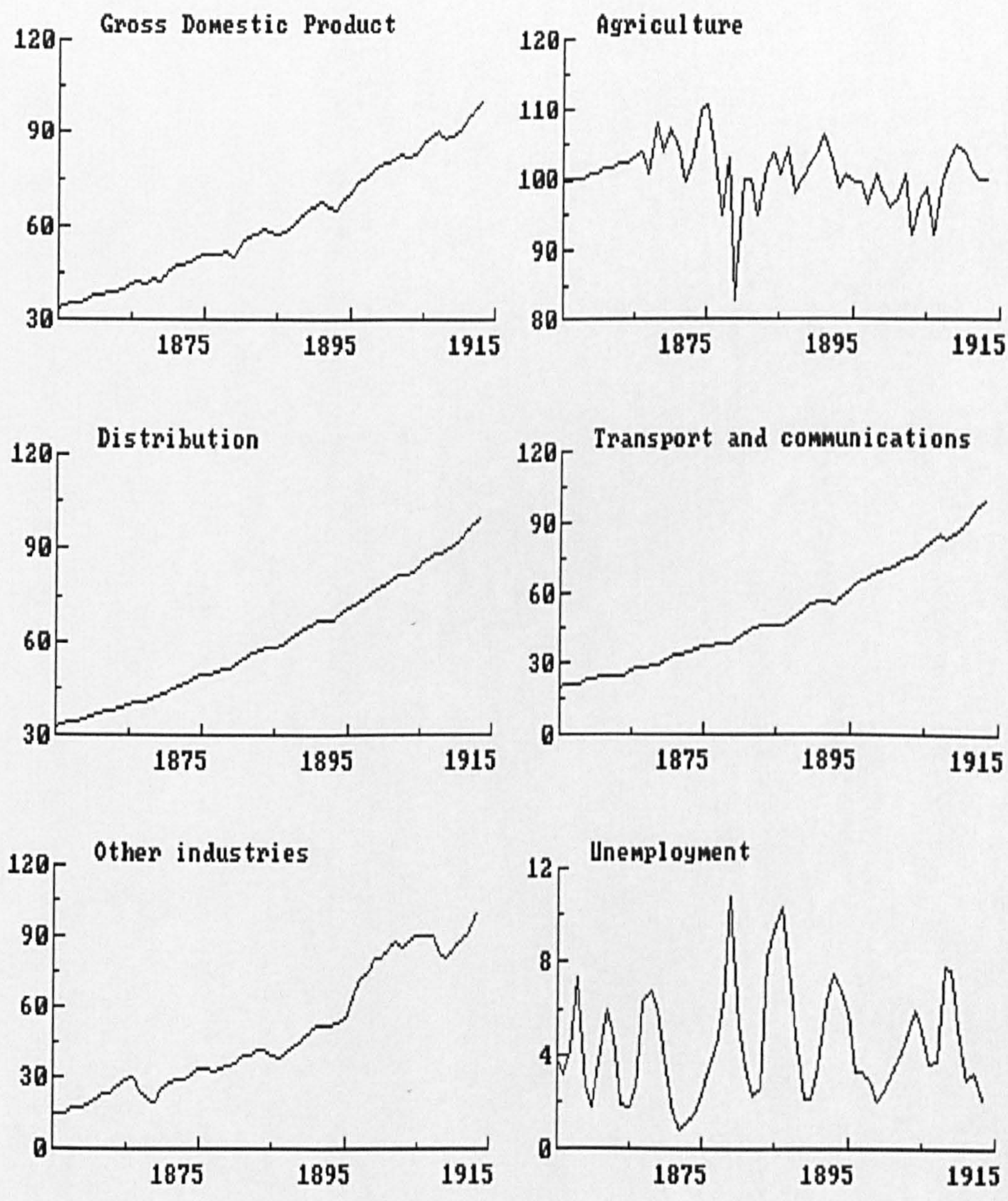


Figure 5.3.2. The Feinstein and Pollard Investment Series 1855–1913

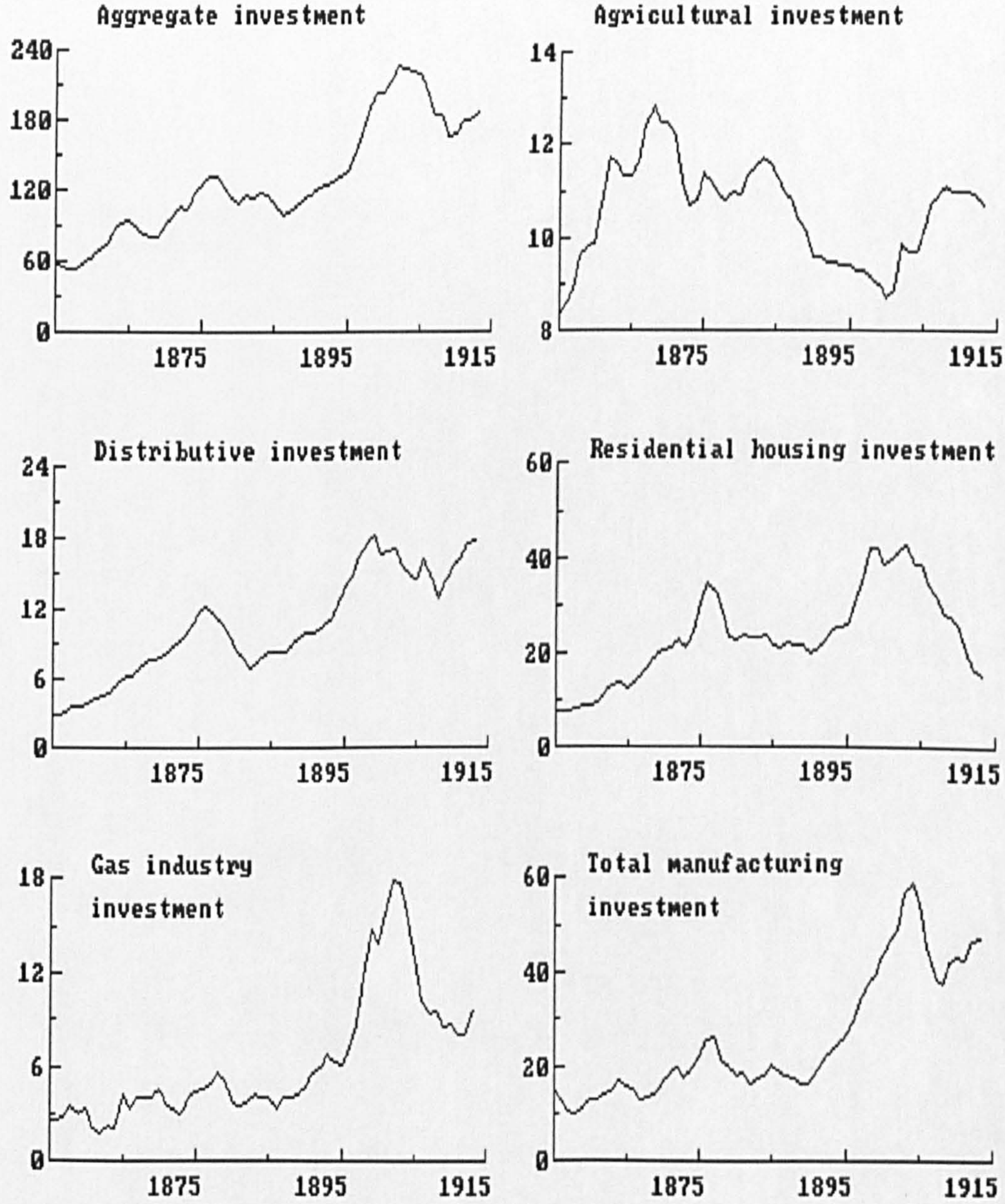


Figure 5.3.2 (Continued)

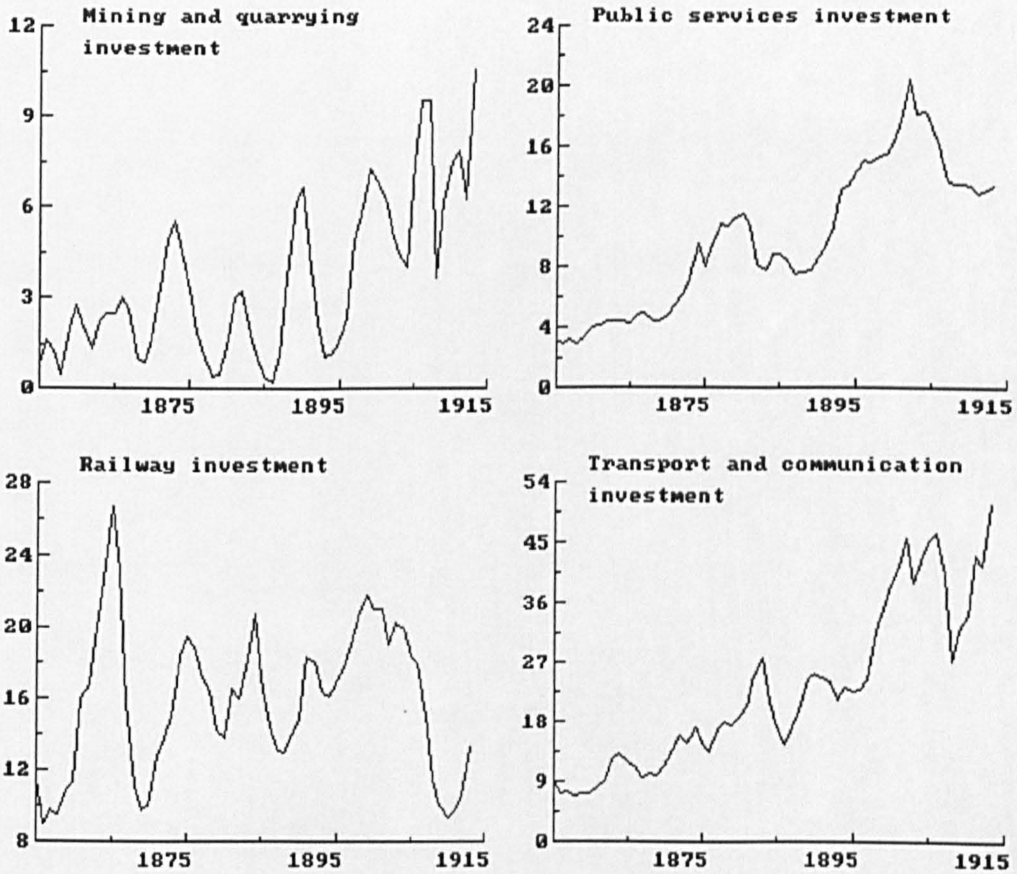


Figure 5.3.3(a). Feinstein's GNP (Expenditure) Estimates (1830-1913)

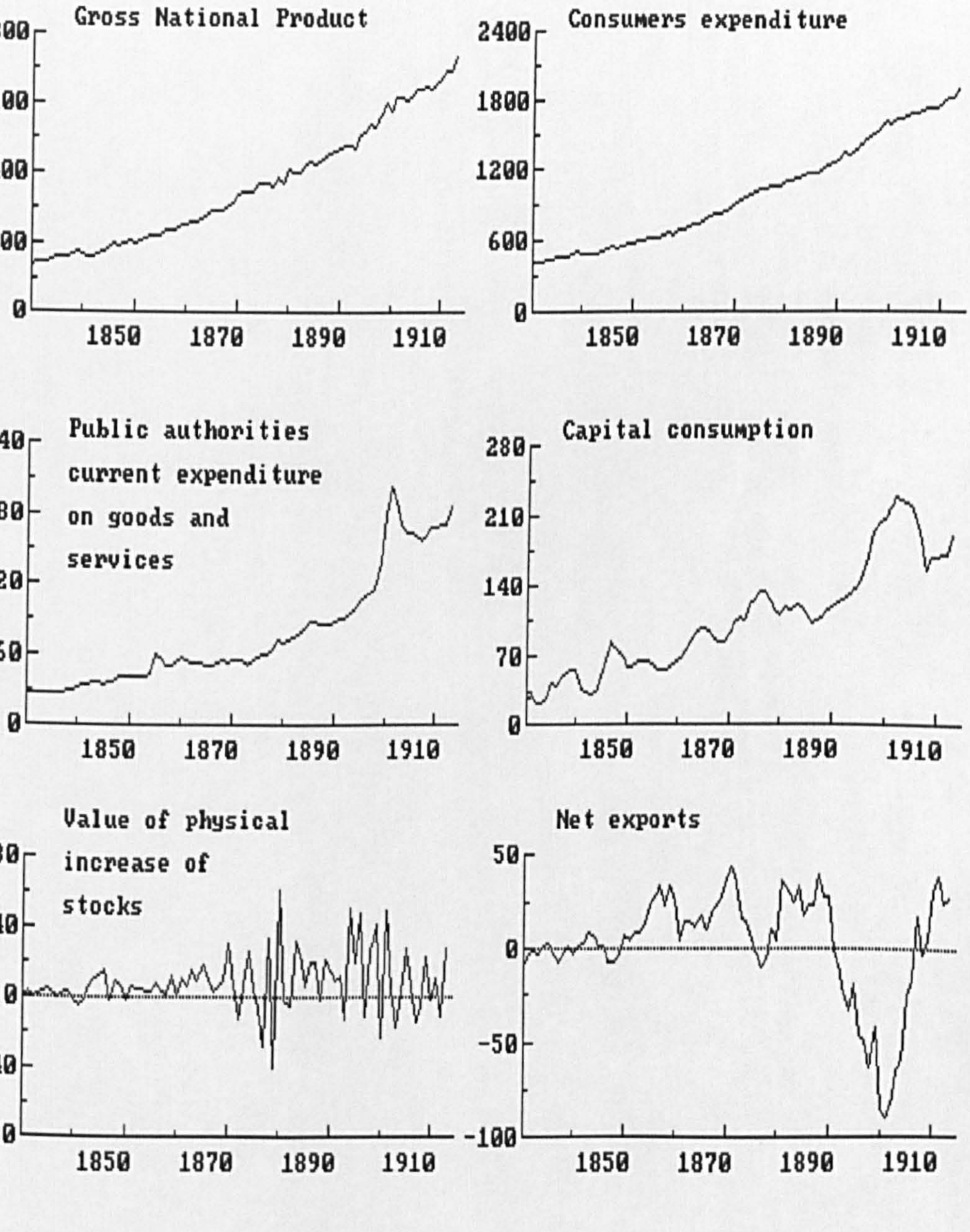


Figure 5.3.3(b). Feinstein's GNP (Expenditure) Estimates 1948–1980 (1980 Prices)

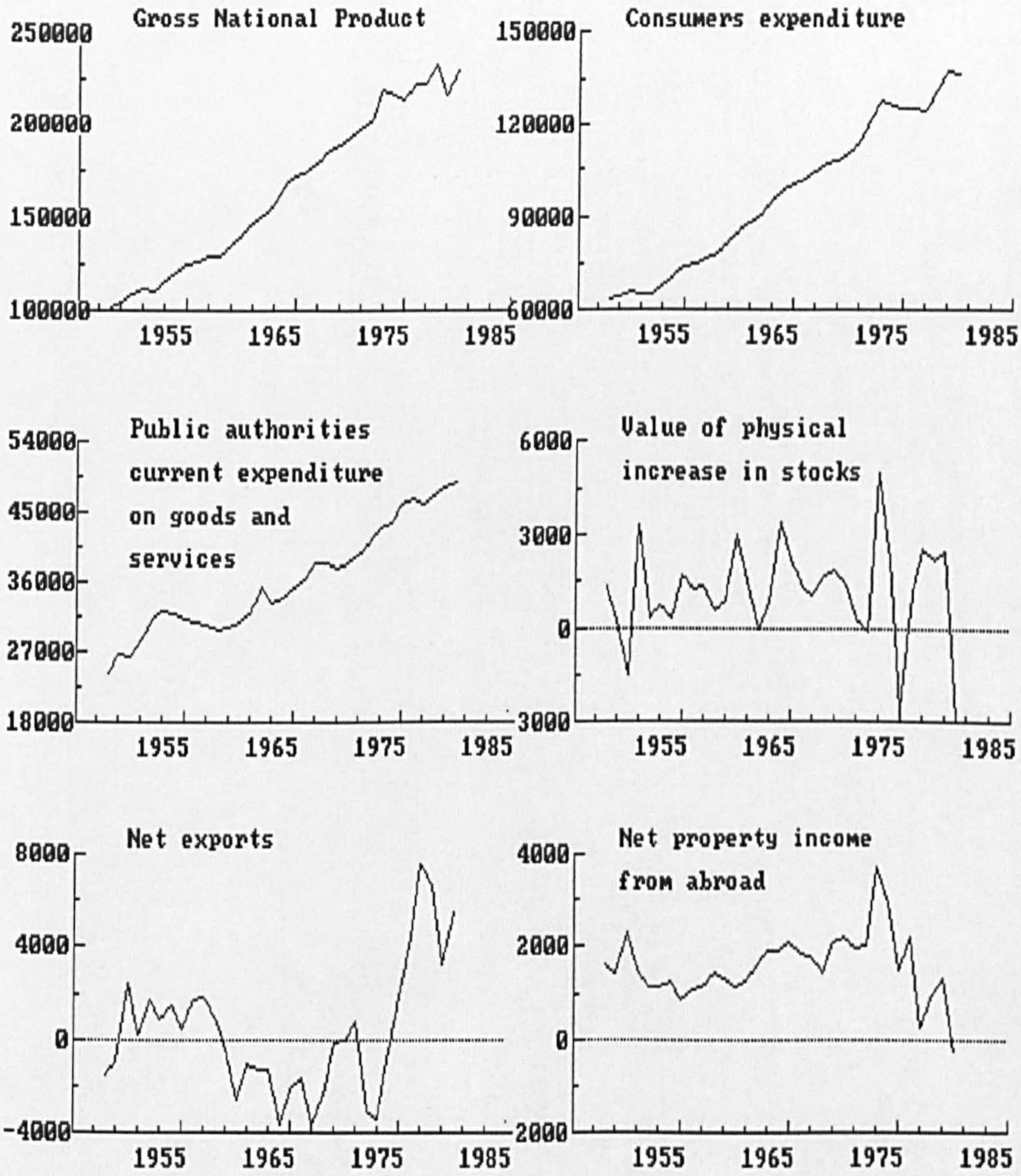


Figure 5.3.4. Lewis's Industrial Production Series 1852–1913

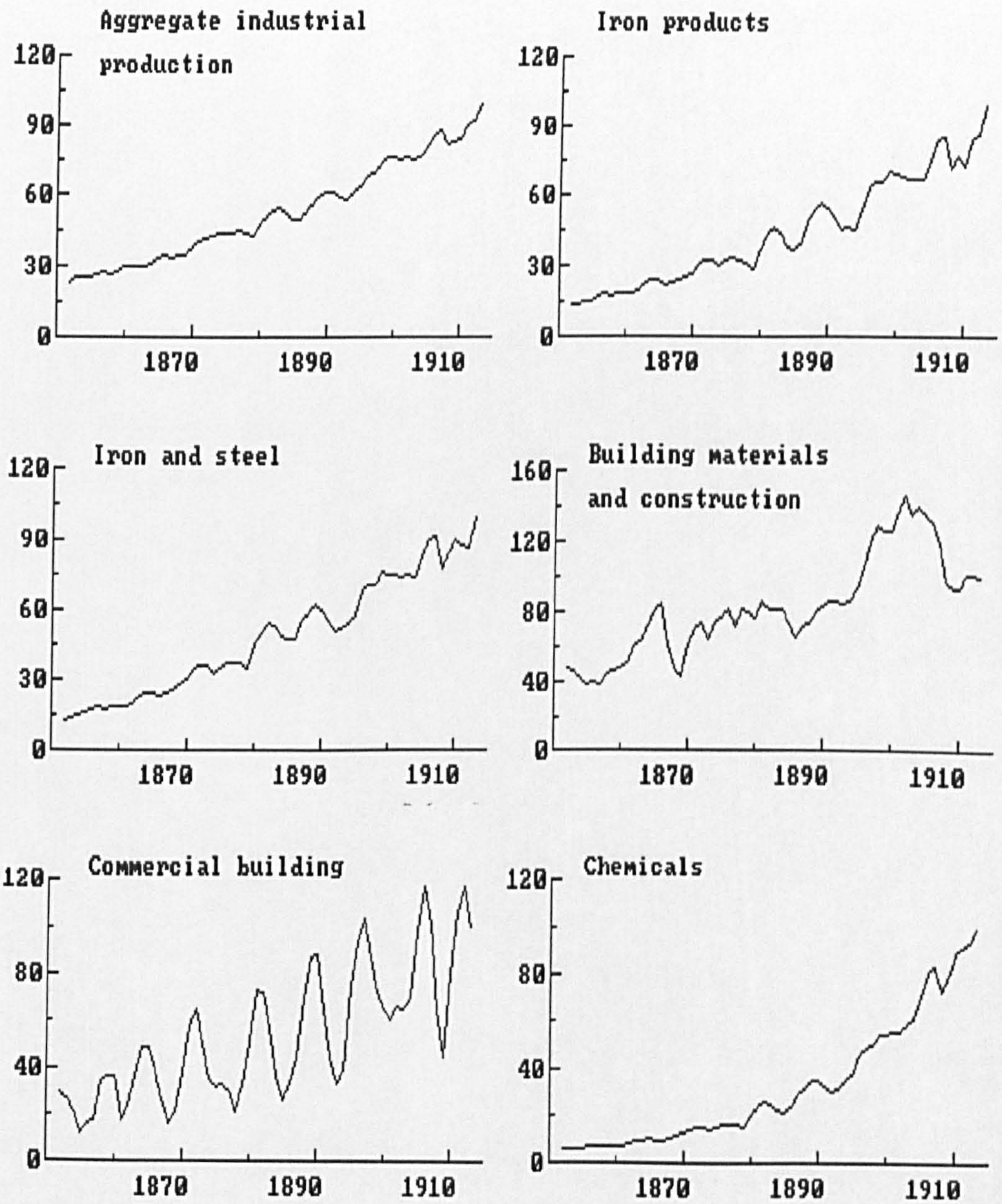


Figure 5.3.4 (Continued)

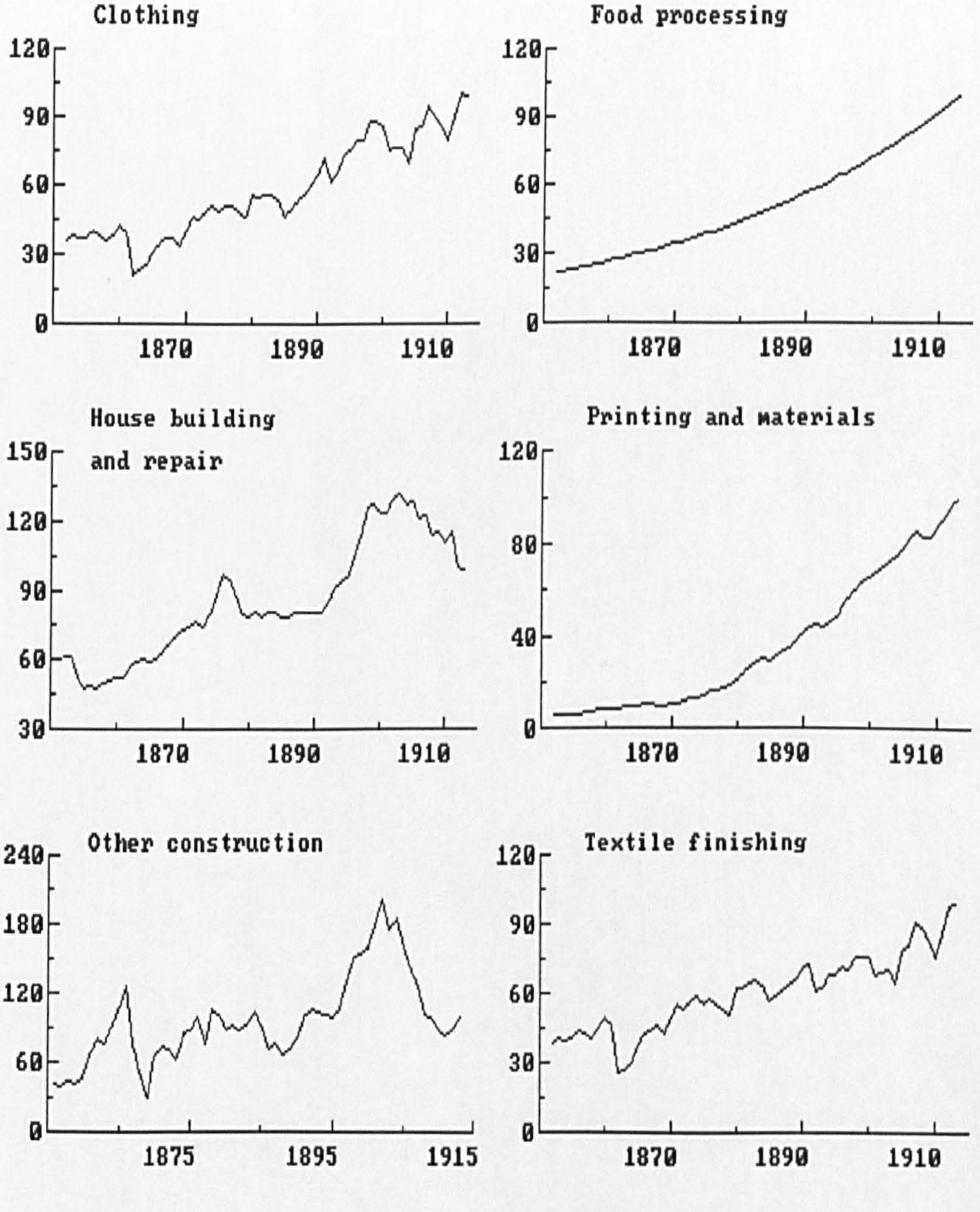
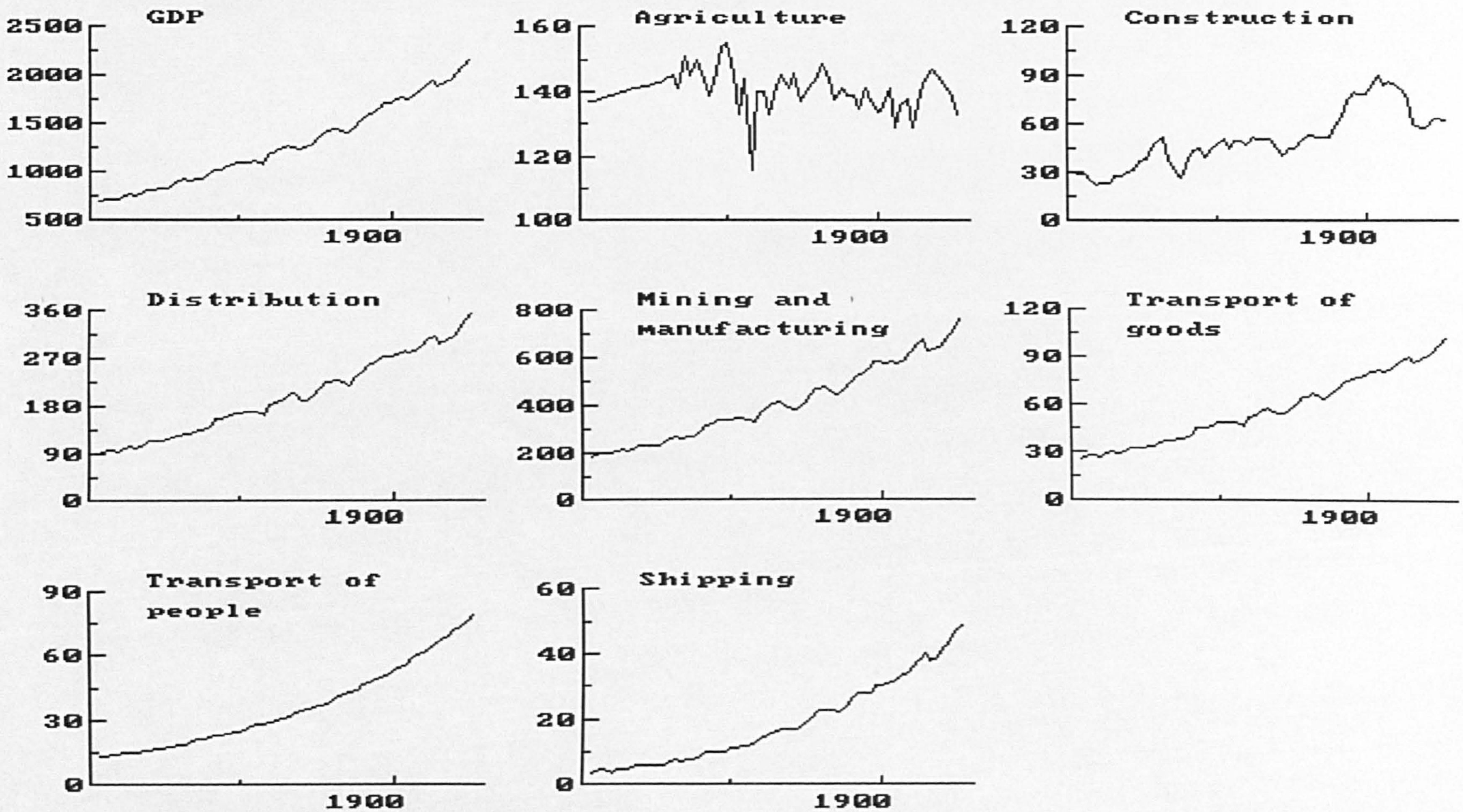


Figure 5.3.5. Lewis's UK GDP Series 1852–1913



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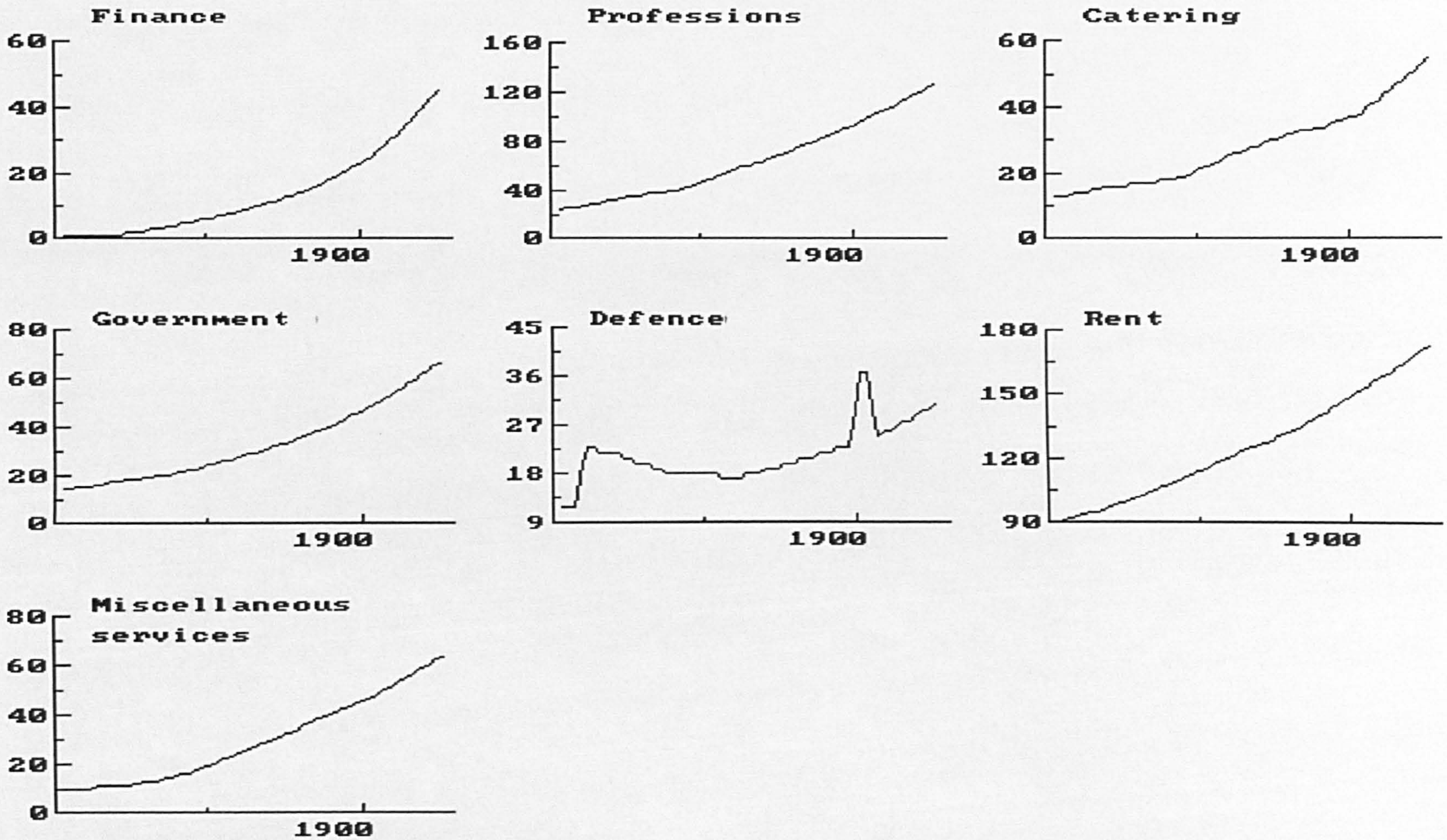


Figure 5.3.6. Lewis's UK Price Series 1871–1913

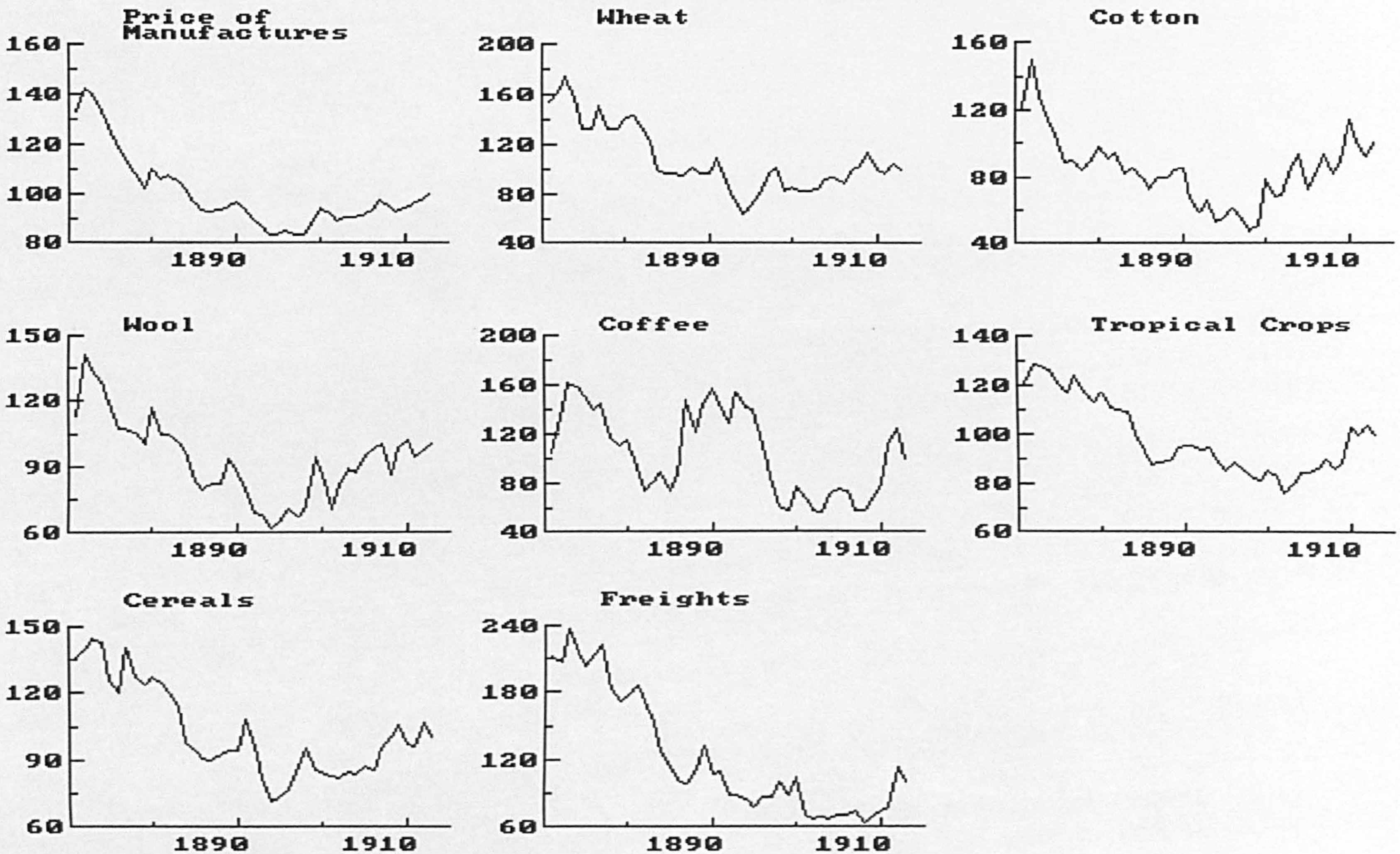


Figure 5.3.7(a). Frequency Distributions, Annual Data: Perfect Clustering of Turning Points
Hypothetical Four-, Six- and Eight-year Cycles, Ten Series, 60-year Sample Period

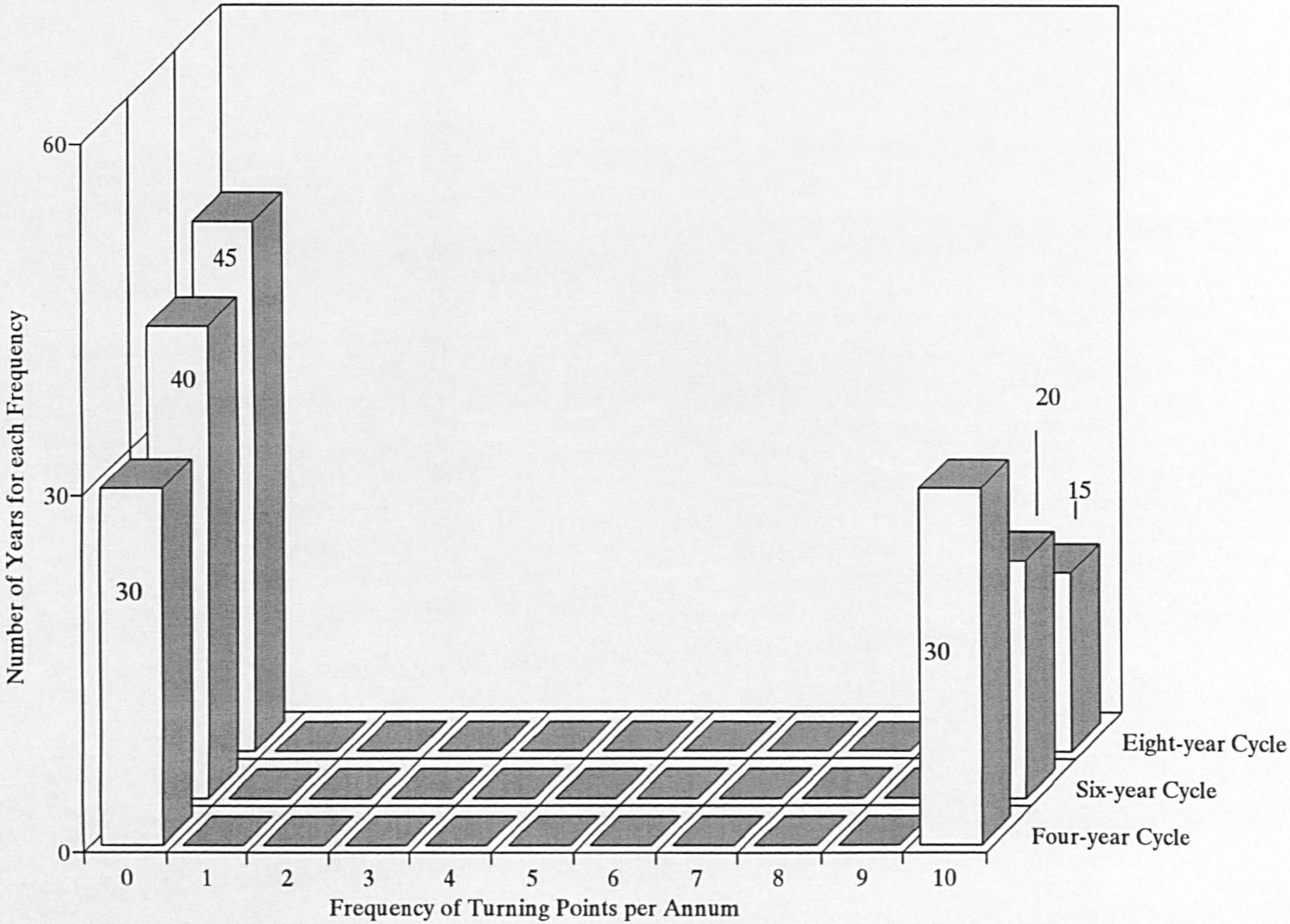


Figure 5.3.7(b). Frequency Distributions, Annual Data, Various Turning-Point Incidences

Hypothetical Eight-year Cycle, 16 Series, 64-year Sample Period

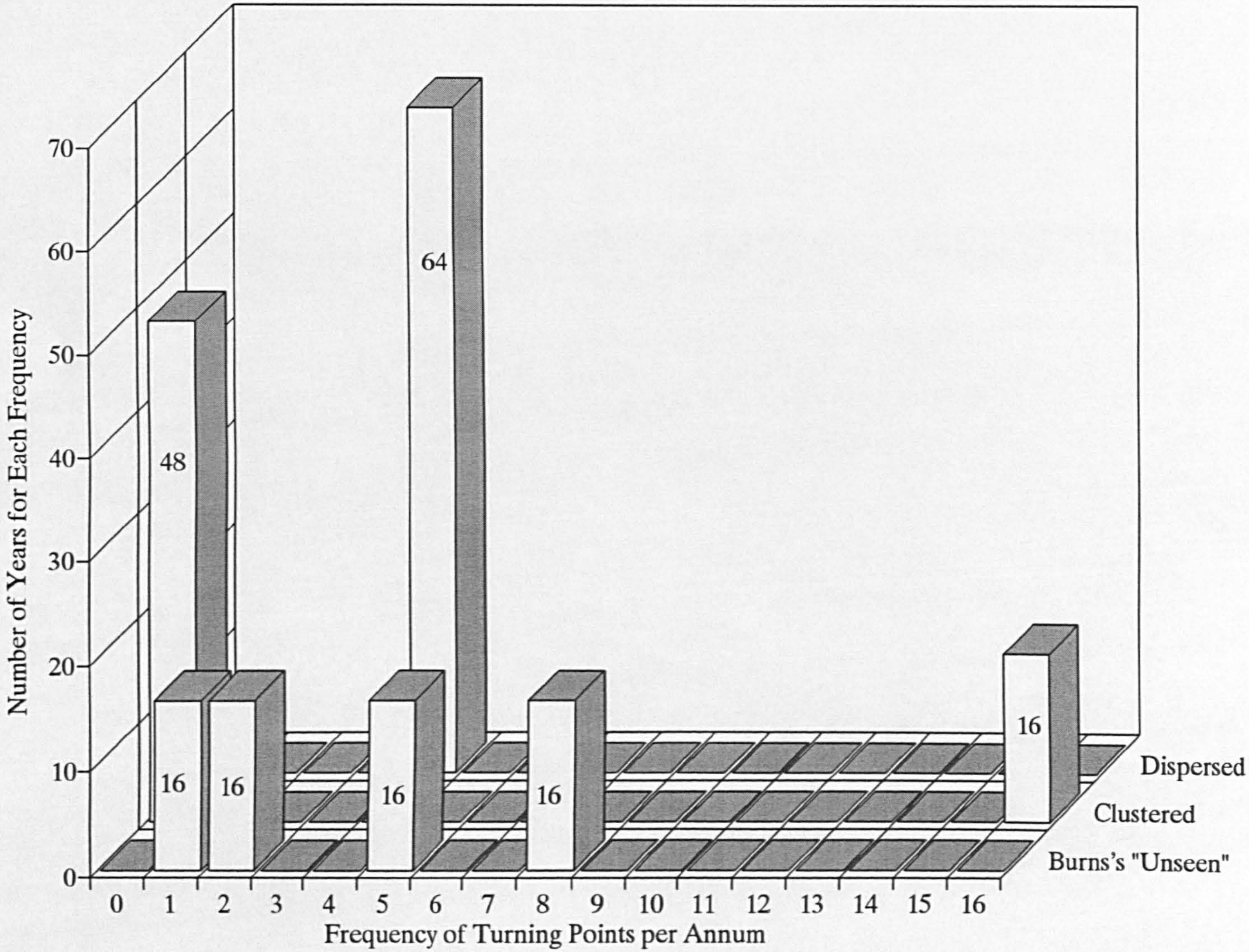
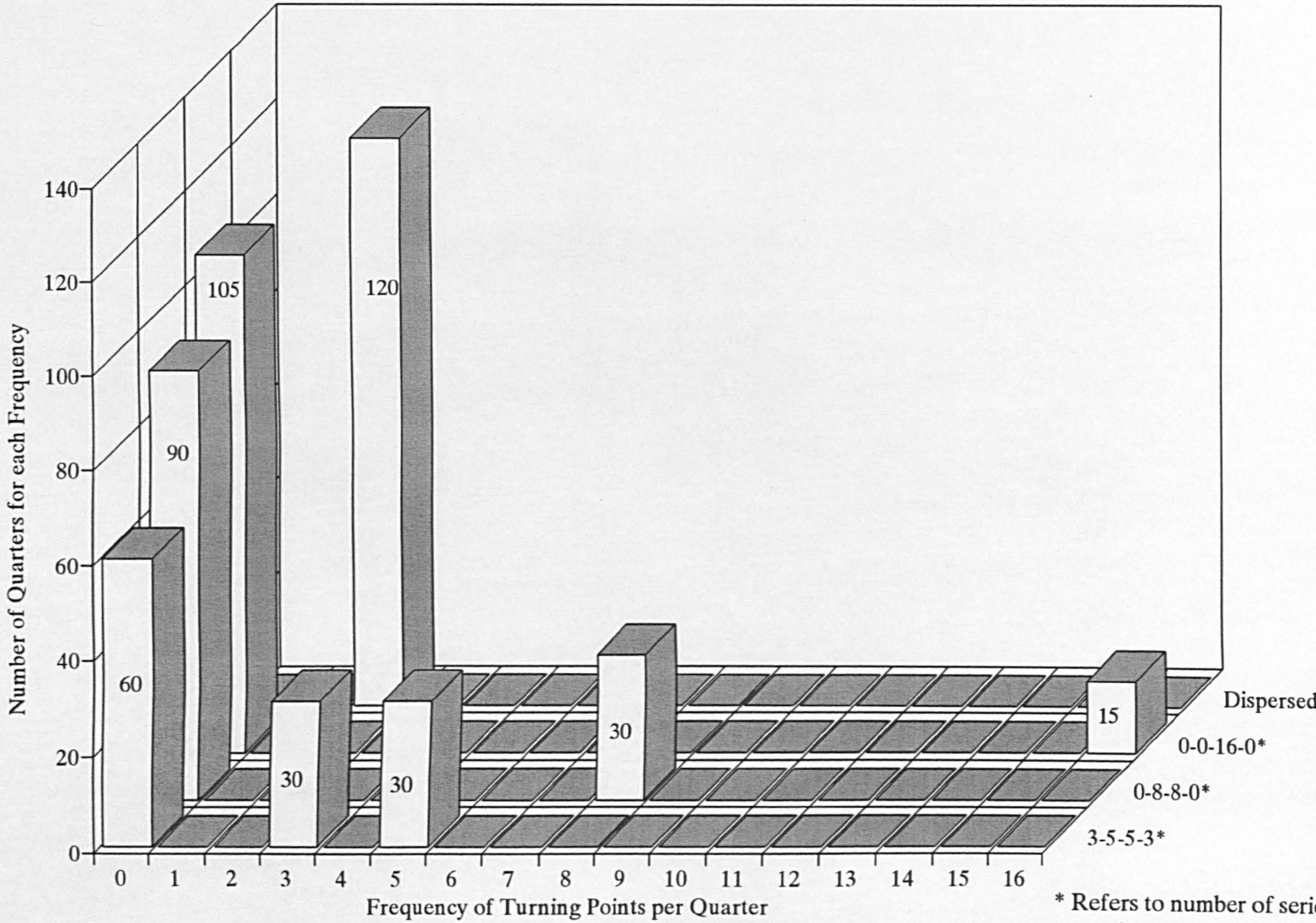


Figure 5.3.8. Frequency Distributions, Quarterly Data: 16 Series, Four-year Cycle

Perfect Clustering and Dispersion of Turning Points



* Refers to number of series turning per quarter

Figure 5.4.1. Beveridge's UK Industrial Production Series 1785-1913: Turning Point Frequencies

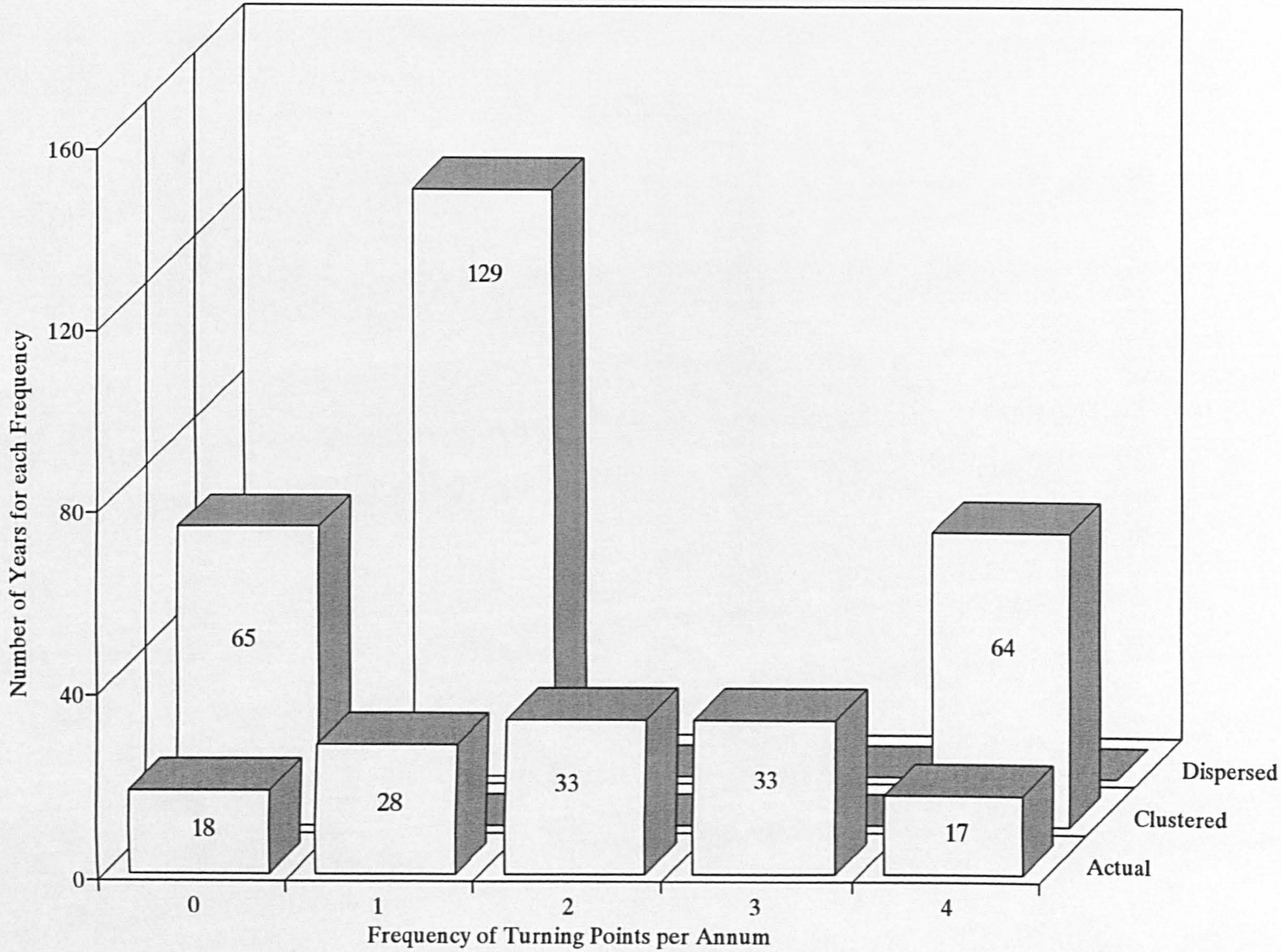


Figure 5.4.2. Feinstein's 13 UK Industrial Production Series 1855-1913
Turning Point Frequencies

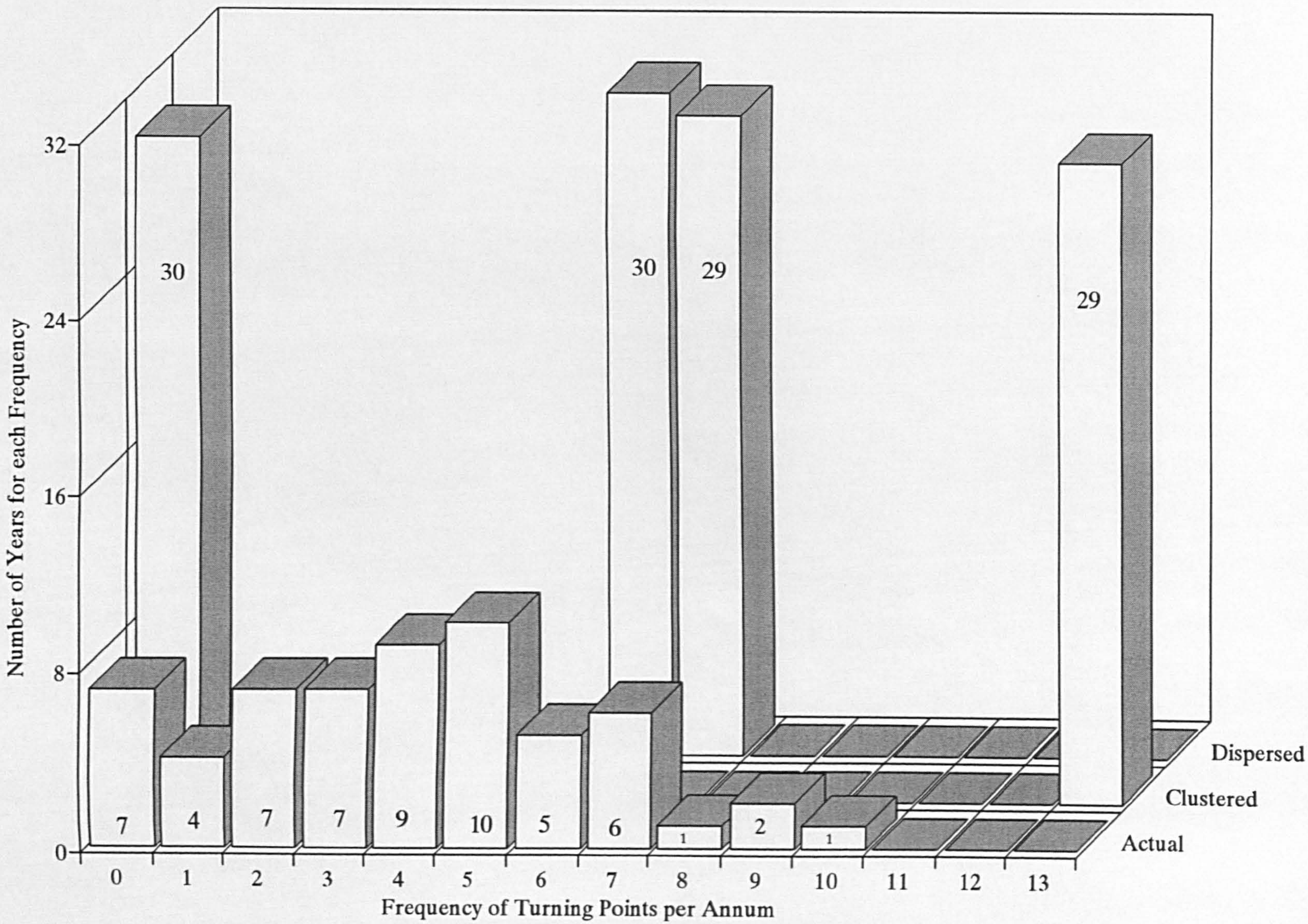


Figure 5.4.3. Feinstein's 12 UK GDP Series 1855-1913
Turning Point Frequencies

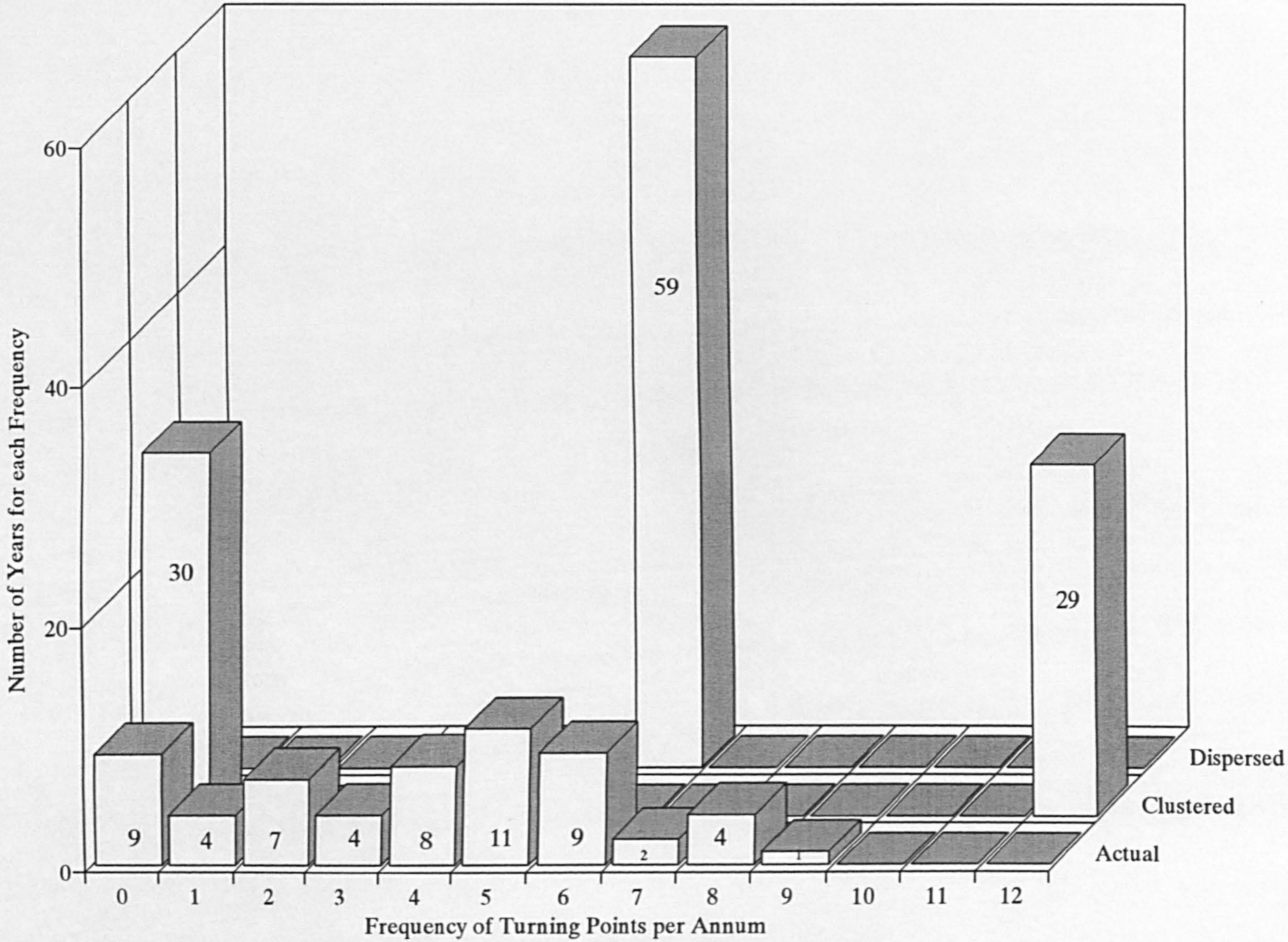


Figure 5.4.4. Feinstein and Pollard Ten UK Investment Series 1855-1913

Turning Point Frequencies

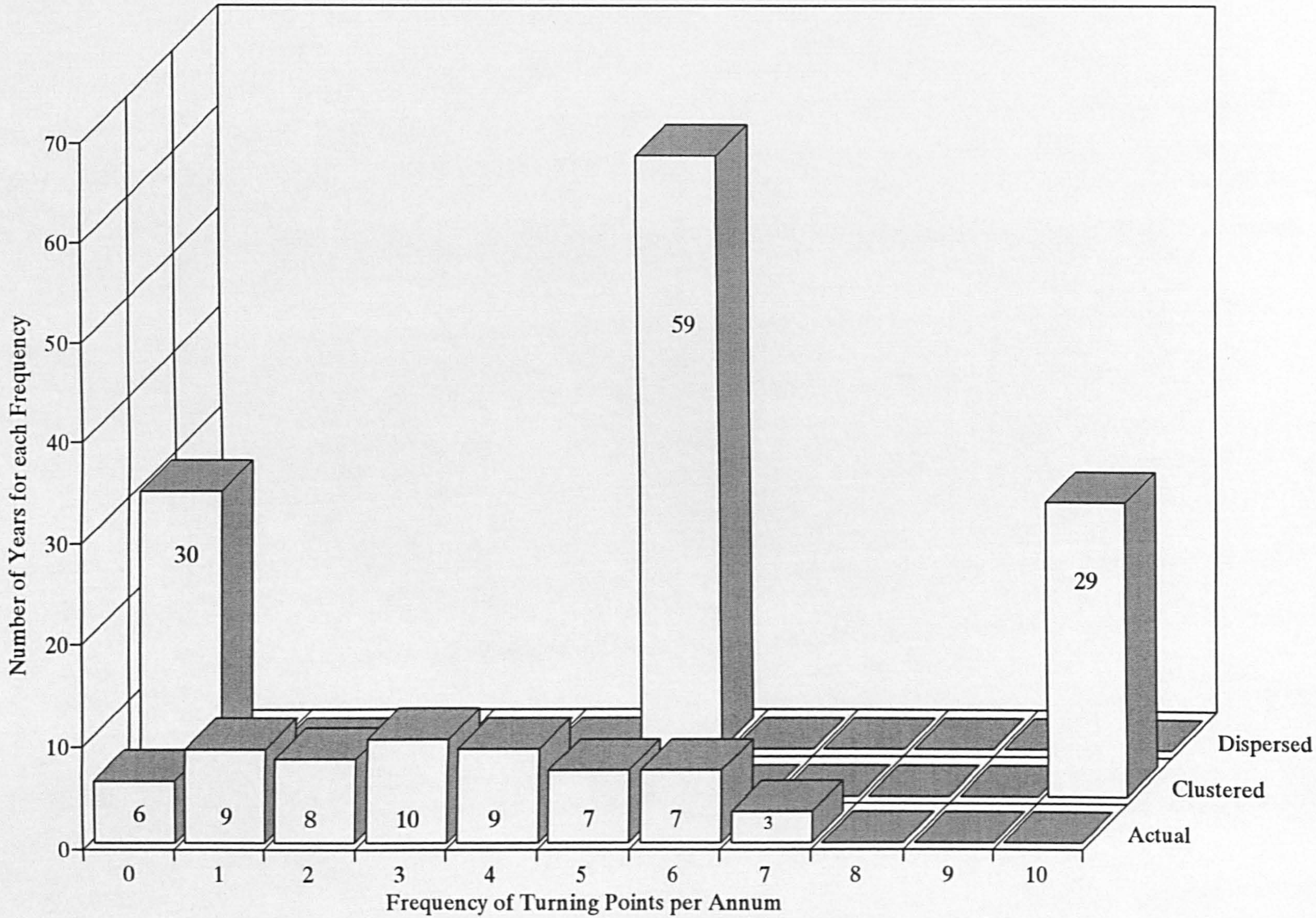


Figure 5.4.5. Feinstein (and Pollard) Industrial Production and Investment Series 1855-1913

Turning Point Frequencies

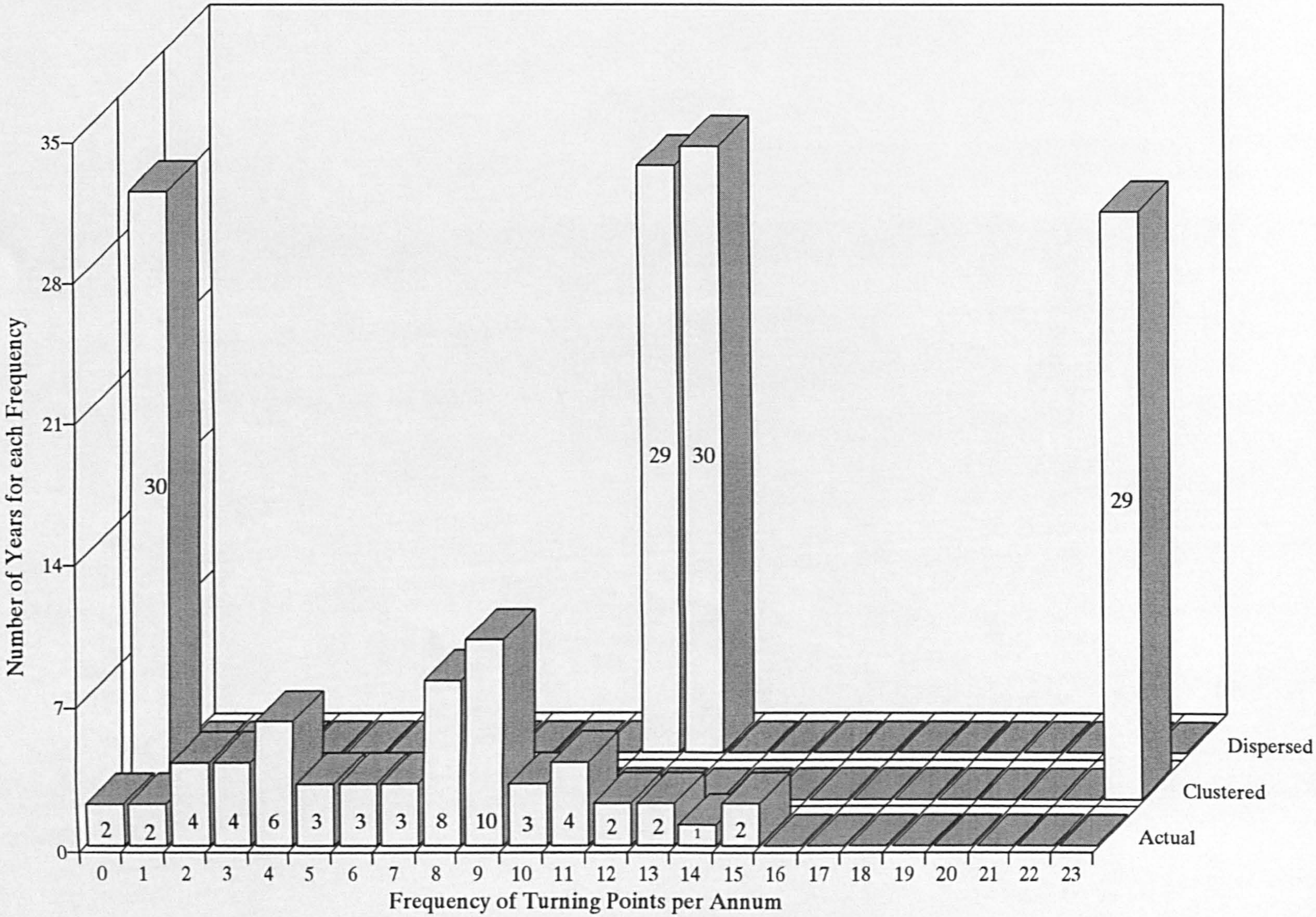


Figure 5.4.6. Lewis's UK Industrial Production Series 1852-1913
Turning Point Frequencies

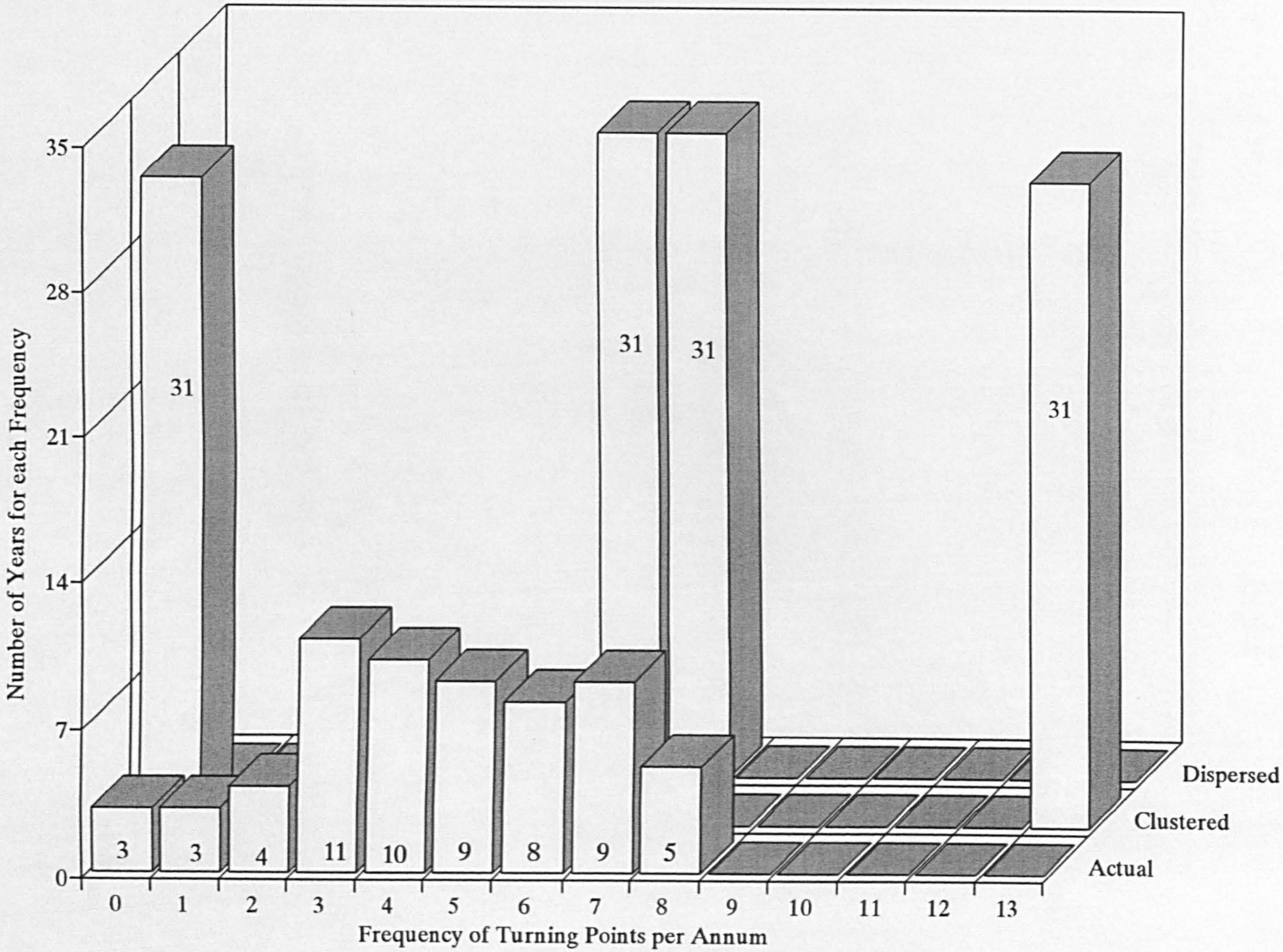


Figure 5.4.7. Seven Lewis UK GDP Constituent Series with Fluctuations 1852-1913

Turning Point Frequencies

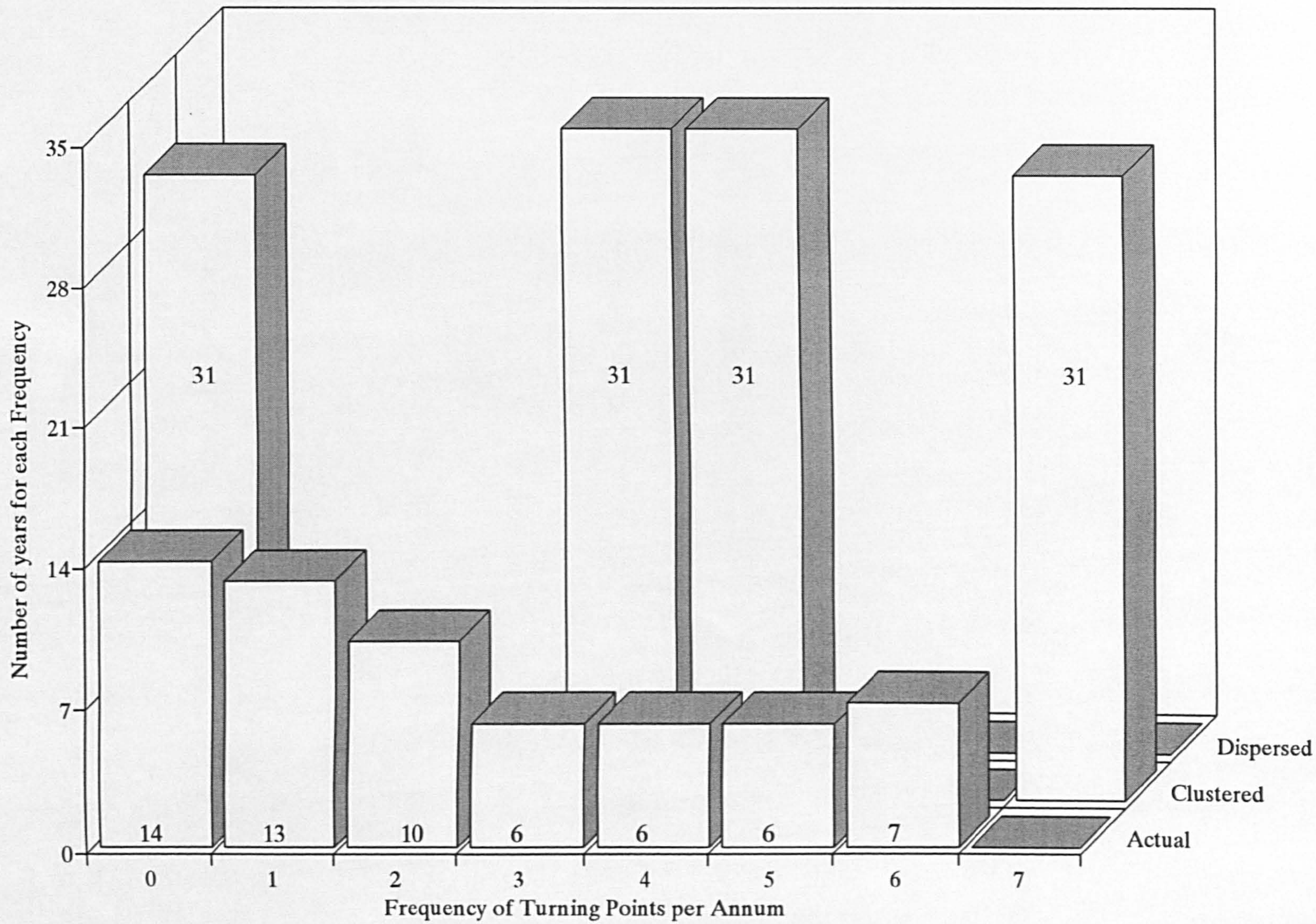


Figure 5.4.8. Lewis's Commodity Price Series 1852-1913

Turning Point Frequencies

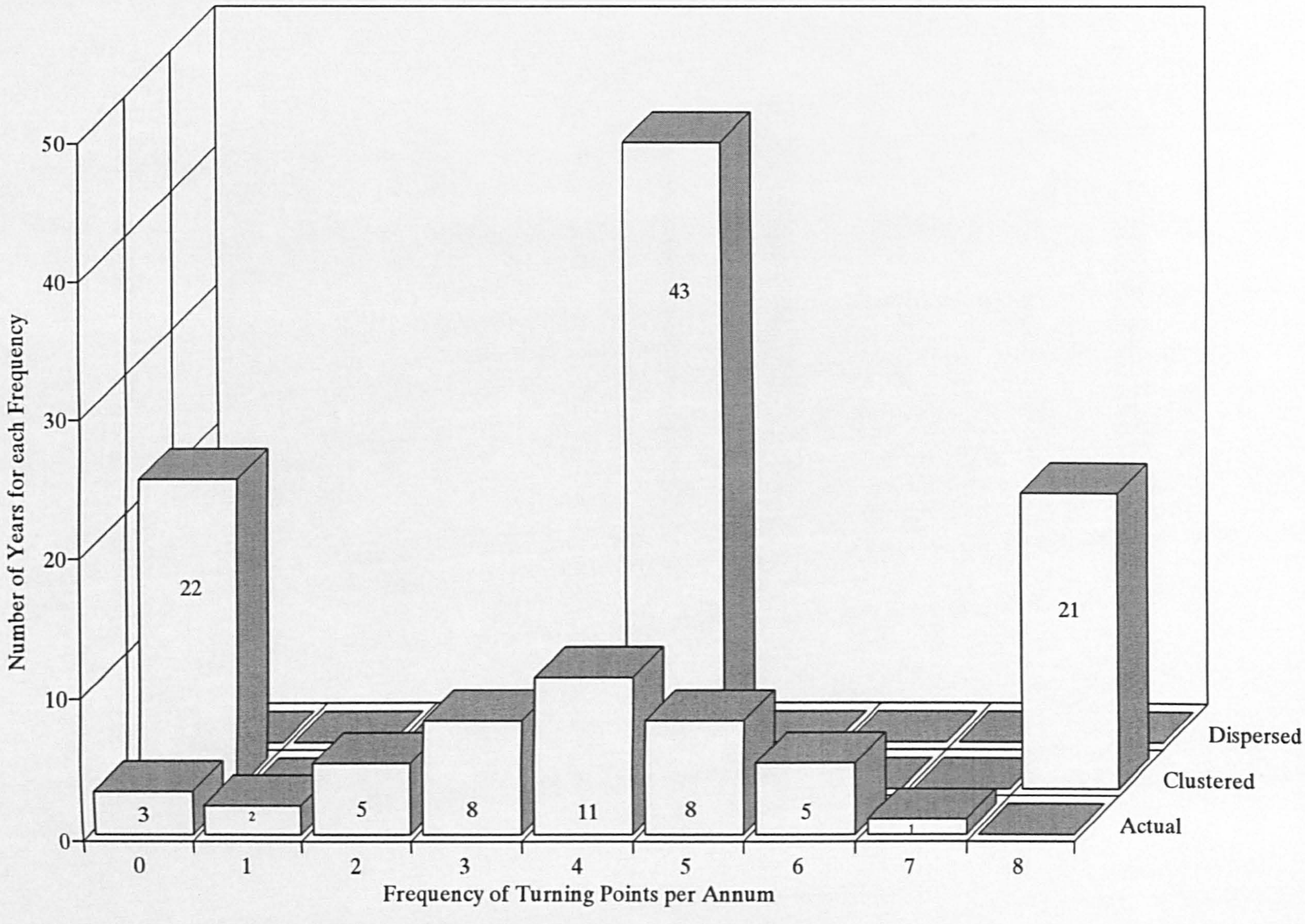


Figure 5.4.9. Board of Trade UK Price Series 1871-1913

Turning Point Frequencies

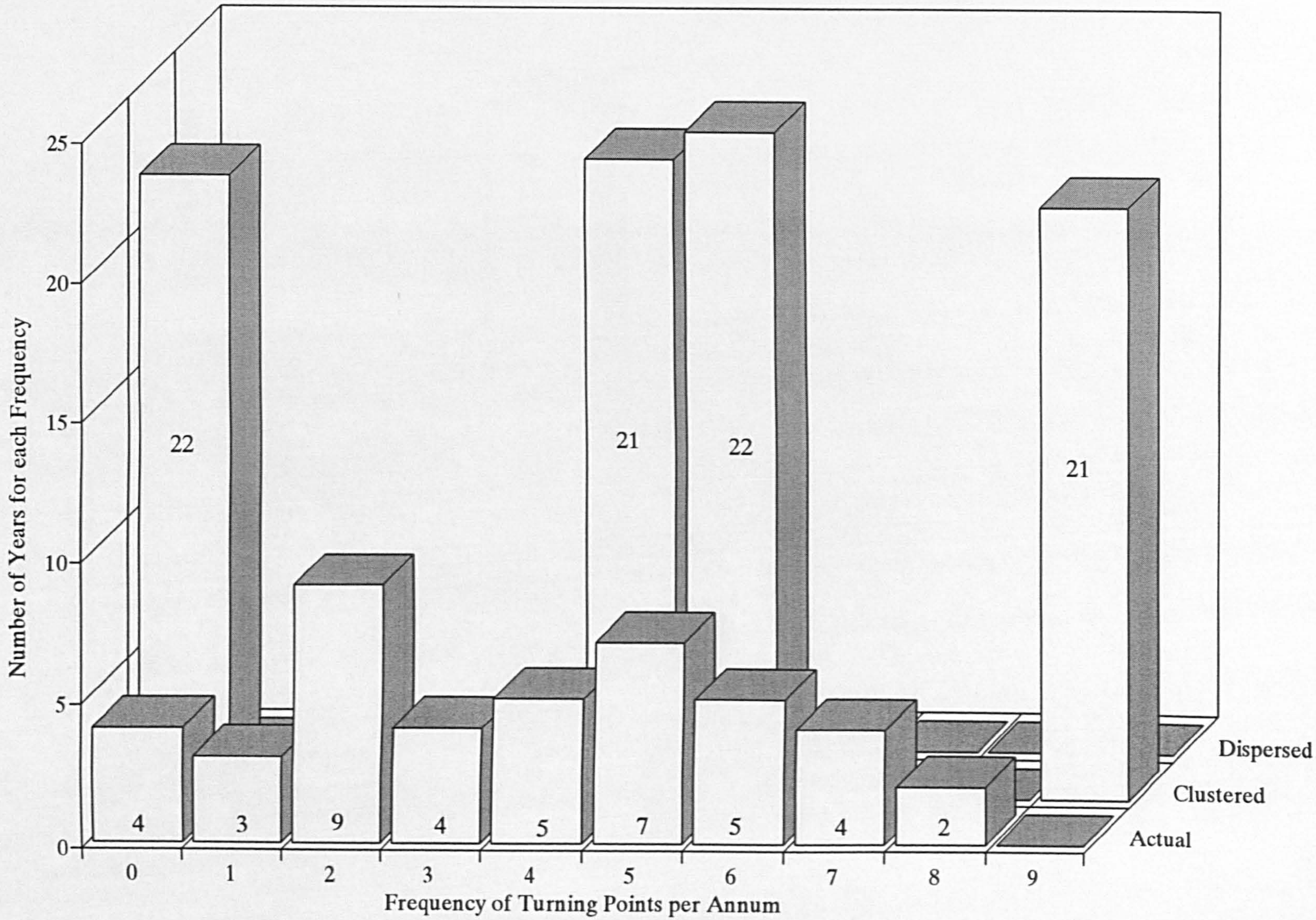
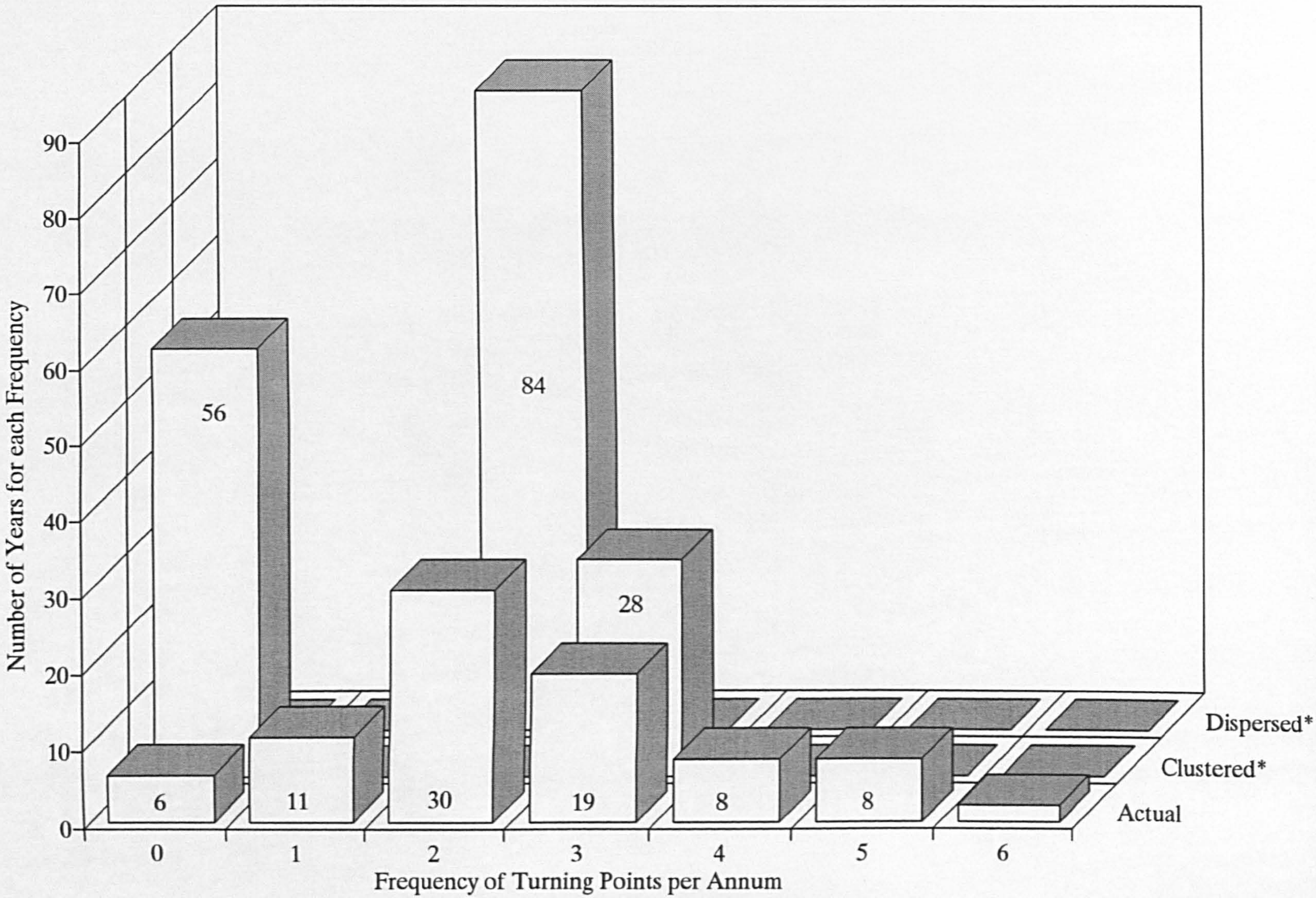


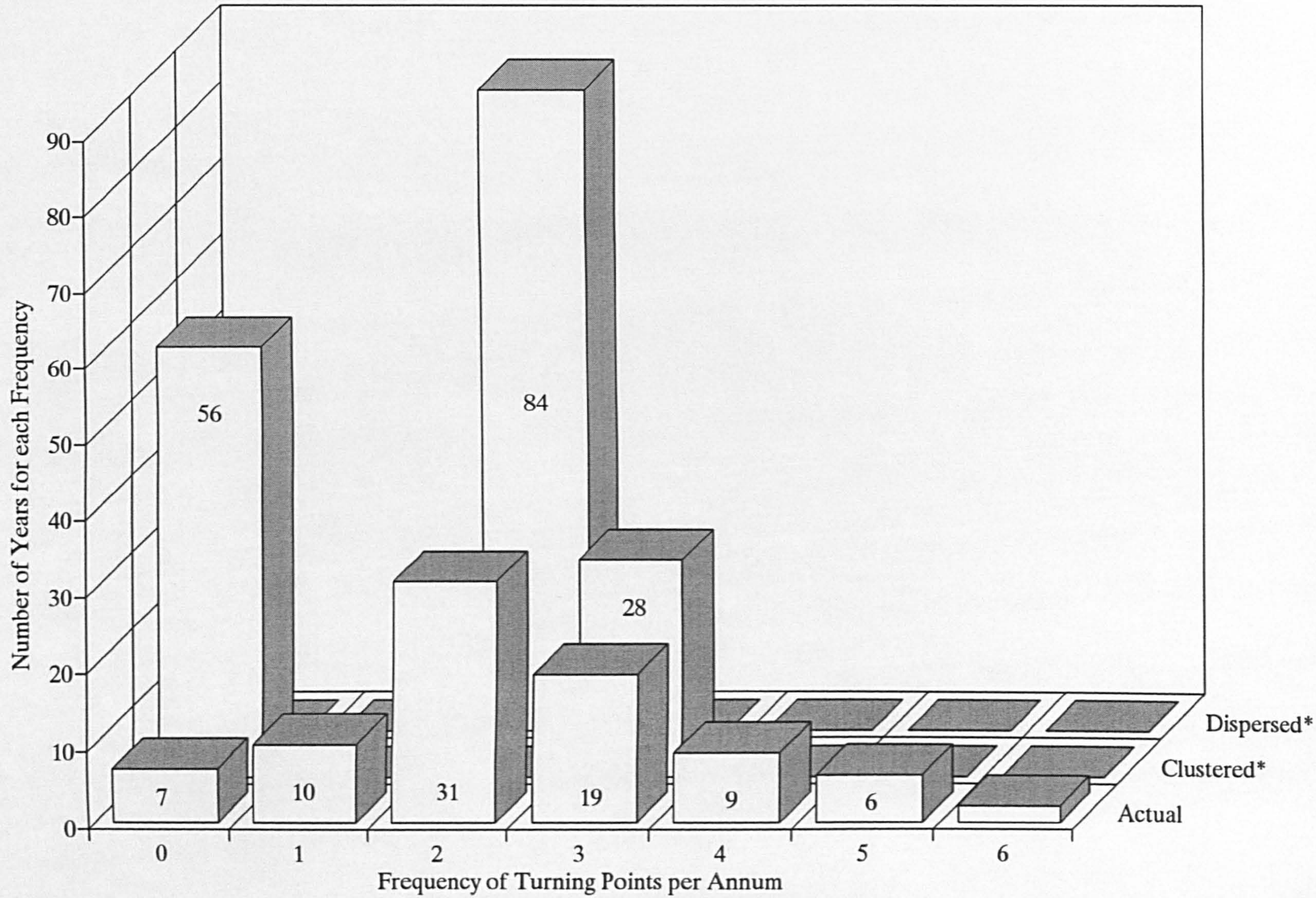
Fig. 5.4.10. Feinstein's UK GNP (Expenditure) Series and Constituents 1830-1913

Turning Point Frequencies (Multiple Turns Included)



*Assuming a six-year cycle

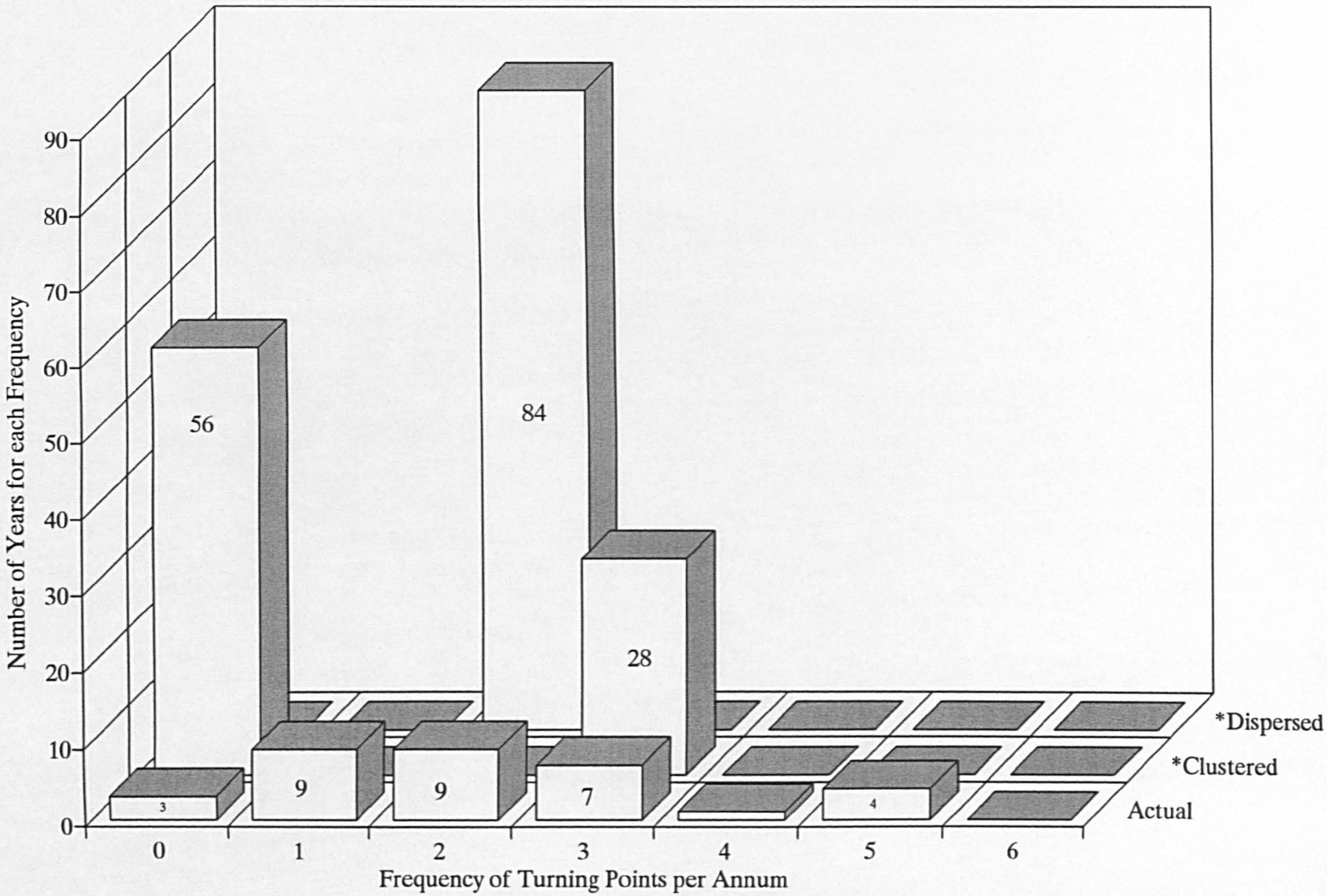
Fig. 5.4.11. Feinstein's UK GNP (Expenditure) Series and Constituents 1830-1913
Turning Point Frequencies (Multiple Turns Excluded)



*Assuming a six-year cycle

Fig. 5.4.12 . Feinstein's UK GNP (Expenditure) Series and Constituents 1948-1980

Turning Point Frequencies



*Assuming a six-year cycle

Figure 5.4.13. Eleven Quarterly US Macroeconomic Time Series 1947-1983: Turning Point Frequencies (Multiple Turns Included)

Figure 5.4.14. Eleven Quarterly US Macroeconomic Series 1947-1983:

Turning Point Frequencies (Multiple Turns Excluded)

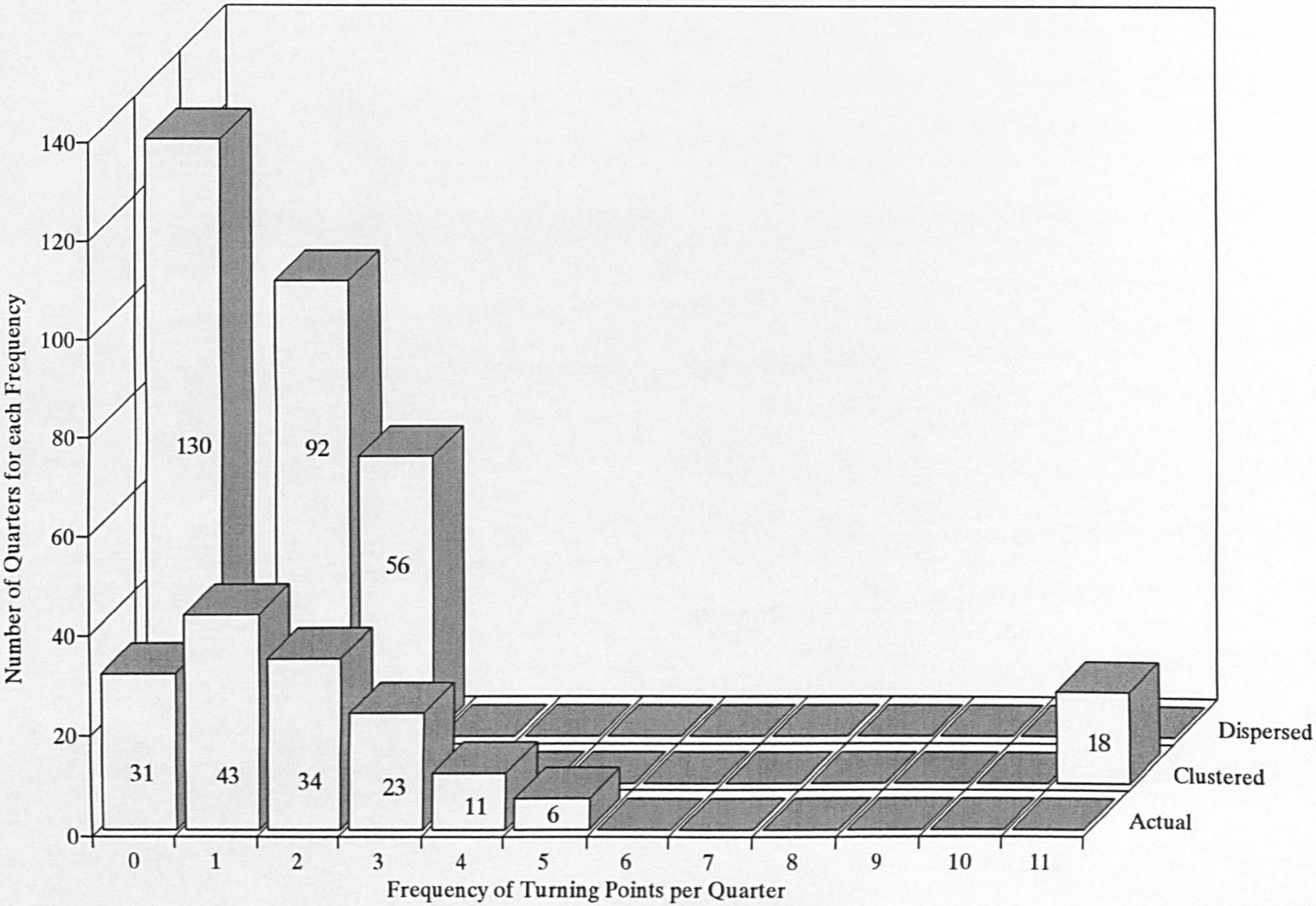


Figure 5.4.15. 23 Quarterly UK Macroeconomic Series 1957-1992:

Turning Point Frequencies (Multiple Turns Included)

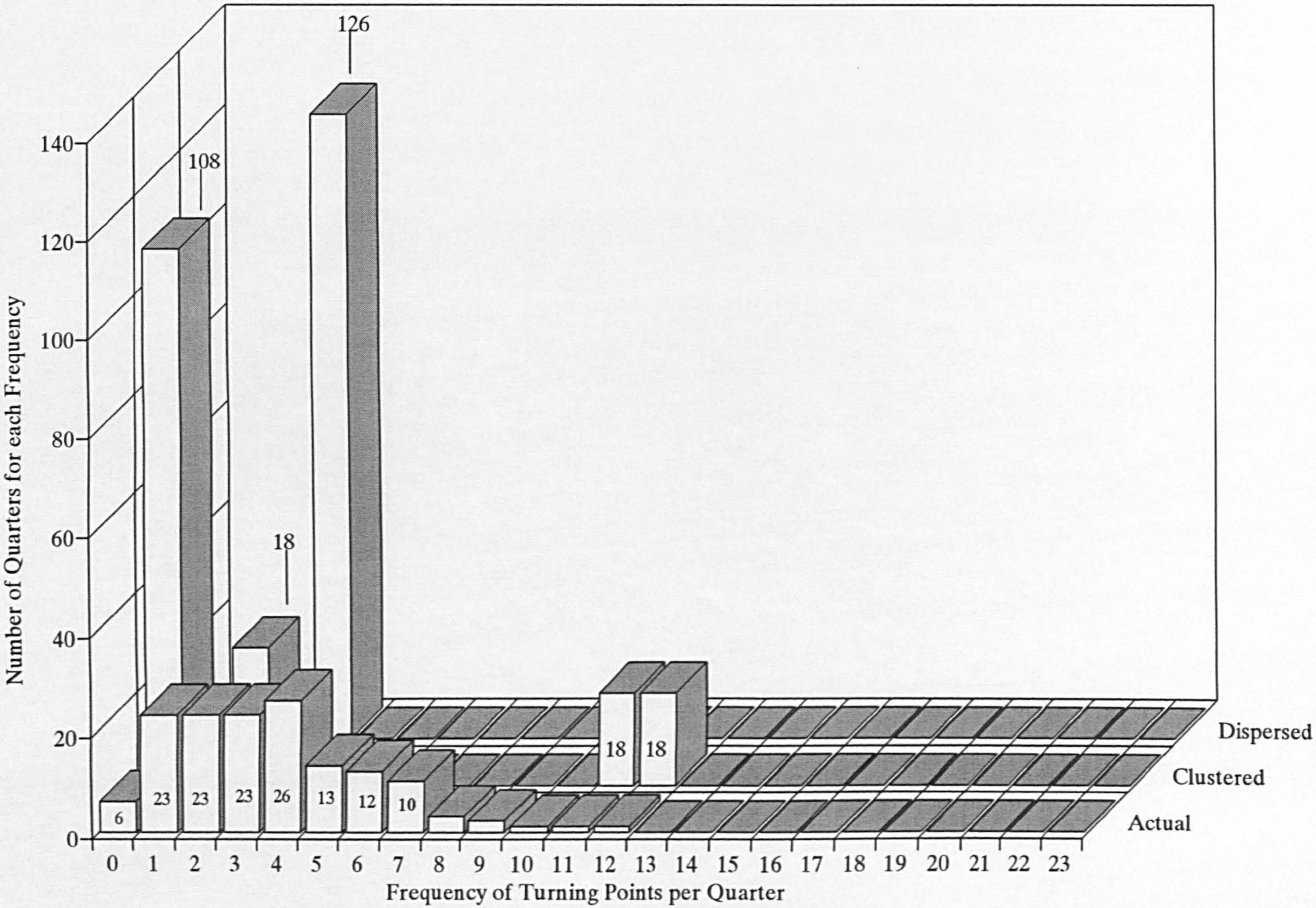


Figure 5.4.16. 23 Quarterly UK Macroeconomic Series 1957-1992:

Turning Point Frequencies (Multiple Turns Excluded)

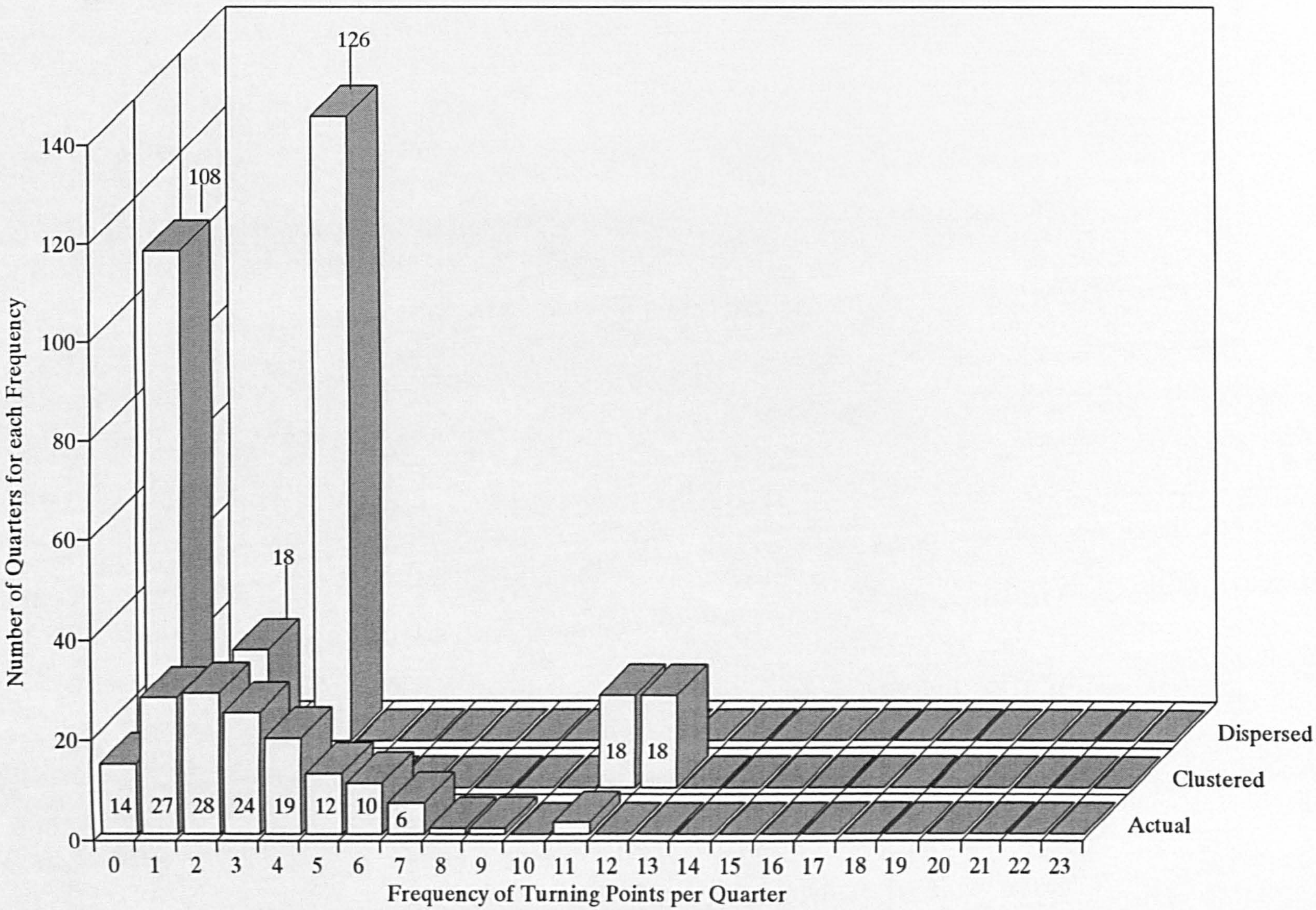


Figure 5.4.17. UK Real GNP and Consumers Expenditure, 1855-1913

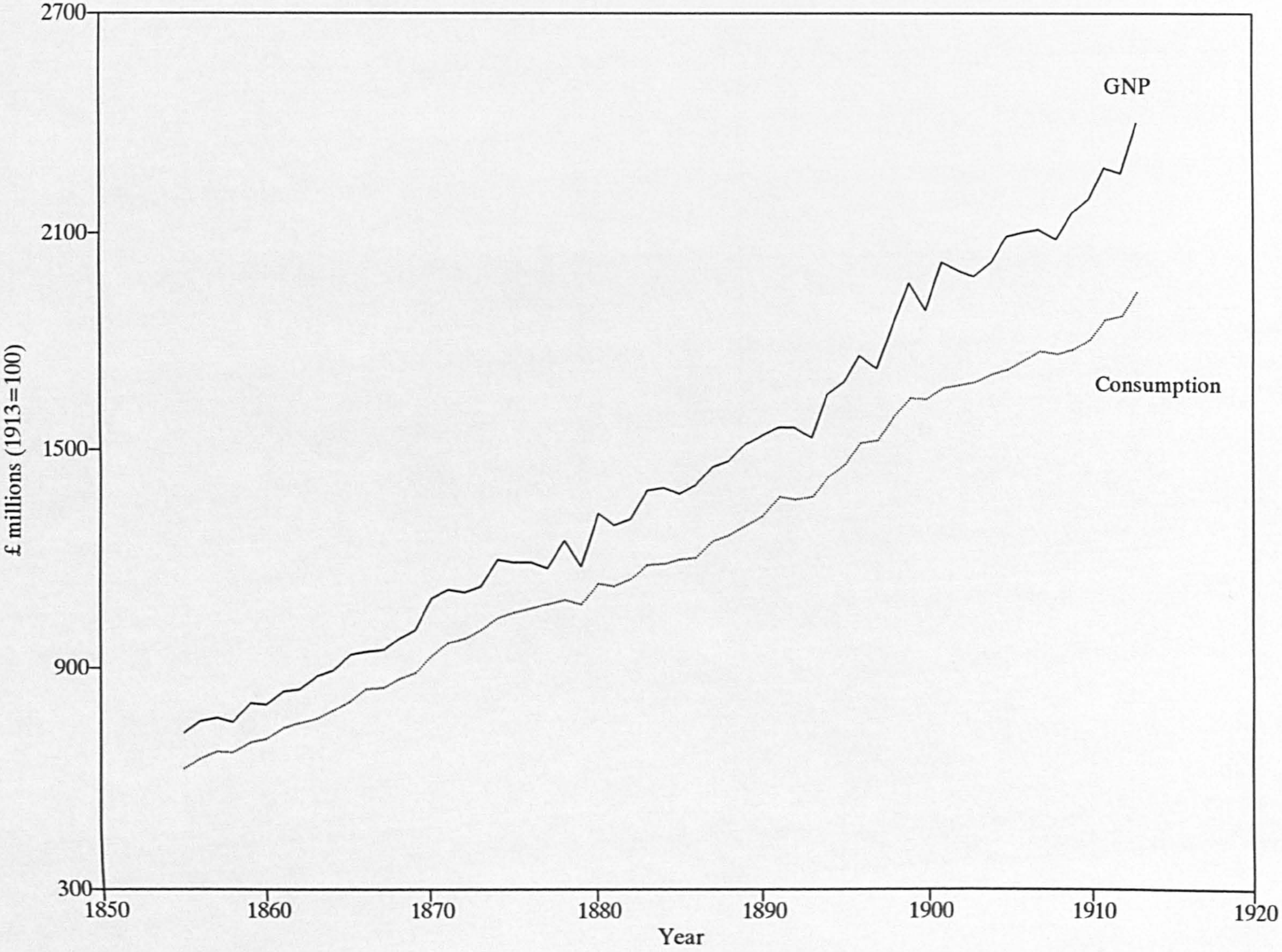


Figure 5.4.18. Pairwise Comparisons of Leads and Lags Producers Durable Equipment against US GNP

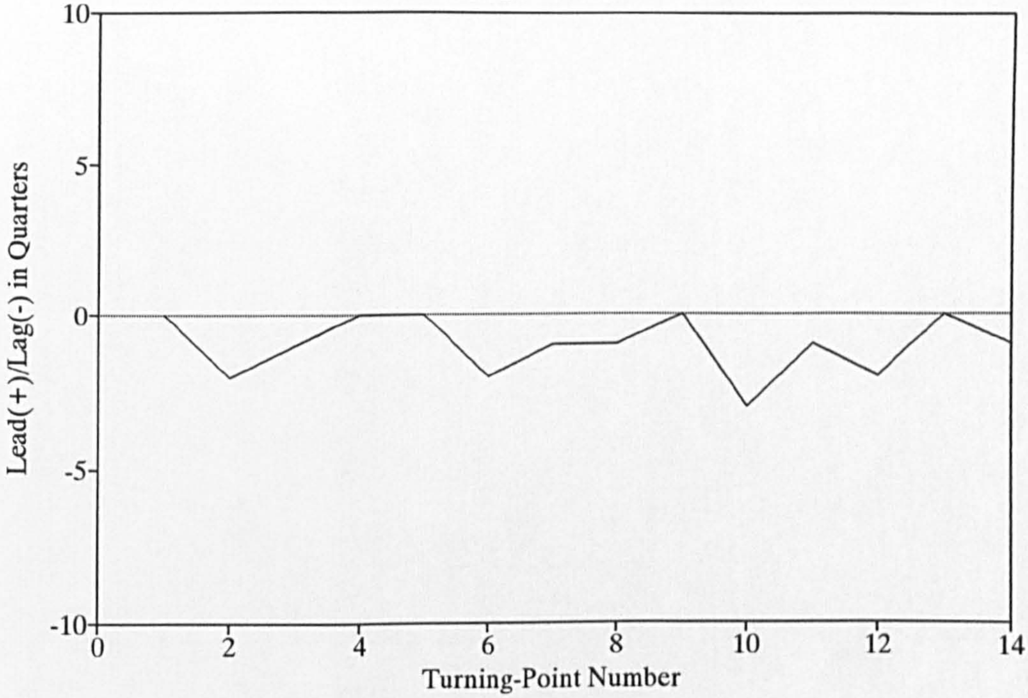


Figure 5.4.19. Pairwise Comparisons of Leads and Lags Residential Structures against US GNP

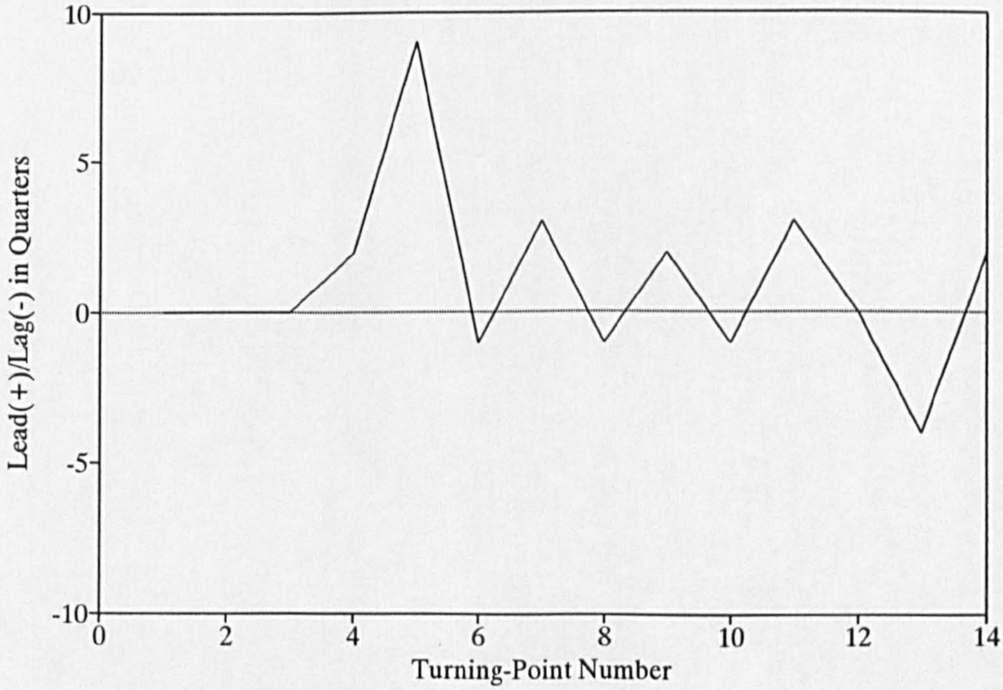


Figure 5.4.20. Pairwise Comparison of Leads and Lags
Nonresidential Structures against US GNP

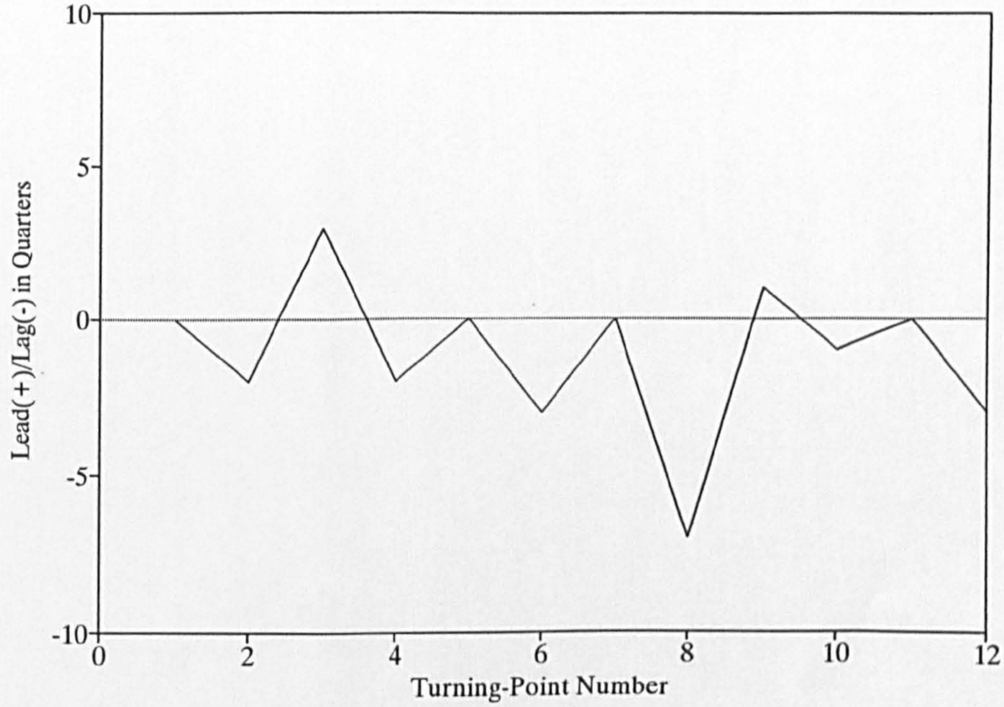


Figure 5.4.21. Pairwise Comparison of Leads and Lags
Durable Goods against US GNP

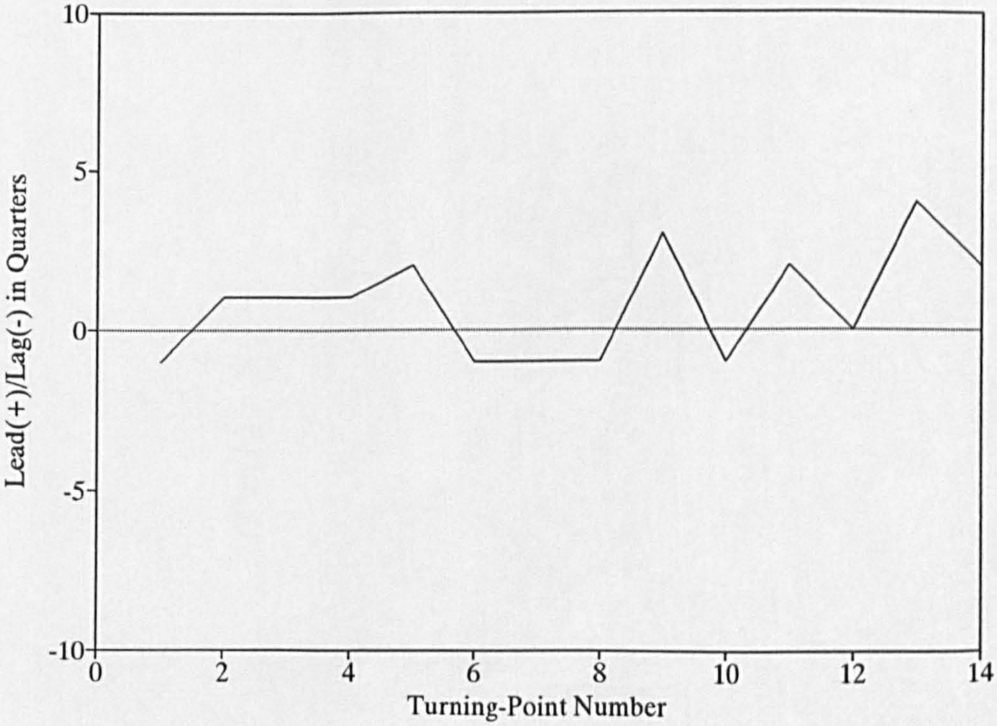


Figure 5.4.22. Pairwise Comparison of Leads and Lags
Nondurable Goods against US GNP

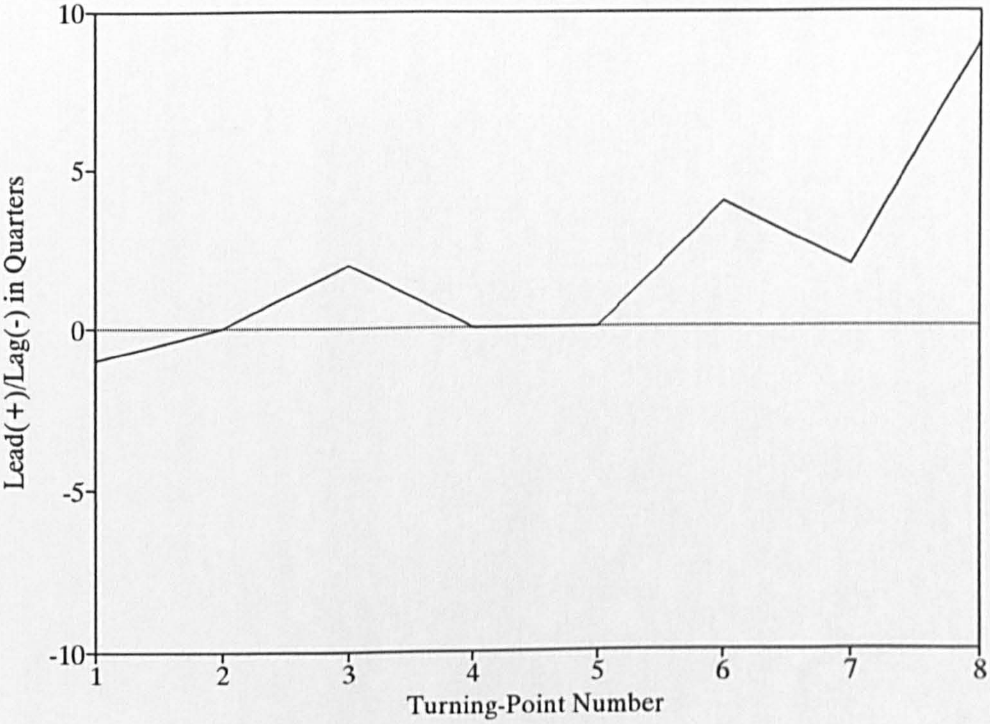


Figure 5.4.23. Pairwise Comparison of Leads and Lags
Government Expenditure against US GNP

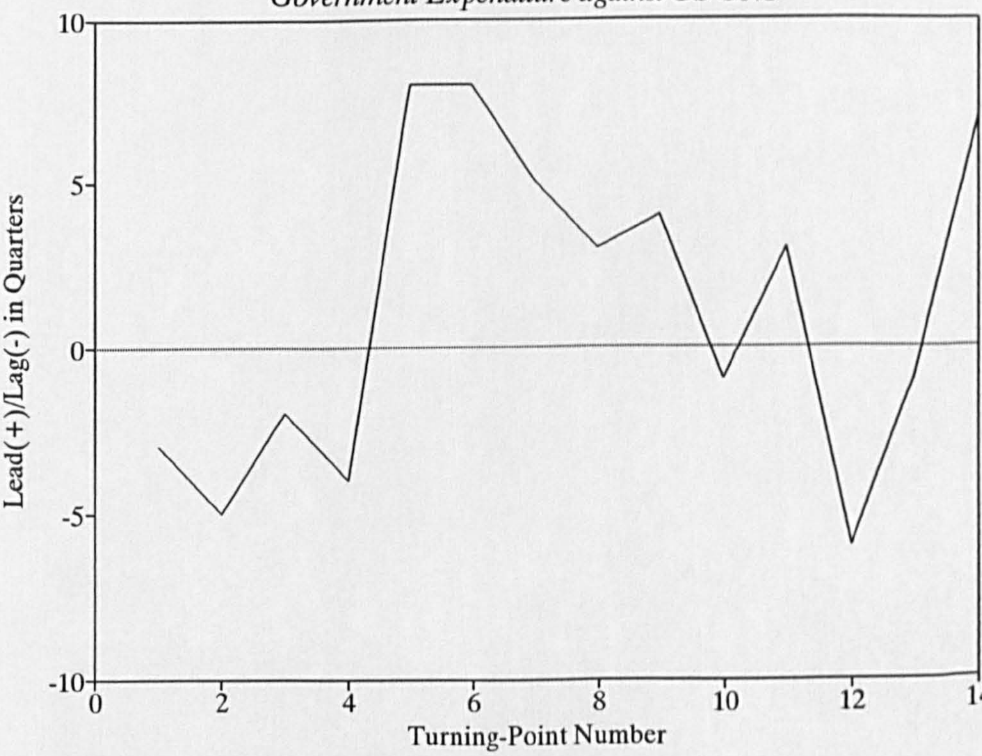


Figure 5.4.24. Pairwise Comparison of (Postwar) Leads and Lags
UK Industrial Production against GDP

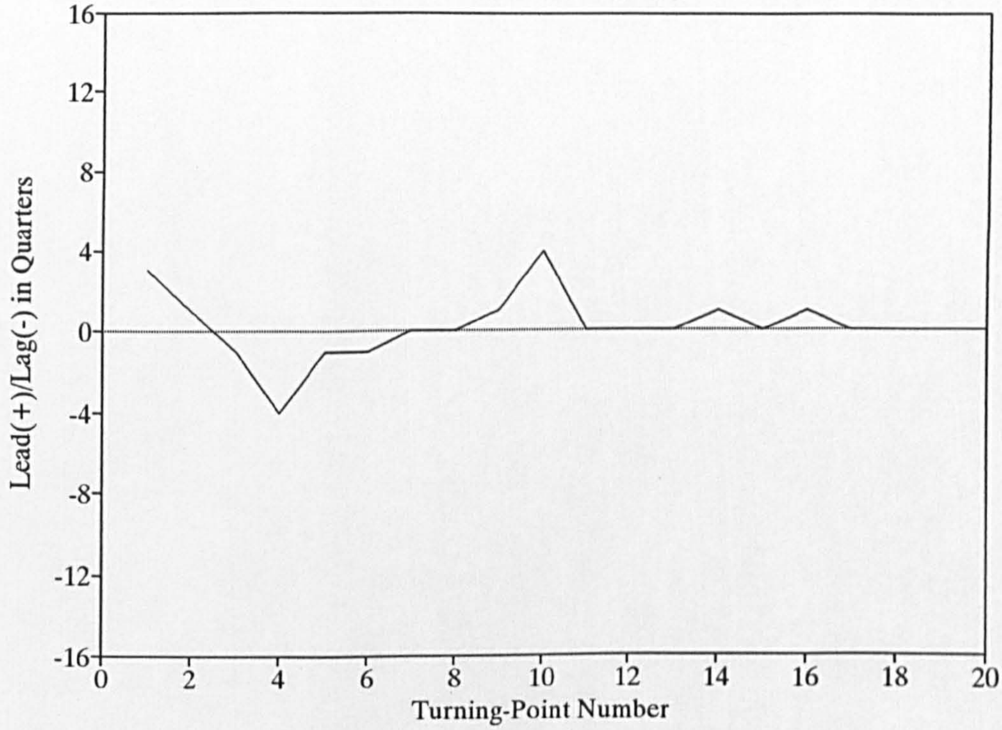


Figure 5.4.25. Pairwise Comparison of (Postwar) Leads and Lags
UK Retail Sales against GDP

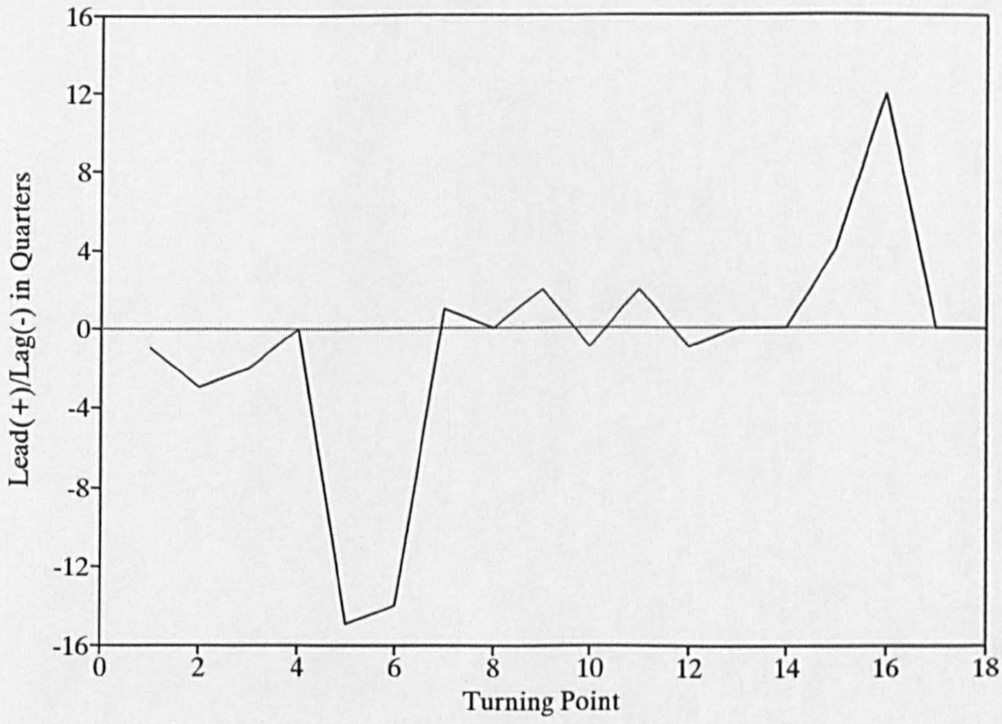


Figure 5.4.26. Pairwise Comparison of (Postwar) Leads and Lags
UK Government Final Consumption against GDP

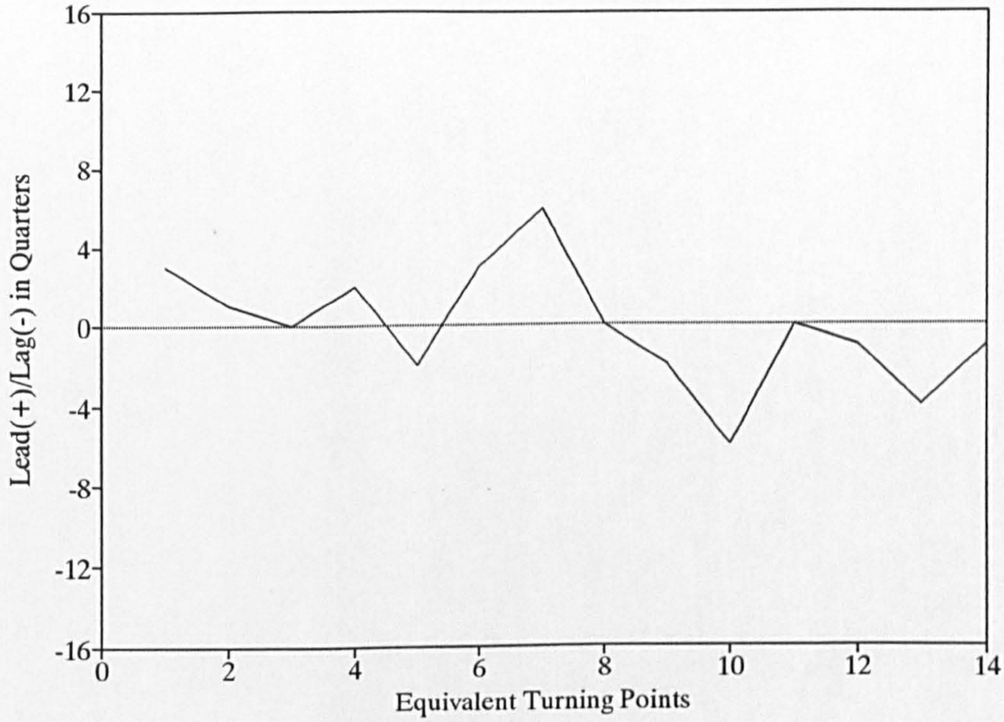


Figure 5.4.27. Pairwise Comparison of (Postwar) Leads and Lags
UK Retail Sales against Industrial Production

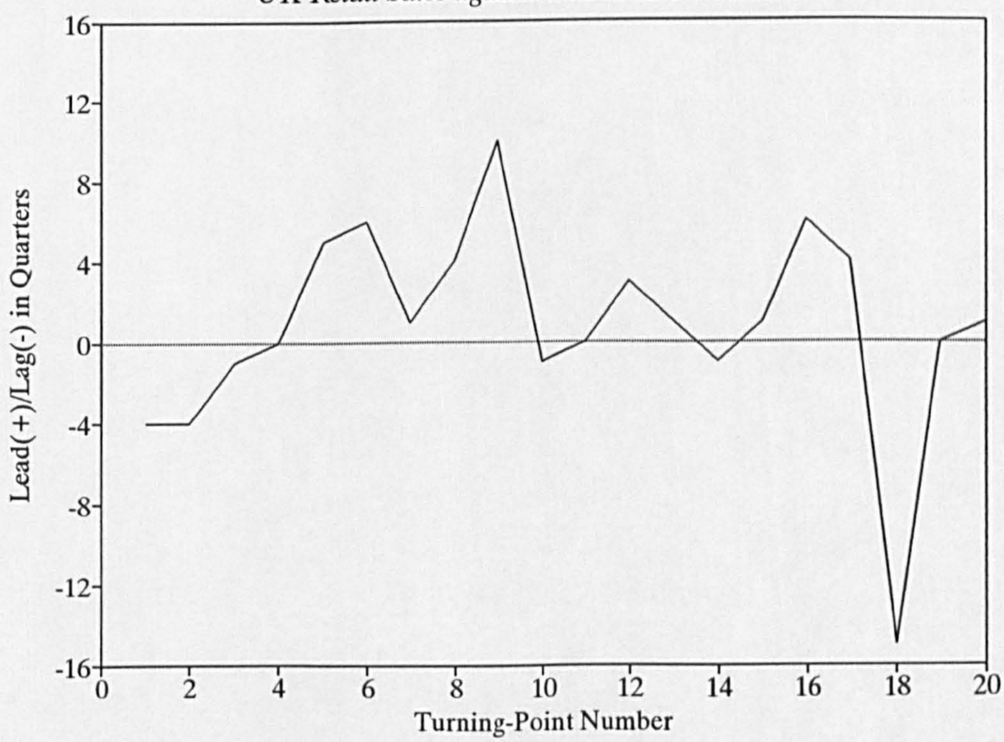


Figure 5.4.28. Pairwise Comparison of (Postwar) Leads and Lags
UK Construction against Industrial Production

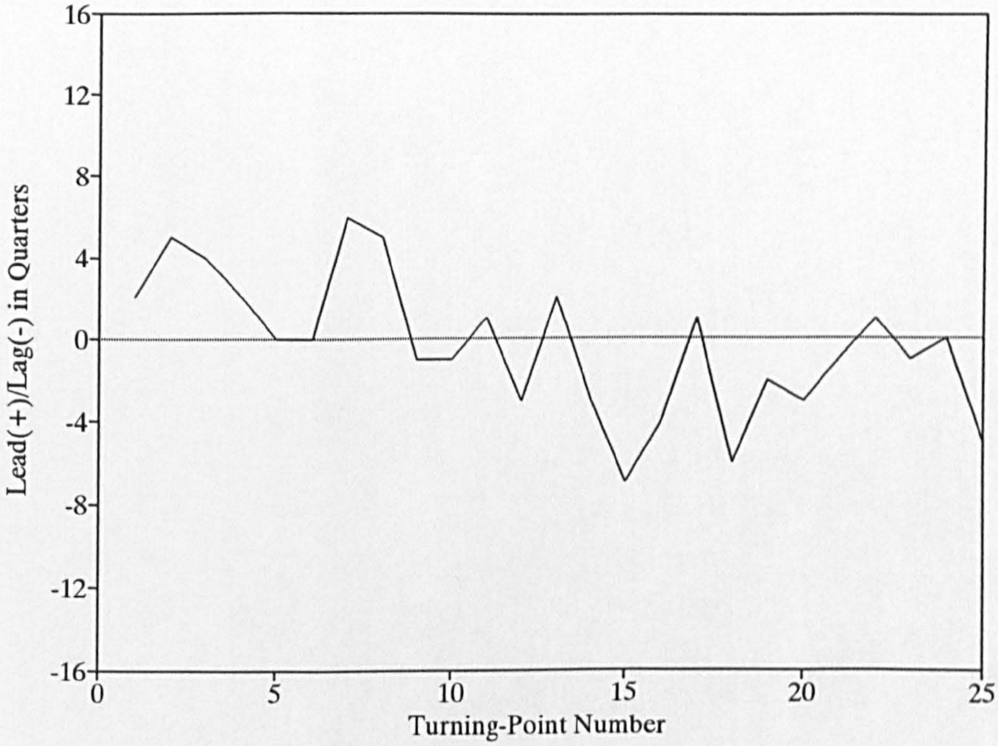


Figure 5.4.29. Pairwise Comparison of (Postwar) Leads and Lags
UK Distribution against Industrial Production

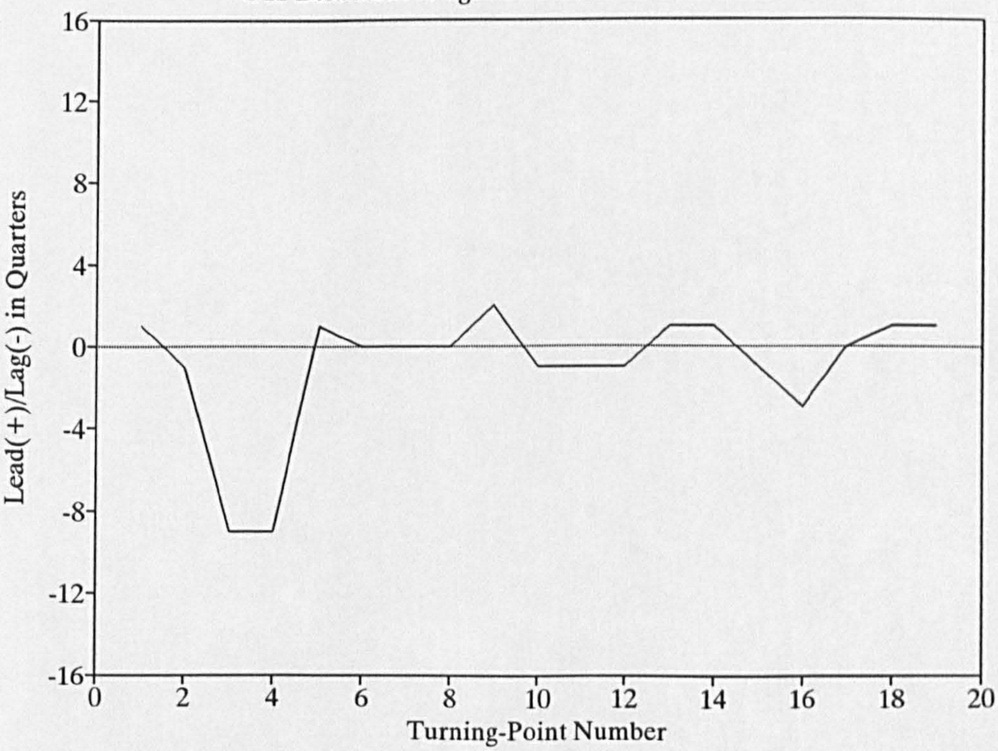


Figure 5.4.30. Pairwise Comparison of (Postwar) Leads and Lags UK Transport and Communication against Industrial Production

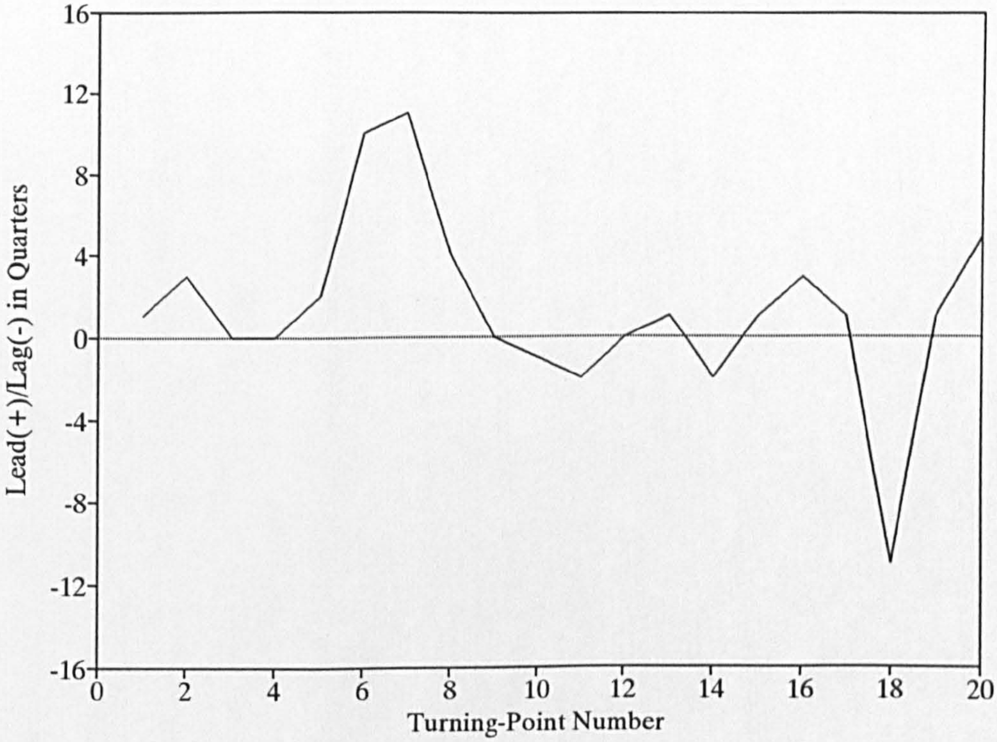


Figure 5.5.1. Lewis's UK GDP Series 1852-1913: Comparison with Annals

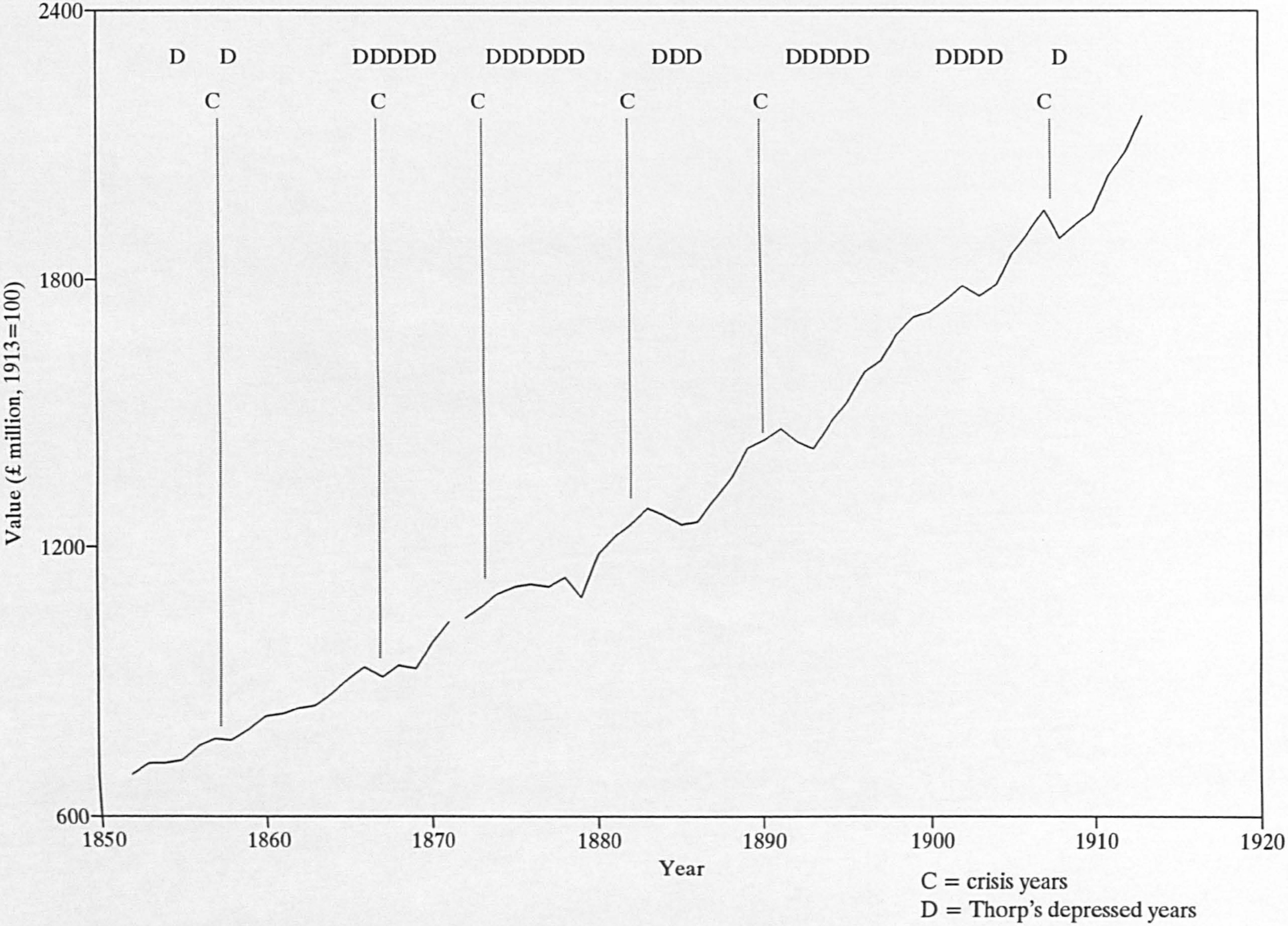
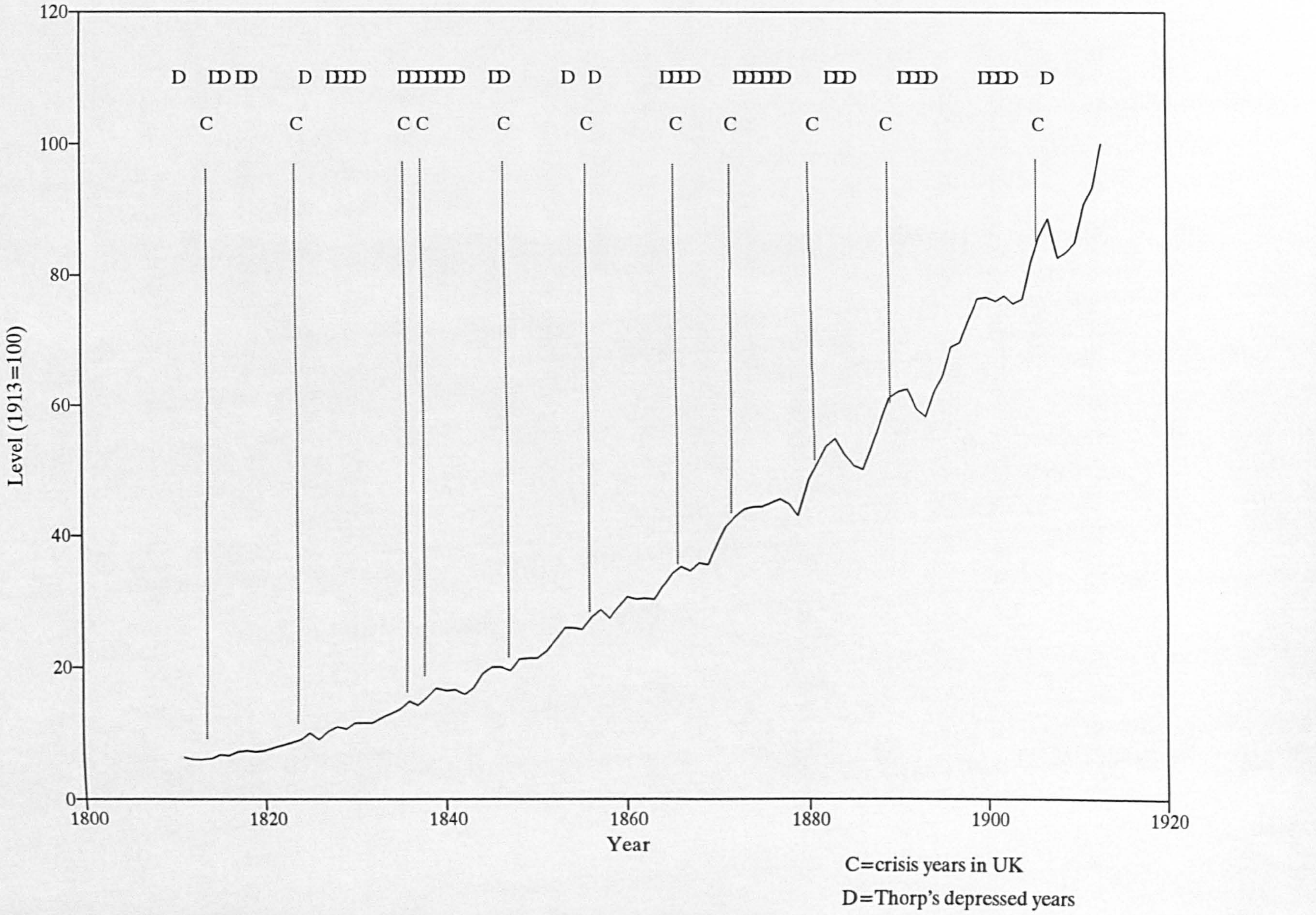


Figure 5.5.2. Crafts-Harley UK Industrial Production Series 1811-1913: Comparison with Annals



CHAPTER 6

CONCLUSIONS

The introduction to this thesis proposed seven general areas of enquiry. These were:

1. How did nineteenth-century experience condition later research?

2. What was the contribution of Mitchell and the NBER to business-cycle theory and what was its influence on later research?

3. What were the results of Mitchell's programme?

4. How have the interpretations of these ideas and results influenced the understanding of business cycles?

5. What has been the contribution of econometric ideas?

6. How does the analysis of modern time-series data affect the understanding of short-run fluctuations?

7. What, therefore, can be inferred about their nature?

The answers to these questions that have emerged from Chapters 2 to 5 are summarized below.

1. The hypothesis of recurrent and general short-run fluctuations originated in the nineteenth century with the occurrence of financial crisis in the United Kingdom approximately every ten years. Explanations of these disturbances focused on quantity-theoretic relations among prices, the monetary circulation and the volume of output. As the century progressed short-run fluctuations in the real and nominal sectors were increasingly perceived to be distinct, as for example in Marshall's evidence to the Gold and Silver Commission and in Juglar's 1900 pamphlet. Nevertheless, by 1914 explanations had crystallized into a 'transitional' dynamics within the framework of Irving Fisher's equation of exchange; that is, that short-term instabilities are driven by price fluctuations transmitted through profits via money to output. In the 1920s Mitchell

proposed a time-subscripted version of the Fisher equation as a theory of business cycles. The modern empirical description of business cycles, as first proposed by Mitchell and Thorp in 1926, became the consensus view in the business-cycle literature until the late 1940s. It later evolved into the 'reference cycle' which is still followed by the NBER dating committee. The empirical findings of MBC, although designed to test Mitchell's theory, were later interpreted as 'measurement without theory' by the Cowles Commission. Marschak's re-interpretation of MBC as a single, unobserved common factor driving all economic fluctuations prepared the ground for, and may have influenced Lucas's conclusion that 'business cycles are all alike'.

2. Burns and Mitchell did not find the evidence of homogeneous business cycles attributed to them by Lucas. In fact, Mitchell concluded that behavioural divergences were more likely than conformities; that all historical episodes were effectively 'unique'; and that short-run fluctuations might actually be a function of 'accelerations and retardations' of the 'more fundamental process' of economic growth. Mitchell's ambiguities and methodological lapses unfortunately complicated later interpretations of his empirical results. Far greater confusion has been caused by ignoring his interest in theory, because his research programme was designed as empirical verification of a quantity-theoretic hypothesis and can be properly understood only in this context. His four-phase cycle, subdivided into eight stages, was followed only by GRS, a contemporary study. It was quickly abandoned after his death and is consequently nowadays only of historical interest. However, the core of the MBC 'definition' — recurrent 'comovements' in 'many economic activities' — has been accepted as 'fact' based on the evidence of that volume. All later research, whether theoretical and empirical, follows this interpretation of Burns and Mitchell by assuming a cycle in 'general business conditions' as fundamental. The modern NBER reference-dating procedure follows methods originally laid down in MBC, according to Romer [1992]; yet although claiming to identify the US 'business cycle' the dating committee appears instead, from the evidence of Table 5.3.2,

to be tracking GNP turning points. This is done with great skill and accuracy but is nevertheless a process which falls outside the NBER's own business-cycle definition, as articulated by Zarnowitz.

3. Time-series models of economic fluctuations have traditionally made the identifying assumption of three or four (unobserved) components: 'trend', 'cyclical', irregular and in some cases seasonal. Originally heuristic, these assumptions were later justified by the 'evidence' of comovements in MBC; but since this was based a misunderstanding of Burns and Mitchell's results, the process of identification has become effectively circular. Recent research has departed from this approach by arguing that trend-cycle decompositions are no longer designed to represent underlying empirical DGPs, but rather to represent what is 'known' about business cycles (as in Harvey [1985], Harvey and Jaeger [1993], CLM [1989]). They cannot therefore be treated as empirical 'evidence'. In a similar manner, RBC studies which simulate business-cycle-style fluctuations in model economies are designed to show that such behaviour can arise in conditions of competitive equilibrium with optimizing agents, rather than to capture empirical behaviour up to an error term with known properties (for example Cooley and Prescott [1995] and the several Kydland and Prescott papers).

4. The asymmetries between short-period expansions and contractions observed in empirical data are not captured by any of the linear models of trend and cycle discussed above. Evidence from the experimental and empirical results of the present study suggests that asymmetric behaviour may be a function of secular tendencies rather than an intrinsic property of a separable cyclical component. This lends weight to the hypothesis of trend and cycle as integral and stochastic, and also calls into question the idea that the two can be sensibly decomposed, especially in linear models. Evidence of this from recent empirical research is mixed. Some supports the integrated representation: Harvey [1985] finds that a unit-root is not rejected for postwar US GNP; and the evidence of King *et al.* [1991] favours a common stochastic trend representation for its

set of US economic variables while Stock and Watson [1989] find that, for a different data set, cointegration is not accepted. On the other hand, Crafts and Mills [1995] find that a nonlinear, deterministic trend representation is not rejected by nineteenth century industrial production data for the UK economy; but here a cyclical component is identified only for the period 1874-1913.

5. Time-series output data may not be sufficiently accurate to permit inferences to be drawn about short-run fluctuations from the statistical analysis of durations and amplitudes of turning points, particularly when strong secular tendencies are present. The nineteenth century presents special difficulties and output estimates such as those of Feinstein [1972] and Crafts-Harley [1992] are consequently described as 'best guesses'. The Feinstein reliability table shows margins of error ranging from > ±5 percent to > ±25 percent. Other distortions are evident: Beveridge 'detrended' his series; Lewis filtered his by moving averages; and Feinstein 'adjusted' his expenditure estimates to take account of Thorp's *Annals*, any of which practices may induce bias. However, recent data also present problems: CSO postwar UK series have error estimates on the order of ±2½ — ±7½ percent. Such wide margins can subsume most if not all turning points in output data since the amplitudes of fluctuations marked off by such peaks and troughs are normally small in comparison both to growth rates and to the acknowledged error margins. As discussed above, peaks and troughs in US GNP alternate naturally and, for quarterly economic time-series data, exceptionally. The behaviour of short-run fluctuations in general cannot be hence inferred from that of the US aggregate, whether or not its levels have been measured accurately. Economic data contain, perhaps, better information about long-run (growth) features, such as the presence or absence of cointegrating vectors, than about short-run variability. The problem of distinguishing the short- from long-run is not addressed by current decomposition practices — either the filters used in theoretical research or the identifying assumptions employed in structural time-series models.

6. Although there is agreement in the current literature about empirical 'business-cycle facts', there are divergent explanations of the phenomena among theorists. These divergences concern the character of impulse and propagation mechanisms and the importance of microfoundations. In all cases, the 'facts' are assumed to be the generalized comovements believed to have been documented by Burns and Mitchell. For this reason most empirical evaluations of RBC *v.* neo-Keynesian hypotheses have employed significant levels of sectoral disaggregation (e.g. Shapiro [1987] and Kydland and Prescott [1990]). Empirical evaluation of the nature and persistence of shocks from the evidence of Solow residuals is problematical; for as Stadler has pointed out, these are difficult to estimate and are interpretable only against the maintained assumption of competitive equilibrium. Despite such debates, recent research appears to be moving towards the more consensual view (either implicit or explicit) that rather than being driven by separate forces, short-run fluctuations are integral with the dynamics of growth. Investigations of Solow residuals as impulse mechanisms for 'real business cycles' are actually assessing the effects of growth-rate innovations. Recent RBC literature, such as Cooley and Prescott [1995], appears to be treating growth and 'cycles' as generated by the same process in a theoretical model. The time-series models of Stock and Watson and Hamilton are also formulated in growth rates rather than in levels. Indeed, Hamilton's turning points (Table 5.3.2) suggest that in the case of US GNP, growth-rate fluctuations are asymmetric and that their peaks and troughs are virtually coincident with those in the levels series. Zarnowitz is the exception, distinguishing 'growth cycles' from 'business cycles' in levels; but he does not propose a distortion-free decomposition of 'trend' and 'cycle'.

7. Evidence from the application to a large number of data sets of an unrestricted turning-point algorithm suggests that the hypothesis of comovements in levels is not supported (subject to the above 'health warning' about accuracy). Nor are there stable timing relations within or across data sets. There is a wide variation in numbers of turning

points in time series within the same data set, and this variation occurs in all data sets in the present study. The relatively flat frequency distributions of turning points consequent upon these 'extra' peaks and troughs appear both in price and output data, suggesting that they are a common behavioural feature. Although the specific dating of turns in output series cannot be relied upon because of measurement error and other inaccuracies, it can nevertheless be tentatively inferred that divergences in numbers of turning points are the rule rather than the exception in time-series data. The implication for policy analysis is that the nature of macroeconomic fluctuations is better understood if examined sector by sector rather than inferred from the behaviour of aggregates.

References

Adelman, Irma and Frank [1959]. 'The Dynamic Properties of the Klein-Goldberger Model.' *Econometrica* **4**, 596-625.

Aldcroft, D.A and P. Fearon [1972]. *British Economic Fluctuations 1790-1939.*

Bagehot, Walter [1873]. *Lombard Street.* 1915 (13th) edition. Smith, Elder & Co. London.

Balke, N.S. and R.J. Gordon [1986]. 'Appendix B Historical Data', in (R.J. Gordon, ed.) *The American Business Cycle.* National Bureau of Economic Research Conference Volume. University of Chicago Press. London.

Baumol, W.J. [1955]. *Economic Dynamics.* Macmillan, London.

Beveridge, Sir W.H. [1909]. *Unemployment, a Problem of Industry.* 1910 (second) edition.

——————[1944]. *Full Employment in a Free Society.* Allen and Unwin, London.

Blanchard, O.J., and S. Fischer [1989]. *Lectures on Macroeconomics.* MIT University Press. London.

——————and Danny Quah, [1987]. 'The Dynamic Effects of Aggregate Demand and Supply Shocks.' MIT Working Paper. September.

——————and Mark Watson [1986]. 'Are Business Cycles all Alike?' In R.J. Gordon (ed.) *The American Business Cycle*, 123-182.

Bry, G. and C. Boschan [1961]. *Cyclical Analysis of Time Series: Selected Procedures and Computer Programs.* NBER Technical Paper No. 20. Columbia University Press.

Burns, Arthur F. [1951]. 'Mitchell on What Happens during Business Cycles', in U.N.B.C. *Conference on the Business Cycle*, 3-14. NBER. New York.

——————[1951]. 'Reply by Mr. Burns', in *ibid.*, 25-33.

——————[1952]. 'Introductory Sketch' in *Wesley Clair Mitchell, the Economic Scientist,* 3-54. NBER, New York.

——————[1961]. 'New Facts on Business Cycles.' In Shishkin, J., Hultgren, T. and G.H. Moore (eds.) *Business Cycle Indicators* I, 13-44.

Campbell, J. and Mankiw, N.G. [1987]. 'Are Output Fluctuations Transitory?' *Quarterly Journal of Economics* **102**, 4(Nov.), 857-880.

Canova, F., Finn, M. and A.R. Pagan [1991]. 'Econometric Issues in the Analysis of Equilibrium Models.' Incomplete working paper given at the Canadian Econometric Study Group meeting, Quebec City, September 1991.

Caporale, G.M. [1993]. 'Productivity Shocks and Business Cycles.' *Applied Economics* **25**, 1065-1070.

Chan, K. Hung, Jack C. Haya and J. Keith Ord [1977]. 'A Note on Trend Removal Methods: The Case of Polynomial Regression versus Variate Differencing.' *Econometrica* **45**, 737-744.

Cooley, T.F. and E.C. Prescott [1995]. 'Economic Growth and Business Cycles.' In *Frontiers of Business Cycle Research*, T.F. Cooley (ed.), 1-38.

Costello, D.M. [1993]. 'A Cross-Country, Cross-Industry Comparison of Productivity Growth.' *Journal of Political Economy* **101**, 207-222.

Crafts, N.F.R, S.J. Leybourne and T.C. Mills [1989]. 'Trends and Cycles in British Industrial Production, 1700-1913'. *Journal of the Royal Statistical Society A* **152**, 43-60.

Crafts, N.F.R. and C. K. Harley [1992]. 'Output Growth and the British Industrial Revolution: a Restatement of the Crafts-Harley View.' *Economic History Review,* XLV 4(1992), 703-730.

Crafts, N.F.R. and T.C. Mills [1995]. 'Trend Growth in British Industrial Output, 1700-1913: a Reappraisal'. Unpublished working paper, Loughborough University and the London School of Economics.

Deane, P. and W.A. Cole [1962]. *British Economic Growth 1688-1959.* Cambridge University Press.

Dimsdale, N.H. [1990]. 'Money, Interest and Cycles in Britain'. *Greek Economic Review* **12**, Supplement, 193-196.

Dornbusch, R. and J.A. Frenkel [1984]. 'The Gold Standard and the Bank of England in the Crisis of 1847'. In Bordo and Schwartz (eds.), *A Retrospective on the Classical Gold Standard, 1821-1931.* National Bureau of Economic Research conference volume. University of Chicago Press.

Eichengreen, B.J. [1983]. 'The Causes of British Business Cycles'. *Journal of European Economic History* **12**, 145-161.

England, M.T. [1912]. 'Fisher's Theory of Crises: a Criticism', *Quarterly Journal of Economics* **XXVII**, 95-106.

Falk, B. [1986]. 'Further Evidence on the Asymmetric Behavior of Economic Time Series over the Business Cycle'. *Journal of Political Economy* **94**, 1097-1109.

Feinstein, C.H. [1972]. *National Expenditure and Output of the United Kingdom 1855-1965.* Cambridge University Press.

——————and S. Pollard [1988]. *Studies in Capital Formation in the UK 1750-1920.* Cambridge University Press.

Fisher, Irving and H.G. Brown [1911]. *The Purchasing Power of Money, its Determination and Relation to Credit, Interest and Prices.*

——————[1925]. 'Our Unstable Dollar and the So-called Business Cycle'. *Journal of the American Statistical Association* **20**, 179-202.

Ford, A.G. [1981]. 'Trade Cycles 1870-1914.' In Floud, R.C and D.N. McCloskey (eds.) *The Economic History of Britain since 1700* (first edition) **II**, 131-159.

Friedman, B.M. [1986]. 'Money, Credit and Interest Rates in the Business Cycle', in R.J. Gordon (ed.), *The American Business Cycle.*

Friedman, M. [1952]. 'The Economic Theorist', in Burns (ed.), *Wesley Clair Mitchell, the Economic Scientist,* 237-282.

Frisch, Ragnar [1927]. *The Analysis of Statistical Time Series.* Unpublished mimeo.

——————[1933]. 'Propagation and Impulse Problems in Dynamic Economics.' In *Economic Essays in Honour of Gustav Cassel,* 171-205. Allen and Unwin, London.

Gayer, A.D., W.W. Rostow and Anna Schwartz [1953]. *The Growth and Fluctuation of the British Economy*, Vols. I and II. Clarendon Press, Oxford.

Giffen, Sir R. [1885]. 'Recent Changes in Prices and Incomes Compared' in *Economic Enquiries and Studies.*

——————[1888]. 'Trade Depression and Low Prices' in *The Contemporary Review.*

Goldberger, A.S. [1959]. *Impact Multipliers and the Dynamic Properties of the Klein-Goldberger Model*, North-Holland, Amsterdam.

——————[1972]. 'Structural Equation Methods in the Social Sciences.' *Econometrica* **40**, 979-1001.

Goodwin, R.M. [1953]. 'The Problem of Trend and Cycle.' *Yorkshire Bulletin of Economic and Social Research* **5**, 89-97.

Gordon, R.J. (ed.) [1986]. *The American Business Cycle.* NBER, New York.

Hall, R.E. [1991]. 'Economic Fluctuations.' *NBER Reporter*, Summer 1991.

——————[1991/2]. 'The Business Cycle Dating Process.' *NBER Reporter*, Winter 1991/2.

Harvey, A.C. [1981]. *Time Series Models.* Philip Allan, London.

——————[1985]. 'Trends and Cycles in Macroeconomic Time Series', *Journal of Business and Economic Statistics* **3**, 216-227.

——————[1989]. *Forecasting, Structural Time Series Models and the Kalman Filter.* Cambridge University Press, Cambridge.

——————and A. Jaeger [1993]. 'Detrending, Stylized Facts and the Business Cycles.' *Journal of Applied Econometrics* **8**, 231-247.

Hamilton, James D. [1989]. 'A New Approach to the Economic Analysis of Nonstationary Time Series and the Business Cycle.' *Econometrica* **57** No. 2, 357-384.

Hoffman, W.G. [1955]. *British Industry, 1700-1850.* Blackwell, Oxford.

Howrey, E. Philip [1968]. 'A Spectrum Analysis of the Long-Swing Hypothesis.' International Economic Review 9 No. 2, 228-252.

Hughes, J.R.T. [1960]. *Fluctuations in Trade, Industry and Finance.* Clarendon Press, Oxford.

Hull, G.H. [1911]. *Industrial Depressions.* 1926 (second) edition. Codex Book Company, Inc. New York.

Hyndman, H.M. [1892]. *Commercial Crises of the Nineteenth Century.* 1908 edition. Swan Sonnenschein & Co., Ltd. London.

Jevons, W. Stanley [1863]. 'A Serious Fall in the Value of Gold Ascertained, and its Social Effects Set Forth'. In *Investigations in Currency and Finance* [1884]. 1909 (second) edition, pp.13-111. Macmillan & Co. London.

——————[1865]. 'The Variation of Prices, and the Value of the Currency since 1782'. In *Ibid.*, pp. 112-142.

——————[1866]. 'On the Frequent Autumnal Pressure in the Money Market, and the Action of the Bank of England'. In *Ibid.*, pp. 151-174.

——————[1878, 1879]. 'Commercial Crises and Sunspots'. In *Ibid.*, pp. 201-213.

Johnston, J. [1955]. 'Econometric Models and the Average Durations of Business Cycles.' *Manchester School,* **23**, 193-227.

Juglar, Clement [1862] 1889. *Des crises commerciales et leur retour périodiques en France, en Angleterre et aux États-Unies.* Guillaumin, Paris.

——————[1900]. *Les crises commerciales et financières et les crises économiques générales.* Pamphlet, Paris.

——————[1893. *A Brief History of Panics and their Periodical Occurrence in the United States.* (Trans. D.W. Thom). 1966 reprint of 1915 edition. Augustus Kelley, New York.

Kendall, M.G. [1944]. 'On Autoregressive Time Series.' *Biometrika* **33**, 105-122.

——————[1946]. *Contributions to the Study of Oscillatory Time-Series.* NIESR Occasional Paper. Cambridge University Press.

——————[1953]. 'The Analysis of Economic Time Series—Part I: Prices'. *Journal of the Royal Statistical Society,* Part I, 1953.

Keynes, J.M. [1930]. 'Inflation and Deflation' in *Essays in Persuasion.*

Kim, K. and A.R. Pagan [1993]. 'The Econometric Analysis of Calibrated Macroeconomic Models.' Unpublished working paper.

King, R.G., Plosser, C.I., Stock, J.H. and M.W. Watson [1987]. 'Stochastic Trends and Economic Fluctuations.' NBER Working Paper 2229. NBER, Cambridge, Mass.

——————[1991]. 'Stochastic Trends and Economic Fluctuations.' American Economic Review **81**, 819-840.

Klein, L.R., R.J. Ball, A. Hazlewood and P. Vandome [1961]. *An Econometric Model of the United Kingdom*, Basil Blackwell, Oxford.

——————and A.S. Goldberger [1955]. *An Econometric Model of the United States 1929-1952.* Amsterdam North Holland.

Koopmans, T. C. [1947]. 'Measurement without Theory'. *Review of Economic Statistics*, **29**, 161-172. Reprinted in *Readings in Business Cycles*, R.A. Gordon and L.R. Klein, eds. [1966]. Geo. Allen & Unwin, London.

————[1949]. 'A Reply'. *Review of Economics and Statistics*, **31**, May, 1949, 86-90. Reprinted Gordon and Klein, *op. cit.*

————[1957]. *Three Essays on the State of Economic Science*. McGraw Hill Book Co. Inc., New York. Reprinted in Gordon and Klein, *op. cit.*

Kuhn, T.S. [1962]. *The Structure of Scientific Revolutions*. Second Edition 1970. University of Chicago Press.

Kuznets, Stephen [1929]. 'Random Events and Cyclical Oscillations.' *Journal of the American Statistical Association*, **24**, pp. 258-275.

————[1937]. *National Income and Capital Formation, 1919-1935* . NBER, New York.

Kydland, F.E. and E.C. Prescott [1990]. 'Business Cycles: Real Facts or a Monetary Myth.' Federal Reserve Bank of Minneapolis *Quarterly Review* **14**, 3-18.

————[1991]. 'The Econometrics of the General Equilibrium Approach to Business Cycles.' *Scandinavian Journal of Economics* **93**, 161-178.

————[1995]. 'The Computational Experiment: an Econometric Tool.' Federal Reserve Bank of Minneapolis Research Department Staff Report 178.

Lewis, W.A. [1978]. *Growth and Fluctuations, 1870-1913.* George Allen & Unwin, Ltd. Cambridge.

Long, J. and C.I. Plosser [1983]. 'Real Business Cycles.' *Journal of Political Economy* **91**, 39-69.

Loyd, S. (Lord Overstone) [1837]. *Reflections Suggested by a Perusal of Mr. J. Horsley Palmer's Pamphlet on the Causes and Consequences of the Pressure on the Money Market.* Pamphlet, London.

Lucas, Robert E. [1977]. 'Understanding Business Cycles.' In K. Brunner and A.H. Meltzer (eds.) *Stabilization of the Domestic and International Economy*, 7-29. Amsterdam North Holland.

————[1981]. *Studies in Business Cycle Theory*. Basil Blackwell, Oxford.

Luukonen, Ritva and Timo Teräsvirta [1991]. 'Testing Linearity of Economic Time Series Against Cyclical Asymmetry'. *Annales d'Économie et de Statistique,* **20/21** (1991), 125-142.

Malthus, R. [1836] 1964. *Principles of Political Economy*. Kelley, New York.

Marschak, J. [1951]. 'Comment' on Burns, *op. cit.*, in U.N.B.C. *Conference on the Business Cycle*, 14-24.

Marshall, Alfred. [1926]. *Official Papers*. Macmillan & Co. London.

Matthews, R.C.O. [1954a]. 'The Trade Cycle in Britain, 1790-1850'. *Oxford Economic Papers*, **6**, 1-32.

————[1954b]. *A Study in Trade Cycle History*. Cambridge University Press.

McElvaine, Robert S. [1984]. *The Great Depression. America 1929-41*. Times Books, New York.

McQueen, G. and S Thorley [1993]. 'Asymmetric Business Cycles Turning Points'. *Journal of Monetary Economics* **31** (1993) 341-362.

Mill, J.S. [1857] 1885. *Principles of Political Economy.* Longmans, Green & Co. London.

Mills, John [1867]. 'On Credit Cycles and the Origin of Commercial Panics' in *Transactions of the Manchester Statistical Society* 1867-8, 11–40.

Mitchell, Brian [1988]. *British Historical Statistics.* Cambridge University Press.

Mitchell, Wesley C. [1896]. 'The Quantity Theory of the Value of Money'. *Journal of Political Economy*, **4**, March 1896, 139-165.

——————[1903]. *A History of the Greenbacks, with Special Reference to the Economic Consequences of their Issue: 1862–65.* University of Chicago Press.

——————[1904]. 'The Real Issues in the Quantity Theory Controversy'. *Journal of Political Economy*, **12**, June 1904, 403–408.

——————[1908]. *Gold, Prices and Wages under the Greenback Standard.* University of California Publications in Economics **1**. University of California Press, Berkeley.

——————[1912]. Review of Irving Fisher's *The Purchasing Power of Money.* In *Political Science Quarterly*, **27**, March 1912, 160–164.

——————[1913]. *Business Cycles.* California University Memoirs, Vol. III. Berkeley.

——————[1915]. *The Making and Using of Index Numbers.* Bulletin 173, Bureau of Labor Statistics, revised 1921 in Bulletin 284.

——————[1927]. *Business Cycles: the Problem and its Setting.* NBER, New York.

——————[1929a]. 'Testing Business Cycles'. *News-Bulletin of the National Bureau of Economic Research*, No. 31, March 1, 1929, 1-8. [Not signed.]

——————[1929b]. 'A Review'. In *Recent Economic Changes.* **2**. National Bureau of Economic Research, 1929. xxxvi, 841–910.

——————[1937]. *The Backward Art of Spending Money and other Essays.* McGraw-Hill.

——————[1951]. *What Happens during Business Cycles: A Progress Report.* (Edited and with an introduction by Arthur Burns). NBER.

——————and Arthur F. Burns [1935]. 'The National Bureau's Measures of Cyclical Behavior'. *News-Bulletin* No. 57. NBER, July 1, 1935.

——————[1936]. 'Production during the American Business Cycle of 1927–1933. *News-Bulletin* No. 61. NBER, November 9, 1936.

——————[1938]. 'Statistical Indicators of Cyclical Revivals'. *News-Bulletin* No. 69. NBER, May 28, 1938.

——————[1946]. *Measuring Business Cycles.* NBER, New York.

Morgan, Mary S. [1990]. *The History of Econometric Ideas.* Cambridge University Press, Cambridge.

National Bureau of Economic Research [1991]. 'NBER Business Cycle Dating Committee Determines that Recession Began in July 1990.' Press Release.

Neftçi, S. N. [1984]. 'Are Economic Time Series Asymmetric over the Business Cycle?' *Journal of Political Economy*, **92**, 307-328.

Nelson, C.R. and Kang, H. [1981]. 'Spurious Periodicity in Inappropriately Detrended Time Series,' *Econometrica*, **49**, 741-751.

Nelson, C.R. and Plosser, C.I. [1982]. 'Trends and Random Walks in Macroeconomic Time Series', *Journal of Monetary Economics*, **10**, 139-162.

Orcutt, G.H. [1948]. 'A Study of the Autoregressive Nature of the Time Series used for Tinbergen's Model of the Economic System of the United States, 1919-1932.' *Journal of the Royal Statistical Society* B, **10**, 1-45.

Osborn, Denise R. [1993]. 'Moving Average Detrending of Integrated Processes'. Unpublished working paper, University of Manchester.

Perron, Pierre [1989]. 'The Great Crash, the Oil Price Shock, and the Unit Root Hypothesis.' *Econometrica* **57**, 1361-1401.

Persons, W.M. [1919]. 'An Index of General Business Conditions.' *Review of Economic Statistics* **1**, 111-205.

——————[1925]. 'Statistics and Economic Theory.' *Review of Economic Statistics* **7**, 179-197.

Petty, Sir William [1899] 1963. *The Economic Writings of Sir William Petty* **I**, (C.H. Hull, ed.). Kelley, New York.

Ricardo, David [1951]. *Works of David Ricardo* **I**. (P. Sraffa, ed.). Cambridge University Press, Cambridge.

Romer, C.D. [1992]. 'Remeasuring Business Cycles'. NBER Working Paper No. 4150.

Rostow, W.W [1948]. *British Economy of the Nineteenth Century*. Clarendon Press. Oxford.

——————[1972]. 'Cycles in the British Economy, 1790-1914'. In Aldcroft, D.H. and P. Fearon (eds.) *British Economic Fluctuations 1790-1939*. Macmillan St. Martin's Press. London.

Sargent, T.J. and Sims, C.A., [1977]. 'Business Cycle Modelling without Pretending to have Too Much a priori Economic Theory', in Sims et. al., *New Methods in Business Cycle Research*. Minneapolis: Federal Reserve Bank of Minneapolis.

Schumpeter, J. [1939]. *Business Cycles*, **I**, **II**. McGraw, Hill Book Co. New York and London.

——————[1952]. 'The General Economist.' In *Wesley Clair Mitchell, the Economic Scientist*, 321-342.

——————[1954]. *History of Economic Analysis*. Oxford University Press, New York.

Scott, W.R. [1912]. *The Constitution and Finance of the English, Scottish and Irish Joint-Stock Companies to 1720* **I**. Reprinted 1951. Peter Smith, New York.

Shapiro, M.D. [1987]. 'Are Cyclical Fluctuation in Productivity Due More to Supply Shocks or Demand Shocks.' *American Economic Review (Papers and Proceedings)* **77**, 118-124.

Sichel, D.E. [1993]. 'Business Cycle Asymmetry: a Deeper Look.' *Economic Inquiry*, **XXXI**, 224-236.

Sims, C. A. [1989]. 'Modelling Trends.' Federal Reserve Bank of Minneapolis, University of Minnesota, Discussion Paper 22.

Slutsky, Eugen [1937]. 'The Summation of Random Causes as the Source of Cyclic Processes', *Econometrica* **5**, 105-146.

Solomou, S. [1987]. *Phases of Economic Growth, 1850-1973.* Cambridge University Press.

——————[1994]. 'Economic Fluctuations, 1870-1913' in Floud, R. and D. McClosky (eds.) *The Economic History of Britain since 1700* **II**, 247-264. Cambridge University Press.

Solow, R.M. [1956]. 'A Contribution to the Theory of Economic Growth.' *Quarterly Journal of Economics* **LXX**, 65-94.

——————[1957]. 'Technical Change and the Aggregate Production Function.' *Review of Economics and Statistics* **39**, 312-320.

Stadler, G.W. [1994]. 'Real Business Cycles.' *Journal of Economic Literature* **XXXII**, 1750-1783.

Stock, J.H. and M. W. Watson [1988]. 'A Probability Model of the Coincident Economic Indicators.' NBER Working Paper No. 2772.

——————[1989]. 'New Indexes of Coincident and Leading Economic Indicators.' *NBER Macroeconomics Annual 1989*, O.J. Blanchard and S. Fischer, eds., 351-394. NBER Cambridge, Mass.

——————[1991]. 'A Probability Model of the Coincident Economic Indicators.' NBER Working Paper No. 2772. National Bureau of Economic Research, Cambridge, Mass.

——————[1992]. 'A Procedure for Predicting Recessions with Leading Indicators: Econometric Issues and Recent Experience.' NBER Working Paper No. 4014. National Bureau of Economic Research, Cambridge, Mass.

Thorp, Willard [1926]. *Business Annals.* NBER, New York.

Tinbergen, J. [1935]. 'Annual Survey: Suggestions on Quantitative Business Cycle Theory'. *Econometrica,* **3**, 241-208.

——————[1939]. *Business Cycles in the United States of America II,* League of Nations Economic Intelligence Service. Geneva.

——————[1940]. 'Econometric Business Cycle Research'. *Review of Economic Studies* **7**, 73-90.

——————[1951]. *Business Cycles in the U.K. 1870-1914*, North-Holland, Amsterdam.

——————[1959]. *Jan Tinbergen: Selected Papers.* L.H. Klaasen, L.M. Koyck and H.J. Witteveen (eds.). Amsterdam: North Holland.

Tooke, Thomas [1841-1856]. *History of Prices*, **I, II, V, VI.**

Veblin, Thorstein [1904]. *The Theory of Business Enterprise*, New York.

Viner, Jacob [1932]. *Studies in the Theory of International Trade.* 1955 edition.

Vining, R. [1949a]. 'Koopmans on the Choice of Variables to be Studies and of Methods of Measurement.' *Review of Economic and Statistics* **XXXI**, 77-85.

——————[1949b]. 'A Rejoinder.' *Review of Economics and Statistics* **XXXI**, 91-94.

Wade, John [1833]. *History of the Middle and Working Classes.* London.

Wallis, K.F. [1977]. 'Multiple Time Series Analysis and the Final Form of Econometric Models.' *Econometrica* **45**, 1481-1497.

Watson, M.W. [1992]. 'Business Cycle Durations and Postwar Stabilization of the U.S. Economy.' NBER Working Paper No. 4005.

——————[1994]. 'Business Cycle Durations and Postwar Stabilization of the U.S. Economy.' *American Economic Review* **84**, 24-46.

Wicksell, Knut [1899]. *Interest and Prices.*

Yule, G. Udny [1926]. 'Why do we sometimes get Nonsense-Correlations between Time-Series? — a Study in Sampling and the Nature of Time-Series'. *Journal of the Royal Statistical Society*, **89**, 1-64.

——————[1927]. 'On a Method of Investigating Periodicities in Disturbed Series, with special reference to Wolfer's Sunspot Numbers'. *Philosophical Transactions of the Royal Society of London*, (A), **226**, 267-298.

——————[1971]. *Statistical Papers of George Udny Yule.* Selected by A. Stuart and M.G. Kendall. Charles Griffin & Co. London.

Zarnowitz, V. [1992]. 'What is a Business Cycle?' *The Business Cycle: Theories and Evidence*, Belongia, M.T. and M.R. Garfinkel (eds.), 3-72.

—————— and P. Braun [1990]. 'Major Macroeconomic Variables and Leading Indexes: Some Estimates of their Interaction.' In P.A. Klein (ed.) *Analyzing Modern Business Cycles*, 177-205. M.E. Sharpe, Armonk New York.