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HAVE POSTWAR ECONOMIC FLUCTUATIONS BEEN STABILIZED?

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ABSTRACT

Previous investigations of whether the volatility of the U.S. economy diminished after World War II have been inconclusive because of questionable prewar macroeconomic aggregates. We examine, more broadly, the hypothesis of the stabilization of the postwar economy by focusing on the duration of business cycles, rather than their amplitude; in the process, we avoid the debate about the quality of prewar aggregates. Using distribution-free statistics, we find clear evidence of postwar duration stabilization in terms of a shift toward longer expansions and shorter contractions. Moreover, we find no shift in whole-cycle durations, which suggests a reallocation of the business cycle away from contraction and toward expansion.

1. *Introduction*

Arthur Burns (1960, p.2) was perhaps the first to articulate clearly the view that business cycles in the postwar era had changed in character:

> Between the end of the Second World War and the present, we have experienced four recessions, but each was a relatively mild setback. Since 1937 we have had five recessions, the longest of which lasted only thirteen months. There is no parallel for such a sequence of mild--or such a sequence of brief--contractions, at least during the past hundred years in our own country.

The steady growth of the 1960s reinforced the general acceptance of what we shall call the stabilization hypothesis, namely, that since World War II, the U.S. economy has been more stable than in the prewar period. Prominent proponents of the stabilization hypothesis included Baily (1978) and DeLong and Summers (1986). However, it should be noted that these authors, as well as the general stabilization debate, focused only on the changing *volatility of business cycle fluctuations, arguing that the variability of various* macroeconomic aggregates about trend had diminished in the postwar period.

The early consensus on volatility stabilization was seriously challenged by Romer (1986a, 1986b, 1986c, 1988, 1989). She argued that the higher volatility of prewar macroeconomic aggregates (whether real GNP, industrial production, or the unemployment rate) was the result of different prewar and postwar data construction methodologies, and that when consistent methodologies were employed, the difference in prewar and postwar volatility was greatly lessened.[1] Under this interpretation, the moderation of the business cycle was simply a spurious consequence of inconsistent data.

1. Sheffrin (1988) confirms Romer's U.S. results for a number of European countries. See also Backus and Kehoe (1989).

Romer's contention has not gone undisputed. Various authors have constructed still more alternative versions of prewar aggregates (Weir (1986) and Balke and Gordon (1989)) and, on examination, have reached traditional conclusions about lower macroeconomic volatility in the postwar period. Others, such as Lebergott (1986), have argued that--like the original series--Romer's reconstructed aggregates depend in important ways on unverifiable assumptions and therefore cannot be ranked as unambiguously better than the original series.

The conclusion that we draw from the above literature on postwar volatility stabilization is that, given the limited availability of prewar source data, it is very difficult to construct incontrovertible quantitative aggregate measures of the prewar U.S. economy. This is true even at the annual frequency employed by previous authors. Moreover, the quantitative size of fluctuations in these constructed macroeconomic aggregates will be crucial for the resolution of the volatility debate. Thus, the underlying inadequacy of quantitative measures of the prewar economy makes any comparison of prewar and postwar volatility rather uncertain.

We do not resolve this uncertainty; instead, we provide new evidence on the stability of the postwar economy by concentrating on a different aspect of stabilization and by employing a different type of data. Drawing upon the duration perspective suggested by Diebold and Rudebusch (1990), we focus on stabilization in terms of the relative duration, rather than the relative volatility, of prewar and postwar business cycles. In modern terminology, the duration perspective considers the frequency of business cycles, rather than just their amplitude (as in the volatility debate). Duration is clearly one aspect of postwar stabilization that Burns (cited above) had in mind, for he referred to the brevity of postwar contractions as well as their mildness.[2]

Because we examine durations rather than amplitudes of business fluctuations, we only require a chronology of turning points and do not employ data on a macroeconomic aggregate. The informational advantages of a chronology over an aggregate measure are twofold. First, because designation of turning points largely requires only a *qualitative* sense of the general direction of business activity, it requires much less information to construct than a *quantitative* aggregate measure. It is much easier to determine, for example, that the second quarter of 1894 was a cyclical peak than it is to determine that real GNP rose x percent and fell y percent in the second and third quarters of that year. By eschewing examination of the amplitude of business fluctuations, we avoid focusing on, and relying on, the quantitative movements in a prewar macroeconomic aggregate, which are critical to conclusions about volatility. A second advantage of a business cycle chronology is that the chronology can incorporate a greater variety and number of sources of cyclical information. A chronology can be constructed from a broad set of indicators of business activity, rather than being limited to just the components of real GNP or industrial production. In the construction of the National Bureau of Economic Research (NBER) business cycle chronology, which we use below in our analysis, a large number of sources of information have been used including the cyclical price movements of stocks and other assets as well as

2. Duration considerations have been largely been ignored by previous researchers with the exception of DeLong and Summers (1988) and Zarnowitz (1989), who address similar issues with other techniques.

narrative accounts of business activity from descriptive business annals. Sources such as these have necessarily been ignored in the volatility stabilization debate; thus, our use of the NBER business cycle chronology implicitly brings new information to bear on the debate about the changing nature of business fluctuations. A further advantage of this larger information set is that it can capture higher frequency cyclical movements. Previous volatility comparisons have only been able to construct the requisite aggregates at an annual frequency, which is somewhat crude for assessing cyclical properties. The NBER turning point chronology is able to incorporate monthly data.

In our analysis, however, we do not accept the NBER chronology unquestioningly. One clear truth in economic history is that the quantity and quality of economic data have increased markedly over the last century. The poor quality and relative paucity of earlier data may affect the comparability of prewar and postwar turning point dates. Such data considerations may be important for judging changes in cyclical duration, just as similar data problems were crucial for the volatility debate.

The paper proceeds as follows. In section 2, we discuss the NBER business cycle dating methodology as well as the consistency of the resulting cyclical dates. In section 3, we describe and perform tests of the null hypothesis of no duration stabilization, that is, that the distributions of prewar and postwar durations are identical. The tests are designed to have high power against alternatives involving shifts in location (i.e., mean duration). We also construct confidence intervals for the location shift. We provide extended discussion and interpretation of our results in section 4, and we offer a summary in section 5.

2. The NBER Business Cycle Chronology

The dates of business cycle peaks and troughs as designated by the NBER for the United States are shown in table 1 along with the associated durations of expansions, contractions, and whole cycles measured from peak to peak and from trough to trough. As noted above, the earlier volatility debate has hinged on the issue of the comparability of prewar and postwar data, and we focus the discussion in this section on similar issues: the consistency of the prewar and postwar NBER turning point dates and, equivalently, the comparability of prewar and postwar durations.

A brief review of the NBER dating procedure is in order. One of the earliest statements of these methods is in Burns and Mitchell (1946, pp. 76-77):[3]

> Our first step toward identifying business cycles was to identify the turns of general business activity indicated by [descriptive business] annals. Next, the evidence of the annals was checked against indexes of business conditions and other series of broad coverage. In most cases these varied records pointed clearly to some one year as the time when a cyclical turn occurred. When there was conflict of evidence, additional statistical series were examined and historical accounts of business conditions consulted, until we felt it safe to write down an interval within which a cyclical turn in general business probably occurred. We then proceeded to refine the approximate dates by arraying the cyclical turns in the more important monthly or quarterly series we had for the time and country.

The last step is the most important as it directly focuses on the amount of cyclical co-movement or coherence among economic variables. For Burns and Mitchell, this co-movement is the prime definitional characteristic of the

3. A more recent description is Moore and Zarnowitz (1986), which provides an excellent overview of the NBER methods and related issues.

business cycle: "... a cycle consists of expansions occurring at about the same time in many economic activities, followed by similarly general recessions, ..." (Burns and Mitchell, 1946, p. 3). Thus, in determining monthly business cycle turning point dates, Burns and Mitchell considered hundreds of individual series, including those measuring commodity output, income, prices, interest rates, banking transactions, and transportation services. The peaks and troughs of these individual series are not randomly distributed; rather, they form clusters of similar turning points. The monthly dates of such clusters, which are designated as the turning points of the general business cycle, are listed by Burns and Mitchell (1946, p. 105) for the period from 1854 through 1938. Dates in the postwar period have been designated by the NBER by an identical methodology (see Moore and Zarnowitz (1986)).[4] Note that, in contrast to popular folklore, NBER researchers have never used two consecutive quarterly declines in real GNP as the criterion for dating downturns.

The historical continuity and constancy of the procedures used in designating turning points by NBER researchers supports the use of these dates in prewar/postwar comparisons. However, although procedures may not have changed, both the number and quality of the underlying individual series examined have increased greatly over time. For example, in Burns and Mitchell's analysis (1946, p. 82) only 19 individual monthly or quarterly series were available for dating in the 1860s, while 199 were available for the dates after 1890, and 665 were available after 1920. The increases in

4. Two detailed illustrations of the postwar application of the NBER dating methodology are Zarnowitz and Moore (1977) for the 1973-1975 recession and Zarnowitz and Moore (1983) for the 1980 recession.

the number of underlying individual series, which were also accompanied by increases in quality, led to increases in the reliability of the NBER dates. The "clustering zones" of individual turning points are quite narrow in the postwar period; in contrast, there is more uncertainty about some of the dates in the prewar period because of the lack of data. These changes in the reliability of the dates could subtly affect their consistency, as certain series necessarily assume more importance in the absence of others. In other words, changes in the reliability of the NBER dates could affect their consistency for a prewar/postwar comparison. The rest of this section addresses this issue and describes some of the variations that we consider of the canonical NBER chronology in order to ensure the robustness of our results.

All of the researchers who have designated NBER turning points have cautioned that there is some uncertainty about the precise month of the general turn in business activity. One way to get an indication of the uncertainty about the designated dates is to consider their distance from a number of alternative dates that have been suggested by NBER researchers and by independent observers. Let us first consider the reliability of the postwar dates. The NBER turning point dates during the early part of the postwar period were the subject of some controversy, with several alternative chronologies hotly debated (Cloos, 1963a, 1963b; Zarnowitz 1963a, 1963b; Trueblood, 1961; Moore, 1961). For our purposes, the differences between the proposed alternatives and the official postwar chronology are minor; of the eight dates examined by Cloos, for example, his suggested changes would shift one peak back by one month, another forward by two months, and one trough back by 3 months.

The choice of more recent dates in the postwar period (since 1960), and indeed the entire NBER turning point methodology, has gained additional support from recent research by Stock and Watson (1989).[5] They have attempted to formalize the notion that the business cycle is defined by the co-movements of many macroeconomic time series by specifying a dynamic factor model that identifies the unobserved common component in the movements of many coincident variables. The cyclical peaks and troughs of the extracted common component coincide with the NBER chronology; the only exception is in 1969, when the NBER-dated peak is two months later than the one designated by Stock and Watson.

As suggested above by the large changes in the number of time series employed by Burns and Mitchell, the prewar dates are of varying quality. The dates in the interwar period (1918-1938) appear to be little more questionable than those in the postwar period. Of the original 12 turning points in this period specified by Burns and Mitchell, careful re-evaluations by the NBER staff led to three changes of one month and two shifts of two months (Moore and Zarnowitz, 1986). This is broadly indicative of the amount of uncertainty in the interwar dates.

The turning point dates before World War I are more uncertain. Again, we can compare alternative business cycle chronologies for this period, such as Ayres (1939), Kitchin (1923), and Persons (1931), in order to gauge the uncertainty associated with the NBER's choices.[6] From this perspective,

5. The postwar NBER chronology is also broadly confirmed by Hamilton (1989), who posits an underlying nonlinear regime-switching model and uses optimal signal extraction techniques to estimate turning-point dates.
6. Indeed, this is one of the procedures used by Burns and Mitchell (1946, p. 108) to examine the dependability of their dates.

the NBER dates appear to be reasonable choices, with no clear bias in choosing peaks and troughs that are either too early or too late. However, the range in variation among alternatives is larger, with an average shift of about four months. Careful examinations of the early NBER dates, notably Fels (1959) and Zarnowitz (1981), place the greatest uncertainty on the timing of the dates before 1885. Before the mid-1880s, very few comprehensive statistics are available at a monthly frequency; consequently, the "clusters" of individual series available for Burns and Mitchell are rather sparse and diffuse. In our empirical analysis, we shall examine the robustness of our results when the turning points before 1885 are excluded.

Although the early NBER dates appear to provide a reasonably unbiased delineation of good times from bad times, there is a remaining question about whether some of the designated recessions represent true cyclical contractions or rather are simply periods of very slow growth, i.e., growth recessions. This distinction is more difficult to make for recessions in the pre-World War I period because several of the data series are only available on a trend-adjusted basis, making actual declines in real economic activity difficult to judge. In the period after 1885, the 1887-1888 recession is the most dubious, although the 1899-1900 recession was also very mild (Kitchin, 1923; Eckler, 1933; Fels, 1959; Zarnowitz, 1981). Although we remain undecided on the growth recession issue, we examine the consequences of treating 1887-1888 and 1899-1900 as growth slowdowns rather than as business cycle contractions. Our procedure replaces these prewar contractions and their immediate preceding and succeeding expansions by two long expansions; this provides the least favorable case for the postwar duration stabilization hypothesis.

3. Analysis of Postwar Duration Stabilization

3a. Testing the Stabilization Hypothesis

Consider two samples of prewar and postwar durations of size n_1 and n_2, $\{X_1, \ldots, X_{n_1}\}$ and $\{Y_1, \ldots, Y_{n_2}\}$. Denote the corresponding population prewar and postwar duration distribution functions by F and G. The null hypothesis of no postwar duration stabilization implies that these distributions are identical ($F = G$). Depending on the situation, we shall subsequently be interested in both one-sided and two-sided alternatives. The interpretation of the one-sided alternative that Y is stochastically larger than X is that (1) $F \neq G$, and (2) $G(k) \leq F(k)$, for all k (or equivalently, $P(Y > k) \geq P(X > k)$, for all k). The inequalities are reversed for the one-sided alternative that X is stochastically larger than Y. The two-sided alternative, $F \neq G$, has obvious interpretation.

We shall test the null hypothesis of no postwar stabilization using the Wilcoxon, or rank-sum, test. We replace the observations $\{X_1, \ldots, X_{n_1}, Y_1, \ldots, Y_{n_2}\}$ by their ranks, $\{R_1, \ldots, R_n\}$, where $n = n_1 + n_2$.[7] Then the Wilcoxon test statistic is formed as

$$W = \sum_{i=n_1+1}^{n} R_i,$$

the sum of the ranks in the second sample. The key insight is that the distribution of W under the null hypothesis of no stabilization is invariant

7. In the case of a tie, the relevant ranks are replaced by the average of the ranks of the tied observations.

to the true underlying distribution of durations. This invariance follows from the fact that the null distribution of the ranks is independent of the true underlying distribution, and is given by

$$P(R_1=r_1, \ldots, R_n=r_n) = 1/n!, \qquad (1)$$

for all permutations $(r_1, \ldots, r_n)$ of $(1, \ldots, n)$. Equation (1) enables computation of exact finite-sample critical values of W, which are tabled in Bradley (1968) for $n_1, n_2 < 25$. For $n_1, n_2 > 8$, the asymptotic distribution of the standardized Wilcoxon statistic,

$$Z = \frac{W - 1/2\ n_2(n_2+1)}{[1/12\ n_1 n_2 (n+1)]^{1/2}} \overset{a}{\sim} N(0, 1), \qquad (2)$$

is quite accurate.

The Wilcoxon test is a *nonparametric*, or distribution-free, test designed to have particularly high power against alternatives involving a shift of location. Intuition can be gained, therefore, by comparing the Wilcoxon test to the classical t-test for equality of two population means,

$$t = (n_1 n_2/n)^{1/2}\ [(\bar{Y} - \bar{X})\ /\ s],$$

where

$$s = (n-2)^{-1/2}\ [\sum_{i=1}^{n_1} (X_i-\bar{X})^2 + \sum_{j=1}^{n_2} (Y_j-\bar{Y})^2\ ]^{1/2}.$$

The t-test is appropriate for testing the null hypothesis that $E(X) = E(Y)$, when the underlying populations are normally distributed. Unfortunately, normality is a distinctly inappropriate distributional assumption for duration data. The Wilcoxon test may be interpreted as a distribution-free t-test, obtained by replacing the observations $\{X_1, \ldots, X_{n_1}, Y_1, \ldots, Y_{n_2}\}$ by their ranks, $\{R_1, \ldots, R_n\}$, which yields

$$t^* = (n_1 n_2/n)^{1/2} [(\bar{R}_2 - \bar{R}_1) / s^*],$$

where $\bar{R}_1$ and $\bar{R}_2$ denote the mean ranks of the X and Y samples, and

$$s^* = (n-2)^{-1/2} [\sum_{i=1}^{n_1} (R_i-\bar{R}_1)^2 + \sum_{j=n_1+1}^{n} (R_j-\bar{R}_2)^2]^{1/2}.$$

Straightforward but tedious algebra reveals t^* to be a monotone transformation of W.

Because the Wilcoxon test is exact, we are assured of correct-test size, even in small samples. But what of power? It turns out that the tradeoff between the relaxation of distributional assumptions and the loss of power is extremely favorable--the Wilcoxon test is only slightly less powerful than the t-test when the distributional assumption (normality) underlying the t-test is true, and it is typically much more powerful when the distributional assumption is false.[8]

3b. Estimating the Shift in Mean Duration

Under the maintained assumption that the distributions of durations differ only by a shift in location, i.e., $G(k) = F(k+\Delta)$ for all k, we can produce a confidence interval for the location shift, Δ. Consider the n_1n_2-element sequence of differences $\{D_{ij}\}$, $i = 1, \ldots, n_1$, $j = 1, \ldots, n_2$, where

8. See Bickel and Doksum (1977) for a comparison of the performance of the Wilcoxon and t-test.

$D_{ij} = Y_j - X_i$, and order them so that $D_{(1)} < D_{(2)} < \ldots < D_{(n_1 n_2)}$. For a given significance level α, let k_α be an integer defined from the confidence interval

$$P(k_\alpha \leq U \leq n_1 n_2 - k_\alpha) = 1-\alpha,$$

where

$$U = W - 1/2\ n_2(n_2+1)$$

is the Mann-Whitney U-statistic, a monotone transformation of W.[9] Then it can be shown (Bickel and Doksum, 1977) that

$$P(D_{(k_\alpha)} \leq \Delta \leq D_{(n_1 n_2 - k_\alpha + 1)}) = 1-\alpha.$$

Thus, a two-sided $(1-\alpha)\%$ confidence interval for Δ is $(D_{(k_\alpha)}, D_{(n_1 n_2 - k_\alpha + 1)})$. Alternatively, the two one-sided confidence intervals are $(-\infty, D_{(k_{2\alpha})})$ and $(D_{(n_1 n_2 - k_{2\alpha} + 1)}, \infty)$.

3c. Empirical Results

We use a variety of samples in our empirical examination of the stabilization hypothesis in order to assess the robustness of our results, especially in light of some of the concerns about the consistency of dating noted in section 2. There are four major changes to the official chronology that we consider:

(1) exclusion of the pre-1885 turning point dates in order to avoid potentially unreliable dates in the very early period,

9. The finite-sample distribution of U is tabulated in Bickel and Doksum (1977), for n_1, $n_2 < 8$. For larger samples, the asymptotic distribution (2) may be used.

(2) elimination[10] of the 1887 and 1899 recessions to account for the possibility that these were merely growth recessions,

(3) three different ending dates for the prewar sample (June 1938, August 1929, December 1914) to exclude the influence of the Great Depression or the interwar period in general, and

(4) exclusion of wartime expansions and whole cycles that include wartime expansions to avoid possible spuriously long observations.

A detailed listing of all sample periods and the associated mnemonics (with the A, B, C samples all loosely termed "prewar" samples) is as follows:

<u>Pre-World War II (December 1854 - June 1938)</u>

A1. All observations
A2. Excluding observations before May 1885
A3. A2, eliminating 1887 and 1899 contractions
A1X. A1, excluding wartime observations
A2X. A2, excluding wartime observations
A3X. A3, excluding wartime observations

<u>Pre-Great Depression (December 1854 - August 1929)</u>

B1. All observations
B2. Excluding observations before May 1885
B3. B2, eliminating 1887 and 1899 contractions
B1X. B1, excluding wartime observations
B2X. B2, excluding wartime observations
B3X. B3, excluding wartime observations

<u>Pre-World War I (December 1854 - December 1914)</u>

C1. All observations
C2. Excluding observations before May 1885
C3. C2, eliminating 1887 and 1899 contractions
C1X. C1, excluding wartime observations
C2X. C2, excluding wartime observations
C3X. C3, excluding wartime observations

<u>Post-World War II (February 1945 - July 1990)</u>

Z. All observations
ZX. Z, excluding wartime observations

10."Elimination" of a recession means that we replace that contraction and the preceding and succeeding expansions by one long expansion.

Results appear in tables 2 and 3 for expansions and contractions. For each pair of prewar and postwar samples, we report the sample sizes, mean durations, exact Wilcoxon statistic and its one-sided p-value, approximate Wilcoxon statistic and its one-sided p-value, and approximate 90 percent and 80 percent one-sided confidence intervals.[11] For example, the first line of table 2 compares the prewar expansion sample A1 (with 21 observations and mean duration of 26.5 months) and the postwar expansion sample Z (with 9 observations and mean duration of 49.9 months). For these two samples, the exact and approximate Wilcoxon p-values under the null hypothesis of no change in distribution are less than .01, and the confidence interval estimates suggest that we can be 90 percent certain that the postwar increase in mean expansion duration was at least 9 months. Similar results are obtained for the other pairs of expansion samples in table 2 and for contraction samples in table 3. Almost without exception, the tests reject the null hypothesis of no stabilization in favor of longer postwar expansions or shorter postwar contractions. Rejection is typically at the 1 percent level or better; twelve of eighteen Wilcoxon p-values for expansions are less than or equal to .025, as are *all* p-values for contractions.[12]

Even more persuasive evidence for duration stabilization is given by the results of a test of the *joint* stabilization hypothesis that expansions

11. The obvious alternatives of longer postwar expansions and shorter postwar contractions make one-sided tests and confidence intervals appropriate.
12. The p-values for the W and Z statistics move closely together; this makes the Z statistics particularly useful, because the coarseness of the tabled W distribution makes calculation of precise p-values difficult. Thus, for example, in the few cases of expansion W p-values above .1, we can make reliable guesses (from the Z p-values) that they are only slightly greater than .1.

have lengthened and contractions have shortened. We test this joint hypothesis by examining the distribution of the ratio of the expansion duration to the preceding contraction duration. Under duration stabilization, postwar ratios of expansion duration to contraction duration will be unambiguously larger than in the prewar period. The test statistics are reported in table 4. All p-values are less than .01 and all but two are less than or equal to .001; we interpret these results as the most compelling evidence in favor of overall duration stabilization.

It is comforting and unusual in empirical macroeconomics to obtain such high significance levels, particularly with such small samples. But what of the more important question: Are the postwar shifts *significant* from an economic perspective? Clearly, the answer is yes. The mean postwar expansion duration is *double* that of its prewar counterpart, while the mean postwar contraction duration is *half* that of its prewar counterpart. The distributional shifts for expansions and contractions are illustrated graphically in figures 1 and 2 (with A1 prewar samples and Z postwar samples).[13] (The axes in each figure are scaled identically, so the two figures are fully comparable.) Figure 1 shows the estimated distribution functions for expansions; duration stabilization is clearly manifest in the rightward shift of the postwar distribution function. Duration stabilization for contractions is shown in figure 2 by the leftward shift of the postwar distribution.

13. The empirical distribution functions are, of course, step functions. The figures show the piecewise-linear functions obtained by connecting the midpoints of each step (which are denoted by boxes).

Results for whole cycles, which appear in tables 5 (peak-to-peak) and 6 (trough-to-trough), are very different.[14] The p-values of the Wilcoxon tests rarely indicate significant change in the postwar period; in fact, they are typically greater than .2. Thus, the data suggest an unchanged distribution of whole cycle durations, but with a revised allocation of the whole cycle into expansions and contractions: Postwar expansions are longer, and contractions shorter.

4. Duration Stabilization: Interpretations and Implications

In this section, we examine our results that postwar expansions have been longer and postwar recessions shorter than their prewar counterparts. We discuss welfare considerations related to this duration stabilization, as well as possible causes of such stabilization. Finally we relate our findings to earlier research.

4a. Welfare Considerations

In light of our evidence of duration stabilization, a natural question is whether it should be regarded as welfare improving. Interpretation of the properties of business cycles in welfare terms requires an economic model, and different models will clearly produce different welfare rankings. Although there are certainly models (typically Keynesian) in which stabilization of the sort found here is welfare-improving, there are other (typically classical) models in which unstabilized fluctuations are Pareto optimal. Full specification of the welfare gains and losses of duration stabilization will have to await the arrival of a consensus theory.

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14. The lack of obvious alternative hypotheses suggests the use of two-sided tests and confidence intervals, apart from which the statistics reported in tables 5 and 6 match those of tables 2 and 3.

However, it is interesting, however, to sketch some of the mechanisms by which welfare might be affected by changes in cyclical duration as opposed to cyclical volatility.

For duration to have an effect on welfare, the accrual of costs and benefits must depend on the length of time that an economic state continues. For physical capital it is doubtful that such costs are important; that is, the costs of restarting a machine that has been idle for two years are probably little more than the startup costs for a machine that has been idle for six months. However, for human capital, a large literature suggests that the costs of idleness are substantial and increase with duration. It has long been recognized that a crucial factor in human capital accumulation is the opportunity to maintain and update skills through employment or, to use Arrow's (1962) felicitous phrase, the opportunity for learning by doing. Workers who are unemployed face an atrophy of skills over time, which reduces the effective supply of labor. Extensive discussion of these effects can be found in Phelps (1972) and Hall (1976). Thus, the fact that recessions have shortened and expansions have lengthened may have additional human capital benefits beyond the simple fact that less time is spent below potential.

4b. Sources of Stabilization

It is interesting to speculate as to what changes in the postwar period are most likely to have contributed to duration stabilization. Many factors have been suggested as explanations for the (possible) postwar volatility stabilization of business fluctuations, and some combination of these factors is also likely to provide an explanation for postwar duration stabilization. To the extent that postwar volatility actually *was* stabilized, one expects, ceteris paribus, concomitant duration stabilization due to the upward trend in aggregate economic activity.[15] However, there are also a variety of avenues through which such factors may have worked to stabilize durations, *independently* of whether volatility was stabilized.[16] We shall consider a variety of factors, with some attention paid to independent duration stabilization and some attention paid to concurrent duration and volatility stabilization.

The potential factors underlying postwar stabilization can be broadly classified into three categories: (1) postwar changes in the nature of macroeconomic shocks, (2) postwar improvements in discretionary government policy, and (3) postwar policy and non-policy structural changes. The first category, namely a direct change in the nature of postwar shocks, must certainly be admitted as a logical possibility, but there is no available evidence, either econometric or anecdotal, that has been given as support.[17] In particular, we know of no evidence that macroeconomic shocks have changed in such a way as to lead either to duration stabilization independent of volatility stabilization (a change in pattern but not size), or to

15. To see this, consider an extreme case. In an upwardly trending economy, if volatility approaches zero, expected expansion duration grows without bound and expected contraction duration collapses to zero. That is, permanent expansion prevails.
16. The link between volatility stabilization and duration stabilization may be affected by other changes in the nature of postwar business cycles, for example, changes in the asymmetry of the cycle.
17. It may be interesting to apply the structural procedure of Blanchard and Watson (1986) in a comparative analysis of the size and pattern of prewar and postwar shocks. However, this analysis would have to rely on questionable prewar aggregates.

concomitant duration and volatility stabilization (a change in size and perhaps pattern).

As for the second category, the start of the postwar period saw both a broadening and deepening of the powers of monetary and fiscal policy and the public commitment of their use to stabilize the economy.[18] There is some evidence (e.g., Delong and Summers (1988)) that this commitment alleviated fears of macroeconomic catastrophic risk, ruling out very long, deep depressions.[19] However, policy attempts at smoothing out the moderate swings in business activity have been judged, even by those who might be expected to somewhat sympathetic (e.g., Gordon (1980), Okun (1980), Blinder (1981)), as neutral at best, with successes offset by failures. Overall, therefore, it would appear that if postwar discretionary government policy produced duration stabilization, it did so independently of volatility stabilization. Such a scenario is not unreasonable, if policy makers perceived a link between expansion and contraction durations and welfare, perhaps along the lines discussed above, and took policy action accordingly.

The last set of factors, postwar structural changes in the economy, also includes likely sources of duration stabilization.[20] Structural changes that have been emphasized in the past for volatility stabilization

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18. The potency of fiscal policy was greatly enhanced by the increased size of the federal government after World War II, while organized and independent monetary policy was only available after the Banking Act of 1935 and the Treasury Accord of 1951. The public consensus and commitment for stabilization was engendered by the Employment Act of 1946.
19. Given the infrequency of full-scale depressions in the prewar period, the absence of postwar contractions of the magnitude of the Great Depression is not completely convincing evidence in this regard.
20. In addition to the above papers, see Burns (1960), Baily (1978), and DeLong and Summers (1986) for discussion of structural changes.

include those that have occurred independently of policy as well as those that are part of the postwar Keynesian institutional order. Prominent in the former category are the increased share of acyclic services in total output,[21] increased availability of consumer credit (with a reduction in the number of liquidity-constrained non-consumption-smoothing households), and technical improvements leading to better inventory management. The important stabilizing policy programs include automatic stabilizers (countercyclical entitlement programs such as unemployment insurance and a progressive tax system) and deposit insurance and regulation (which acts indirectly through stabilization of the financial system). Not all of these will induce duration stabilization. Notably, automatic stabilizers may work to reduce the severity of contractions rather than to shorten them; for example, it is fairly well established that the existence of unemployment insurance *increases* the spell of individual unemployment durations, while reducing the severity of adverse effect of unemployment on personal income (e.g., Meyer (1990)). Others probably have direct effects on duration. More adept inventory control may speed up inventory corrections and shorten contractions, while deposit insurance by eliminating bank panics may have eliminated one stage of prewar contractions.

Finally, we consider one other possible structural source of duration stabilization that would possibly operate independently of volatility stabilization. Because business cycles are delineated on a non-trend-adjusted basis, any differences in the trend growth of the economy in the

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21. However, this may have been offset by the diminished importance of the acyclic agricultural sector.

prewar and postwar periods may affect the duration comparisons. If the postwar economy had a higher average rate of growth than in the prewar period (each with identical trend-adjusted cyclical movements), the duration of postwar expansions would be longer and the duration of postwar contractions would be shorter than in the prewar period. However, as shown in table 8, the mean growth rate of real output in the postwar period was generally lower than in the prewar samples (the roughly corresponding sample mnemonics are given in parentheses).[22] The fact that growth in the postwar period was lower on average than in the prewar period would suggest that, *ceteris paribus*, postwar expansions should be shorter and postwar contractions longer. Thus, our results on duration stabilization do not reflect changes in trend growth.

4c. Relation to Previous Work

In earlier work, Diebold and Rudebusch (1990), we suggested that the tools of duration analysis might profitably be used in the study of business cycles, and we focused attention on the question of business cycle *duration dependence*, that is, whether termination probabilities of expansions and contractions depend on duration.[23] To a first approximation, we found that such termination probabilities are independent of duration. In the language of duration analysis, we found that the hazard functions of expansions and contractions, which chart the dependence of termination probabilities on duration, are approximately constant. The fact that the *slopes* of expansion

22. Note that we rely on the prewar measure of GNP only for average growth estimates, rather than for the more contentious cyclical fluctuations.
23. In addition, for international evidence see Rudebusch, Sichel, Diebold (1990).

and contraction hazards are approximately zero in both the prewar and postwar periods does not preclude the possibility of changes in their *heights*.[24] Such changes, toward a larger postwar contraction hazard and a smaller postwar expansion hazard, imply a decrease in mean contraction duration and an increase in mean expansion duration, precisely the findings that emerged so clearly in tables 2 through 4 and figures 1 and 2.

Finally, our earlier work indicates that, if there *were* any postwar shifts in slopes of hazard functions, the shift was toward a gentler expansion hazard slope and a steeper contraction hazard slope. Such shifts can also be interpreted in terms of the dispersion of durations, with postwar expansions being less tightly clustered in length and postwar contractions more tightly clustered in length than their prewar counterparts. The present paper focuses on *location* (studied via distribution functions) rather than *dispersion* (studied, in our earlier work, via hazard functions), but the results of the both papers are consistent.

5. Summary

We have investigated the stabilization hypothesis from the perspective of duration, or frequency, as opposed to volatility, or amplitude. Our analysis made use of the qualitative information contained in the NBER's business cycle chronology, and was robust to criticisms of conventional measures of prewar aggregate output data. Using distribution-free statistical procedures, we found strong evidence of a postwar shift toward

24. Indeed, in a parametric framework, Sichel (1990) finds just such a significant height shift in the estimated hazards for expansions and contractions in the postwar period.

longer expansions and shorter contractions, which is consistent with a broad interpretation of the stabilization hypothesis. Moreover, we found no evidence for a postwar shift in the distribution of whole-cycle durations, which suggests a zero-sum reallocation of the business cycle away from contraction and toward expansion.

We provided some discussion of the welfare implications of our findings, as well as a discussion of the possible sources of stabilization. It appears that some of the duration stabilization is associated with concomitant volatility stabilization.[25] We believe it highly unlikely, however, that all of the postwar duration stabilization is associated with volatility stabilization. To the extent that volatility actually was reduced, the reduction was small and hard to detect. The postwar shift toward duration stabilization, however, is large and difficult to deny. It is likely, therefore, that duration stabilization arose, at least in part, *independently* of volatility stabilization.

25. *All* postwar volatility estimates of which we are aware--including Romer's --indicate lower postwar volatility. The debate is only over the size and significance of the volatility reduction.

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Table 1
NBER Business Cycle Dates and Durations

Trough	Peak	Contractions	Expansions	Trough to Trough	Peak to Peak
December 1854	June 1857	--	30	--	--
December 1858	October 1860	18	22	48	40
June 1861	April 1865	8	46	30	54
December 1867	June 1869	32	18	78	50
December 1870	October 1873	18	34	36	52
March 1879	March 1882	65	36	99	101
May 1885	March 1887	38	22	74	60
April 1888	July 1890	13	27	35	40
May 1891	January 1893	10	20	37	30
June 1894	December 1895	17	18	37	35
June 1897	June 1899	18	24	36	42
December 1900	September 1902	18	21	42	39
August 1904	May 1907	23	33	44	56
June 1908	January 1910	13	19	46	32
January 1912	January 1913	24	12	43	36
December 1914	August 1918	23	44	35	67
March 1919	January 1920	7	10	51	17
July 1921	May 1923	18	22	28	40
July 1924	October 1926	14	27	36	41
November 1927	August 1929	13	21	40	34
March 1933	May 1937	43	50	64	93
June 1938	February 1945	13	80	63	93
October 1945	November 1948	8	37	88	45
October 1949	July 1953	11	45	48	56
May 1954	August 1957	10	39	55	49
April 1958	April 1960	8	24	47	32
February 1961	December 1969	10	106	34	116
November 1970	November 1973	11	36	117	47
March 1975	January 1980	16	58	52	74
July 1980	July 1981	6	12	64	18
November 1982	?	16	92	28	108
?		--	--	101	--

Note to table 1: The 92-month duration of the last expansion, the 108-month duration of the last peak-to-peak cycle, and the 101-month duration of the last trough-to-trough cycle are conservative estimates. They assume a peak in July 1990 and, for the last of these, a trough nine months later. Wartime expansions and cycles containing wartime expansions are underlined.

Table 2
Wilcoxon Tests of No Stabilization, Expansions

Sample		Sample Size		Mean Duration		Exact Wilcoxon Test		Approximate Wilcoxon Test		Confidence Interval	
1	2	n_1	n_2	$\bar{x}_1$	$\bar{x}_2$	W	$P_1(W)$	Z	$P_1(Z)$	90%	80%
A1	Z	21	9	26.5	49.9	193.5	<.01	-2.45	.00	<-9	<-12
A2	Z	15	9	24.7	49.9	154.0	<.01	-2.48	.01	<-12	<-14
A3	Z	13	9	30.8	49.9	127.5	<.10	-1.60	.05	<-3	<-8
A1X	ZX	19	7	24.5	42.6	132.5	<.025	-2.20	.01	<-5	<-9
A2X	ZX	14	7	23.3	42.6	106.0	<.025	-2.17	.02	<-6	<-12
A3X	ZX	12	7	29.8	42.6	85.5	>.10	-1.31	.10	<0	<-4
B1	Z	20	9	25.3	49.9	190.5	<.005	-2.62	.00	<-10	<-14
B2	Z	14	9	22.9	49.9	151.0	<.005	-2.71	.00	<-13	<-15
B3	Z	12	9	29.3	49.9	124.5	<.05	-1.81	.04	<-4	<-12
B1X	ZX	18	7	23.1	42.6	130.5	<.01	-2.40	.01	<-6	<-10
B2X	ZX	13	7	21.2	42.6	104.0	<.01	-2.42	.01	<-10	<-12
B3X	ZX	11	7	27.9	42.6	83.5	<.10	-1.54	.06	<-3	<-6
C1	Z	15	9	25.5	49.9	154.5	<.01	-2.507	.01	<-9	<-12
C2	Z	9	9	21.8	49.9	56.0	.005	-2.61	.00	<-12	<-15
C3	Z	7	9	32.4	49.9	47.5	>.10	-1.27	.10	<4	<-4
C1X	ZX	14	7	24.0	42.6	106.5	<.025	-2.20	.01	<-5	<-7
C2X	ZX	9	7	21.8	42.6	80.0	<.025	-2.17	.02	<-6	<-12
C3X	ZX	7	7	32.4	42.6	45.5	>.10	-.90	.19	<6	<0

Table 3
Wilcoxon Tests of No Stabilization, Contractions

Sample		Sample Size		Mean Duration		Exact Wilcoxon Test		Approximate Wilcoxon Test		Confidence Interval	
1	2	n_1	n_2	$\bar{x}_1$	$\bar{x}_2$	W	$P_1(W)$	Z	$P_1(Z)$	90%	80%
A1	Z	21	9	21.2	10.7	75.0	<.005	2.93	.00	>3	>5
A2	Z	15	9	17.8	10.7	68.0	<.005	2.67	.00	>3	>3
A3	Z	13	9	18.2	10.7	66.0	<.01	2.51	.01	>3	>3
B1	Z	19	9	20.5	10.7	73.0	<.005	2.84	.00	>3	>5
B2	Z	13	9	16.2	10.7	66.0	<.01	2.52	.01	>2	>3
B3	Z	11	9	16.4	10.7	64.0	<.025	2.33	.01	>2	>3
C1	Z	15	9	22.5	10.7	61.0	<.001	3.09	.00	>6	>7
C2	Z	9	9	17.7	10.7	117.0	<.005	2.80	.00	>4	>5
C3	Z	7	9	18.3	10.7	84.0	.005	2.61	.00	>5	>7

Table 4
Wilcoxon Tests, Expansion Relative to Preceding Contraction

Sample		Sample Size		Mean Duration		Exact Wilcoxon Test		Approximate Wilcoxon Test		Confidence Interval	
1	2	n_1	n_2	$\bar{x}_1$	$\bar{x}_2$	W	$P_1(W)$	Z	$P_1(Z)$	90%	80%
A1	Z	20	9	1.5	4.5	216.0	<.001	-3.82	<.001	<-2.0	<-2.2
A2	Z	14	9	1.5	4.5	169.5	<.001	-3.88	<.001	<-2.0	<-2.1
A3	Z	12	9	1.6	4.5	149.5	<.001	-3.59	<.001	<-1.8	<-2.1
A1X	ZX	18	7	1.3	3.7	152.5	<.001	-3.72	<.001	<-1.8	<-2.0
A2X	ZX	13	7	1.4	3.7	117.5	<.001	-3.49	<.001	<-1.8	<-1.8
A3X	ZX	11	7	1.6	3.7	101.5	<.001	-3.17	<.001	<-1.6	<-1.8
B1	Z	19	9	1.6	4.5	207.0	<.001	-3.77	<.000	<-2.0	<-2.2
B2	Z	13	9	1.5	4.5	160.5	<.001	-3.81	<.000	<-1.9	<-2.1
B3	Z	11	9	1.6	4.5	140.5	<.001	-3.50	<.000	<-1.8	<-2.0
B1X	ZX	17	7	1.3	3.7	145.5	<.001	-3.69	<.000	<-1.8	<-2.0
B2X	ZX	12	7	1.4	3.7	110.5	<.001	-3.42	.001	<-1.7	<-1.8
B3X	ZX	10	7	1.6	3.7	94.5	<.001	-3.08	.001	<-1.6	<-1.8
C1	Z	15	9	1.6	4.5	171.0	<.001	-3.49	<.001	<-2.0	<-2.2
C2	Z	9	9	1.4	4.5	46.5	<.001	-3.45	.001	<-1.9	<-2.0
C3	Z	7	9	1.7	4.5	31.5	<.005	-2.97	.002	<-1.6	<-1.8
C1X	ZX	13	7	1.2	3.7	117.5	<.001	-3.49	<.001	<-1.8	<-2.1
C2X	ZX	8	7	1.4	3.7	82.5	<.001	-3.07	.001	<-1.8	<-1.9
C3X	ZX	6	7	1.7	3.7	24.5	<.010	-2.50	.006	<-1.3	<-1.6

Table 5
Wilcoxon Tests, Peak-to-Peak Cycles

Sample		Sample Size		Mean Duration		Exact Wilcoxon Test		Approximate Wilcoxon Test		Confidence Interval	
1	2	n_1	n_2	$\bar{x}_1$	$\bar{x}_2$	W	$P_2(W)$	Z	$P_2(Z)$	90%	80%
A1	Z	20	9	47.9	60.6	158.0	>.20	-1.09	.28	(-24,7)	(-19,3)
A2	Z	14	9	43.0	60.6	134.0	<.20	-1.64	.10	(-34,0)	(-26,-5)
A3	Z	12	9	46.8	60.6	115.0	>.20	-1.14	.26	(-33,9)	(-26,3)
A1X	ZX	18	7	46.6	53.3	101.5	>.20	-.64	.53	(-18,10)	(-15,7)
A2X	ZX	13	7	41.2	53.3	89.5	>.20	-1.27	.20	(-32,7)	(-17,0)
A3X	ZX	11	7	45.0	53.3	75.5	>.20	-.82	.42	(-28,11)	(-17,7)
B1	Z	19	9	45.6	60.6	156.0	>.20	-1.26	.21	(-28,4)	(-20,0)
B2	Z	13	9	39.2	60.6	132.0	<.10	-1.91	.06	(-38,3)	(-32,-6)
B3	Z	11	9	42.6	60.6	113.0	.20	-1.41	.16	(-38,4)	(-30,-1)
B1X	ZX	17	7	43.8	53.3	100.5	>.20	-.83	.41	(-22,8)	(-15,5)
B2X	ZX	12	7	36.8	53.3	88.5	<.20	-1.57	.12	(-33,2)	(-28,-3)
B3X	ZX	10	7	40.2	53.3	74.5	>.20	-1.12	.26	(-32,7)	(-27,3)
C1	Z	14	9	47.6	60.6	123.0	>.20	-.95	.34	(-24,7)	(-19,4)
C2	Z	8	9	38.8	60.6	54.0	.10	-1.73	.08	(-39,0)	(-32,-5)
C3	Z	6	9	45.0	60.6	40.0	>.20	-.94	.34	(-38,11)	(-26,7)
C1X	ZX	13	7	47.2	53.3	78.5	>.20	-.40	.69	(-18,11)	(-14,8)
C2X	ZX	8	7	38.6	53.3	66.5	>.20	-1.22	.22	(-34,7)	(-18,0)
C3X	ZX	6	7	45.0	53.3	38.5	>.20	-.50	.62	(-19,14)	(-17,11)

Table 6
Wilcoxon Tests, Trough-to-Trough Cycles

Sample		Sample Size		Mean Duration		Exact Wilcoxon Test		Approximate Wilcoxon Test		Confidence Interval	
1	2	n_1	n_2	$\bar{x}_1$	$\bar{x}_2$	W	$P_2(W)$	Z	$P_2(Z)$	90%	80%
A1	Z	21	9	47.7	60.7	167.5	>.20	-1.27	.21	(-20,2)	(-18,0)
A2	Z	15	9	42.5	60.7	141.0	.10	-1.70	.09	(-24,0)	(-20,-4)
A3	Z	13	9	49.0	60.7	117.0	>.20	-.90	.37	(-24,8)	(-19,3)
A1X	ZX	19	7	45.9	53.4	109.5	>.20	-.87	.38	(-17,7)	(-16,2)
A2X	ZX	14	7	41.9	53.4	93.0	>.20	-1.20	.23	(-20,3)	(-17,1)
A3X	ZX	12	7	48.8	53.4	75.0	>.20	-.42	.67	(-18,12)	(-15,9)
B1	Z	19	9	46.1	60.7	162.0	<.20	-1.55	.12	(-21,1)	(-19,-3)
B2	Z	13	9	39.2	60.7	135.5	<.05	-2.14	.03	(-27,-4)	(-20,-7)
B3	Z	11	9	46.4	60.7	111.5	>.20	-1.29	.19	(-27,3)	(-20,0)
B1X	ZX	17	7	43.9	53.4	106.0	>.20	-1.18	.23	(-19,2)	(-16,1)
B2X	ZX	12	7	38.3	53.4	89.8	<.02	-1.65	.09	(-22,0)	(-19,-4)
B3X	ZX	10	7	45.9	53.4	71.5	>.20	-.83	.40	(-20,8)	(-17,6)
C1	Z	15	9	48.0	60.7	134.5	>.20	-1.31	.18	(-21,2)	(-18,-1)
C2	Z	9	9	39.4	60.7	63.0	<.10	1.99	.04	(-27,-3)	(-20,-6)
C3	Z	7	9	50.7	60.7	52.0	>.20	.79	.42	(-27,15)	(-18,8)
C1X	ZX	14	7	45.9	53.4	89.5	>.20	-.93	.35	(-18,7)	(-16,2)
C2X	ZX	9	7	39.4	53.4	73.0	.20	-1.43	.15	(-22,2)	(-17,-2)
C3X	ZX	7	7	50.7	53.4	50.0	>.20	.31	.74	(-16,18)	(-13,9)

Note: Samples with the suffix X refer to exclusion of wartime cycles.

Table 7
Mean Growth Rate of Real GNP

	$\text{mean}(\Delta \log Y_t)$
Postwar sample	
1946-1989 (Z)	.025
Prewar samples	
1870-1938 (A1)	.031
1886-1938 (A2)	.027
1870-1929 (B1)	.037
1886-1929 (B2)	.034
1870-1914 (C1)	.038
1886-1914 (C2)	.033

Note: The real GNP sample (from 1869 to 1929) comes from Romer (1989, pp. 22-23); the later data come from the NIPA.

Figure 1.

Estimated Distribution Functions for Expansions

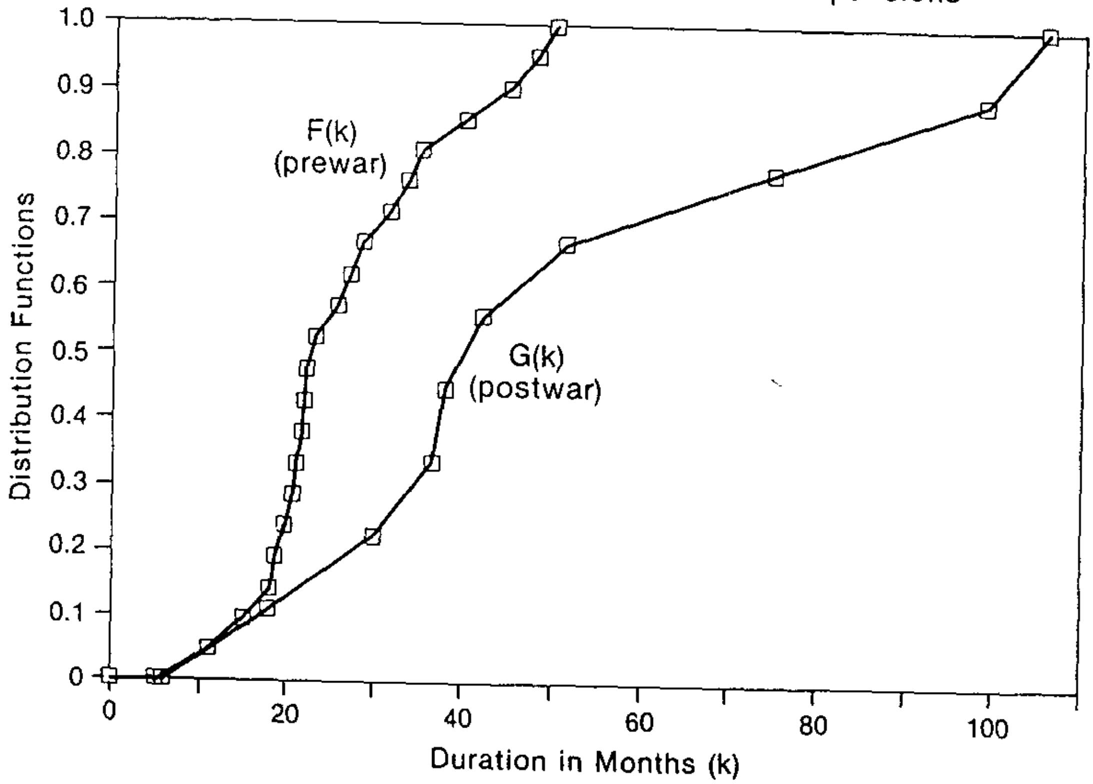

Figure 2.

Estimated Distribution Functions for Contractions

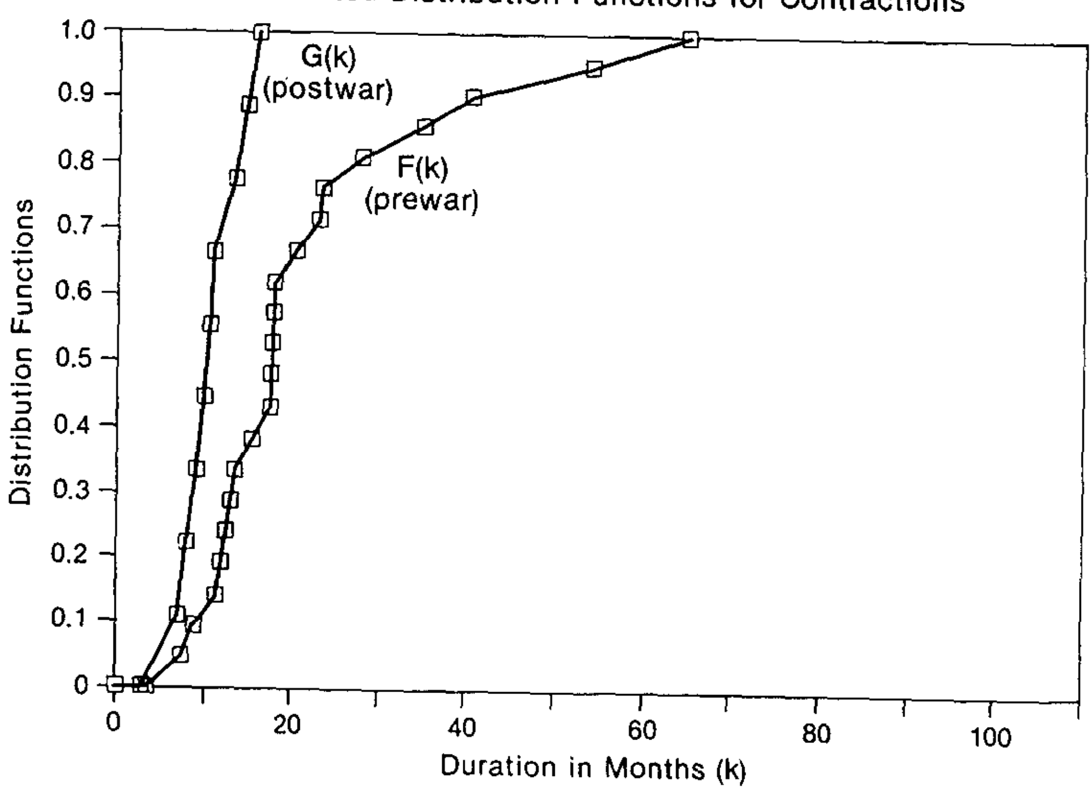

Figure 1.
Estimated Distribution Functions for Expansions

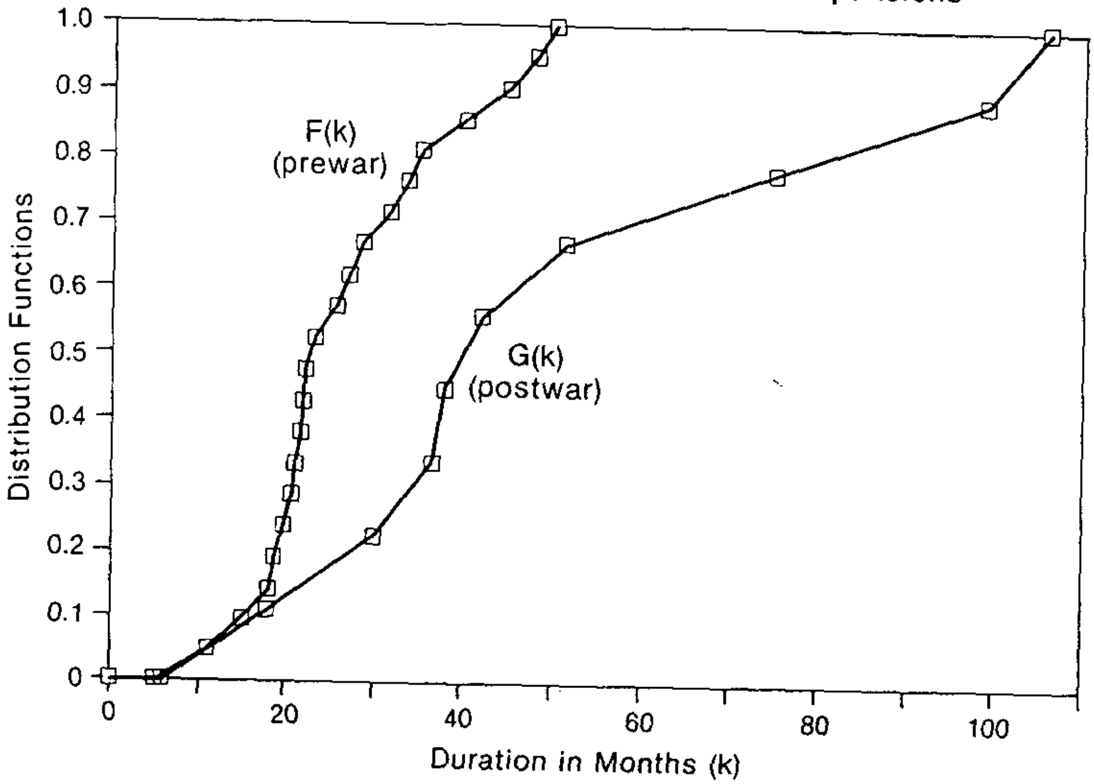

Figure 2.
Estimated Distribution Functions for Contractions