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CHAPTER 10

Secular and Discontinuous Changes in Cyclical Behavior

OUR FIRST problem is to determine whether substantial secular changes can be detected in cyclical behavior as we measure it. Towards this end we have tested in detail the seven American series presented in the preceding chapter. These series cover processes that rank high among the activities stressed in theoretical studies of business cycles. Partly for this reason, partly because of the comparatively long stretch of time covered by these records, we regard our small sample as fairly satisfactory for the present purpose. We analyze also durations of business cycles, the one tolerably reliable set of measures that we have of business cycles as wholes.[1]

I Duration and Amplitude of Specific Cycles

To investigate secular changes in our sample, we have fitted straight lines by the method of least squares to the durations and amplitudes of successive specific cycles in each series.[2] Table 144 and Chart 55 present the results. As the chart shows, the trend lines sometimes fit the cyclical observations badly. Hence we supplement them in Table 145 by subgroup means, each set of cyclical measures being divided into three parts as nearly equal as possible.[3]

1 We are under heavy obligations to Milton Friedman for assistance in handling the technical problems encountered in this and the next two chapters.

2 Measures for the individual specific and reference cycles in the seven series are shown in Appendix B.

3 The bulk of the statistical analysis in Ch. 9-12 was completed before measurements for the business cycle of 1933–38 were ready; hence, with minor exceptions, our analysis stops around 1933. The addition of another cycle, while obviously desirable, would not modify the main results.

TABLE 144

Extreme Ordinates of Straight-line Trends
Fitted to Durations and Amplitudes of Successive Specific Cycles
Seven American Series

Series	Period covered	No. of specific cycles	Duration in months		Amplitude in specific-cycle relatives: Rise & fall		Amplitude in specific-cycle relatives: Rise & fall per month	
			First trend value	Last trend value	First trend value	Last trend value	First trend value	Last trend value
Deflated clearings	1878–1933	15	32	56	38	42	1.1	0.8
Pig iron production	1879–1933	15	44	43	95	139	2.4	3.3
Freight car orders	1870–1933	19	48	31	405	444	8.8	17.6
Railroad stock prices	1857–1932	18	51	49	55	80	1.5	1.4
Shares traded	1878–1933	15	46	42	164	217	3.9	5.2
Call money rates	1858–1931	23	37	38	254	210	7.2	5.4
Railroad bond yields	1860–1931	20	34	51	25	21	0.9	0.4

See Chart 55.

TABLE 145

Average Duration and Amplitude of Three Successive Groups of Specific Cycles
Seven American Series

Series and period covered	Number of specific cycles	Average duration in months	Average amplitude in specific-cycle relatives: Rise & fall	Average amplitude in specific-cycle relatives: Rise & fall per month[a]
Deflated clearings				
1878–1893	5	37	42	1.1
1893–1910	5	41	37	0.9
1910–1933	5	54	41	0.8
Pig iron production				
1879–1896	5	43	107	2.8
1896–1914	5	44	113	2.6
1914–1933	5	44	131	3.2
Freight car orders				
1870–1894	6	48	421	9.7
1894–1914	6	40	319	8.3
1914–1933	7	32	518	20.4
Railroad stock prices				
1857–1889	6	63	76	1.5
1889–1907	6	37	52	1.4
1907–1932	6	49	74	1.4
Shares traded				
1878–1897	5	46	148	3.6
1897–1914	5	42	220	5.2
1914–1933	5	44	204	4.9
Call money rates				
1858–1880	7	38	218	5.9
1880–1904	8	36	289	8.4
1904–1931	8	40	187	4.5
Railroad bond yields				
1860–1876	6	32	27	0.9
1876–1905	7	49	20	0.4
1905–1931	7	45	24	0.6

Where the number of cycles is not exactly divisible by 3, an additional cycle is placed in the last group or in both the second and last.

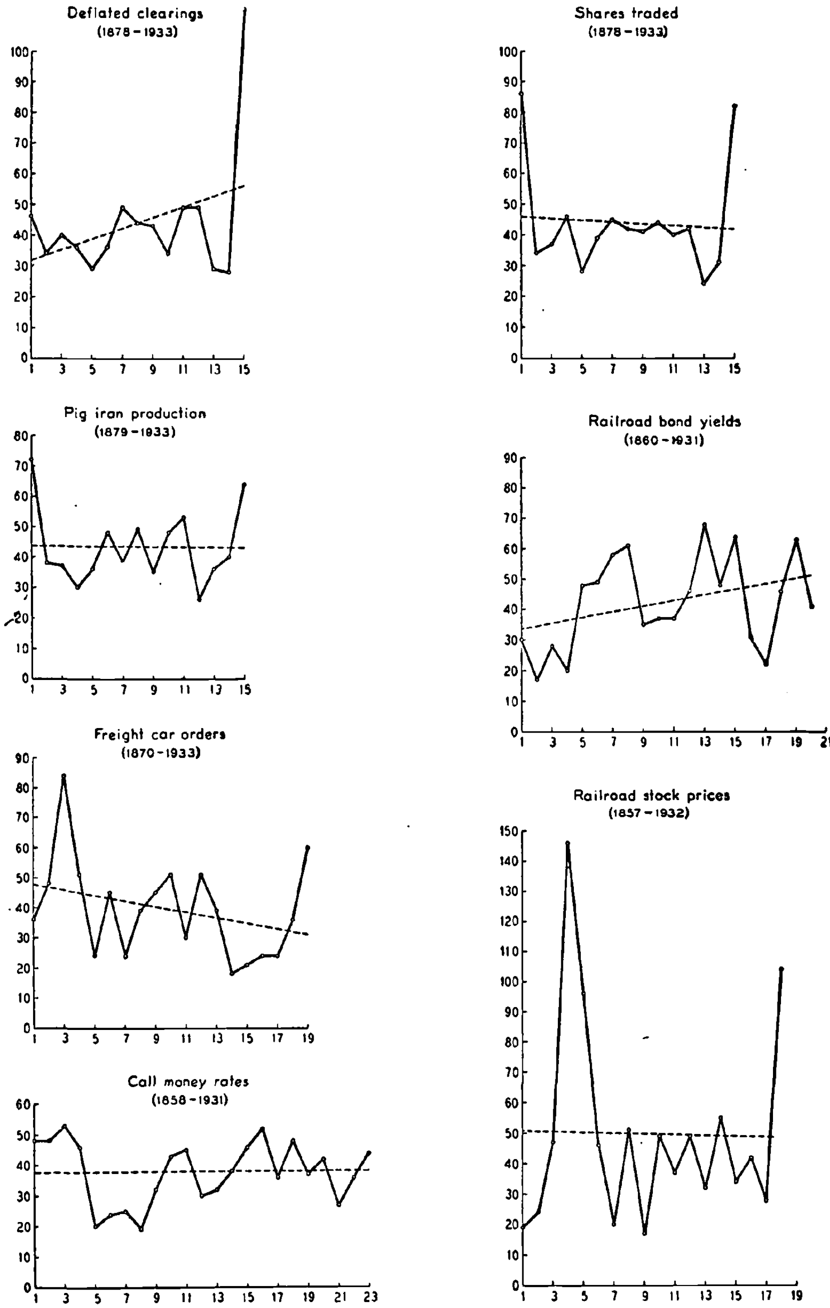
[a] Unweighted average.

CHART 55

Secular Changes in the Duration and Amplitude of Specific Cycles
Seven American Series

Duration of Full Specific Cycles[a]

------ Straight line fitted by 'least squares'



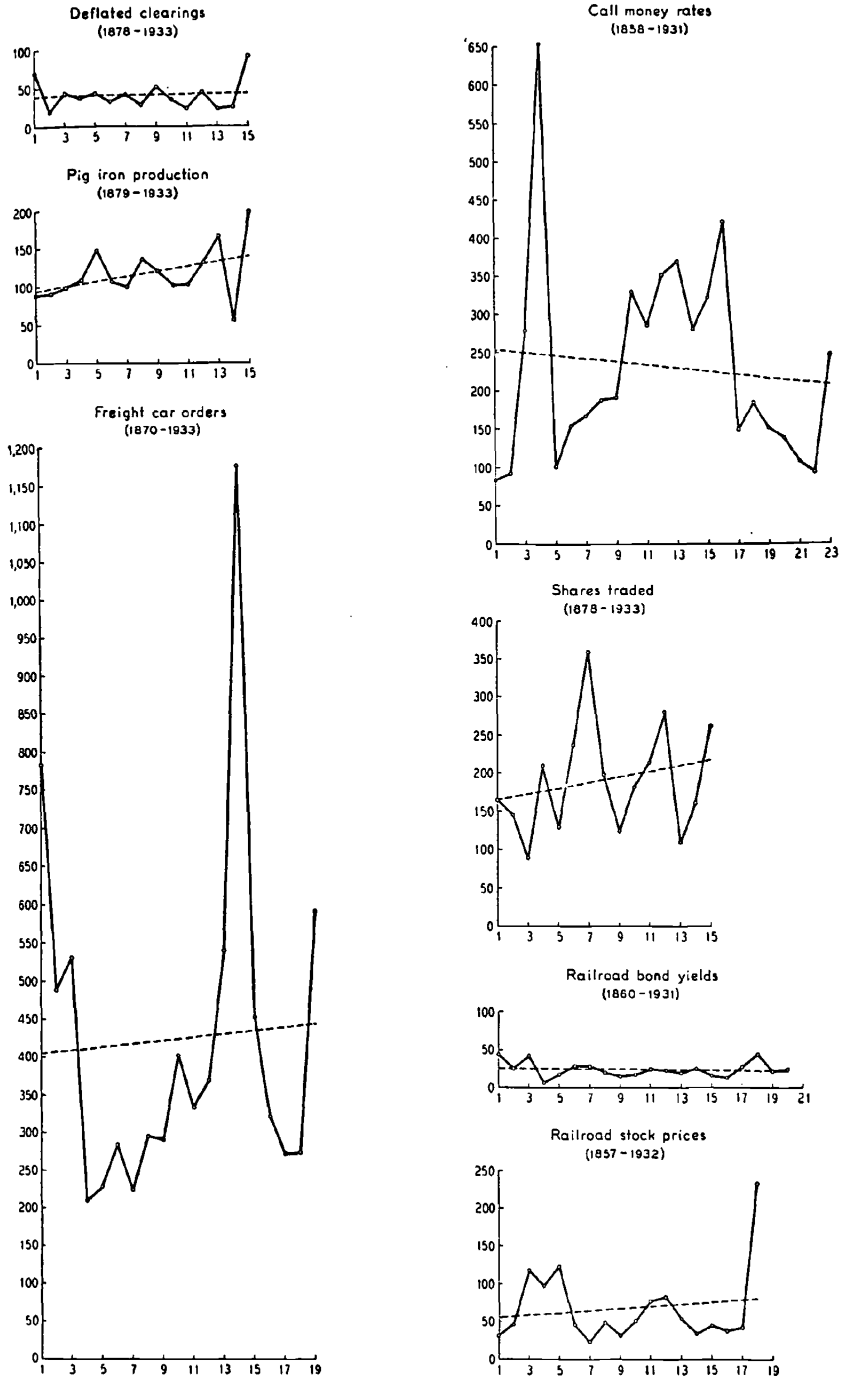
[a] The horizontal scale shows serial numbers of successive specific cycles; the vertical scale represents months

See Table 144 and Appendix Table B1.

CHART 55 (CONT.)

Amplitude of Full Specific Cycles[b]

------ Straight line fitted by 'least squares'



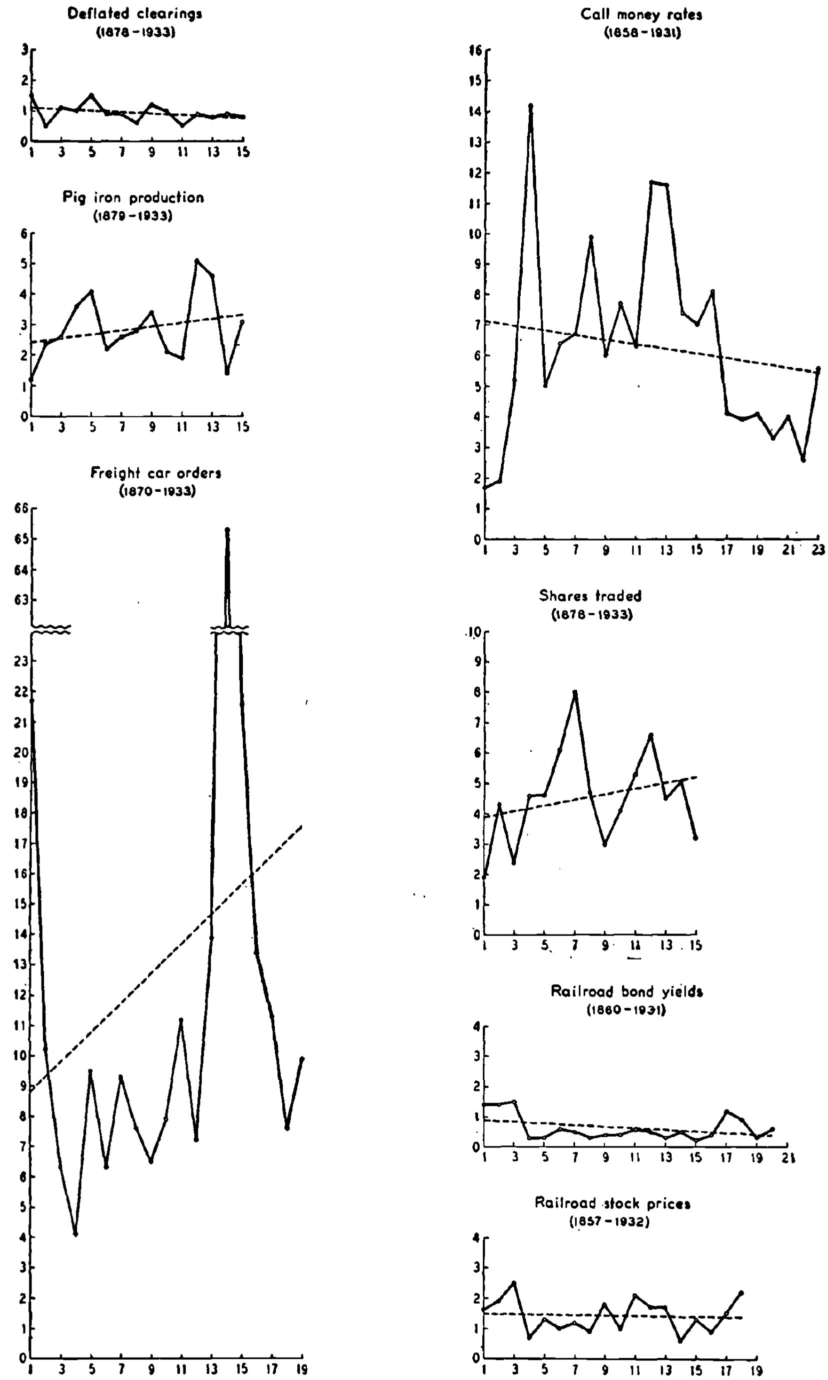
[b] The horizontal scale shows serial numbers of successive specific cycles; the vertical scale represents specific-cycle relatives.

See Table 144 and Appendix Table B1.

CHART 55 (CONT.)

Per Month Amplitude of Full Specific Cycles[c]

------ Straight line fitted by 'least squares'



[c] The horizontal scale shows serial numbers of successive specific cycles; the vertical scale represents specific-cycle relatives.

See Table 144 and Appendix Table B1.

A casual glance at these records might suggest that secular change is a general characteristic of cyclical fluctuations. But the mere fact that the trend lines are not horizontal, or that the subgroup averages differ within each series, does not demonstrate that secular changes have been marked or even that there have been any. Similar results could easily be obtained by fitting trend lines or computing subgroup means for random series; for example, successive values obtained from dice throws or the last places of a table of logarithms. The odds are heavy that a mathematically fitted trend will not be horizontal and that subgroup averages will not be constant. What we have to determine, therefore, is whether the slopes of our trend lines or the variations in the subgroup averages are 'substantial'; and if that does seem to be the case, whether they reflect persistent or haphazard changes. Should we find that secular changes in cyclical measures have as a rule been both 'substantial' and 'statistically significant', we would be forced to regard our notion that long-run averages of cyclical behavior give a useful first approximation of business-cycle behavior as discredited.[4]

A partial answer to our problem is given by measures of correlation between cyclical behavior and time (Table 146). We use two measures of correlation: the square of the coefficient of correlation and the square of the correlation ratio. The former shows the fraction of the total variation of the cyclical measures for each series that is attributable to the linear trend. The latter shows the fraction of the total variation that is attributable to the subgroup averages considered as a step-line of trend.[5] The outstanding feature of the correlation measures is their extremely low level. The square of the coefficient of correlation between cyclical durations and time varies in different series from 0 to .14; the square of the coefficient of correlation between cyclical amplitudes and time varies from 0 to .16 in the total amplitudes and from 0 to .13 in the per month amplitudes. Of course, the correlation ratios squared are higher in most

[4] It is important to distinguish between 'substantial' and 'significant' secular changes. If accounting records show that Mr. X received $1,000.00 during the first year of teaching, $1,000.01 the second year, $1,000.02 the third, and so in regular increments until the fiftieth year when he retired after enjoying a stipend of $1,000.49; then, there has been a definite trend in the teacher's annual earnings. This trend is 'statistically significant', but just as surely it is trifling and of no practical consequence.

Of course, what secular changes are 'substantial' and what 'slight' is a matter of judgment, which is bound to shift from problem to problem.

[5] In each computation the total variation is measured by the sum of the squares of the deviations of individual observations from their mean. Let Y be any observation on (say) durations of successive specific cycles, Y_t successive ordinates of a straight line fitted by least squares, $\overline{Y}$ the mean of all observations, $\overline{Y}_i$ the mean of the i^{th} subgroup, N the total number of observations, and N_i the number of observations in the i^{th} subgroup. Then the square of the coefficient of correlation is $\frac{\Sigma(Y_t - \overline{Y})^2}{\Sigma(Y - \overline{Y})^2}$. The square of the correlation ratio is $\frac{\Sigma N_i(\overline{Y}_i - \overline{Y})^2}{\Sigma(Y - \overline{Y})^2}$. If the number of observations is the same in each subgroup, the latter becomes $\frac{N_i \Sigma(\overline{Y}_i - \overline{Y})^2}{\Sigma(Y - \overline{Y})^2}$.

TABLE 146

Correlation between Specific-cycle Measures and Their Order in Time
Seven American Series

Series	Period covered	Number of		Coefficient of correlation squared			Correlation ratio squared		
		Specific cycles	Subgroups[a]	Duration of full cycles and time	Amplitude of full cycles and time	Per month amplitude of full cycles and time	Duration of full cycles and time	Amplitude of full cycles and time	Per month amplitude of full cycles and time
Deflated clearings...	1878–1933	15	3	.14	.00	.13	.13	.01	.23
Pig iron production.	1879–1933	15	3	.00	.16	.06	.00	.09	.05
Freight car orders...	1870–1933	19	3	.10	.00	.04	.18	.13	.18
Railroad stock prices	1857–1932	18	3	.00	.02	.00	.10	.05	.01
Shares traded......	1878–1933	15	3	.01	.05	.07	.01	.20	.22
Call money rates....	1858–1931	23	3	.00	.01	.03	.04	.11	.29
Railroad bond yields	1860–1931	20	3	.13	.02	.13	.22	.07	.24

The specific-cycle measures (durations in months, amplitudes in specific-cycle relatives) are correlated with serial numbers of the successive specific cycles.

[a] See Table 145 for the number of cycles and periods covered by successive subgroups.

instances than the coefficients of correlation squared, but they are just as unimpressive.[6]

These results are what we should expect from a careful study of Chart 55 and Table 145, apart from formal measures of correlation. Not only do secular changes appear small in comparison with the variability of individual cycles, but there is slight uniformity among the series with regard to the trend of any attribute of cyclical behavior. If we look merely at the trend lines of the full-cycle amplitudes, it seems that cyclical fluctuations have become narrower in the series on interest rates and wider in the series on industrial and speculative activity. But this suggestion is not borne out uniformly by the subgroup averages of the amplitudes or even by the trend lines of the per month amplitudes. The indications of different series concerning secular trends in cyclical duration are likewise contradictory and inconclusive. The dominating impression conveyed by Chart 55 is that the durations and amplitudes of successive cycles have varied in a highly irregular fashion, and that substantial secular changes have not taken place.

On the basis of the evidence presented so far, the most that may be said is that secular changes account for a small part of the variation in the durations and amplitudes of the specific cycles in our sample series. But have we any warrant for believing that secular changes account for *any* part of the variation? This question cannot be answered by measures of correlation alone. Even if measures of correlation were computed from

[6] The cycle measures are grouped by periods in computing correlation ratios, but the correlation coefficients are computed from ungrouped data. In these circumstances the correlation ratio may be smaller than the correlation coefficient. See Jan K. Wiśniewski, Pitfalls in the Computation of the Correlation Ratio, *Journal of the American Statistical Association*, Dec. 1934, pp. 416-7.

purely random series, it is almost certain that they would not be exactly zero. Further, no matter what the nature of the series may be, the size of correlation measures is not independent of the complexity of the regression lines that are fitted, or the number of subgroups into which the data are broken, or the number of observations that are used. To determine whether there is any warrant for believing that the trend lines or subgroup averages truly 'account' for any part of the variability of the cyclical measures in our samples, we should establish whether the correlation measures are larger than might reasonably be expected to arise from chance alone. Such a test is provided by 'variance analysis', a statistical tool devised by R. A. Fisher.

The principles on which the technique of 'variance analysis' rests can be explained most simply in terms of our subgroup analysis. Let us start, for example, with the hypothesis that the durations of successive specific cycles in pig iron production are free from secular change. To determine whether our data are consistent with this hypothesis, we first group the durations of the cycles by periods. Then, as in computing correlation ratios, we divide the total variation in the data—that is, the sum of the squares of the deviations of the individual cyclical measures from their mean—into two parts: the variation among and within the periods. On the hypothesis of no secular change both parts arise from the same source, namely, random variation. Each part can therefore be used to estimate the variability of the universe of cycles from which the original observations are considered to be a sample. The estimates are obtained by dividing each component of the total sum of squares by the number of 'degrees of freedom' on which it is based, that is, by the number of independent comparisons among the relevant observations. For example, if fifteen cycles are grouped into three periods, the estimate of variance derived from the differences among periods is based on two degrees of freedom, while the estimate of variance derived from the differences within periods is based on twelve degrees of freedom. The ratio of the first estimate to the second yields a measure designated *F*, which furnishes an objective test of the hypothesis that secular changes have not taken place.[7]

We should expect the two estimates of variance, or the two variances for short, to be approximately equal if the hypothesis that no secular

[7] If we use the symbols in note 5, the variance estimated from the variation among subgroups (periods) is $\frac{\Sigma N_i(\bar{Y}_i - \bar{Y})^2}{K-1}$, where K is the number of subgroups; that is, $i = 1, 2, \ldots, K$. The variance estimated from the variation within subgroups is $\frac{\Sigma S(Y - \bar{Y}_i)^2}{N-K}$, where S represents the summation within a subgroup. Hence $F = \frac{(N-K)\,\Sigma N_i(\bar{Y}_i - \bar{Y})^2}{(K-1)\,\Sigma S(Y - \bar{Y}_i)^2}$.

In the straight-line trend analysis, the variance estimated from the variation of the ordinates of trend is $\Sigma(Y_t - \bar{Y})^2$; the variance estimated from the variation about the line of trend is $\frac{\Sigma(Y - Y_t)^2}{N-2}$. Hence $F = \frac{(N-2)\,\Sigma(Y_t - \bar{Y})^2}{\Sigma(Y - Y_t)^2}$.

change has occurred in cyclical durations is valid. On the other hand, if a secular change has taken place, the variance among periods should be larger than the variance within periods, and *F* should exceed unity. But *F* may exceed unity, even if calculated from random series. Therefore, to judge any *F*, we must determine the probability of obtaining a greater value of *F* from a random sample grouped at random. If the probability is small, the presumption is that the differences among periods are *significantly* greater than the differences within periods. In other words, we may conclude provisionally that a secular change has occurred in cyclical behavior. How 'small' the probability need be is a matter of choice and of the objectives in view; but it has become customary to consider a probability of .05 (one chance in twenty) or smaller as indicating 'significance', and that is the standard to which we shall adhere.[8]

The probability tables used to test variance ratios (*F*'s) are derived from a theoretical population distributed according to the 'normal curve'. Since definite evidence exists that the frequency distributions of our cyclical measures are frequently skewed,[9] the tests we make are in some degree inexact. Further, the probability tables are based on the assumption that the observations entering the sample are independent. It seems reasonably certain that cyclical measures do not fulfill this condition, although they come closer to doing so than the original data of time series. This too means that our tests are inexact, in the sense that the probability tables are not perfectly applicable to our data. But these technical difficulties do not seriously affect the analysis of this chapter. Our main problem is whether secular changes in cyclical behavior have been substantial, and that question we can usually answer sufficiently well from ordinary data charts and tables. It is helpful, however, to check judgments reached in this fashion by determining which of our cyclical measures are and which are not reasonably consistent with the hypothesis of no secular change. Even rough tests of statistical significance used with reserve and discrimination will serve this limited purpose.

Thus Table 147 shows the variance ratios (*F*'s) derived for the durations and amplitudes of full specific cycles in our seven series. The hypothesis of no secular change is tested in two ways: by dividing the data into subgroups, and by computing linear trends. The trend lines and subgroups that we use are indicated in Chart 55 and Table 145. On the whole the variance tests seem to corroborate the hypothesis that the

[8] R. A. Fisher and F. Yates have published a table showing what values of *F* correspond to the .20, .05, .01, and .001 levels of significance (that is, the values of *F* that will be exceeded in the stated proportion of cases by chance) for specified degrees of freedom among and within groups. See their *Statistical Tables for Biological, Agricultural and Medical Research* (Oliver and Boyd, London, 1938), pp. 29-35. A more detailed table of the values of *F*, but limited to the .05 and .01 levels, is given by George W. Snedecor in his *Statistical Methods* (Collegiate Press, Ames, Iowa, 1938 rev. ed.), pp. 184-7. Both tables show directly values of *F* greater than 1; values between 0 and 1 can be obtained by using reciprocals.

[9] See Ch. 12, Sec. III.

TABLE 147

Tests of Secular Change in Durations and Amplitudes of Specific Cycles
Seven American Series

Series	Number of		Ratio of variance among subgroups to variance within subgroups			Ratio of variance of ordinates of straight-line trend to variance about the trend		
	Specific cycles	Subgroups	Duration of full cycles	Amplitude of full cycles	Per month amplitude of full cycles	Duration of full cycles	Amplitude of full cycles	Per month amplitude of full cycles
Deflated clearings	15	3	0.87	0.07	1.78	2.12	0.05	2.00
Pig iron production	15	3	0.01 †	0.61	0.35	0.01	2.41	0.84
Freight car orders	19	3	1.74	1.18	1.75	1.95	0.05	0.74
Railroad stock prices	18	3	0.87	0.39	0.08	0.01	0.38	0.06
Shares traded	15	3	0.06	1.46	1.64	0.08	0.73	0.91
Call money rates	23	3	0.39	1.23	4.03J	0.01	0.22	0.63
Railroad bond yields	20	3	2.44	0.67	2.70	2.66	0.33	2.60

See note 7 concerning the variance ratios, Table 145 for the number of cycles and periods covered by the successive subgroups, and Chart 55 for the straight-line trends.

J Indicates that the ratio is 'significantly large', i.e., larger than the value that would be exceeded once in twenty times by chance. For the analysis by subgroups this value is 3.88 for deflated clearings, iron production and shares traded, 3.68 for railroad stock prices, 3.63 for freight car orders, 3.59 for railroad bond yields, and 3.49 for call money rates. For the straight-line trend analysis, the corresponding values are 4.67, 4.49, 4.45, 4.41, and 4.32. The differences among these values result from differences in the number of degrees of freedom on which the estimates of variance are based.

† Indicates that the ratio is 'significantly small', i.e., smaller than the value that would be fallen short of once in twenty times by chance. For the subgroup analysis this value is .05 for all series. For the straight-line trend analysis, it is .004 for all series.

durations and amplitudes of the specific cycles in our test series have not been subject to secular changes. Surely, they do not contradict the conclusion already reached: namely, that if secular changes have taken place in these cyclical characteristics, they have in general been slight. In the portion of the table relating to the subgroup analysis, one out of twenty-one variance ratios is greater than the value that would be exceeded by chance once in twenty times, and one is less than the value that would be fallen short of by chance once in twenty times.[10] No value of F in the straight-line trend analysis falls outside either limit. Our data seem to behave the way we should expect data derived from the same universe and grouped at random to behave. Yet we believe that the amplitudes of call money rates have undergone a real change. By facilitating increases of bank reserves in times of pressure and by reducing the dependence of interior banks upon New York, the Federal Reserve system has undoubtedly tended to mitigate the spasmodic fluctuations of call money rates.

II Reference-cycle Patterns

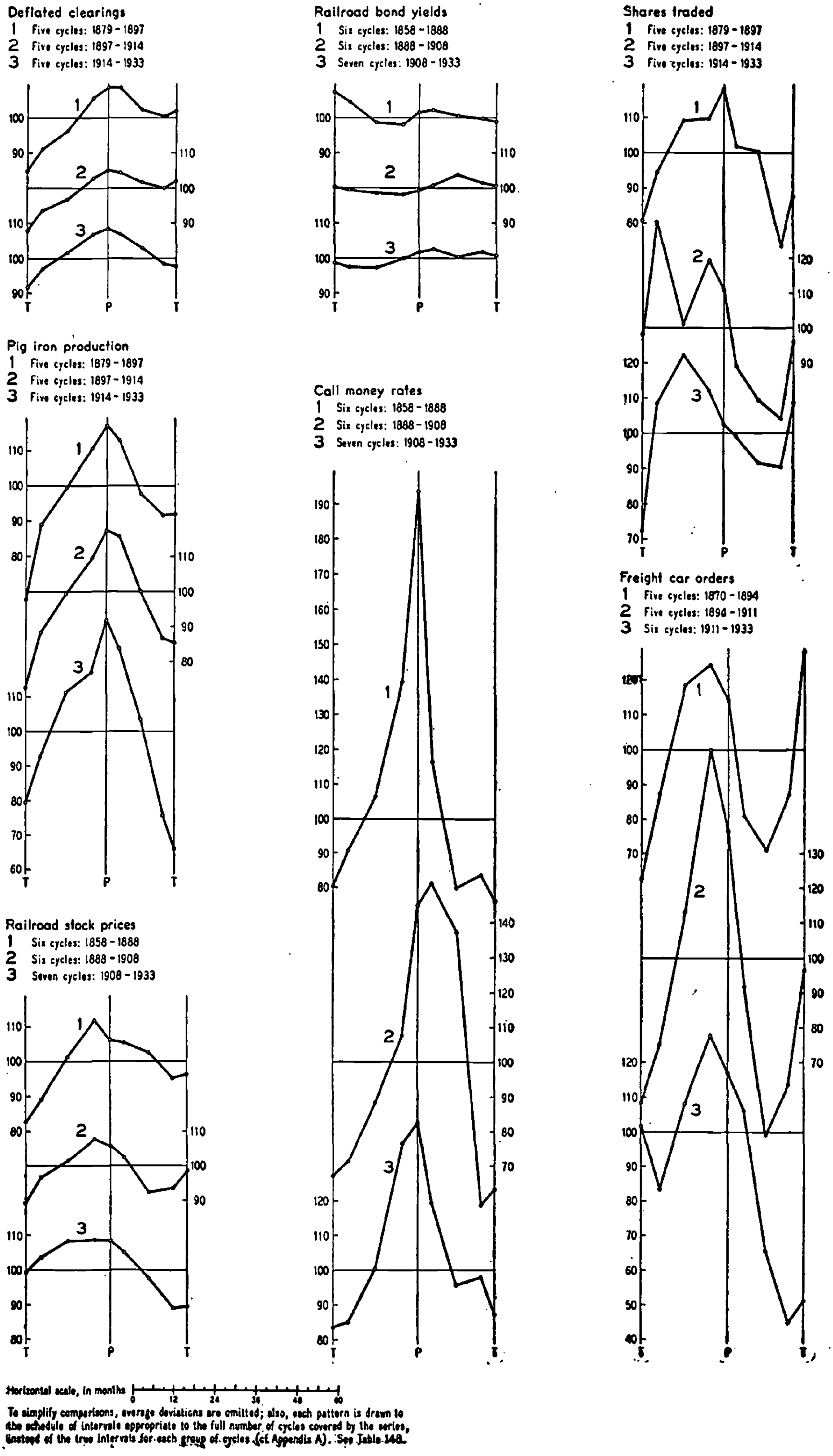
We pass from these tests of specific cycles to tests of reference cycles. The question now is whether the behavior of our sample series has changed materially during the business cycles marked off by our reference dates.

[10] These values are identified in Table 147. Of course, only the values of F that are greater than the value that would be exceeded once in twenty trials by chance can create a presumption of a secular change in cyclical behavior.

CHART 56

Average Patterns of Three Successive Groups of Reference Cycles

Seven American Series



To simplify comparisons, average deviations are omitted; also, each pattern is drawn to the schedule of intervals appropriate to the full number of cycles covered by the series, instead of the true intervals for each group of cycles (cf. Appendix A). See Table 148.

TABLE 148

Average Patterns of Three Successive Groups of Reference Cycles
Seven American Series

Series and period covered	No. of reference cycles	I Initial trough (3 mos.)	II Expansion: First third	III Expansion: Middle third	IV Expansion: Last third	V Peak (3 mos.)	VI Contraction: First third	VII Contraction: Middle third	VIII Contraction: Last third	IX Terminal trough (3 mos.)
Deflated clearings										
1879–1897.........	5	85.0	91.4	96.6	105.8	108.6	108.6	102.4	100.3	102.0
1897–1914.........	5	87.8	93.7	96.9	102.8	105.3	104.6	101.7	100.0	102.2
1914–1933.........	5	91.6	97.0	101.8	106.9	108.5	106.9	102.9	98.3	97.7
Pig iron production										
1879–1897.........	5	67.9	89.1	99.8	110.8	117.3	113.1	97.8	91.9	92.0
1897–1914.........	5	72.5	88.3	99.4	109.7	117.5	115.8	100.0	86.7	85.4
1914–1933.........	5	79.5	92.7	111.2	117.0	131.9	124.0	103.4	75.9	66.0
Freight car orders										
1870–1894.........	5	62.7	87.4	118.6	124.3	114.1	81.0	70.9	87.2	128.0
1894–1911.........	5	58.3	75.2	113.0	160.0	136.6	91.8	49.1	63.8	96.3
1911–1933.........	6	101.7	83.5	108.1	127.9	117.2	106.1	65.3	45.0	51.0
Railroad stock prices										
1858–1888.........	6	82.8	89.0	101.5	112.0	106.2	105.6	102.8	95.3	96.5
1888–1908.........	6	89.5	96.8	101.5	107.7	105.8	102.3	92.5	93.7	98.7
1908–1933	7	99.3	103.7	108.4	108.5	108.4	105.0	97.8	89.1	89.6
Shares traded										
1879–1897.........	5	80.8	94.7	109.3	109.9	118.2	101.9	100.4	73.7	87.8
1897–1914.........	5	98.2	130.2	101.0	119.7	110.9	89.1	79.5	74.4	96.0
1914–1933.........	5	72.4	108.7	122.4	112.3	102.7	99.3	91.7	90.5	108.8
Call money rates										
1858–1888.........	6	80.4	90.6	106.4	139.3	193.7	116.5	80.0	83.8	76.3
1888–1908.........	6	67.3	71.1	88.4	107.8	145.1	151.4	137.4	58.8	63.2
1908–1933.........	7	83.8	85.1	100.2	136.3	142.5	119.3	95.7	98.0	87.3
Railroad bond yields										
1858–1888.........	6	107.3	104.6	98.8	98.1	101.8	102.4	100.5	99.9	99.0
1888–1908.........	6	100.4	99.6	98.8	98.3	99.3	100.8	103.9	101.4	100.6
1908–1933.........	7	98.9	97.6	97.4	100.0	101.8	102.7	100.2	101.8	100.8

Where the number of cycles is not exactly divisible by 3, an additional cycle is placed in the last group.

TABLE 149

Square of Correlation Ratio between Reference-cycle Standings and Time
Seven American Series

Series	Period covered	Number of: Reference cycles	Number of: Subgroups	I	II	III	IV	V	VI	VII	VIII	IX	Pattern as a whole[b]
				Square of correlation ratio between time and standings[a] in reference-cycle stage									
Deflated clearings...	1879–1933	15	3	.10	.10	.25	.16	.10	.11	.01	.01	.04	.07
Pig iron production.	1879–1933	15	3	.05	.01	.13	.06	.19	.23	.02	.09	.23	.11
Freight car orders...	1870–1933	16	3	.08	.02	.01	.16	.02	.04	.09	.15	.15	.09
Railroad stock prices	1858–1933	19	3	.16	.17	.11	.03	.01	.02	.18	.02	.04	.08
Shares traded......	1879–1933	15	3	.13	.20	.37	.08	.06	.13	.28	.14	.04	.12
Call money rates....	1858–1933	19	3	.07	.09	.09	.10	.06	.22	.30	.33	.10	.12
Railroad bond yields	1858–1933	19	3	.33	.36	.02	.05	.08	.06	.15	.03	.02	.14

See Table 148 for the number of cycles and periods covered by the successive subgroups.

[a]That is, between standings in reference-cycle relatives and serial numbers of successive reference cycles.

[b]See note 11.

TABLE 150

Tests of Secular Change in Reference-cycle Patterns
Seven American Series

Series	Number of: Reference cycles	Number of: Subgroups	Standings in reference-cycle stage: I	II	III	IV	V	VI	VII	VIII	IX	Pattern as a whole
			Ratio of variance among subgroups to variance within subgroups for									
Deflated clearings...	15	3	0.65	0.69	1.97	1.13	0.70	0.73	0.03†	0.06	0.23	0.46†
Pig iron production..	15	3	0.33	0.08	0.93	0.37	1.43	1.78	0.12	0.62	1.74	0.75
Freight car orders....	16	3	0.55	0.12	0.09	1.24	0.15	0.24	0.65	1.16	1.11	0.62
Railroad stock prices.	19	3	1.57	1.70	0.96	0.24	0.08	0.13	1.75	0.17	0.32	0.67
Shares traded.......	15	3	0.94	1.47	3.53	0.49	0.38	0.86	2.39	0.99	0.23	0.80
Call money rates....	19	3	0.56	0.75	0.84	0.94	0.52	2.23	3.50	3.88J	0.90	1.07
Railroad bond yields.	19	3	4.00J	4.52J	0.16	0.42	0.66	0.51	1.36	0.25	0.14	1.30

See notes 7 and 11 concerning the variance ratios, and Table 148 for the number of cycles and periods covered by the successive subgroups.

J Indicates that the ratio is 'significantly large', i.e., larger than the value that would be exceeded once in twenty times by chance. For the pattern as a whole this value is 1.68 for railroad bond yields, railroad stock prices, and call money rates; 1.70 for deflated clearings, iron production, and shares traded; 1.69 for freight car orders. For the single stages it is 3.63 for railroad bond yields, railroad stock prices, and call money rates; 3.88 for deflated clearings, iron production, and shares traded; 3.80 for freight car orders.

† Indicates that the ratio is 'significantly small', i.e., smaller than the value that would be fallen short of once in twenty times by chance. In all series this value is .51 for the pattern as a whole, and .05 for the single stages.

To answer this question we proceed as before, except that we engage in no curve fitting. The first step is to divide into thirds the full number of cycles covered by each series and compute means of the reference-cycle patterns in each subgroup. Next we compute for each series squares of correlation ratios between (a) time and (b) the standings in individual cycle stages and all stages combined. Finally, we show the statistical significance of the correlation ratios by using variance analysis.[11] The results of these operations are presented in Chart 56 and Tables 148-150.

The new experiments yield results similar to those for specific cycles. The squares of the correlation ratios for whole reference-cycle patterns range from .07 to .14 in different series. Of the sixty-three correlation measures for single stages, only six are above .25; the largest is .37. None of the correlation measures for whole patterns and only three for single stages are 'significantly large'. It might seem that in these instances the hypothesis of absence of secular change is contradicted. That inference is inadmissible without additional evidence; for if sixty-three variance ratios were computed from random samples, approximately three such instances would be expected. Nor is it surprising that two of the *F*'s are

11 In computing the correlation ratio (squared) for the reference-cycle pattern as a whole, the numerator is the sum of the numerators for the nine separate stages and the denominator is the sum of the corresponding denominators. See above, note 5. Consequently, the correlation ratio (squared) for the whole pattern of a series is a weighted arithmetic mean of the correlation ratios (squared) for the nine stages, the weight being the sum of squared deviations about the mean of the stage. This measure takes no account of the actual sequence of the stages.

In making the variance analysis for the whole pattern, the number of degrees of freedom is $9(K - 1)$ among subgroups and $9(N - K)$ within subgroups, where N is the number of cycles and K the number of subgroups.

less than the values that would be fallen short of once in twenty times by chance, since approximately one such case is to be expected in twenty random samples.

The preceding statements cannot be accepted literally, as statistical experts will be quick to recognize, for our data fail to satisfy fully the

TABLE 151

Average Duration and Amplitude of Expansions and Contractions of Three Successive Groups of Specific Cycles
Seven American Series

Series and period covered	Number of specific cycles	Average duration in months		Average amplitude in specific-cycle relatives			
		Expansion	Contraction	Rise	Fall	Rise per month*	Fall per month*
Deflated clearings							
1878–1893	5	28	9	30	13	1.0	1.7
1893–1910	5	33	8	27	10	0.8	3.0
1910–1933	5	37	17	24	18	0.7	1.1
Pig iron production							
1879–1896	5	29	14	62	44	2.6	3.8
1896–1914	5	32	11	64	48	2.1	5.6
1914–1933	5	25	18	59	72	2.6	4.6
Freight car orders							
1870–1894	6	22	26	215	206	13.9	8.3
1894–1914	6	22	18	163	156	9.4	10.4
1914–1933	7	12	19	254	264	41.3	15.9
Railroad stock prices							
1857–1889	6	35	28	46	31	1.8	1.5
1889–1907	6	23	14	29	23	1.4	1.8
1907–1932	6	28	21	32	42	1.1	1.8
Shares traded							
1878–1897	5	15	31	74	73	7.7	2.5
1897–1914	5	15	28	108	112	9.8	4.2
1914–1933	5	24	20	112	93	5.3	5.6
Call money rates							
1858–1880	7	21	17	110	108	5.2	7.3
1880–1904	8	18	17	142	148	10.5	9.8
1904–1931	8	20	20	96	91	4.7	5.7
Railroad bond yields							
1860–1876	6	15	17	12	14	0.8	1.1
1876–1905	7	22	27	7	13	0.4	0.5
1905–1931	7	25	20	13	11	0.6	1.0
	Number of subgroups	Ratio of variance among subgroups to variance within subgroups					
Deflated clearings	3	0.43	1.38	0.21	0.39	0.90	0.49
Pig iron production	3	0.46	0.62	0.07	1.04	0.35	0.49
Freight car orders	3	1.65	1.16	1.03	1.26	1.11	1.61
Railroad stock prices	3	0.47	1.17	0.69	0.59	1.14	0.18
Shares traded	3	1.05	1.11	1.45	1.31	0.86	4.87J
Call money rates	3	0.13	0.32	0.99	1.32	4.35J	1.22
Railroad bond yields	3	1.03	0.97	1.10	0.30	1.88	0.93

See note to Table 145.

* Unweighted average.

J Larger than the value that would be exceeded once in twenty times by chance. This value is specified in a note to Table 147.

assumptions underlying variance analysis. In particular, we cannot justifiably regard our several series or the standings within a given series in successive cycle stages as being independent of one another; we know that their movements are intercorrelated. But technical difficulties do not cloud the essential point, which is sufficiently plain without probability calculations. If secular changes have taken place in reference-cycle behavior, they have not left a prominent imprint on our sample series. The outstanding feature of Chart 56 is that the average cyclical patterns are roughly similar from period to period within the same series but differ widely among series. This demonstration suffices for our present purpose.

III Other Cyclical Measures

The results of the preceding tests are confirmed when the range of the tests is extended. Table 151 supplements Tables 145 and 147, by analyzing separately the expansion and contraction phases of the specific cycles. We find that the variance among subgroups is larger than the variance within subgroups in half of the forty-two instances covered by the new table. But in only two of the twenty-one instances does the variance ratio exceed unity to a degree that is significant according to probability calculations: the rate of cyclical rise in call money rates and the rate of cyclical fall in share trading. The latter result is perhaps unexpected. The former is confirmed by independent knowledge, as already stated. No other result in the table is even in the vicinity of the 'level of significance'. As a whole the evidence in Table 151 gives no support to the view that real secular changes of substantial scope have been a common feature of the cyclical behavior of economic activities. This remark applies also to the measures of timing and conformity in Table 152, although the timing of some series seems to have undergone a clear-cut secular change.

In Table 153 we summarize the variance tests thus far presented. We show merely the number of 'significantly large' variance ratios relatively to the number computed, without allowing for the duplication that automatically results when closely related measures are analyzed or when certain of these measures are treated on more than one plan. Of the 189 variance ratios, only nine are 'significantly large'. It is interesting that none of the 'significant' ratios is among the measures of cyclical duration and that seven occur in the two series on interest rates. These results may mean that the duration of specific cycles is one of their most stable features in the long run, and that money markets are more susceptible to secular changes in cyclical behavior than are industrial or security markets. Although the evidence at hand is slender and indefinite, it is noteworthy that the series on deflated clearings outside New York, which comes closer to representing economic activity at large than any other in our sample,

TABLE 152

Average Timing of Specific Cycles and
Rates of Change during Three Successive Groups of Reference Cycles
Seven American Series

Series and period covered	Number of reference cycles	Average lead (−) or lag (+) in months at reference[a]		Average change per month in reference-cycle relatives during stages[b] matched with reference		
		Peaks	Troughs	Expansions	Contractions	Cycles[c]
Deflated clearings						
1879–1897	5	+3.8	−6.2	+0.9	−0.7	−1.5
1897–1914	5	+4.2	−7.4	+0.8	−0.4	−1.2
1914–1933	5	+1.2	−3.8	+0.7	−0.4	−1.1
Pig iron production						
1879–1897	5	+0.8	−3.2	+2.1	−1.7	−3.9
1897–1914	5	+3.4	−7.2	+2.2	−1.8	−4.0
1914–1933	5	+1.4	−0.3	+2.4	−3.3	−5.7
Freight car orders						
1870–1894	5	−7.6	−3.2	+2.4	−2.5	−4.9
1894–1911	5	−7.0	−5.0	+5.2	−5.2	−10.5
1911–1933	6	−3.2	−1.4	+3.8	−3.9	−7.8
Railroad stock prices						
1858–1888	6	−7.6	−13.4	+0.9	−0.6	−1.5
1888–1908	6	−2.2	−1.2	+0.9	−0.8	−1.7
1908–1933	7	−7.3	−8.6	+0.5	−0.5	−1.0
Shares traded						
1879–1897	5	−11.4	−2.2	+1.4	−2.2	−3.5
1897–1914	5	−12.0	−4.8	+1.8	−2.2	−4.1
1914–1933	5	−7.8	−6.5	+2.7	−0.6	−3.3
Call money rates						
1858–1888	6	−3.3	−1.2	+4.2	−3.5	−7.7
1888–1908	6	+4.2	+3.0	+3.4	−5.0	−8.4
1908–1933	7	−1.0	+2.6	+3.3	−2.4	−5.7
Railroad bond yields						
1858–1888	6	+9.2	+17.8	+0.2	−0.3	−0.5
1888–1908	6	+9.2	+14.4	+0.1	−0.1	−0.2
1908–1933	7	+4.8	+1.8	+0.4	−0.1	−0.5
	Number of subgroups	Ratio of variance among subgroups to variance within subgroups				
Deflated clearings	3	0.50	0.78	0.57	0.26	0.51
Pig iron production	3	0.26	7.83ʃ	0.09	1.99	1.55
Freight car orders	3	0.37	0.31	1.29	0.92	2.05
Railroad stock prices	3	0.90	2.11	0.58	0.18	0.56
Shares traded	3	0.54	0.57	1.23	1.61	0.14
Call money rates	3	4.23ȷ	0.59	0.16	1.86	0.49
Railroad bond yields	3	1.61	7.61ʃ	1.02	1.44	0.89

See note to Table 148.

[a] The number of timing observations is not defined exactly by the number of reference cycles; see Tables 62 and 141. To avoid duplication, the timing at the last reference trough of the first period is excluded from the average for that period but included in the average for the second, and the timing at the last reference trough of the second period is excluded from the average for that period but included in the average for the third.

[b] These stages are indicated in Table 140.

[c] Difference between contraction and expansion (see Table 47, col. 8).

ȷ Larger than the value that would be exceeded once in twenty times by chance.

ʃ Larger than the value that would be exceeded once in a hundred times by chance.

TABLE 153

Summary of Variance Tests in Preceding Tables

Measure	Number of variance ratios computed		Number of 'significantly large' variance ratios							
	Per series	For all series	Deflated clearings	Pig iron production	Freight car orders	Railroad stock prices	Shares traded	Call money rates	Railroad bond yields	All series
Duration[a]	4	28	...	...	...	...	...	...	...	...
Amplitude[a]	8	56	...	...	...	...	1	2	...	3
Timing[b]	2	14	...	1	...	...	...	1	1	3
Pattern[c]	10	70	...	...	...	...	...	1	2	3
Conformity[b]	3	21	...	...	...	...	...	...	...	...
Total	27	189	...	1	...	...	1	4	3	9

This summary omits 'significantly small' ratios, of which there are two in clearings and one in iron production.

[a] Derived from Tables 147 and 151.

[b] Derived from Table 152.

[c] Derived from Table 150.

has very few ratios exceeding unity, in no instance turns up a 'significantly large' ratio, but does turn up two that are 'significantly small'.

It is no part of our aim to discuss the results for individual series in any detail at present. Nor, to repeat, are 'significant' but 'slight' secular changes of more than incidental interest. It is well to note, nevertheless, that apart from their technical limitations, our probability calculations cannot be taken at face value. Economic analysis and historical knowledge must play a part in whatever judgment is finally reached concerning the presence or absence of secular change in cyclical behavior. For example, our observations suggest that the tendency of pig iron production to lead at general revivals has practically disappeared; the variance test indicates that so large a difference as we find between the timing of recent and earlier cycles could arise from chance causes less frequently than one time out of twenty; which creates some presumption that a secular change in timing has actually occurred. This presumption is materially strengthened by knowledge that the secular trend of iron output has flattened out, for we have independent reasons for believing that a change in cyclical timing tends to be associated with retardation of growth. But retardation in the railroad equipment industry has been even more pronounced than in the iron industry; the mean timing of freight car orders at business-cycle troughs has shifted in the same direction as the timing of pig iron production; the original data on orders leave much to be desired and an erratic factor is bound to enter the placing of orders; hence we are inclined to judge that a secular change may well have occurred also in the timing of freight car orders, in spite of the low variance ratio.[12] Of course,

[12] Freight car orders led the reference trough in 1911 by 17 months. This long lead is placed in the third period in Table 152, by force of the convention adopted in constructing the table. If it were placed instead in the second period, the average timing in the three successive periods would run − 3.2, − 7.0 and + 1.2, and the variance ratio would be 2.02 (the .05 value is 3.74). See below, Sec. VIII.

what we do in such instances is to combine intuitively the knowledge given by probability tests with other knowledge concerning the phenomenon under observation.

IV Duration and Amplitude of Business Cycles

A vital check on some of the preceding results may be obtained by analyzing the business-cycle durations yielded by our reference dates. If there is little or no presumption that the durations of specific cycles have undergone noteworthy secular changes, the same should be true of the durations of business cycles. And since our business-cycle measures cover three countries besides the United States, we are also in a position to determine whether the experience of other countries has been similar to that of the United States.

We therefore group the durations of successive business cycles and their phases of expansion and contraction into three approximately equal classes in each country, and compare the variance among subgroups in each with the variance within subgroups. The subgroup means and the results of the variance analysis are set out in Table 154. There is a rough

TABLE 154

Average Duration of Three Successive Groups of Business Cycles
Four Countries

Country and period covered	Number of business cycles	Average duration in months		
		Expansion	Contraction	Full cycle
United States				
1854–1885	6	31	30	61
1885–1908	7	24	16	40
1908–1933	7	21	21	42
Great Britain				
1854–1886	5	40	36	76
1886–1914	5	43	25	68
1914–1932	5	24	19	43
France				
1865–1887	5	22	30	52
1887–1914	5	39	26	65
1914–1932	5	30	13	43
Germany				
1879–1902	3	46	47	92
1902–1914	3	33	17	50
1914–1932	4	34	20	54
	Number of subgroups	Ratio of variance among subgroups to variance within subgroups		
United States	3	1.89	1.75	3.11
Great Britain	3	2.09	0.93	1.88
France	3	1.77	1.47	1.07
Germany	3	0.78	4.31	6.18[j]

Derived from the monthly reference dates in Table 16. See note to Table 145.

[j] Larger than the value that would be exceeded once in twenty times by chance.

suggestion that business cycles may have become shorter with the passage of time. But this suggestion is only partly borne out by the annual measures in Table 139, which cover a longer period. Our present data yield a 'significant' variance ratio only in one instance, the duration of full cycles in Germany. True, the variance ratio for durations of full cycles exceeds unity in all four countries. But Table 155 suggests that at least the American result is fortuitous, and we know that the timing of business cycles in each of our countries is correlated with that of business cycles in the others.

TABLE 155

Tests of Secular Change in Durations of Business Cycles

United States, 1854–1933

Test	Average duration of successive groups of cycles (mos.) (1)	(2)	(3)	(4)	(5)	Variance ratio (F)[b]
20 cycles divided into						
Two equal groups	51	43	..	..	..	0.99
Three equal groups[a]	61	40	42	..	..	3.11
Four equal groups	58	44	42	44	..	0.84
Five equal groups	48	61	40	44	42	0.86
Straight line fitted by 'least squares'[c]	54[d]	..	..	..	40[e]	1.19

Derived from the monthly reference troughs in Table 16.

[a] The successive groups include 6, 7 and 7 cycles.

[b] None of these ratios is statistically significant.

[c] The x values are the serial numbers of the successive reference cycles.

[d] First trend value ($x = 1$).

[e] Last trend value ($x = 20$).

Unfortunately, we lack at present reliable measures of the amplitude of successive business cycles. In the absence of anything better, the ratings in Table 156, based on several familiar indexes of American business activity, may perhaps serve as a provisional check on our sample series.[13] These ratings fail to disclose any very clear trend in the intensity of successive business-cycle expansions or contractions, and thus corroborate the broad evidence of our sample.[14] The coefficient of correlation between the graded intensities of expansions and their order in time[15] is —.01, or virtually zero; the coefficient for contractions is +.18. On the basis of these results, we can say at most that secular change accounts for a small part of the variation in the amplitudes of business-cycle declines. But even this statement is problematical, since the coefficient of correlation for contractions is not statistically significant.[16]

13 Note, however, the difficulty that at least in the early years these indexes lean heavily on two of our series, clearings and iron production.

14 See also Sec. VI, and Ch. 11, Sec. VI.

15 That is, between the ranks of the average ranks in Table 156 and the serial numbers of the successive cycles.

16 The standard error of a coefficient of rank correlation based on 15 observations is .27; which means that the coefficient would have to be (approximately) .54 in order to satisfy the .05 level of significance.

TABLE 156

Ranks of Amplitudes of Cyclical Expansions and Contractions
Three Indexes of American Business Activity, 1879–1933

Reference expansion[a]	Amplitude of expansions ranked					Reference contraction[a]	Amplitude of contractions ranked				
	A.T.&T. index	Persons index	Ayres index	Average of three ranks	Rank of average ranks		A.T.&T. index	Persons index	Ayres index	Average of three ranks	Rank of average ranks
1879–82	11	14	10	11.7	12	1882–85	11	12	9	10.7	11
1885–87	9	10	9	9.3	9	1887–88	1	4	1	2.0	1
1888–90	4	8	2	4.7	5	1890–91	6	6	5	5.7	6
1891–93	3	5	4	4.0	3	1893–94	12	14	13	13.0	13
1894–95	7	12	11	10.0	10	1895–97	7	11	10	9.3	10
1897–99	13	11	14	12.7	13	1899–00	2	5	4	3.7	5
1900–02	1	3	1	1.7	1	1902–04	4	3	3	3.3	3.5
1904–07	10	4	8	7.3	7.5	1907–08	13	10	12	11.7	12
1908–10	12	9	12	11.0	11	1910–12	3	1	6	3.3	3.5
1912–13	2	1	3	2.0	2	1913–14	10	8.5	8	8.8	8
1914–18	14	13	13	13.3	14	1918–19	8	8.5	7	7.8	7
1919–20	6	6	5	5.7	6	1920–21	14	13	14	13.7	14
1921–23	15	15	15	15.0	15	1923–24	9	7	11	9.0	9
1924–26	8	7	7	7.3	7.5	1926–27	5	2	2	3.0	2
1927–29	5	2	6	4.3	4	1929–33	15	15	15	15.0	15

The mildest contraction or expansion is assigned a rank of 1, the next a rank of 2, and so on. The ranks were determined from our standard measures of specific-cycle amplitude, the analysis being positive. Each of the three indexes is trend-adjusted. For this reason, if for no other, the ranks of the successive cycles are very rough approximations.

The A.T.&T. index is available without trend adjustment since 1900. The ranks of the cyclical movements in the unadjusted and trend-adjusted indexes differ considerably at times. The ranks of the two forms of the index (the ranks of the trend-adjusted index are shown in parentheses) for the nine successive expansions since 1900 are 5(1), 8(6), 6(7), 1(2), 7(8), 2(4), 9(9), 4(5), 3(3); and 3(2), 7(7), 1(1), 4.5 (6), 4.5(4), 8(8), 6(5), 2(3), 9(9) for the nine successive contractions since 1902.

For sources, see Appendix C, notes on series (1), (2) and (4) of Table 21. (The notes in the Appendix are phrased for reference cycles; they apply only approximately to specific cycles when overlapping segments of the same series have been used, as in the A.T.&T. and Persons indexes.)

[a] Years of turning points in our monthly reference chronology (Table 16).

V Business Cycles and Economic Stages

It is possible, of course, that so few noteworthy secular or discontinuous changes in cyclical behavior have emerged in our tests because they have been made by mechanical rule. The trend lines and subgroups we have used involve arbitrary arrangements in every instance; they do not follow hints derived from general economic history or the history of business cycles. We have used mechanical schemes not because we prefer them, but because no other method seemed possible in view of the looseness with which hypotheses concerning secular changes in cyclical behavior have usually been formulated. But there are two notable exceptions to this statement, and they warrant careful investigation.

The first is Frederick C. Mills' suggestion that the duration of business cycles is a function of the stage of industrial development.[17] Mills developed his hypothesis to account for the differences in the durations of business cycles shown by Thorp's *Business Annals*. In its author's words:

[17] An Hypothesis Concerning the Duration of Business Cycles, *Journal of the American Statistical Association*, Dec. 1926, pp. 447-57.

When the modern type of economic organization is in the initial stages of development, the average duration of cycles is relatively long. During the stage of rapid growth, when modern types of business enterprise and modern forms of industrial organization are being applied extensively, business cycles are of relatively short average duration. With the decline in the rate of economic change and the attainment of comparative stability, business cycles increase again in length.

Though he had "no objective criterion for distinguishing the stages in a country's industrial development, or for classifying countries according to their present state of development", Mills attempted a "quite experimental and tentative" classification, which applied as follows to the four countries treated in this study:

A. *Early stage of industrialization*
United States: to 1822 (Annals begin in 1796)
Germany: to 1866 (Annals begin in 1848)
B. *Stage of rapid transition*
England: to 1831 (Annals begin in 1793)
France: to 1876 (Annals begin in 1838)
United States: 1822 to date
Germany: 1866 to date
C. *Stage of economic stability*
England: 1831 to date
France: 1876 to date

Making similar tentative judgments about the stage of industrialization prevailing at different periods in all of the seventeen countries covered by Thorp's annals, Mills obtained the following results:

	No. of business cycles	Mean duration in years	Standard deviation in years
A. Early stage of industrialization	51	5.86	2.41
B. Stage of rapid transition	77	4.09	1.88
C. Stage of economic stability	38	6.39	2.42

He then showed by a sampling test that differences in average duration so large as he had found were most unlikely to happen by chance. Hence he concluded that, if his classification of countries by stages of industrial development is valid and if the durations shown by *Business Annals* are correct, "there is proof here of a definite secular change in the factors determining the duration of business cycles."

In Table 157 we present a detailed test of Mills' hypothesis. On applying variance analysis to the data available to Mills, we confirm his results not only in regard to seventeen countries, but also in regard to the four countries we are using at present. The results are again confirmed if we omit Mills' 'early stage of industrialization', which is not represented in the revised annual reference dates shown in Chapter 4 of this volume; also if we omit the early cycles in the second stage that are not covered by our new chronology. These four samples of durations not only yield averages for the several stages that differ in the direction suggested

TABLE 157

Analysis of Durations of Business Cycles Classified According to Mills' Stages of Industrialization

Data and grouping	No. of business cycles covered	Average duration of cycles in months during stage[a]			Ratio of variance among groups to variance within groups (F)	Probability that larger F would be obtained by chance is	
		A	B	C		Less than	More than
(1) Business-cycle durations in 17 countries, determined from annual records in *Business Annals*. Classified into three groups. The data and classification are those used by Mills......	166	70.3	49.1	76.7	17.79	.001	
(2) Business-cycle durations in 4 countries: United States, Great Britain, Germany and France. In other respects the same as (1).............	84	77.1	49.7	80.4	13.79	.001	
(3) The cycles placed by Mills in stage A are dropped. In other respects the same as (2)......................	77	...	49.7	80.4	23.72	.001	
(4) A few early cycles not covered by the annual reference dates in Table 16 are dropped. The data and other features of the analysis are the same as in (3)........................	70	...	49.0	80.4	23.80	.001	
(5) The calendar-year reference dates of Table 16 are substituted for reference dates taken from *Business Annals*. The periods and other features of the analysis are the same as in (4)......	71	...	53.4	65.5	3.53	.20	.05
(6) A few cycles completed since *Business Annals* was published are added; that is, all cycles, from peak to peak, covered by annual reference dates through 1928–30 are included. Otherwise the same as (5). Going outside Mills' period, we lack the benefit of his classification. Mills assigned the last cycle he covered in U. S. and Germany to stage B. We arbitrarily assign later cycles (since 1923 in U. S. and since 1925 in Germany) to this stage..	80	...	52.6	60.0	1.60		.20
(7) The same as (6) except that, in view of the difficulty just stated, the cycles since 1923 in U. S. and since 1925 in Germany are assigned to stage C....	80	...	53.4	58.2	0.69		.20
(8) Differs from (6) only in that the cycles are taken on a trough-to-trough basis. All cycles covered by our annual reference dates through 1932 are included.	84	...	54.1	60.4	1.34		.20
(9) The same as (6), except that cycle durations are measured from monthly instead of annual reference dates. As a consequence, the period covered is shorter than in (6)................	56	...	50.2	59.4	1.69	.20	.05
(10) The same as (8), except that the durations are measured from monthly instead of annual reference dates and that in consequence the period covered is shorter....................	60	...	51.8	59.4	1.46		.20

[a] Stages A, B and C are identified in the text.

by Mills' hypothesis, but the differences are 'highly significant' in every instance. However, if we substitute durations derived from the annual reference dates of the present investigation for those derived from Thorp's *Business Annals,* the gap between the averages is reduced sharply and the value of *F* is no longer statistically significant. If we use the revised annual reference dates and add several business cycles that have run their course since Thorp's work was done, the gap between the averages becomes still narrower and its 'significance' still more doubtful, whether we count durations from peak to peak or from trough to trough. The somewhat smaller sample of durations measured from the monthly reference dates yields similar results.

Since the use of presumably more reliable and certainly more numerous measures reverses the results of variance analysis applied to earlier measurements of business-cycle durations in four countries classified by 'stages of industrialization', we must accept the conclusion that what we take to be the best collection of measures available at present gives slight support to Mills' hypothesis.[18]

VI Business Cycles before and after 1914

Another challenging hypothesis is the idea voiced repeatedly in recent years that World War I marks a 'break' in the history of business cycles. To test this hypothesis of structural change, we divide the leading cyclical measures for our seven American series and the durations of business cycles in our four countries into two groups; the first of which includes all cycles from the time our monthly records start to 1914, and the second the cycles from 1914 to about 1932. The results are presented in Tables 158-160 and Charts 57-58. To facilitate comparisons, the average reference-cycle patterns before the war are restricted to 1879–1914, the period covered by all seven series.

On the whole these measurements indicate a family likeness between the business cycles that come before and after 1914. As we move from pre-War to later cycles, numerous discrepancies appear within each pair of patterns, but they are overshadowed by the basic similarity that the patterns of the several series bear to one another. The order of magnitude of the cyclical durations, leads or lags, even amplitudes is, broadly speaking, not very different in the two periods. Less than half of the variance ratios in Tables 158-159 exceed unity (42 out of 98); in other words, the differences between the two periods exceed the differences within the periods in less than half of the instances tested. Four variance ratios are 'significantly large', but five are 'significantly small'. Doubtless, changes

[18] C. G. W. Schumann finds that the experience of South Africa from 1806 to 1909 does not bear out Mills' hypothesis. See his *Structural Changes and Business Cycles in South Africa, 1806–1936* (P. S. King, London, 1938), pp. 118-21.

TABLE 158

Average Duration and Amplitude of Specific Cycles before and after 1914

Seven American Series

Series and period covered	No. of specific cycles	Average duration in months			Average in specific-cycle relatives					
					Amplitude of			Per month amplitude[a] of		
		Expansion	Contraction	Full cycle	Rise	Fall	Rise & fall	Rise	Fall	Rise & fall
Deflated clearings										
1878–1914	11	30	10	40	27	11	38	0.9	2.2	1.0
1914–1933	4	39	16	55	27	19	46	0.8	1.2	0.8
Pig iron production										
1879–1914	10	30	13	43	63	46	110	2.4	4.7	2.7
1914–1933	5	25	18	44	59	72	131	2.6	4.6	3.2
Freight car orders										
1870–1914	12	22	22	44	189	181	370	11.6	9.3	9.0
1914–1933	7	12	19	32	254	264	518	41.3	15.9	20.4
Railroad stock prices										
1857–1915	14	28	21	49	35	26	61	1.5	1.6	1.4
1915–1932	4	31	21	52	36	52	89	1.2	2.2	1.5
Shares traded										
1878–1914	10	15	29	44	91	92	184	8.8	3.3	4.4
1914–1933	5	24	20	44	112	93	204	5.3	5.6	4.9
Call money rates										
1858–1915	18	20	18	38	127	129	255	7.6	8.5	6.9
1915–1931	5	19	19	37	77	71	148	4.2	4.6	3.9
Railroad bond yields										
1860–1914	15	22	21	43	10	13	23	0.5	0.7	0.6
1914–1931	5	17	23	41	13	13	25	0.7	1.2	0.7
	No. of groups	Ratio of variance between groups to variance within groups								
Deflated clearings	2	1.09	1.30	1.60	0.00‡	1.00	0.45	0.19	0.31	0.47
Pig iron production	2	0.70	1.13	0.01	0.12	2.21	1.23	0.11	0.01	0.70
Freight car orders	2	3.50	0.33	2.74	1.48	2.10	1.84	2.32	3.16	3.67
Railroad stock prices	2	0.05	0.00§	0.02	0.01	2.67	0.93	0.61	1.14	0.02
Shares traded	2	2.25	2.15	0.00†	0.90	0.00‡	0.26	1.42	6.00J	0.41
Call money rates	2	0.12	0.03	0.03	2.36	2.63	2.66	1.89	2.14	3.89
Railroad bond yields	2	0.52	0.11	0.09	0.39	0.00†	0.24	0.69	1.18	0.09

[a] Unweighted average.

J Larger than the value that would be exceeded once in twenty times by chance.

† Smaller than the value that would be fallen short of once in twenty times by chance.

‡ Smaller than the value that would be fallen short of once in a hundred times by chance.

§ Smaller than the value that would be fallen short of once in a thousand times by chance.

in cyclical behavior occurred or first became marked around 1914 in some single series. The most prominent change turned up by our sample is in the timing of bond yields. A real change occurred also in the amplitude of call money rates, but it is obscured by lumping together all pre-War cycles. From 1858 to 1885 we find nine specific cycles in call money rates and only five business cycles; from 1885 to 1915 we find another nine cycles in call money rates but each corresponds neatly to a business cycle; thus the pre-War period is apparently not homogeneous. If the cycles in call money rates during 1885–1915 are compared with the cycles during

1915–31, we find a 'significant' decline in amplitude.[19] But as already stated, this change is attributable to the Federal Reserve system, not to the War and its sequelae.

It is worth noting, however, that the average total swing of the specific cycles is larger after 1914 than before in every series except call money rates. True, not one of the differences is statistically significant. But our analysis stops in 1933, and we know that the cycle of 1933–38 was sufficiently violent to make its influence felt on the comparisons we are making. For example, the average amplitudes of pig iron production are + 76, −81, 158 for the six cycles from 1914 to 1938, in contrast to + 59, − 72, 131 for the five cycles from 1914 to 1933. If the 1933–38 cycle is added to the post-War period the variance ratio of the cyclical amplitudes also rises materially, though it still fails to meet the .05 level of significance.[20] Apparently, a tendency toward intensified cyclical fluctuations is impressed on our American samples—though not with any great clarity. It is impressed also on the ratings of business-cycle contractions presented in Table 156, but again not very clearly. If this table were extended in both directions, the severe contraction of 1937–38 would be added at the end, the severer contraction of 1873–79 at the beginning. It may be that no lasting change in the severity of cyclical contractions has taken place. On the other hand, a lasting change may have occurred, but we do not have as yet a sufficient number of cyclical observations to establish this result with confidence.[21]

If we are to judge from Table 160, the duration of business cycles in each of our four countries has been shorter on the average since 1914 than in pre-War times. However, the difference is small for this country,[22]

19 The measures in full are:

	Average in specific-cycle relatives		Variance ratio
	1885–1915	*1915–1931*	
Rise	151	77	10.62
Fall	149	71	8.82
Rise & fall	299	148	11.80
Rise per month	6.8	4.2	4.99
Fall per month	10.0	4.6	4.36
Rise & fall per month	7.5	3.9	7.62

The .05 and .01 values of the variance ratio are 4.75 and 9.33, respectively.

20 The variance ratio (F) for the rise is .64, the fall 4.19, the joint rise and fall 3.24. The .05 value of F is 4.60.

21 It is well to observe specifically that since the sample of series analyzed in this chapter leaves us uninformed about the cyclical amplitude of total industrial production or employment, no inference concerning the presence or absence of secular changes in the amplitude of these fundamental magnitudes is possible. Furthermore, if we suppose for a moment that the amplitude of cyclical fluctuations in industrial employment has been constant in the long run, that would not imply constancy in the amplitude of fluctuations in total employment. On the contrary, it would imply that the amplitude of fluctuations in total employment has actually increased; for the number of persons engaged in agricultural work, which is a rather steady branch of employment (though not of output or income) during business cycles, has been a declining fraction of the gainfully occupied population.

22 It is scarcely visible in the specific-cycle durations of the series in Table 158.

and it would virtually disappear if the 1933–38 business cycle were included in the analysis. In foreign countries the difference is substantial, though in no instance statistically significant.

TABLE 159

Average Timing of Specific Cycles and
Rates of Change during Reference Cycles before and after 1914
Seven American Series

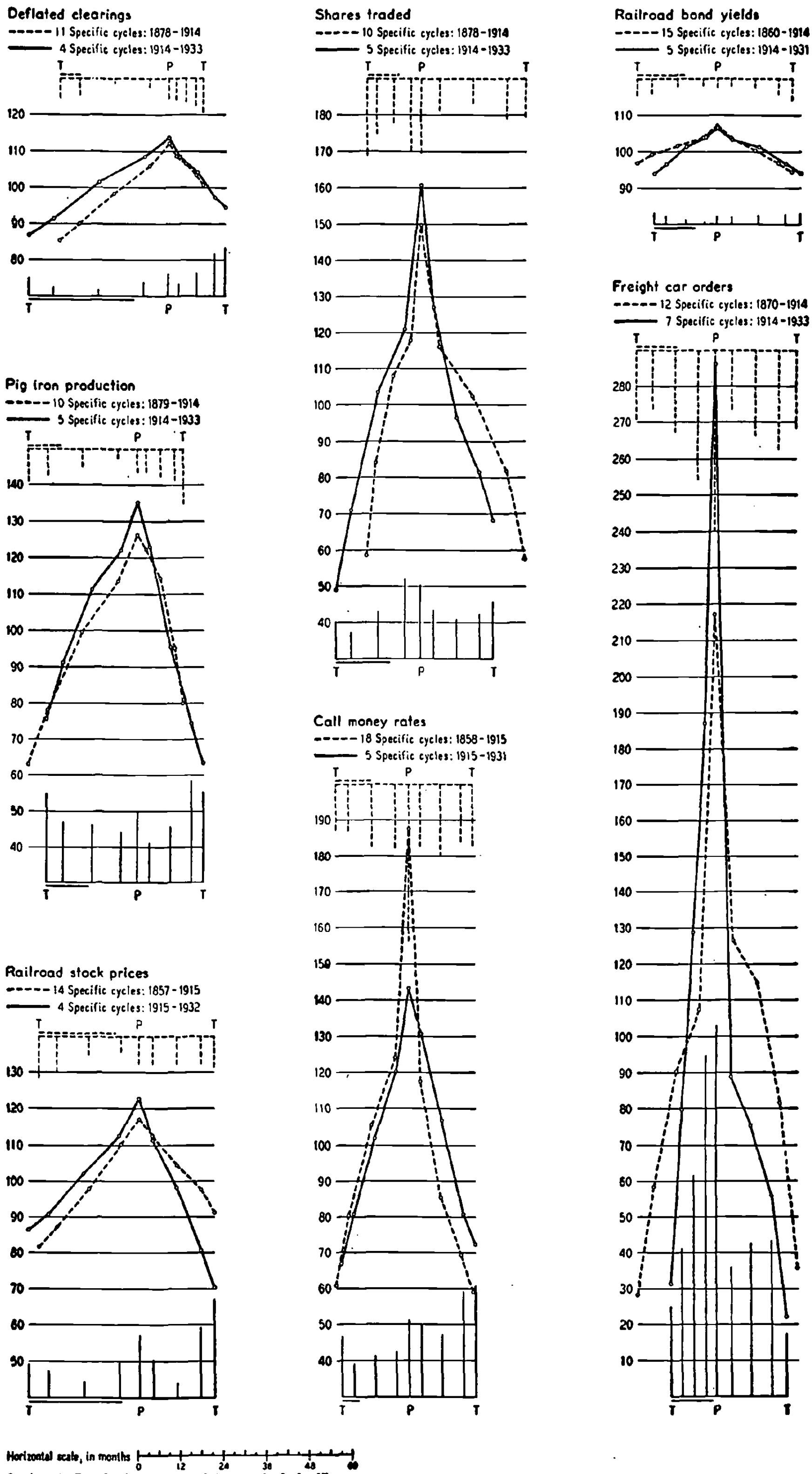
Series and period covered	Number of reference cycles	Average lead (−) or lag (+) in months at reference[a]: Peaks	Troughs	Average change per month in reference-cycle relatives during stages[b] matched with reference: Expansions	Contractions	Cycles[c]
Deflated clearings						
1879–1914	10	+4.0	−6.8	+0.8	−0.5	−1.4
1914–1933	5	+1.2	−3.8	+0.7	−0.4	−1.1
Pig iron production						
1879–1914	10	+2.1	−5.2	+2.2	−1.8	−3.9
1914–1933	5	+1.4	−0.3	+2.4	−3.3	−5.7
Freight car orders						
1870–1914	11	−6.8	−5.3	+4.2	−4.1	−8.3
1914–1933	5	−3.4	+1.2	+3.1	−3.5	−6.5
Railroad stock prices						
1858–1914	14	−4.7	−7.6	+0.9	−0.7	−1.6
1914–1933	5	−8.5	−7.0	+0.4	−0.4	−0.8
Shares traded						
1879–1914	10	−11.7	−3.5	+1.6	−2.2	−3.8
1914–1933	5	−7.8	−6.5	+2.7	−0.6	−3.3
Call money rates						
1858–1914	14	+0.6	+1.0	+3.8	−3.8	−7.7
1914–1933	5	−2.0	+3.0	+3.1	−2.8	−5.9
Railroad bond yields						
1858–1914	14	+9.3	+15.6	+0.2	−0.2	−0.3
1914–1933	5	+3.2	+0.2	+0.4	−0.2	−0.6
	Number of groups	**Ratio of variance between groups to variance within groups**				
Deflated clearings	2	1.08	1.49	1.10	0.15	0.55
Pig iron production	2	0.05	8.46J	0.18	4.30	3.34
Freight car orders	2	0.48	3.29	0.52	0.11	0.45
Railroad stock prices	2	0.73	0.01	2.27	0.29	2.00
Shares traded	2	1.15	0.80	2.27	3.47	0.15
Call money rates	2	0.85	0.27	0.20	0.63	0.47
Railroad bond yields	2	7.02J	14.32♪	1.32	0.01	1.00

[a] The number of timing observations is not defined exactly by the number of reference cycles; see Table 141. To avoid duplication, the timing observation at the reference trough of 1914 is included in the later period only.
[b] These stages are indicated in Table 140.
[c] Difference between contraction and expansion (see Table 47, col. 8).
J Larger than the value that would be exceeded once in twenty times by chance.
♪ Larger than the value that would be exceeded once in a hundred times by chance.

CHART 57

Average Specific-cycle Patterns before and after 1914

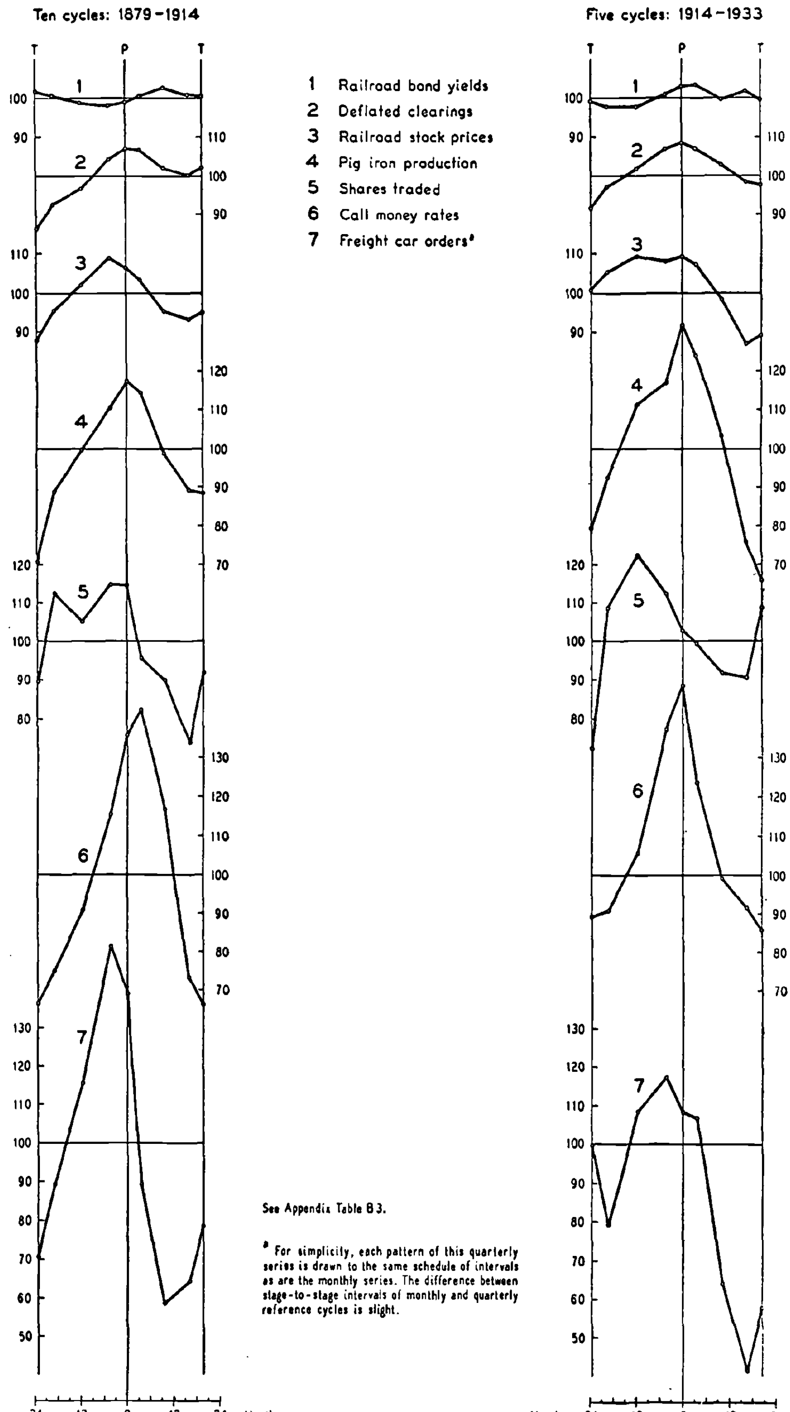
Seven American Series



See Appendix Table B1. For explanation of chart, see Ch. 5, Sec. VI.

CHART 58

Average Reference-cycle Patterns before and after 1914
Seven American Series



See Appendix Table B 3.

* For simplicity, each pattern of this quarterly series is drawn to the same schedule of intervals as are the monthly series. The difference between stage-to-stage intervals of monthly and quarterly reference cycles is slight.

TABLE 160
Average Duration of Business Cycles before and after 1914
Four Countries

Country and period covered	Number of business cycles	Average duration in months		
		Expansion	Contraction	Full cycle
United States				
1854–1914	15	25	23	48
1914–1933	5	24	20	44
Great Britain				
1854–1914	10	41	30	72
1914–1932	5	24	19	43
France				
1865–1914	10	31	28	58
1914–1932	5	30	13	43
Germany				
1879–1914	6	39	32	71
1914–1932	4	34	20	54
	Number of groups	Ratio of variance between groups to variance within groups		
United States	2	0.13	0.10	0.19
Great Britain	2	4.35	1.06	3.81
France	2	0.00†	2.96	1.44
Germany	2	0.36	0.93	1.19

Derived from the monthly reference dates in Table 16.
†Smaller than the value that would be fallen short of once in twenty times by chance.

VII Conclusions from Tests

Before attempting to draw conclusions from the tests in this chapter, it is well to note their limitations. All our tests are based on a small sample of series. They are restricted to this country except for measures of the duration of business cycles. They are arranged for the most part according to mechanical criteria. The probability tests we have used are based on assumptions that are not fully met by cyclical measures. More troublesome still, the number of cycles in our test series is small, only twenty-three at its largest. These series cover a long stretch relatively to most monthly series in our collection, but not relatively to the history of business economy. Hence our experiments may mean that cyclical behavior in fact has usually been free from 'substantial' and 'significant' secular changes; but they also may mean that the influence of secular changes is too small to be detected reliably by means of the data and techniques employed.

These remarks severely limit the conclusions that we may draw concerning secular changes in cyclical behavior. But the aims of this chapter are also modest. We have not been concerned with the question whether secular or structural changes are characteristic of cyclical behavior, but with the narrower question whether such changes have been so prominent as to discredit the use of averages. We can say with confidence that, on the whole, secular or structural changes have not impressed their influence very strongly on the cyclical behavior of our sample of seven American series or on the durations of business cycles in our four countries. From experience in handling numerous time series, we judge that our sample is tolerably representative in this respect of the great bulk of economic series, except those of very narrow coverage. Hence, we see no serious obstacle, so far as secular changes are concerned, to the use of averages to express in a preliminary way what cyclical behavior has been characteristic of different economic activities in recent decades.

If secular changes usually account for only a small portion of the cyclical variability in our time series, two corollaries follow. First, averages covering all cycles in a series can usually generalize the cyclical behavior of the series, if that is to be done at all, about as well as evolving averages or subperiod averages. Second, it is safe, ordinarily, to use in different connections averages based upon different groups of cycles for the same series, and we may even compare averages for different series based upon different periods.

VIII Preparation for Later Work

These conclusions, however, cannot be applied casually or mechanically. Our analysis has disclosed what we take to be several genuine instances of secular or structural change in cyclical behavior. It also suggests, however faintly, that the cyclical behavior of money markets has been more sensitive to secular factors than the cyclical behavior of security or industrial markets, that the duration of specific cycles has been influenced less than their timing or amplitude, and that the amplitude of business fluctuations may have increased since 1914. Quite apart from our specific results, it hardly seems possible that the widespread secular changes that have taken place in economic organization—such as the increasing scale of business enterprise, the spread of absentee ownership, the building up of colonial empires, the disappearance of our frontier, the commercialization of agriculture, the declining rate of population growth, the development of installment selling, the increasing role of government in economic affairs, and many others—have not left their mark on business cycles. Hence, we scrutinize closely the cyclical measures for each series in succeeding monographs, note secular changes in cyclical behavior that appear in the light of the surrounding facts to be significant, and thus prepare materials that should prove suggestive in a later attack on the problem of cumulative changes in business cycles.

These steps would be essential even if we knew in advance that business cycles were uniform in the long run. In an expanding economic system, characterized by continual accessions of new industries and an absolute or relative decline of old industries, secular changes might be found in the cyclical behavior of each industry and yet be absent in industry as a whole; just as retardation in the growth of individual industries is compatible with a uniform rate of growth or even acceleration of total industrial production. To show how changing practices in industry, commerce, and finance penetrate the world of business and what their cyclical repercussions are, is one of the most important problems to be tackled in the theoretical volume of this series.

Studies of secular movements by several investigators have shown that many industries have similar histories. During the experimental years of a new industry, or the period when an old industry gets its first taste of the Industrial Revolution, the rate of growth is usually moderate. When the methods of production have been stabilized and a wide demand for the product at a profitable price has been assured, the rate of growth is rapid. As technical improvements are added, prices fall relatively to the products of older industries, and markets keep expanding. But after a point, every fresh advance encounters increasingly stiff resistance from older industries and even more from new industries endowed with the freshness and vigor of youth. Finally, a stage of decline may set in, when the product is superseded by some other article that gives more satisfaction in proportion to its cost.[23]

Some of our time series are long enough to show industries passing through two or more of these 'stages', and the secular changes ordinarily affect cyclical behavior as we measure it. The intra-cycle trend and the tilt of the cyclical patterns shift with the rate of growth. The duration of the expansions and contractions of specific cycles, the amplitude of rise relative to the fall, the leads or lags at the reference turns, the indexes of conformity, indeed, nearly all of our measures may be influenced.[24] Sometimes the record is long enough to yield two or three sets of averages, one representing the stage of rapid growth, another the stage of moderate growth; or one representing the high tide, another the stage of comparative stability, and perhaps a third the decline of the industry. In other instances, the best we can do is to exclude from the averages two or three early or late cycles that represent a fragment of life history too short to yield significant averages, but too different from most of the record to be lumped with it.

An instructive illustration of secular change in cyclical behavior is provided by the history of American railroads.[25] Chart 59 shows cyclical

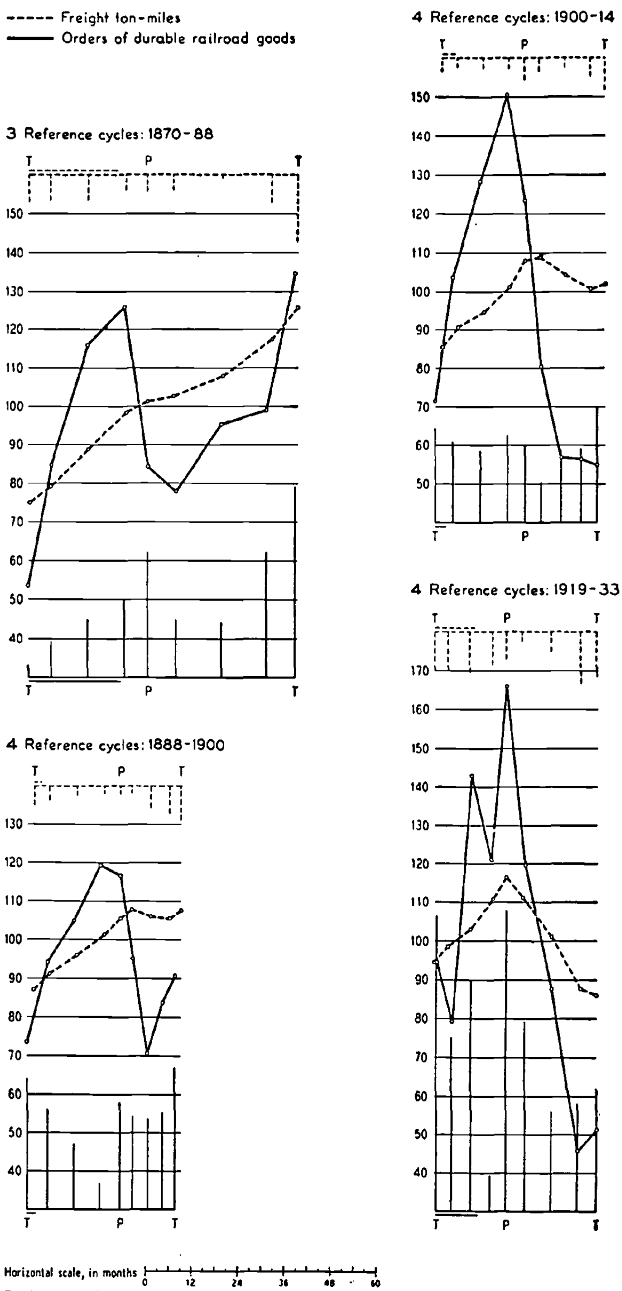
23 See Simon Kuznets, *Secular Movements in Production and Prices* (Houghton Mifflin, 1930), Ch. I, III, V-VI, and Arthur F. Burns, *Production Trends in the United States since 1870*, pp. 79-82, 96-173, and 270-81.

24 See W. C. Mitchell and A. F. Burns, The National Bureau's Measures of Cyclical Behavior (National Bureau of Economic Research, *Bulletin 57*, July 1, 1935), pp. 16-17; also above, Ch. 7, particularly Sec. VI.

25 Secular changes in the cyclical behavior of railroad investment will be discussed at length in our monograph on construction work.

CHART 59

Average Patterns of Four Successive Groups of Reference Cycles
Railroad Traffic and Investment, United States, 1870–1933



The disturbed cycle 1914–19 is omitted. For sources of data, etc., see Appendix C; for explanation of chart, Ch. 5, Sec. VII. The intervals between successive stages for ton-miles and orders differ slightly, since the former series is monthly and the latter quarterly.

patterns of orders for durable railroad goods and of freight ton-miles of traffic during four groups of business cycles since 1870. For a time the opportunities for profits in railroads seemed boundless, and investments in railway plant increased at a vigorous rate. A cyclical depression in general business checked new investment, but the check was comparatively brief; for even during depressions railroads were able to gain new traffic from other transport agencies and to carry that traffic over longer distances. Hence we find railroad construction leading by long intervals and playing an 'active' part in cyclical revivals. But the sharp rate of growth of new railroad lines could not be continued indefinitely. Once the business of rival agencies was fairly well captured and the continent crisscrossed with railroad lines, the addition of new mileage often resulted merely in a duplication of existing facilities. A period of rate wars, maneuvers for control of competing lines, and outright consolidations set in. The incentive to construct new lines diminished, while the need for betterments and additions increased. These changes reinforced one another, so far as their effects on cyclical timing are concerned. The expansion of auxiliary industries to a point where they could meet orders by railroads more quickly than in earlier times worked to the same end. Railroad enterprises tended to become more cautious, to give less weight in making investment decisions to favorable building costs and money market conditions, and to give more weight to the traffic in sight. These tendencies were quickened as new transport agencies—interurban railways, trucks, motor buses, passenger automobiles, pipe lines, airplanes, and revived waterways—emerged and battled the railroads for traffic as mercilessly as the railroads in their youth had battled their rivals. Chart 59 shows the progressive changes in the cyclical timing of fixed railroad investments that resulted from these complex forces. As the trend of railway traffic flattened out and its cyclical fluctuations became intensified, the leads of new investments at business-cycle revivals became irregularly weaker, vanished, and finally were replaced by lags.[26]

In certain activities cyclical behavior is free from secular change, yet is not steady in the long run. For an extended period a series may follow a characteristic pattern, then shift abruptly to another pattern. When we encounter such cases, we strike separate averages for the period preceding and following the discontinuous shift. For example, railroad freight rates in the United States, so far as we may judge from receipts per ton-mile, tended to conform positively to business cycles before about 1890; after that date they usually move invertedly. This shift is due, at least in part, to the regulative activities of the Interstate Commerce Commission established in 1887, which made the adjustment of freight rates to

26 By applying variance analysis to the leads or lags (in cycle-stage units) of the series on orders of durable railroad goods at reference troughs, using the groups shown on the chart, we get a variance ratio of 3.42, which is very close to the .05 point (3.59).

cyclical changes in general business a more deliberate and time-consuming process. Another example is the reduction in the cyclical amplitude of short-term interest rates after the introduction of the Federal Reserve system, on which we commented above. A still neater example is found in the history of silver prices. During the two centuries prior to the 1870's, the price of silver relatively to the price of gold was virtually free from cyclical fluctuations. Since that time, the progressive 'demonetization' of silver has been accompanied by exceptionally violent flucuations in its gold price.[27]

Many of our foreign series seem to support the view that an abrupt change occurred around 1914 in the economic activities of Western Europe. The change is evident in certain series on production, trade and employment. It is reflected most sharply in monetary series, as should be expected from the intermittent existence of the gold standard and the changes in the methods of operating it since 1914. For instance, the metallic reserves of the Bank of France moved invertedly to business cycles without exception from 1872 to 1914, but bear an extremely irregular relation to business cycles since 1914. The ratio of reserves to liabilities in the Bank of England conforms about as well to business cycles after 1914 as before, but the cycles have become more intense. In these and similar instances, as already stated, we strike separate averages for the period before and after the discontinuous shift.

We trust that these illustrations convey a more adequate notion of the manner in which we actually use averages than do the brief statements in preceding chapters. Our primary interest at this stage of our work, to repeat, is in average cyclical behavior. We proceed on the belief that business cycles have been sufficiently stable 'in the long run' to justify the effort to develop a systematic and detailed picture of what happens during an 'average' or 'typical' business cycle. But we conceive of this picture as being merely a first approximation to our ultimate goal, which is to explain the business cycles of actual life. Hence we are also preparing materials as well as we now can on secular and discontinuous changes in cyclical behavior. Once our basic analysis of time series is completed, we plan to collate the results for the longest series, note what regularities they suggest, then scrutinize all our series, those covering only a half-dozen cycles as well as those covering ten or more cycles, in the light of these suggestions. We expect that these studies will help us not only to determine what secular or discontinuous changes have taken place in specific and business cycles, but also to explain how the business cycles of actual life typically have run their course.

27 See George F. Warren and Frank A. Pearson, *Prices* (John Wiley, 1933), pp. 142-4.