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This article investigates the forecast value of U.S. interventions in the foreign exchange market, which have become increasingly rare in the last seven years. Evidence of superior forecasting skill would imply that U.S. monetary authorities typically act with better information than the market and that intervention could alter foreign exchange traders' expectations about rates. However, the analysis presented here shows that this was not the case for recent interventions (May 1, 1990–March 19, 1997), and that official transactions by U.S. monetary authorities do not seem to improve the efficiency with which the foreign exchange market obtains information.

Inventories and the Business Cycle: An Overview 11
by Terry J. Fitzgerald

The literature on business inventory investment provides a good example of how theory and data interact in the ongoing process of research. This review of work on the relationship between inventory investment and business cycle fluctuations focuses on the developments of the last 15 years, a period characterized by renewed interest in the role that inventories play in the aggregate economy. A central issue underlying the literature is the relative importance of demand and supply shocks as sources of business cycle fluctuations—a question that continues to be debated today.

The Long-run Demand for Labor in the Banking Industry 23
by Ben Craig

Until the last decade, U.S. banks were considered nearly impervious to the employment swings that affect most other industries. Between 1989 and 1995, however, banking payrolls shrunk more than 6 percent, while U.S. employment and the overall labor force experienced a steady expansion. Equally interesting is the fact that the loss of banking jobs occurred as aggregate output in the industry rose 15 percent in real terms. This article uses call report data to examine two often-mentioned reasons for the decline in banking employment—new technology and industrywide consolidation—and finds that technical change explains the downturn only for large banks, and that acquisition accounts for very little of the overall employment change.

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Recent U.S. Intervention: Is Less More?

by Owen F. Humpage

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Introduction

In the past seven years, U.S. interventions in the foreign exchange market have become increasingly rare.[1] This paper offers an explanation for the reluctance to intervene. The apparent frequency with which recent U.S. interventions have stabilized key dollar exchange rates seems attributable primarily to the random-walk nature of movements in these rates. Official transactions by U.S. monetary authorities generally do not appear to improve the efficiency with which the foreign exchange market obtains information.

As discussed in the next section, U.S. interventions do not seem to affect fundamental determinants of exchange rates; rather, they change the way the market perceives and interprets information about those fundamentals. Sections II and III offer a definition of a successful intervention and ask if exchange rate movements consistent with this definition occur more frequently when the United States intervenes. Although the success criterion used is somewhat arbitrary, it encompasses outcomes that most economists would consider desirable. The empirical tests follow a methodology proposed by Merton (1981) and Henriksson and Merton (1981) and applied by Leahy (1995) in a study of U.S. profits from intervention. The results are given in section IV, and section V concludes with a brief discussion of some shortcomings that limit the interpretation of the results.

I. Intervention and the Channels of Influence

Economists' doubts about the effectiveness of U.S. intervention originate with the Federal Reserve's practice of preventing official exchange-market transactions from interfering with monetary policy.[2] When, for example, the United States sells German marks in an attempt to prevent a dollar depreciation, the Federal Reserve receives payment in dollars

1 Under the Gold Reserve Act of 1934, the U.S. Treasury, through its Exchange Stabilization Fund (ESF), maintains primary responsibility for the nation's interventions. The Federal Reserve intervenes both as the ESF's agent and on its own behalf, typically splitting any transactions equally between the two accounts.

2 Almekinders (1995), Dominguez and Frankel (1993), and Edison (1993) provide useful surveys of exchange-market intervention.

by debiting the reserve accounts of the appropriate commercial banks. Other things being equal, this action shrinks bank reserves, the monetary base, and ultimately the U.S. money stock. The German money stock will tend to rise. Although dollar exchange rates should respond favorably, the mechanism can interfere with the inflation objectives of monetary policy when the initial underlying cause of the dollar's depreciation is anything other than a domestic monetary impulse. Moreover, if the Federal Reserve tolerated such interference, the U.S. Treasury, which has primacy regarding intervention in this country, could influence monetary policy and violate the Fed's independence (see Humpage [1994]).

To avoid possible conflicts between exchange rate and domestic price objectives, the Federal Reserve routinely offsets the monetary-base effects of U.S. intervention through open-market transactions in Treasury securities. To continue with the example of the mark begun earlier, the Fed purchases Treasuries and credits banks' reserve accounts. (The Bundesbank tends to do likewise.) Although this eliminates the most obvious, direct influence on exchange rates—relative changes in the U.S. and German money stocks—the process alters the currency composition of publicly held government debt. After the offset, the public holds fewer dollar-denominated securities and more mark-denominated securities. According to the portfolio-balance approach to determining exchange rates, if Ricardian equivalence does not hold and if investors regard these bonds as imperfect substitutes, changes in the currency composition of outstanding debt will cause the dollar to depreciate, independent of our monetary policy stance. Unfortunately, empirical studies find virtually no evidence that intervention alters exchange rates through this channel (see Edison [1993]).

Even if intervention does not alter market fundamentals, it could still influence exchange rates by affecting either the market's perception of current fundamentals or expectations about how they might change. Foreign exchange dealers face strong incentives to acquire all possible information about current and anticipated economic developments that could influence exchange rates. If these dealers are successful, current quotations will incorporate all available information, and only new information that revises traders' expectations will affect exchange rates. To the extent that traders formulate their expectations without systematic errors, revision will be random, and exchange rate changes will approximate a random walk.

Although economists generally regard foreign exchange markets as highly efficient processors of information, markets do not always respond to news instantaneously or completely. Information is costly, and some time must elapse—whether minutes, hours, or days—between the receipt of new information and its full incorporation into exchange rates. Traders' expectations can be dissimilar or highly uncertain. Consequently, monetary authorities could sometimes possess better information than other market players and could use intervention to convey it to the market. For example, a central bank could have superior knowledge about an impending change in monetary policy. Nevertheless, the notion that it *routinely* has better information than the market—even about monetary policy—remains debatable.

II. Success Criterion

If U.S. monetary authorities can routinely affect the information flow within the foreign exchange market, then one would regularly observe an adjustment in the spot exchange rate when intervention occurs. Furthermore, if intervention can promote an exchange rate policy, one would expect these adjustments to conform to that policy's objective.

The stated aim of U.S. intervention policy is to counter disorderly market conditions, a goal that eludes a simple, precise, or even impartial definition. Sometimes, reported interpretations of this objective, such as reintroducing a sense of two-way risk, also elude a verifiable description in terms of exchange rate movements. At other times—especially over the period considered in this study—U.S. actions seem to signal support for the initiative of other central banks, rather than ardent conviction about the dollar. Nevertheless, official descriptions of efforts since May 1, 1990, suggest that U.S. monetary officials usually determine the success or failure of their interventions with reference to movements in spot dollar exchange rates.[3] Although the success criterion offered below is somewhat arbitrary, it is nevertheless consistent with the objective of countering disorderly markets and is readily verifiable. One could, of course, propose and test other criteria.

3 This statement is based on a survey of "Treasury and Federal Reserve Foreign Exchange Operations," which appeared quarterly in the *Federal Reserve Bulletin* between October 1990 and June 1997.

A General Success Criterion

Since one is never precisely certain whether the intended goal of intervention on any given day is to dampen exchange rate movements, to reverse their direction, or to encourage them along their present path, I adopt a broad success criterion—jointly expressed by (1a) and (1b) below—that subsumes all of these purposes. For U.S. sales of foreign exchange,

$$(1a) \quad ws_t = \begin{cases} 1 & \text{if } I_t > 0 \text{ and } \Delta S_t > 0 \text{ or} \\ & \Delta S_t > \Delta SAM_t, \text{ and} \\ 0 & \text{otherwise.} \end{cases}$$

For U.S. purchases of foreign exchange,

$$(1b) \quad wb_t = \begin{cases} 1 & \text{if } I_t < 0 \text{ and } \Delta S_t < 0 \text{ or} \\ & \Delta S_t < \Delta SAM_t, \text{ and} \\ 0 & \text{otherwise.} \end{cases}$$

The variables are defined as follows:

ws_t and wb_t are dichotomous success variables,

I_t is official U.S. intervention, with positive values indicating sales of foreign exchange and negative values designating purchases,

ΔS_t measures the change in the exchange rate between the morning opening of the New York market (9:00 a.m.) and the afternoon closing (4:00 p.m.), and

ΔSAM_t measures the change in the exchange rate from the morning opening on day $t-1$ to the morning opening on day t.

The respective parts of the dichotomous success criterion (ws_t and wb_t) take a value of one if U.S. intervention sales or purchases of foreign exchange are successful. An intervention sale of foreign exchange ($I_t > 0$) is successful if it is associated with a dollar appreciation ($\Delta S_t > 0$) or a smaller depreciation ($\Delta S_t > \Delta SAM_t$) when both ΔS_t and ΔSAM_t are negative. An intervention purchase of foreign exchange ($I_t < 0$) is successful if it is associated with a dollar depreciation ($\Delta S_t < 0$) or a smaller appreciation ($\Delta S_t < \Delta SAM_t$) when both ΔS_t and ΔSAM_t are positive.

In this paper, I_t refers to official data on actions against German marks or Japanese yen, the only foreign currencies that are subject to U.S. intervention. I assume that all such events occur in the New York market between its morning opening and afternoon closing (see Goodhart and Hesse [1993]). All exchange rates are bid quotes in German marks per dollar or Japanese yen per dollar.

The criterion pertains to movements in the exchange rate during the current day or compares current changes with movements over the previous 24 hours. In a highly efficient market, dealers' quotations will quickly incorporate useful information arising from intervention. In considering U.S. actions alone, the following tests assume that the market fully processes the relevant news about intervention on the day of the official transaction.

Because the United States and foreign monetary authorities closely coordinated their interventions during the sample period, I modified the success criterion slightly to lengthen the timing convention and to capture possible effects of foreign intervention. This was done by substituting ΔSPM_t for ΔS_t and ΔSPM_{t-1} for ΔSAM_t in expressions (1a) and (1b), where *SPM* is the afternoon closing exchange-rate quotation for the New York market. These substitutions measure success by comparing changes in today's closing quotation with yesterday's and by comparing movements in today's and yesterday's exchange rate. Foreign interventions, undertaken before the U.S. market opens and possibly with the acquiescence of U.S. officials, could affect the opening quotation in New York before American authorities act. Subsequent U.S. intervention may not supply any further information to the market or have any effect on the exchange rate, but one might wish to consider the overall intervention (domestic and foreign) a success.[4]

Sample Period: May 1, 1990 – March 19, 1997

I applied the success criterion described in expressions (1a) and (1b) to U.S. interventions between May 1, 1990 and March 19, 1997. During this period, the nation demonstrated a growing reluctance to intervene. Initially, this hesitation appears to have resulted from a series of dissents on Federal Open Market Committee votes related to U.S. intervention policies in late 1989 and early 1990. These dissents touched on various aspects of official policy, but generally expressed skepticism about the efficacy

4 The tests utilize only official U.S. intervention data because foreign data are unavailable.

TABLE 1

U.S. Intervention Amounts and Frequencies

	Count	Mean[a]	Standard Deviation[a]	Min[a]	Max[a]
Louvre Period: February 23, 1987–April 30, 1990					
Against German marks					
Absolute value	147	146.6	114.2	15.0	695.0
Sales of marks	110	155.1	116.6	25.0	695.0
Purchases of marks	37	121.2	104.1	15.0	395.0
Against Japanese yen					
Absolute value	147	148.6	121.8	3.0	720.2
Sales of yen	83	156.7	110.7	6.0	555.0
Purchases of yen	64	138.2	135.0	3.0	720.2
Number of observations = 805					
Sample Period: May 1, 1990–March 19, 1997					
Against German marks					
Absolute value	45	220.6	226.5	20.0	850.0
Sales of marks	39	241.2	236.8	20.0	850.0
Purchases of marks	6	86.7	21.6	21.6	100.0
Against Japanese yen					
Absolute value	21	331.6	215.5	30.0	800.0
Sales of yen	17	396.1	186.2	165.0	800.0
Purchases of yen	4	57.5	29.9	30.0	100.0
Number of observations = 1,733					

a. In millions of dollars.
SOURCE: Author's calculations.

of intervention and concern about adverse spillovers onto monetary policy (see Humpage [1994]). At this writing, the United States has not intervened in the foreign exchange market since August 15, 1995—the longest period of abstinence since the dollar began to float.[5]

Between May 1, 1990 and March 19, 1997, the United States intervened on 45 occasions against the mark and on 21 occasions against the yen (see table 1). The vast majority of these events involved official sales of marks or yen. The number of actions during this period was far smaller than in the previous one, which had been influenced by the Louvre Accord of February 1987. In addition, instances of intervention in the sample period usually did not persist as they did in the 1987–90 period immediately following the Accord (see figure 1). Often, they lasted no more than a single day.

Although the frequency was lower, the average amount of intervention *sales* of marks or yen was substantially greater during the sample period than in 1987–90. The average amount of intervention *purchases* of foreign exchange was smaller during the sample period, but the United States undertook very few of these.

I also break the sample period into subperiods: May 1 to July 31, 1990 and August 1, 1990 to March 19, 1997. During the first subperiod, the only U.S. intervention involved selling marks on 17 occasions. The Federal Reserve undertook these sales as agent for the Treasury's ESF. The operations were intended to adjust ESF balances and to facilitate a reversal of outstanding warehousing operations with the Federal Reserve System. A warehousing operation is a swap transaction between the ESF and the System, whereby the System acquires foreign exchange (German marks in this case) and the ESF receives U.S. dollars. The warehousing operation unwinds at a set future date (see Humpage [1994]). Although these transactions were not designed to affect the mark–dollar exchange rate, they remain interesting because even interventions without any intended effect on exchange rates should frequently appear successful when rates follow a random walk.[6]

Success and the Random Walk

In a highly efficient market, exchange rate changes will approximate a random walk (see Baillie and McMahon [1989]). Consequently, even completely ineffectual interventions frequently seem successful.

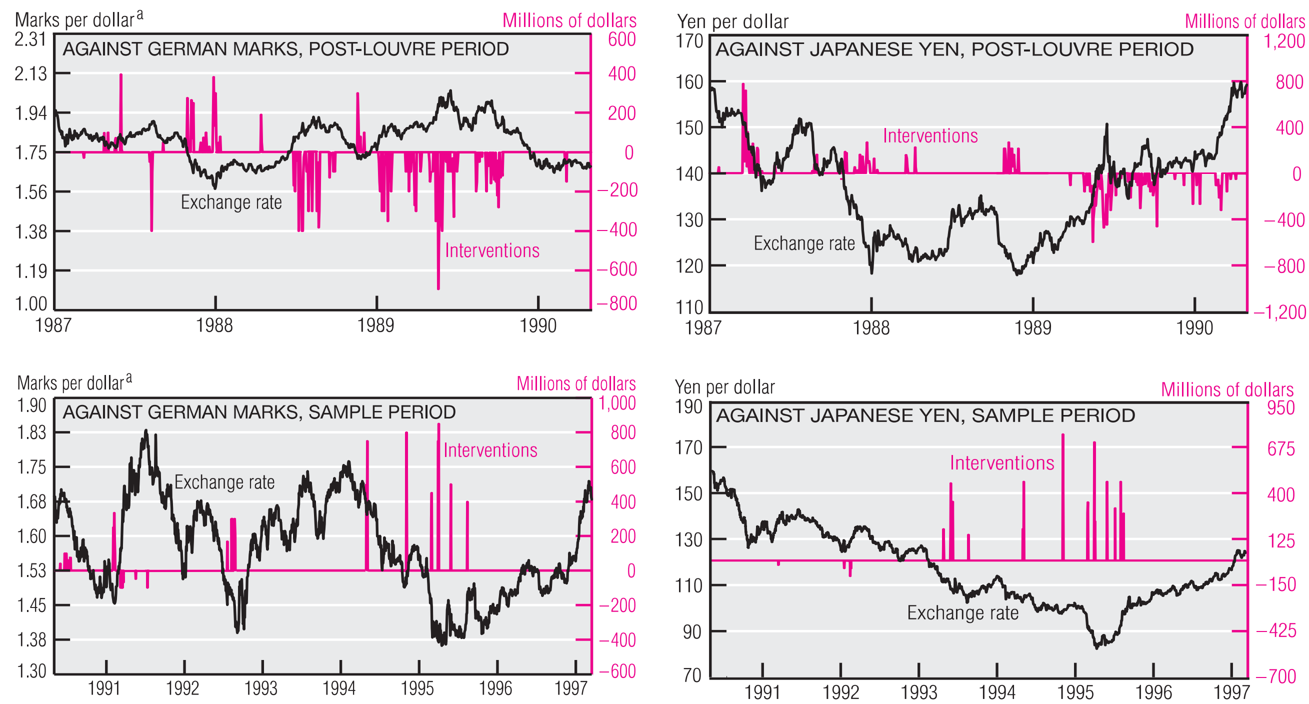
Figure 2 illustrates this point. Imagine that at the beginning of day $t-1$, 1.85 German marks trade for one dollar, but that over the day, the dollar depreciates 5 percent against the mark. At the start of day t, therefore, 1.76 marks trade for one dollar. Under the random-walk hypothesis, the best guess for the mark–dollar exchange rate on day $t+1$ is 1.76, but an appreciation or a depreciation away from 1.76 is equally probable. Consequently, the chance of observing a dollar appreciation (depreciation) following the sale (purchase) of foreign exchange—even when the effort is completely ineffectual—will approach 50 percent. (One must also allow for the chance of no change.) Indeed, during the sample period, the dollar depreciated against the mark 48 percent of the time, appreciated against the mark 48 percent of

5 I base this statement on official published summaries of "Treasury and Federal Reserve Foreign Exchange Operations" and news accounts of currency markets. Official data used in this paper terminate in December 1995.

6 The "Foreign Exchange" column of the *Wall Street Journal* made no mention of these interventions on the days they took place.

FIGURE 1

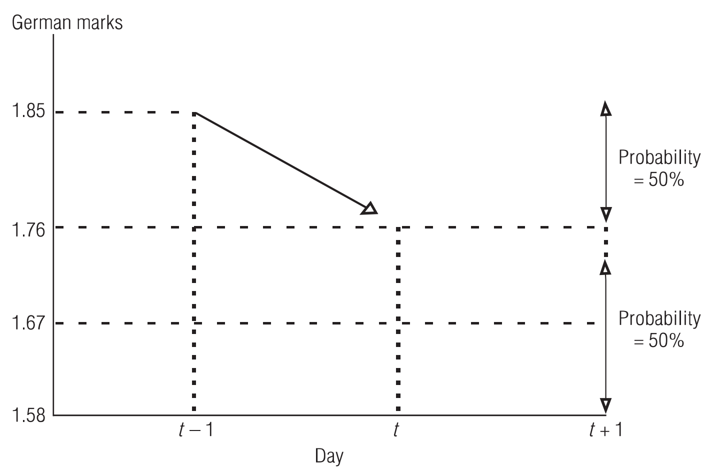
U.S. Interventions



a. Rounding to two decimal places causes the appearance of variation among intervals.
SOURCES: Board of Governors of the Federal Reserve System; and U.S. Department of Commerce, Bureau of Economic Analysis.

FIGURE 2

Random Walk and the Probability of Success



SOURCE: Author's calculations.

the time, and otherwise remained unchanged. The results are similar for the yen–dollar exchange rate, and dropping observations that include intervention does not substantially alter the proportions.

If the 5 percent depreciation of the dollar on day $t-1$ continued throughout day t, the exchange rate would be 1.67 at the start of day $t+1$. As figure 2 demonstrates, the probability of seeing an appreciation (or a smaller depreciation) on day $t+1$ must be greater than 50 percent. Hence, we expect that the probability of observing a success according to the general criterion—expressions (1a) and (1b)—will exceed 50 percent. The frequency of observing exchange rate movements consistent with these definitions is approximately 63 percent for the entire sample (that is, with or without interventions). This probability does not change when one drops intervention days from the sample (see Humpage [1996]).

III. The Probability of Success

If exchange rate changes followed a random walk without any drift, one could view each change as an independent event and analyze the frequency of success using standard statistical distributions (see Humpage [1996]). Exchange rate changes, however, are generally not strict random walks. Even when they exhibit such behavior over an entire, lengthy sample, they may deviate from a random walk around times when intervention occurs.[7]

Merton (1981) and Henriksson and Merton (1981) develop a nonparametric test to evaluate investment managers' ability to predict the relative performance of stocks and bonds, which have statistical properties similar to those of exchange rates. To apply the test, I treat expression (1) as an official forecast of near-term exchange rate movements that U.S. monetary authorities reveal by intervening. When the Federal Reserve sells foreign exchange, for example, it forecasts a near-term appreciation of the dollar or a smaller depreciation than recently observed. A purchase of foreign exchange has a corresponding interpretation. Evidence of exceptional forecasting skills would suggest that U.S. monetary authorities act with better information than the market and successfully convey that information to it.

The chief advantage of this procedure is that it does not require specific assumptions about either the distribution of exchange rate changes or the probabilities of individual events. A disadvantage is that it investigates only the number of times intervention is successful, not the magnitude of any effect.

To illustrate the test, consider U.S. intervention sales of foreign exchange. Following Merton, I define the conditional probabilities as:

(2a) $$p1 = \text{prob}\,[I > 0 \mid \Delta S > 0 \text{ or } \Delta S > \Delta SAM], \text{ and}$$

(2b) $$1 - p1 = \text{prob}\,[I \le 0 \mid \Delta S > 0 \text{ or } \Delta S > \Delta SAM].$$

(2c) $$p2 = \text{prob}\,[I \le 0 \mid \Delta S \le 0 \text{ or } \Delta S \le \Delta SAM], \text{ and}$$

(2d) $$1 - p2 = \text{prob}\,[I > 0 \mid \Delta S \le 0 \text{ or } \Delta S \le \Delta SAM].$$

Expression (2a) is the probability that the exchange rate behaves in a manner consistent with the criterion for success—expression (1a)—and the United States intervenes. Expression (2c) is the probability that the exchange rate does *not* conform with the success criterion and the United States does *not* sell foreign exchange. The conditional probabilities defined in (2b) and (2d) are for events complementary to those considered in (2a) and (2c).

U.S. intervention sales would have no value as a forecast of the success criterion if the probability of observing an official sale of foreign exchange given a dollar appreciation or smaller depreciation ($p1$) was no greater than the probability of observing an official sale of foreign exchange given exchange rate behavior inconsistent with the success criterion ($1 - p2$). In a test of the forecast value of intervention, the null hypothesis—that U.S. intervention has no predictive value—becomes

(3) $$H_0\colon p1 = 1 - p2 \Rightarrow p1 + p2 = 1.$$

In this case, traders would not modify their prior estimates of the distribution of exchange rate movements as a result of intervention.[8] Intervention has positive forecast value if $p1 + p2 > 1$. If, for example, intervention conveyed perfect information to the market, then $p1 = 1$, $p2 = 1$, and $p1 + p2 = 2$. Similarly, intervention would have negative forecast value if $p1 + p2 < 1$.[9]

I obtain estimates of conditional probabilities from the sample data (see table 2). In the case of U.S. sales of German marks, for example, $n1$ is the number of successful mark sales (23); $n2$ represents unsuccessful mark sales (16 = 39 – 23); $N1$ is the number of *virtual successes*, that is, the number of days on which the dollar–mark exchange rate appreciates or dampens a depreciation (1,101); and $N2$ is the remaining number of observations (632 = 1,733 – 1,101). It follows that $E(n1/N1) = p1$ and $E(n2/N2) = 1 - p2$. Hence, $\hat{p}1 + \hat{p}2 = 0.996$.

Henriksson and Merton (1981) show that under the null hypothesis ($p1 + p2 = 1$), the number of correct interventions will have a hypergeometric distribution. This provides a direct test of the null hypothesis that depends neither on the underlying exchange-rate process nor on an underlying guess of the probability of an

7 The author thanks an anonymous referee for comments about the random-walk hypothesis.

8 Merton (1981; proposition III, p. 384) shows this to be a necessary and sufficient condition for the forecast to have no value.

9 Ironically, an intervention that is consistently wrong also conveys useful information to the market. The market can profit by betting against the intervention: Buy when the Federal Reserve sells foreign exchange.

TABLE 2

Analysis of U.S. Interventions: May 1, 1990–March 19, 1997

	Count	Intervention Successes	Percentage	Virtual Successes	Percentage	$p1$	$p2$	$p1 + p2$	$1 - CDF$
Against German marks									
Opening to closing									
Purchases	6	4	66.7	1,147	66.2	0.003	0.997	1.000	0.341
Sales	39	23	59.0	1,101	63.5	0.021	0.975	0.996	0.670
Closing to closing									
Purchases	6	3	50.0	1,058	61.1	0.003	0.996	0.998	0.566
Sales	39	24	61.5	1,048	60.5	0.023	0.978	1.001	0.385
Number of observations = 1,733									
Against Japanese yen									
Opening to closing									
Purchases	4	3	75.0	1,131	65.3	0.003	0.998	1.001	0.181
Sales	17	12	70.6	1,143	66.0	0.010	0.992	1.002	0.260
Closing to closing									
Purchases	4	2	50.0	1,048	60.5	0.002	0.997	0.999	0.483
Sales	17	13	76.5	1,066	61.5	0.012	0.994	1.006	0.059
Number of observations = 1,733									

SOURCE: Author's calculations.

individual success (see Humpage [1996]). Assuming that $n1$ is a hypergeometric random variable, I reject the null hypothesis that $p1 + p2 = 1$ in favor of $p1 + p2 > 1$, if the probability of observing an equal or greater number of successes—that is, one minus the cumulative density function ($1 - CDF$)—is very small. I reject the null hypothesis in favor of $p1 + p2 < 1$ if the probability of observing an equal or greater number of successes ($1 - CDF$) is very large.

IV. Empirical Results

Table 2 reports the results of the experiment for the entire period (May 1, 1990–March 19, 1997), and table 3 breaks out two subperiods. As the first column of each table indicates, I test both purchases and sales of German marks and Japanese yen against the success criterion defined with opening-to-closing and with closing-to-closing changes in the exchange rate. As noted above, the longer time frame accommodates cooperation between U.S. and foreign monetary authorities, which occurred frequently over the sample period. Columns 2 and 3 indicate the number of interventions and successful interventions, respectively, for each category listed in column 1. Approximately 64 percent of the interventions succeeded according to criteria (1a) and (1b). In column 4, this statistic ranged from a low of 50 percent to a high of 76.5 percent. Over the sample period, U.S. interventions generally seem more successful against yen than against marks. Column 5 counts the virtual successes, that is, the number of days over which exchange rate movements conformed with the general success criteria, irrespective of U.S. intervention. When I measure exchange rate changes from opening to closing, the frequency of a virtual success is approximately 65 percent. When I measure exchange rate changes from closing to closing, the frequency is somewhat lower (approximately 61 percent). In general, therefore, the frequency of a successful intervention is not substantially different from the frequency of a virtual success. Random interventions would seem to have done as well.

Estimates of the relevant conditional probabilities follow in columns 7 and 8. It is unsettling that the value of $p1$ is very low in cases where the number of interventions is small, but nearly all are successful, as in the case of U.S. purchases of Japanese yen. Nevertheless, if all interventions were successful, $p2$ alone would equal one, and the statistical test would always reject the null hypothesis.

TABLE 3

Analysis of U.S. Intervention: Two Subperiod Samples

	Count	Intervention Successes	Percentage	Virtual Successes	Percentage	$p1$	$p2$	$p1 + p2$	$1 - CDF$
		May 1, 1990–July 31, 1990							
Against German marks									
Opening to closing									
Purchases	0	0	—	40	62.5	—	—	—	—
Sales	17	11	64.7	39	60.9	0.282	0.996	1.279	0.256
Closing to closing									
Purchases	0	0	—	40	62.5	—	—	—	—
Sales	17	10	58.8	38	59.4	0.263	0.996	1.259	0.411
Number of observations = 64									
		August 1, 1990–March 19, 1997							
Against German marks[a]									
Opening to closing									
Purchases	6	4	66.7	1,107	66.3	0.004	0.997	1.000	0.344
Sales	22	12	54.5	1,062	63.6	0.011	0.985	0.996	0.751
Closing to closing									
Purchases	6	3	50.0	1,018	61.0	0.003	0.996	0.999	0.565
Sales	22	14	63.6	1,010	60.5	0.014	0.989	1.003	0.306
Number of observations = 1,669									
Against Japanese yen									
Opening to closing									
Purchases	4	3	75.0	1,090	65.3	0.003	0.998	1.001	0.182
Sales	17	12	70.6	1,104	66.1	0.011	0.992	1.003	0.265
Closing to closing									
Purchases	4	2	50.0	1,008	60.4	0.002	0.997	0.999	0.482
Sales	17	13	76.5	1,027	61.5	0.013	0.994	1.007	0.059
Number of observations = 1,669									

a. Excludes interventions associated with warehousing.
SOURCE: Author's calculations.

Column 9 records the test statistic for the null hypothesis of no forecast value, which I assume to have a hypergeometric distribution. As the final column indicates, I can reject the null hypothesis in only one case—that of U.S. sales of Japanese yen when exchange rates are measured from closing to closing. Here, one can reject the null with 94 percent confidence in favor of the positive forecast value. The inability to reject the null hypothesis for U.S. sales of Japanese yen when the tests include opening-to-closing exchange rate movements suggests that foreign, not U.S., intervention may have provided the forecast value, but I did not test this proposition directly.

The findings do not change when I remove the 17 sales of German marks that were undertaken to adjust the ESF's portfolio and unwind its warehousing operation. The results shown in table 3 parallel those in table 2, implying that, with the exception already noted, recent U.S. intervention did not systematically affect the mark–dollar or yen–dollar exchange rates.

V. Conclusion

This paper investigates the forecast value of U.S. intervention policy, using a methodology that Merton (1981) and Henriksson and Merton (1981) proposed and that Leahy (1995) applied to an analysis of intervention profits. Evidence of superior forecasting skill would imply that U.S. monetary authorities typically act with better information than the market and that intervention could alter foreign exchange traders' expectations about rates. My analysis, however, indicates that for recent U.S. interventions (May 1, 1990–March 19, 1997), this was not the case. The random-walk nature of exchange rate movements—rather than superior information—seems capable of explaining the frequency of success.

This paper has some shortcomings that limit its interpretation. For one thing, although broad and readily verifiable, the success criterion used is necessarily arbitrary. Under some alternative criteria, intervention could appear successful and have positive forecast value. In addition, the time frame for analysis is short. A narrow period—opening to closing or closing to closing—is consistent with the notion that exchange markets are highly efficient processors of information. A broader time frame, however, might produce different results. A third shortcoming is my treatment of success as a dichotomous variable. I do not consider the possibility that the magnitude of exchange rate movements during the limited instances of successful interventions may be substantially different than at other times. Moreover, this study does not condition the probability of success on whether the United States coordinated its interventions or on the size of its transactions. Humpage (1996) found that coordination—and, to a lesser extent, large dollar amounts—increased the probability of an intervention's success. Despite these shortcomings, the results offer a plausible reason for not expecting more from less intervention.

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Inventories and the Business Cycle: An Overview

by Terry J. Fitzgerald

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Introduction

Investment in business inventories has averaged roughly one-half of 1 percent of real GDP in the United States over the post–World War II period. Given its relatively minor role as a component of output, it might seem curious that inventory investment has traditionally drawn a great deal of interest from macroeconomists and policymakers. One reason is that although the level of inventory investment is quite small relative to GDP, fluctuations in inventory investment are not so small relative to the fluctuations in GDP. For example, changes in inventory investment are, on average, more than one-third the size of quarterly changes in real GDP over the postwar period.[1]

Perhaps more strikingly, table 1 shows the peak-to-trough decline in GDP and the associated decline in inventory investment during postwar recessions. The fall in inventory investment for most of these periods is generally substantial relative to the fall in real GDP, and sometimes even exceeds it. Using similar data, Blinder and Maccini (1991a, p. 291) report that "the drop in inventory investment has accounted for 87 percent of the drop in GNP during the average postwar recession in the U.S."

Movements in inventory levels over the business cycle are also closely associated with movements in output during the postwar period, with output leading inventories slightly (see figure 1).[2] Furthermore, changes in inventory holdings are, on average, roughly 60 percent the size of quarterly changes in output.[3]

Such observations about the behavior of inventories over the business cycle, long familiar to economists, have led some to speculate that understanding the reason for inventory fluctuations may provide the key to understanding the

1 Following Christiano (1988), I define the volatility of a variable, say x, as the time average of absolute changes in x, expressed as a percentage of gross output, $v_x \equiv 100 * \frac{1}{T} \sum_{t=1}^{T} \frac{|\Delta x_t|}{y_t}$. From 1947:IQ through 1997:IQ, the ratio of v_{di} to v_y (using the time series for real inventory investment and real GDP) is 0.36.

2 The correlation between the cyclical component of inventories and the cyclical component of output is 0.54 and peaks at 0.83 when output is lagged by two quarters. This lagged response of inventory levels is consistent with the fact that cyclical inventory investment is most highly correlated with contemporaneous cyclical output.

3 Using the measure discussed in footnote 1, the ratio of the volatility of inventory levels to output from 1947:IQ through 1997:IQ is 0.605.

TABLE 1

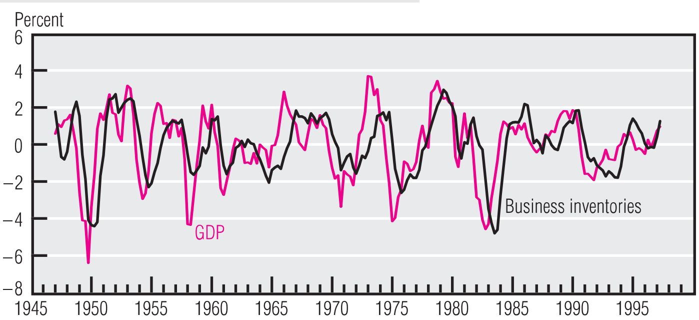
Inventory Investment and Postwar Recessions

GDP Peak to Trough	Change in GDP	Change in Inventory Investment
1948:IVQ–1949:IVQ	–24.4	–33.3
1953:IIQ–1954:IIQ	–48.8	–20.0
1957:IIIQ–1958:IQ	–81.4	–18.4
1960:IQ–1960:IVQ	–40.7	–47.9
1969:IIIQ–1970:IVQ	–20.3	–38.4
1973:IVQ–1975:IQ	–146.2	–77.0
1980:IQ–1980:IIIQ	–116.7	–52.7
1981:IIIQ–1982:IIIQ	–140.9	–43.4
1990:IIQ–1991:IQ	–124.1	–60.7

NOTE: Dates correspond to the largest peak-to-trough decline in GDP associated with each postwar recession. Each date is within one quarter of the quarter containing the peak or trough month as defined by the National Bureau of Economic Research. Data are in billions of chained 1992 dollars.
SOURCE: U.S. Department of Commerce, Bureau of Economic Analysis.

FIGURE 1

Deviations from Trend of Real GDP and Inventory Holdings



NOTE: Quarterly data have been logged and detrended using the Hodrick–Prescott filter (λ =1,600). Raw data are in billions of chained 1992 dollars.
SOURCE: U.S. Department of Commerce, Bureau of Economic Analysis.

business cycle itself.[4] For example, Blinder (1990, p. viii) states that “business cycles are, to a surprisingly large degree, inventory cycles.”

The present article reviews the literature on the relationship between inventory investment and business cycle fluctuations, focusing on developments over the past 15 years. This literature provides a good example of how theory and data interact in the ongoing process of research, and the discussion will be organized around this interaction.

Beginning in the early 1980s, economists began to point out that the standard theoretical model of inventory behavior, the production smoothing model, was not consistent with key features of U.S. data regarding production, inventories, and sales. This inconsistency led to a sizable body of research showing how to modify the standard model to make it accord better with the empirical observations. At the same time, other researchers were developing alternative models of inventory behavior that could also be consistent with the data.

This literature has been largely motivated by two overriding questions. First, does inventory investment play a key role in amplifying and propagating exogenous shocks to the economy? More than 50 years ago, Metzler (1941) provided a model demonstrating that exogenous, uncorrelated shocks, combined with a certain structure of inventory investment, could produce serially correlated movements in GDP that resemble the business cycle. Much of the subsequent work has been motivated by the desire to know whether the process of adjusting inventory holdings in response to exogenous shocks may help explain the magnitude and persistence of changes in real output growth over the cycle.

The second overriding question is, does inventory behavior illuminate the underlying source of the shocks that give rise to business cycle fluctuations? That is, does the statistical relationship between inventory investment and other economic variables provide information on the nature of the shocks that lead to aggregate fluctuations? The answers to these two questions are particularly important to policymakers because they are likely to provide information on the nature of optimal policies, both fiscal and monetary.

I begin this article by briefly presenting what was considered, at least through the early 1980s, the standard model of inventory behavior—the production smoothing model. Next, I discuss some of this model's empirical predictions and review some facts about inventories that are at odds with the simplest version of the model. I then provide an overview of how economists have responded to the discrepancy between theory and data and examine how the interaction between theory development and data has continued to evolve.

4 Abramovitz (1950) provides early documentation on the importance of inventories in aggregate fluctuations during the interwar period.

This review is intended to introduce readers who are unfamiliar with the literature on inventory behavior and cyclical fluctuations to its central issues and developments. Accordingly, the discussion provides a general background, without the more technical details that underlie the research. Readers interested in these details should consult the references given throughout this article.

I. Theory: A Production Smoothing Model

The production smoothing model has provided the microeconomic foundation for most research on the behavior of inventories over the business cycle. The key assumptions of this model are straightforward: Firms face variable demand for their goods, the cost of production is convex, and goods are storable. Loosely speaking, these assumptions imply that a profit-maximizing firm will have an incentive to use inventories to smooth production through time in the face of fluctuating sales.

In examining how the literature on inventories has evolved, it will be useful to have a simple version of the production smoothing model in hand.[5] Consider an individual firm that produces a single storable good. Let the total sales and the price of its good at each date t be given by S_t and p_t, respectively, where these variables may vary through time. The model is silent as to how sales and prices are determined.[6]

The firm faces the following current-period cost function:

$$(1) \qquad C_t = \gamma_1 Y_t + \gamma_2 Y_t^{\,2} + \gamma_3 I_t^{\,2},$$

where $\gamma_1, \gamma_2 > 0$, $\gamma_3 \geq 0$, Y_t is production during period t, and I_t is the stock of inventories at the end of period t. The first two terms reflect the current costs of production, and the assumption that γ_2 is strictly positive implies that marginal costs are increasing in output. The last term represents the cost of holding inventories (such as handling and storage costs), which is assumed to be an increasing function of inventory holdings.

The link between inventory accumulation, production, and sales is given by

$$(2) \qquad I_t - I_{t-1} = Y_t - S_t,$$

with inventory holdings subject to the nonnegativity constraint

$$(3) \qquad I_t \geq 0.$$

Inventory investment is equal to current output minus current sales. Current sales can be met through current output and previously accumulated inventory holdings.

In this environment, a firm's decision problem is to organize its production schedule through time, given the processes for sales and prices, by choosing output and inventory holdings so as to maximize the expected discounted value of its profits

$$(4) \qquad E_0 \sum_{t=0}^{\infty} \beta^t (p_t S_t - C_t),$$

subject to constraints (1), (2), and (3), where E_0 denotes the expectation conditional on information known at time 0. The parameter β is a discount factor implied by a constant real rate of interest, where $\beta = 1/(1 + r)$, and is between 0 and 1.

Since prices and sales are determined outside the model, this problem can be written more succinctly as the firm choosing production and inventories in a way that minimizes the expected discounted present value of costs

$$(5) \qquad E_0 \sum_{t=0}^{\infty} \beta^t C_t,$$

subject to constraints (1), (2), and (3).

It follows immediately from this setup that the firm will have an incentive to smooth the flow of production through time by holding inventories in order to minimize cost. To state it differently, the variance of output will be lower if firms can accumulate inventories than if they cannot, assuming inventories are sometimes held in the solution to the problem. Given that sales vary through time, inventories will be held by the firm as long as the cost of holding them is not too large, the discount factor β is not too small, and the cost of production is sufficiently convex.

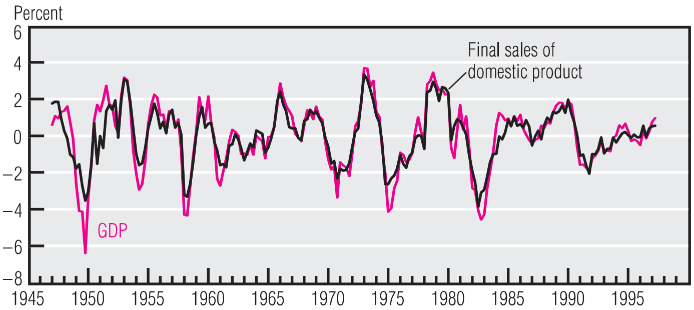
For example, suppose that sales alternated predictably between 1,000 and 2,000 units each period. If the cost of production is linear in output ($\gamma_2 = 0$), then the firm would have no

5 This model is a simple version of the linear quadratic model of optimal inventory behavior introduced by Holt et al. (1960).

6 The firm's decision problem can be thought of as a subproblem in a more general model where a profit-maximizing monopolistic firm also chooses production levels and prices.

FIGURE 2

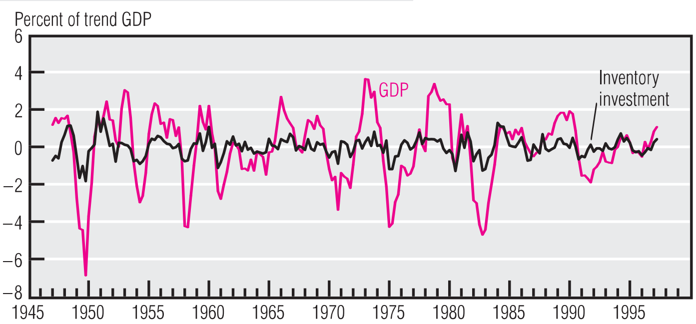
Deviations from Trend of Real GDP and Final Sales of Domestic Product



NOTE: Quarterly data have been logged and detrended using the Hodrick–Prescott filter (λ = 1,600). Raw data are in billions of chained 1992 dollars.
SOURCE: U.S. Department of Commerce, Bureau of Economic Analysis.

FIGURE 3

Deviations from Trend of Real GDP and Inventory Investment



NOTE: Because inventory investment is sometimes negative, the levels of the quarterly data have been detrended using the Hodrick–Prescott filter (λ = 1,600), and both series are expressed as a percentage of the trend in GDP. Raw data are in billions of chained 1992 dollars.
SOURCE: U.S. Department of Commerce, Bureau of Economic Analysis.

incentive to accumulate inventories, since the marginal cost of production would be the same in all periods. In that case, the firm would simply match output with sales, period by period. If, on the other hand, the cost of production is convex, firms will have an incentive to produce a surplus when sales are low, and to use this surplus to reduce output when sales are high. Consider the case where $\gamma_3 = 0$ and $\beta = 1$.[7] Then, inventories are costless to hold, and the firm minimizes costs by producing 1,500 units each period. This is the basic intuition of production smoothing.

In addition, if sales are stochastic, inventories may also play what is commonly called a buffer-stock role in production. The intuition here is that firms will respond to unexpected increases in sales by reducing inventory holdings and increasing production, with production increasing less than sales. If the firm must make its production decision before observing the sales shock, then the increase in sales will come entirely out of inventories.

II. Inconsistencies between Theory and Data

Two empirical predictions of the production smoothing model follow directly from the discussion above. The first is that the variance of sales exceeds the variance of production. The second is that inventory investment and output move in opposite directions. It is natural to ask (as economists began doing in the early 1980s) how well these predictions accord with the data.

Let's begin our exploration of the facts by looking at aggregate data on inventories, output, and sales. Figure 2 shows postwar data on the cyclical components of real GDP and real final sales of domestic product, defined as GDP minus inventory investment. This figure shows that at the aggregate level, output is *more* variable than sales—just the opposite of what the production smoothing model predicts. The standard deviation of cyclical real GDP over the postwar period is 1.81 percent, compared to 1.44 percent for final sales.

In addition, figure 3 shows that output and inventory investment tend to move in the *same* direction over the business cycle, rather than in opposite directions. In fact, the correlation between cyclical inventory investment and cyclical output is strongly positive (0.57).

The empirical findings that output is more variable than sales and that output and inventory investment are positively correlated have also been found to hold when less aggregated data are used. Papers by Blanchard (1983), Blinder (1981, 1986), Blinder and Maccini (l991a, b), and West (1986) reported that these findings held when industry-level data were used. These results were judged to cast a large shadow over the production smoothing model,

■ 7 Strictly speaking, I am referring to the properties of the solution as β approaches 1.

a view expressed in the title of Blinder's 1986 paper, "Can the Production Smoothing Model of Inventory Behavior Be Saved?"

These empirical findings led to a series of papers seeking to modify the existing theory or to develop other theories that could explain them. The next section will summarize this research. Before proceeding, though, I note several challenges to the finding that production is typically more variable than sales. A number of papers, including Lai (1991), Miron and Zeldes (1989), Fair (1989), Krane and Braun (1991), and Krane (1994), present evidence that this finding, at least for some industries, may result from measurement problems with the data or from aggregation biases. While this research suggests that the empirical findings may not be as striking or as prevalent as earlier work reports, it does not entirely resolve the issue, and I will proceed under the assumption that a basic inconsistency remains between the theory and the data for at least some industries.

III. Theory Responses

I have noted that the discrepancy between the predictions of the standard production smoothing model and the properties of the data led to a new burst of research aimed at reducing the discrepancy. This section provides an overview of several approaches that have been taken, some of which can be viewed as modifications of the production smoothing model. After outlining these strategies, I will briefly discuss some other approaches.

Modifications of the Production Smoothing Model

Modifications of the model in response to the empirical findings can be broadly classified into three groups: adding cost shocks, adding a target inventory level, and adding nonconvexities in technology.

Adding Cost Shocks

One approach to resolving the discrepancy between the theory and the facts (arguably the most obvious one) is to add shocks to the firm's production costs. Cost shocks can be introduced by replacing equation (1) in the production smoothing model with

$$(6)\qquad C_t = (\gamma_1 + \tau_t)Y_t + \gamma_2 Y_t^{\,2} + \gamma_3 I_t^{\,2},$$

where τ_t is a shock that varies through time.

Adding cost shocks to the model provides, at least theoretically, a straightforward explanation of the facts. This can be seen most readily in a version of the model with constant sales. In that case, production varies as costs change, with production being high when costs are low (and vice versa), and inventory investment covering the gap between sales and output. Clearly, output will be more variable than sales in this example, and inventory investment will be procyclical. Furthermore, this suggests that a model with both sales and cost shocks may also be consistent with the facts.[8]

Early research that added cost shocks to the production smoothing model included Blanchard (1983), Eichenbaum (1984, 1989), Maccini and Rossana (1984), Blinder (1986), and Christiano and Eichenbaum (1989). Empirical results from these papers were mixed, but generally indicated that cost shocks play an important, if limited, role in explaining inventory behavior. All of these papers invoke unobserved cost shocks to make their point, that is, they do not use cost shocks that are directly measured from data. More recently, West (1990) found that unobserved cost shocks appear to be a dominant source of fluctuations in aggregate inventory holdings, and Kollintzas (1995) reported further evidence that such shocks are an important factor for understanding inventory behavior.

In a separate branch of the literature that developed during the same period, Kydland and Prescott (1982) found that the cyclical fluctuations in aggregate data were surprisingly consistent with a general equilibrium model driven exclusively by unobserved productivity shocks. They introduced inventories as a factor of production and found that cyclical fluctuations in the inventory stock and the correlation of cyclical movement in inventories and output in their model were roughly consistent with the data. Christiano (1988) demonstrated that, by modifying the Kydland–Prescott framework so that inventories buffer unexpected shocks to preferences and technology, the volatility of inventories and the correlation of inventory investment with output could be largely explained. From the viewpoint of this theory, the apparent inconsistency between theory and data discussed in the previous section is not an inconsistency

8 Blinder (1986) argues that highly serially correlated sales shocks combined with relatively small cost shocks can lead the variance of production to exceed the variance of sales.

at all. The patterns in the data are what this theory would predict. Furthermore, the explanation of inventory investment as a residual component contrasts sharply with the prominent role that some economists envisioned for inventories as a central means of propagating exogenous shocks.

While it had become clear that introducing cost shocks could successfully resolve at least some of the discrepancies between theory and data, many economists were troubled by the unobserved nature of the cost shocks. Researchers asked whether the unobserved shocks that were being invoked corresponded to actual, measurable movements in observed price and cost data. Initially, work that attempted to locate the cost shocks in the data was unsuccessful. Miron and Zeldes (1988) found little evidence that observed cost shocks in the form of raw material, energy, and wage prices helped to save the production smoothing model. More recently, Durlauf and Maccini (1995) reported evidence that observed cost shocks in the form of material and energy prices and wage rates do contribute significantly to explaining inventory movement at the industry level. This issue continues to be a subject of active research, but a consensus finding has yet to emerge.

Adding a Target Inventory Level

While adding cost shocks to the production smoothing model provides one possible explanation of the data, other researchers have found that they can explain the facts using only sales (or demand) shocks. A second modification that is capable, at least in theory, of reconciling the model with the data is to assume that firms have a strictly positive inventory-to-sales ratio from which it is costly to deviate, and that shocks to sales are persistent.[9] The assumption of a target inventory level is incorporated into the model by replacing equation (1) with

$$(7) \qquad C_t = \gamma_1 Y_t + \gamma_2 Y_t^{\,2} + \gamma_3 (I_t - \alpha S_t)^2,$$

where $\alpha > 0$. Thus, inventory costs are minimized by setting inventories at a fixed fraction of sales. This assumption is motivated by the observation that the cost of carrying inventories, which increases with inventory holdings, must be balanced against the cost of stocking out or backlogging orders, which falls with inventory holdings.

That the assumption of a target inventory level and persistent sales shocks can make the variance of output exceed the variance of sales was shown by Blanchard (1983) and West (1986), among others. The intuition for this result is as follows: Suppose an unexpected increase in sales occurs in period t. Further assume that the firm's production decision is made before the current-period shock is realized. The firm will respond this period by lowering its inventory holdings by the amount of the shock. In the next period, the firm will increase production not only to meet the expected higher level of sales, but also because its target level of inventory holdings has increased along with sales. This creates a so-called accelerator effect, leading production to increase by more than the unexpected increase in sales. Furthermore, it suggests an avenue by which output and inventory investment may be positively correlated.

Kahn (1987) provides a theoretical basis for a target inventory level by explicitly modeling a stockout avoidance motive for inventory accumulation. Maccini and Zabel (1996) extend to a more general environment Kahn's finding that production is more volatile than sales in a stockout avoidance model. Bils and Kahn (1996) have recently put forth a model in which sales are simply assumed to be an increasing function of inventory holdings.

Empirical results in West (1986), Eichenbaum (1989), and Miron and Zeldes (1988) are unfavorable to early specifications of target inventory models. More recently, Kahn (1992) reports that a stockout avoidance motive in the face of fluctuating demand largely suffices to explain inventory behavior in the automobile industry, while Durlauf and Maccini (1995) find that the stockout avoidance motive helps explain inventory behavior, but does not provide a complete solution. This issue is the subject of continuing research.

Adding Nonconvexities in Technology

A third approach to modifying the production smoothing model in order to reconcile it with the data is to assume that the marginal cost of production is decreasing, rather than increasing, over a relevant range of firm output. This amounts to assuming that γ_2 is less than zero in the cost function given by equation (1).

■ 9 Some versions of the model use expected next-period sales instead of current-period sales.

In an output range with decreasing marginal costs, firms would generally lower their costs by producing high output in some periods, resulting in low marginal costs, and less output in other periods, resulting in high marginal costs. Thus, firms would minimize costs by "bunching" rather than "smoothing" production.

Exploring this possibility, Ramey (1991) finds evidence of declining marginal costs in several manufacturing industries. She also demonstrates that decreasing costs imply that the variance of production exceeds the variance of sales in a model with demand shocks only. Looking at the same industries, Durlauf and Maccini (1995) report evidence of rising marginal costs. The prevalence of declining marginal costs remains an open issue.

Another Approach

While some economists were at work modifying the production smoothing model to bring it into line with the data, others were developing alternative approaches to explain inventory behavior.

(S,s) Models

The production smoothing model is often thought to apply most naturally to manufacturers' inventories of finished goods. Most of the empirical work already mentioned looks at precisely these data. Yet, Blinder and Maccini (1991a, b) report that manufacturers' inventories of finished goods account, on average, for less than 15 percent of inventory investment in the manufacturing and trade industries. Furthermore, they find that this component of inventory investment is the least volatile, with the most volatile being retail inventories and manufacturers' inventories of raw materials and supplies. They argue that these facts suggest that a disproportionate emphasis has been placed on manufacturers' finished goods inventories.

An alternative theory of inventory behavior is provided by the so-called (S,s) model, which focuses attention on the timing of deliveries rather than the timing of production. Because it concentrates on deliveries, this model is commonly viewed as a theory of retail inventories and manufacturers' raw materials and supplies.

In an (S,s) model, a firm's decision rule about inventories has the following characteristics: The firm optimally picks some number, *s*, below which it does not let inventories fall. When inventory stocks reach that level, the firm orders a new batch, increasing the stocks to an optimally chosen level, *S*. The quantity *S* minus *s* is referred to as the optimal lot size. The firm orders more inventories only when the stock again falls to *s*.

One assumption that leads to (S,s) inventory behavior is that the cost of acquiring goods includes a fixed cost plus a constant marginal cost. Reinterpreting the cost function in the production smoothing model as the cost of acquiring goods, an (S,s) model firm faces the cost function

$$(8) \quad C_t = \begin{cases} \gamma_0 + \gamma_1 * [I_t - (I_{t-1} - S_t)] \\ \qquad \text{if} \quad I_t > I_{t-1} - S_t \text{ and} \\ 0 \qquad \text{if} \quad I_t = I_{t-1} - S_t, \end{cases}$$

where γ_0 reflects the fixed cost of placing and processing an order, γ_1 is the constant marginal cost, and S_t is current-period sales.[10] Notice that costs are incurred only when goods are acquired, which is indicated by end-of-period inventories (I_t) exceeding beginning-of-period inventories minus current-period sales ($I_{t-1} - S_t$). Here, the costs of holding inventories are set to zero.

A justification for this cost function is that marginal costs represent shipping costs, which are assumed to be a constant function of the quantity ordered, and ordering a shipment requires paying a fixed cost per order. If relatively large fixed costs exist, firms will order infrequently and will bring in large shipments when they do order (that is, the optimal lot size, *S* – *s*, will be large).

The intuition as to why the variance of shipments (production) can exceed the variance of sales in this setup is clearly illustrated when sales are constant. In that case, shipments will alternate between zero and the optimal lot size, while sales will not vary. This suggests that shipments may also vary more than sales when sales are not constant, at least in cases where the variance of sales is not too large.

Comparing the properties of aggregated data at the industry- or economywide level with the predictions of an (S,s) model is greatly complicated by the difficulties associated with aggregating across firms. There is no representative firm in this model. Instead, one must keep track of the distribution of inventory holdings across firms, since firms will behave differently in response to shocks, depending on their inventory holdings.

10 Scarf (1960) showed that the (S,s) behavior of inventories is optimal given this cost structure.

Early work examining the implications of (S,s) inventory behavior in partial equilibrium models includes Blinder (1981) and Caplin (1985), who provide evidence that (S,s) models are consistent with the facts discussed in section II. Caballero and Engel (1991) present a more sophisticated framework for exploring the aggregate dynamics of (S,s) inventory behavior.

The recent work of Fisher and Hornstein (1997) develops a dynamic general-equilibrium framework with a retail sector in which the aggregate implication of (S,s) inventory policies can be studied. Their model economy replicates salient features of the business cycle and is consistent with the data observations in section II. In addition, they are able to examine quantitatively the effect of (S,s) policies on business cycle shocks. They find that the policies have little effect on the propagation and amplification of productivity disturbances, but contribute substantially to the amplification of a type of demand shock.[11]

Other Research

While the approaches already discussed broadly characterize the bulk of research on the cyclical behavior of inventories, several alternative theories have been developed. Bental and Eden (1993) present a general equilibrium model of sequential trade in which buyers for a product arrive in batches. Demand uncertainty arises from uncertainty about whether a batch will show up for a given product. Inventories accumulate whenever a batch does not arrive. The authors show that this approach provides, at least theoretically, a model that is consistent with the empirical observations discussed earlier. While the specifics of the model differ substantially from the work of Kahn (1987, 1992), this paper can be viewed as providing an alternative theoretical basis for target inventory behavior.

Kashyap, Lamont, and Stein (1994) argue that financial constraints may play a crucial role in understanding inventory behavior during recessions associated with restrictive monetary policy. They find that the inventory investment of firms with no access to public bond markets was significantly liquidity constrained during 1974–75 and 1981–82, recessions in which restrictive monetary policy is thought to have played a large role. They report some evidence suggesting that financial constraints may explain a substantial fraction of inventory movements during these downturns.

Other approaches may also help increase our understanding of inventory behavior. Haltiwanger and Maccini (1990) show that allowing multiperiod labor contracts and a distinction between temporary and permanent adjustments to the workforce can bring theory more into line with the data. Rotemberg and Saloner (1989) demonstrate that strategic behavior by duopolists leads them to accumulate inventories when demand is high so as to deter cheating from an implicitly collusive arrangement. This strategy results in a positive correlation between inventories and sales.

IV. Data Responses

The basic production smoothing model may be inconsistent with certain properties of the data, but we have seen that there are a handful of modifications that may, at least in theory, resolve this inconsistency. To the extent that the alternative models which underlie these explanations have different implications for the two overriding questions posed in the introduction—Does inventory investment play a key role in the amplification and propagation of shocks? What does inventory behavior tell us about the underlying source of the shocks?—it is important to know how much each of these explanations contributes to reconciling theory with the data. This is a quantitative issue.

One procedure to separate out the more plausible alternatives is to compare their predictions with a broader set of facts that characterize the relationship between inventories and variables at the aggregate and industry levels. For example, which alternatives are consistent with the behavior of inventory-to-sales ratios? More generally, since we are ultimately interested in the aggregate implications of inventory behavior, which alternatives are consistent with the aggregate behavior of output, inventories, investment, consumption, and productivity?

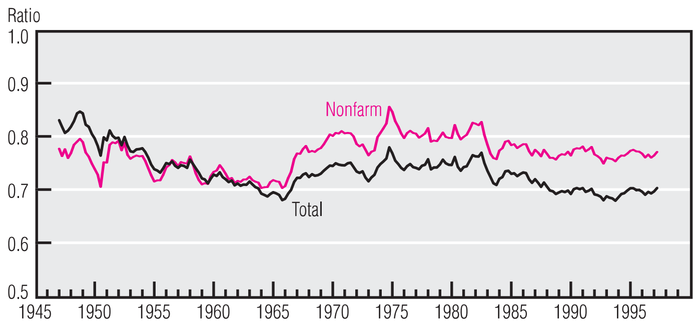
Figure 4 shows that the ratio of inventories to final sales of domestic product declined from the late 1940s through the mid-1960s and has leveled off since then.[12] The ratio of nonfarm inventories to final sales of nonfarm business has shown no decline over this period. This may surprise some, given the extensive reporting in recent years on changes in inventory management practices, such as just-in-time and

11 The authors consider discount rate shocks.

12 The ratio of inventories to final sales of domestic business displays a similar pattern, except that it has fallen relatively more over the past 15 years.

FIGURE 4

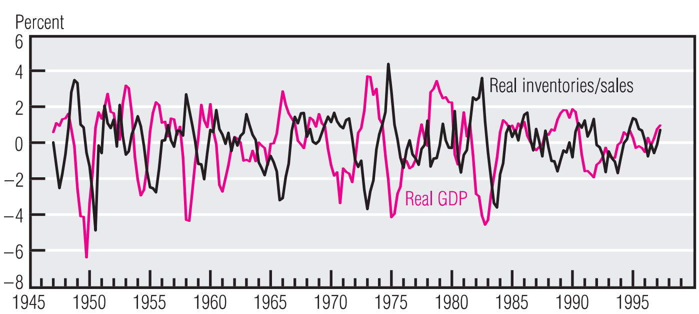
Inventory-to-Sales Ratios



NOTE: *Total* represents total business inventories divided by final sales of domestic product. *Nonfarm* represents nonfarm business inventories divided by final sales of nonfarm domestic business, which is defined as final sales of domestic product less gross product of households and institutions, general government, and farm business. Final sales are at a quarterly rate. Raw data are in billions of chained 1992 dollars.
SOURCE: U.S. Department of Commerce, Bureau of Economic Analysis.

FIGURE 5

Deviations from Trend of Real GDP and Inventory-to-Sales Ratio



NOTE: Quarterly data have been logged and detrended using the Hodrick–Prescott filter (λ = 1,600). Raw data are in billions of chained 1992 dollars.
SOURCE: U.S. Department of Commerce, Bureau of Economic Analysis.

lean production strategies. Figure 5 shows that the ratio of inventories to final sales is countercyclical. The correlation between the cyclical component of this ratio and output is –0.34. An obvious question is, which alternative theories are consistent with these observations on the trend and cyclical behavior of inventory-to-sales ratios?[13]

In addition, it has been shown that different components of inventories behave in substantially different ways over the business cycle. Work by Reagan and Sheehan (1985) and Blinder and Maccini (1991a, b) describes some of these differences. Their findings lead one to ask whether understanding the differences in the behavior of inventory components is crucial for understanding the implications of inventory behavior for business cycle fluctuations.[14]

V. Will Theory Respond?

Given that the issues of interest are the macroeconomic implications of inventory behavior, general equilibrium models of inventory behavior are essential tools. Furthermore, general equilibrium models allow the predictions of alternative inventory models to be compared across a broader set of relevant facts, such as those commonly used in the equilibrium business-cycle literature (see Cooley [1995, table 1.1]).

At this time, however, many of the alternative inventory theories have been offered only in partial equilibrium contexts. Although these models provide possible explanations of industry- and plant-level data, they are of limited use in analyzing the economywide implications of inventory behavior.

Exceptions to this shortcoming include the general equilibrium business-cycle theory put forth by Kydland and Prescott (1982) and modified by Christiano (1988) and a host of others, who model inventories as a factor of production. Fisher and Hornstein (1997) have taken a first step in embedding (S,s) inventory behavior in a general equilibrium framework and analyzing its aggregate implications. Bental and Eden (1993) develop an alternative approach with sequential trade. Other general equilibrium studies of inventory behavior include Christiano and Fitzgerald (1989) and Chatterjee and Ravikumar (1993).

The next step in evaluating the quantitative significance and implications of other inventory theories is to embed those theories in general equilibrium frameworks, so that their aggregate quantitative implications can be compared with data and with other models.

■ **13** Bils and Kahn (1996) argue that the countercyclical behavior of the inventory-to-sales ratio poses a major puzzle to business cycle theories that rely on productivity shocks as the source of uncertainty. They also argue that this behavior supports their own model, in which sales are assumed to be an increasing function of inventories and are subject to stochastic demand shocks.

■ **14** Recent research by Humphreys, Maccini, and Schuh (1997) takes a step toward incorporating input inventories (materials and works in process) and finished goods inventories separately in an otherwise standard inventory framework.

VI. Concluding Remarks

Prior to the 1980s, the predominant view of the business cycle was that fluctuations were driven by demand shocks, which were conceived of as aggregate disturbances to components such as consumer durables and investment. This view was commonly part of a broader vision in which business cycle fluctuations were considered inefficient; therefore, it was thought, they should be actively mitigated by the central government (one possible interpretation of sunspot models is that they provide a modern formalization of this perspective). This vision generated a vast body of research on ways the government could intervene to improve the economy's performance.

Data on inventory behavior over the business cycle initially seemed to pose a serious challenge to the demand-shock view, since they appeared to show that cost or technology shocks, originating on the production side of the economy, were the major source of economic disturbances. A broader vision of many proponents of the cost-shock view of business cycle fluctuations was that the economy reacted efficiently to such shocks (the modern formalization of this vision appears in real business cycle models). This vision carried with it the notion that government attempts to improve the performance of the economy would frequently be counterproductive.

The facts about inventory investment brought the conflict between the demand- and cost-shock views of business cycle fluctuations into sharp focus. While the initial impression was that the evidence supported the cost-shock view and conflicted with the demand-shock view, demand-shock proponents responded with revised theories of inventory investment that were consistent with empirical observations. Advocates of the cost-shock view had little need to revise their theory, since it was consistent with the inventory observations from the beginning.

The underlying source of the shocks that drive business cycle fluctuations continues to be a matter of considerable debate. The next step in advancing this debate is to formulate general equilibrium models that allow us to explore the broader implications of the two views. The real business-cycle literature supplies one set of such models. Research on inventory behavior, which provides one of many avenues for this exploration, is currently in progress.

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The Long-run Demand for Labor in the Banking Industry

by Ben Craig

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Introduction

For years, banking was considered the paragon of stable employment. Since peaking in 1989, however, the industry's payrolls have shrunk—in marked contrast to the expansion of the U.S. labor force and the growth in overall employment (see figure 1). Furthermore, the contraction has been steady, apparently unaffected by the aggregate business cycle. Between 1989 and 1995, banking employment fell more than 6 percent, while aggregate output in the industry (measured by total assets) increased 15 percent in real terms. Clearly, this differs from the situation in the U.S. steel industry during the 1970s, when a decline in demand for the industry's output provided an easy explanation for the employment loss.

Ready explanations for the contraction in banking employment have not been lacking. Casual observation of industry patterns from 1988 to the present suggests that two important changes have coincided with the shift in demand for banking labor: Technology has revolutionized the way banking is done, and consolidation has transformed banking's industrial structure.

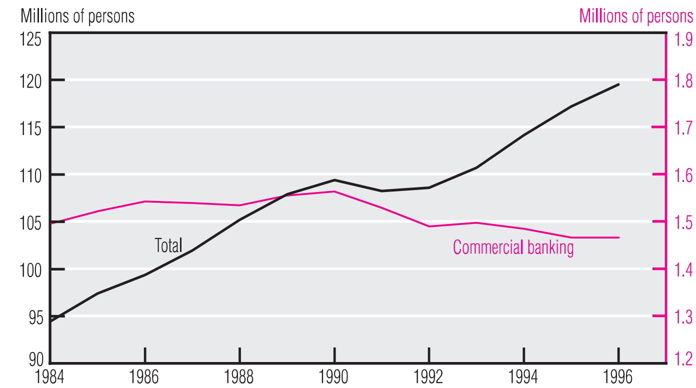
Technical progress may have obvious effects on labor demand. For example, all else equal, a firm may choose to employ fewer workers if the price of a substitute for labor goes down. The explosion in the number of ATM transactions in the 1980s is often cited as a primary reason for banking's dwindling payrolls (see figure 2). Even the name—automated teller machine—suggests the substitution use.

But a closer look suggests that the effect of technical progress may be more complex. Because they offer new opportunities to banks, ATMs may expand the range and amount of output that banks sell. The most visible effect of ATMs has been to transform the multitude of fully staffed branch offices that existed in the 1970s into today's sparsely staffed single branch. However, ATMs also offer services that were not easily obtainable 20 years ago, like allowing people to get cash easily, even at out-of-the-way places. Thus, it's possible that more bank services are being used, which should have a positive effect on employment.

Although ATMs are the most visible sign of technical progress to customers, they are not the only example of banks' adopting new technology. Some technical changes, like cash sorters and electronic readers, are embodied in

FIGURE 1

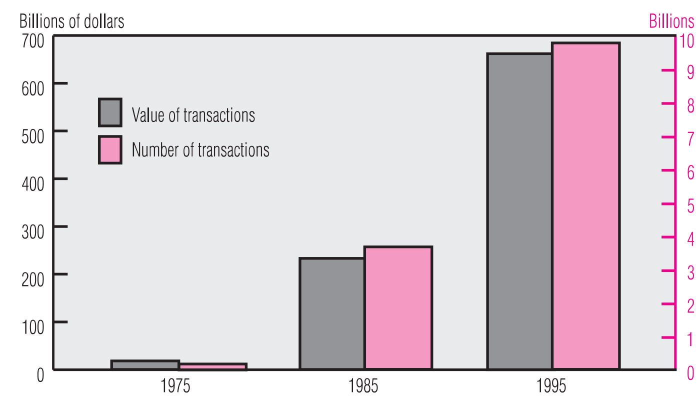
U.S. Employment



SOURCE: U.S. Department of Labor, Bureau of Labor Statistics.

FIGURE 2

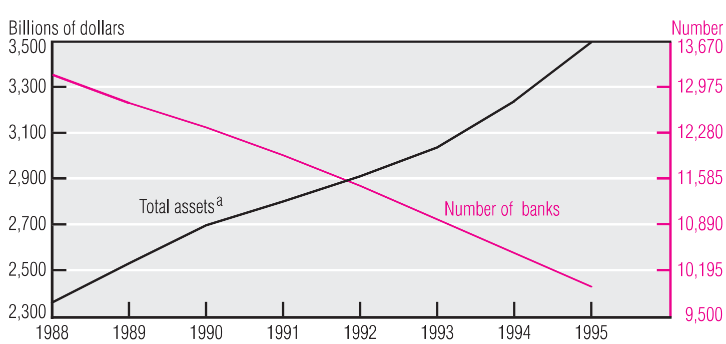
ATM Use[a]



a. Based on Federal Reserve System estimates.
SOURCE: Board of Governors of the Federal Reserve System.

FIGURE 3

U.S. Commercial Banking



a. Includes total loans and securities; seasonally adjusted data.
SOURCES: Board of Governors of the Federal Reserve System; and Federal Deposit Insurance Corporation.

new machinery. Other advances are less obvious, but may be just as important. For example, an accurate formula for assessing a loan's risk may allow a bank to substitute a small amount of an unskilled employee's time for that of a highly paid loan officer. Clearly, technology enters into a bank's employment decision in subtle and complex ways.[1]

One of the purposes of this paper is to document the effects of technical change on the demand for banking labor. Technical change is difficult to measure, however, so I approximate it by using the variable *time* for the period between 1984:IQ and 1996:IVQ. Because the use of ATM machines seems to approach a linear function of time during this interval, time may be a good proxy for technical progress when other, more easily measured labor demand factors are held constant. I also address a number of other questions, such as, when faced with the same measured economic environment, how many workers are employed by a bank today compared with the same bank a decade ago? How much of the decline in labor demand can be traced to "technical progress?" And how does technical progress differ in its effect on large versus small banks?

The second major development in banking over the last 10 years is the dramatic shift in industry composition, which has radically transformed the nature of banking employment.[2] While banking output (measured by total assets) has steadily increased, the number of individual banks has steadily fallen (see figure 3). This compositional change could affect the industry's demand for labor in several ways. A smaller bank's being swallowed by a bank holding company could result in duplicate positions being eliminated in research, marketing, management, and so on. Moreover, entire branches with duplicate functions could be wiped out.[3]

This paper also addresses some intriguing questions about the impact of this compositional change on labor demand. How is a banking organization's total employment

■ **1** See Griliches (1995) for an examination of the complexity of technical change in empirical estimation.

■ **2** For a detailed look at the change in industry composition, see Humphrey (1993), Berger, Kashyap, and Scalise (1995), Boyd and Gertler (1995), and Humphrey and Pulley (1997).

■ **3** Much of the anecdotal literature on the impact of mergers concentrates on gross employment effects, rather than on the net effects examined here. Thus, a management purge aimed at making a takeover easier would not affect net employment if replacements (presumably more docile workers) were hired from the outside.

affected by an acquisition?[4] That is, how does the post-acquisition picture differ from the defacto organization made up of the sum of the banks involved in the takeover? And which banking organization is most affected by an acquisition—the acquiring bank holding company or its target?

The next section lays out some of the theoretical and empirical issues surrounding labor demand in the banking industry. Section II then reports estimates of labor demand when the observation unit is a single firm, and section III uses a sample of acquisitions to explore how consolidation may affect employment. Section IV summarizes and concludes.

I. Labor Demand Estimation

Interpretations of the labor demand estimates reported here must be sensitive to a number of factors, including the formal static theory of labor demand implicitly assumed in the discussion, the limitations of the data used, and the way that industrial consolidation is handled. This paper adopts a static analysis. Thus, even though many short-run dynamics may be affecting labor demand, the issues of interest lie not in the dynamics of adjustment, but rather in the magnitude of long-run demand. I concentrate on the long-run elasticity of demand with respect to wages, the effects of changing prices of close substitutes for labor, and the effects of a change in banking's industrial structure.

The point of departure for most static input demand studies is the cost function (see Berger and Humphrey [1992, 1997]). These papers summarize a wide literature that estimates cost functions for the banking industry, often generated with the same call report data used in this paper. However, their emphasis is usually on efficiency (broken down by category), not on overall labor demand. Indeed, this literature is so focused on efficiency that, often, the coefficients of the cost function required to derive the labor demand elasticities are not even reported. Rather, the papers report efficiency statistics derived from the behavior of residuals from the estimated cost function.

If we had all the correct prices faced by individual firms, as well as their input amounts, then the cost function could be written as $C(Q, P)$, where Q is a vector of outputs and P is a vector of input prices. When viewed this way, unique problems posed by the banking industry become evident. For instance, what is an output and what is an input? Researchers have proposed several solutions to this question.[5] Outputs are usually multivalued, because it is unclear whether deposits, for example, represent inputs or outputs, and whether loans should be considered a primary output or be broken into separate categories. In the banking cost-function literature, Q is often composed of four outputs: deposits and three categories of loans—commercial/industrial, real estate, and other. Inputs are usually composed of labor, physical capital, and funds available from sources other than deposits. The reason for this particular breakdown is not that it is the best possible statistical model of banking industry behavior. Rather, it represents a huge compromise forced on the researcher because of the available data.

The major source of firm-level data for the banking industry is the call report, which every bank in the United States is required to file on a quarterly basis. Included are details on an institution's balance sheet, earnings, and expenses, as well as the number of "full-time equivalent" employees at the end of the reporting period.[6] Because these data are collected for regulatory purposes, they have advantages and disadvantages for the empirical researcher. On the plus side, the data set is large, embracing the entire banking industry. Also, because the information is collected from the same forms, it is comparable across banks. On the minus side, the data are not collected for the purpose of input demand estimation. This leads to major problems, some of which can be illustrated by looking at the measurement of changes that occur within a single bank.

Differentiating the cost function with respect to an input price yields (through Shepherd's lemma) an *output constant* demand curve for its associated input:

$$(1) \quad \frac{\partial C}{\partial P_i} = L_i(Q, P).$$

Here, the i^{th} input is denoted as L_i. This is an incomplete demand curve in that it does not include the changes that might occur if the quantities associated with the output vector were allowed to vary. For example, a wage change could affect the firm's demand for labor in several ways. First, if labor costs increase, the firm

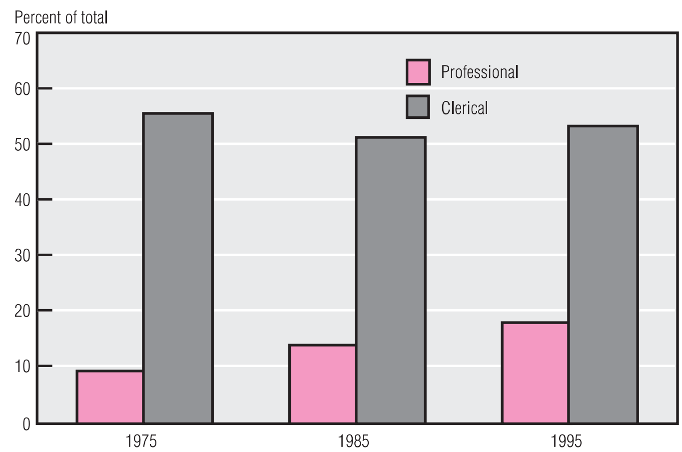
4 An acquisition is distinguished from a merger in that a merger wipes out the charter of one of the banks, whereas an acquisition allows both the acquirer and the acquired firm to retain their charters.

5 See Fixler (1992) for a discussion of the cost-function specification.

6 A full-time equivalent number is obtained by adding up the appropriate fractions corresponding to part-time employment.

FIGURE 4

Occupational Mix in Banking



SOURCE: U.S. Equal Employment Opportunity Commission.

may decrease its output and thus reduce its use of the labor input. Second, even if output remains constant, the firm will substitute the now relatively cheaper inputs for the more expensive labor. Measurement of equation (1) tells us something only about the second effect.[7]

The output constant demand curve implies an estimating equation that poses some measure of labor employed by a firm as a function of wages, prices of physical capital and funds, deposits, and levels of the various loan outputs. Suppose we were to estimate the constant wage elasticity demand function

$$(2)\quad \ln L_{it} = \beta_1 + \beta_2 \ln W_{it} + \beta_3 \ln P_{kit} + \beta_4 t + X_{it}\gamma + \varepsilon_{it},$$

where L is employment, W is the wage rate, P_k is the price of capital, X is a row vector of other included variables in the demand equation, and γ is a column vector of the parameters to be estimated. The parameter β is often of chief interest. Equation (2), when fitted to call report data, must be interpreted with caution in light of recent developments in the banking industry.

Some of the important variables driving labor demand, including new techniques for evaluating loan applications, will not have an available proxy in the call report data. These variables are subsumed in the general interpretation of the coefficients of variables involving time. Perhaps more important are issues of aggregation. Many different labor types are combined into the single variable L, provided by the call report. Theory suggests that aggregation over a group of inputs, $i = 1 \ldots m$, requires the cost function to be written as

$$(3)\quad C(y, P_1, \ldots P_m, P_{m+1}, \ldots P_n)$$

$$= C[y, \theta(P_1, \ldots P_m), P_{m+1}, \ldots P_n],$$

where θ is a price index that aggregates the prices $P_1, \ldots P_m$. A sufficient condition for (3) to be true is that the production function must be strongly separable between the group of inputs, $i = 1 \ldots m$, and all of the other inputs. There is some a priori evidence that this is not the case. For example, data from other industries suggest that skilled labor is complementary to capital inputs. Furthermore, technical change may be easier to accomplish if workers are more skilled. An examination of the occupational makeup of the banking industry suggests that workers are indeed more skilled than they were 20 years ago (see figure 4). Recent work by Demsetz (1997) reinforces this finding, showing that the skill set of bankers, like the skill set of workers in all financial, insurance, and real estate industries, has steadily increased over the last decade. Without more evidence, the direction of the bias for technical change estimates is hard to determine.

The best policy at this point is to use caution when interpreting estimates of labor demand based on call report data. Similar caveats apply to the physical capital variable, clearly an aggregation over many types and vintages. Buildings probably interact with labor in a fundamentally different way than an ATM does, so elasticities with respect to the capital variable computed from call report data should be viewed circumspectly.

A second way the firm's decisions will affect employment is more lumpy. The firm decides whether to open or close a plant, or, more drastically, whether to go out of business.[8] In the context of banking, this means that a bank decides whether to open or close a branch office, as banks generally reorganize through mergers or acquisitions rather than going out of business. Of approximately 9,000 banks operating in the United States during the mid-1990s, fewer than 20 failed each year. In addition, many new banks were chartered during the same period.

7 Differentiation of equation (1) with respect to P_j and a simple application of Young's theorem lead to the symmetry restrictions of convex analysis: $\frac{\partial L_i}{\partial P_j} = \frac{\partial L_j}{\partial P_i}$. These restrictions are usually rejected in cost function analyses of the banking industry. Given this, I concentrate on simple demand functions in what follows.

8 Obviously, the latter decision is sometimes imposed on the firm from the outside.

TABLE 1

Employment and Wages by Bank Size

Banks Assets:[a]	$0–$100 million	$100 million–$500 million	$500 million–$5 billion	More than $5 billion
1984				
Number of banks	6,014	1,775	385	47
Employment/bank[b]	39.8	150.5	1,106.3	10,308.1
	(22.8)	(89.2)	(845.5)	(12,940.3)
Wage[c]	5.04	5.15	5.71	6.96
	(2.01)	(0.95)	(1.16)	(1.30)
Assets/worker[d]	1,421	1,406	1,536	1,712
1987				
Number of banks	6,645	1,914	417	67
Employment/bank	35.6	138.4	1,008.0	8,400.1
	(20.2)	(100.1)	(785.4)	(10,477)
Wage	5.29	5.45	6.02	7.49
	(1.20)	(1.22)	(1.48)	(2.00)
Assets/worker	1,493	1,600	1,795	1,928
1990				
Number of banks	6,531	1,768	406	77
Employment/bank	34.2	139.8	1,005.7	7,686.3
	(20.0)	(82.9)	(803.4)	(8,970.8)
Wage	5.32	5.53	6.09	7.52
	(1.31)	(1.98)	(1.74)	(2.41)
Assets/worker	1,481	1,668	2,284	2,234
1993				
Number of banks	6,641	1,637	401	75
Employment/bank	33.4	137.7	947.3	7,950.4
	(21.8)	(82.9)	(810.4)	(10,833.3)
Wage	5.38	5.64	6.46	8.09
	(1.34)	(3.00)	(3.65)	(3.58)
Assets/worker	1,451	1,615	2,647	2,128
1996				
Number of banks	5,901	1,569	392	86
Employment/bank	33.2	132.7	845.2	8,023.1
	(21.9)	(80.1)	(711)	(9,617.7)
Wage	5.54	5.84	6.69	8.14
	(1.37)	(1.80)	(2.53)	(3.55)
Assets/worker	1,463	1,663	4,585	2,196

a. Real 1984 dollars.

b. Full-time equivalent employees per chartered bank.

c. Thousands of 1984 dollars per full-time equivalent worker per quarter.

d. Thousands of 1984 dollars per full-time equivalent worker.

NOTE: Numbers in parentheses are standard deviations. Footnotes b, c, and d apply to all years.

SOURCE: Author's calculations based on call report data.

This paper concentrates on the patterns surrounding bank acquisitions in order to examine the structural changes that have taken place in the industry. There are several compelling reasons for adopting this approach. First, acquisitions account for at least half the consolidation in the banking industry when measured by number of events. Recent evidence indicates that, relative to mergers, acquisitions are increasing in importance. Second, acquisitions do not destroy the target as a data-reporting organization, meaning that we can empirically observe both the acquirer and its target in the period following a takeover.

Thus, I examine changes in the long-run demand for banking labor using two approaches. The first concentrates on the behavior of a single firm as it minimizes costs subject to a changing environment. The second looks at two organizations—the acquiring bank holding company and its target—as they adjust to an acquisition. The next section looks at the behavior of single banks.

II. Full-Sample Estimates

The call report data examined here embrace the entire U.S. banking industry, with more than 9,000 quarterly observations. Clearly, a data set this rich can be analyzed in several ways. I first look at raw averages computed for several classes of banks over different periods. These numbers are helpful in detecting broad patterns in the data. However, they are less useful in answering the question more relevant to policymakers, that is, how has banks' demand for labor changed, abstracting from the effect of other measured changes in their economic environment? If, for example, average employment has decreased and average wages have increased, then it is difficult to tell from simple averages whether the employment loss is due to higher wages or secular changes (such as technical progress) that are altering the labor demand curve. The discussion progresses from simple averages, to holding measured variables in the data set constant through regression analysis, to holding unmeasured individual characteristics of each bank constant through fixed-effect models. I start with the simple averages.

Table 1 summarizes the labor and wage data by asset class for each of the five years in the sample. Asset size is expressed in real 1984 dollars, so that the structure of the industry reflects genuine growth in bank size rather than an artifice of inflation. Several patterns are evident. To begin with, larger banks are more important employers than smaller banks. Although firms in the smallest two asset categories accounted for 95 percent of all U.S. banks in 1984, they employed only about a third of the industry's workforce. This pattern was even more pronounced in 1996, when the smaller banks accounted for 94 percent of banks by number, but only 28 percent of total employment.

Another important finding is that in every year, larger banks pay a higher average wage. Furthermore, the large institutions differ from the small ones in the rate at which they have adjusted to the changes of the last decade. All bank categories paid a higher average real wage in 1996 than in 1984. However, the increase for small banks was only 10 percent, whereas for large banks it was 17 percent.[9] Also, banks in every size grouping saw their employment levels go down, but smaller firms still had an average employment of about 83 percent of 1984 levels, while larger banks trimmed their payrolls to 77 percent of their former size.

It is interesting to note that banks are getting bigger (in terms of total assets) and that larger banks have more assets per employee. However, the same trend is not evident for smaller institutions. Based on the data presented in table 1, it is hard to reject the notion that smaller banks may have reduced their employment because they have fewer assets to manage. The same cannot be said for the largest banks, particularly those in the $500 million to $5 billion category. Clearly, large banks are different institutions from the point of view of labor demand.

The patterns reported in table 1 must be considered when conducting a regression analysis. A simple labor demand function derived from the firm's static cost-minimization problem represented by equation (2) is reported in table 2. The data represent ordinary least squares (OLS) regressions of log labor on outputs and inputs, prices of substitute inputs, time, and structure variables. Of course, other specifications were tried, but the general pattern of results remained the same. First, the output constant wage elasticity of labor demand is about 40 percent; that is, for every 10 percent rise in the wage rate, the firm's demand for labor decreases by 4 percent. This is lower than many estimates for manufacturing (which cluster around unity), but is still well within the wide band of estimated elasticities reported for the service sector (see Hamermesh [1986, 1993]). Second, it is clear from the table that capital is a much-used substitute for labor. Its cross-price elasticity is quite high at 40 percent, indicating that the price of overnight funds does not affect the demand for labor.

All of a bank's outputs seem to require labor in the sense that the coefficients of outputs are positive. The easiest loans to service appear to be real estate and commercial/industrial loans. Core deposits are the most labor intensive input/output. Ceteris paribus, a 10 percent increase in deposits will boost the demand for labor by 3 percent. This is compatible with the view of banks as firms that use labor to service cheap deposits (relative to the funds market) and convert them into loans.

Although much of the recent research has focused on the structure of the banking industry, when it comes to employment variation, the prima facie evidence indicates that the unexamined seasonal component may be more important. Employment in the summer and fall quarters declines 2 percent relative to the spring (April–June) quarter. Belonging to a bank holding company is associated with an employment decrease of slightly less than 2 percent.[10]

The table 2 regressions show that labor demand has clearly shifted over the last decade. The coefficient of the time variable, which represents the number of quarters from the beginning of the sample period in 1984:IQ, indicates that employment at a firm having the same price structure and the same loan and deposit portfolio declines by half a percentage point per quarter. This stunning observation is the focus of the regression reported in the second column of table 2.

The second regression looks at the time pattern of labor employment by examining the coefficients on two variables defined as products of time and another variable. The first variable is time multiplied by the dummy variable for the firm's holding company status. In this case, the interpretation of the coefficient is the effect of time on banks that belong to bank holding companies, compared to the effect of time on the reference group of independent banks. The second variable is time multiplied by the logarithm of total assets held by the bank. Here, the interpretation of the coefficient is analogous: Compared to a bank with few assets, what is the effect of time on larger banks? A negative coefficient indicates that larger banks have experienced a greater percentage decline in employment demand.

■ **9** This occurred despite the fact that the largest banks were already paying their workers more in 1984 than small banks paid in 1996.

■ **10** The coefficient of this variable compares bank holding company members to a reference group of independent banks.

TABLE 2

Log Labor Demand Regressions

Variable	Ordinary Least Squares		Fixed Effects
Intercept	−2.648	−4.001	−4.149
	(317.8)	(329.0)	(51.4)
Log wage	−0.419	−0.324	−0.250
	(223.9)	(474.7)	(34.8)
Log price capital	0.408	0.476	0.112
	(589.9)	(474.7)	(34.6)
Log price funds	−0.0005	−0.0005	−0.002
	(0.478)	(5.31)	(2.4)
Log real estate loans	0.0116	0.0149	0.009
	(32.2)	(45.9)	(7.5)
Log commercial/ industrial loans	0.0191	0.0123	0.0225
	(43.1)	(30.7)	(10.7)
Log other loans	0.185	0.055	0.0815
	(232.1)	(66.2)	(15.5)
Log core deposits	0.314	0.077	0.0814
	(285.1)	(60.2)	(5.3)
Spring	0.0055	0.0053	0.005
	(4.79)	(5.18)	(16.1)
Summer	−0.0157	−0.009	−0.005
	(13.2)	(8.7)	(13.4)
Fall	−0.0159	−0.0044	−0.004
	(13.1)	(4.02)	(6.6)
Bank holding company	−0.0187	−0.0177	0.0148
	(18.7)	(10.1)	(3.5)
Bank holding company ∗ time		−0.0178	−0.0005
		(10.18)	(3.5)
Time	−0.00570	0.00132	0.0110
	(129.3)	(39.8)	(9.8)
Log total assets		0.509	0.535
		(286.8)	(30.4)
Log total assets ∗ time		−0.00135	−0.00106
		(44.5)	(10.2)
Number of observations	399,266	399,266	395,000
Number of banks	12,664	12,664	12,255

NOTE: Numbers in parentheses are *t* ratios.
SOURCE: Author's calculations based on call report data.

This is exactly what is found—not a surprising result given the averages reported in table 1. An independent bank of average asset size (logarithm of total assets of 10) experiences no discernible employment decline over the sample period, holding wages and all else constant. A huge bank (logarithm of assets equal to 18) experiences a drop of nearly *40 percent.* Interestingly, larger banks seemed to have started the period with higher employment for the same variables than did smaller banks. Some of this decline may have resulted from a shift out of scale diseconomies.

The regressions reported in the first two columns of table 2 leave out many possible variables that might be included in labor demand. Some of these may exist in call report data. Others are measured poorly, if at all, by any data. An example of the latter is managerial taste in using new machinery. To the extent that this factor is correlated with labor demand and with an included variable (such as the price of physical capital), bias can result.

To compensate for this problem, I added a "fixed effect" to the error scheme. In a sense, this is necessary to maintain the interpretation of a firm's cost-minimizing labor demand. The coefficient of interest is the effect on a single firm's employment policy if a change occurs in a measured variable, such as wages. How do such measured environmental shifts affect the firm's decisions, holding all else constant? A fixed effect decomposes the unobserved error term into two terms:

$$(5) \qquad \varepsilon = \varepsilon_i + \varepsilon_{it},$$

where the firm fixed effect, ε_i, may be correlated with included observed variables. The fixed effect accounts for idiosyncratic elements facing the firm, such as local conditions, that remain somewhat constant over time.

The last column of table 2 reports the results from these regressions. The estimates are computed with consistent standard errors under a wide variety of assumptions, and are balanced to account for the possibly different number of time-series observations per bank. The standard errors and estimated coefficients are consistent, for example, if the nonidiosyncratic error for each observation, ε_{it}, is correlated through an autoregressive process with the error in the prior period, ε_{it-1}.

For the most part, the fixed-effects regressions yield similar estimates to the middle-column regression. However, there are some differences that may reflect how local conditions or management traditions are correlated with the variable whose coefficient changes. First, the wage elasticity of demand is somewhat smaller for the fixed-effect estimates. Second, the cross-price elasticity of capital is much smaller. This may be because banks paying a higher price for capital in a cross-section also require more labor. This cross-sectional variation is less interesting than the average variation within a single bank's behavior because the latter is more useful in answering the question, "If a policy were to change the price of capital facing a single bank, how would that bank alter its employment?" The fixed-effect estimates indicate that the impact of a technical innovation that lowers the price of capital by 10 percent should reduce employment by only 1 percent.

The fixed-effect estimates differ from the OLS estimates most radically with respect to the bank structure variables. The coefficient of belonging to a bank holding company is now positive and significant, and the cross-time effect, though still negative, is much smaller. The total effect of being in a bank holding company is slightly positive at the beginning of the sample period and then decreases to a negligible amount by the end of the period. This contrasts with the OLS estimates, which imply a negative employment effect at the beginning of the sample period that increases to 70 percent by the end of the period.

The difference in estimates may be due to an underlying unobserved factor in a bank's labor policy that also makes it more likely to be part of a bank holding company. Although this unobserved factor is reflected in the simple OLS estimates, it is purged in the fixed-effect data. In predicting employment trends for the next decade, analysts must be more concerned about the effect of consolidation on a bank's employment policy than about the underlying policies of banks that happen to be consolidated.

This makes the fixed-effect estimate, which shows that consolidation has only a minimal impact on bank employment, more relevant. A Hausman–Wu test rejects the random-effects model at any reasonable level of significance with a p value of $3\text{x}10^{-7}$. Clearly, local conditions affect how a bank employs labor, and these conditions cannot be entirely accounted for through use of the simple measured variables employed here. They are correlated with wages, the price of capital, and especially the effect of bank structure over time in such a way that studies excluding unobserved local effects will yield misleading results. In particular, consolidation seems to matter little for labor demand. Below, I explore this issue more closely by looking at how participants in a sample of acquisitions reacted to consolidation.

III. Effects of Acquisitions

We can observe the effect of consolidation more directly by looking at a subsample of banks acquired in the 1984–94 period. I collected data on 200 acquisitions, covering both the acquiring banking organization and its target bank. In all cases, the target institution was an independent bank. Such a criterion was much too restrictive for the acquirer, however, essentially ruling out all acquisitions except of one small rural bank by another. In the case of a bank holding company, I aggregated all of the banks in the organization (except for the target of the current acquisition) into a defacto "superbank." Both banks (the acquiring organization and its target) could then be compared before and after the acquisition. Note that the fictional organization of the acquiring bank holding company formed by the sum of its component banks stays constant throughout the comparison period. I look at the broad patterns suggested both by averages over the periods surrounding the acquisition and by regression analyses.

Table 3 presents some of these averages, comparing the post-acquisition institution with the same bank two quarters prior to takeover. The top number is the mean difference in log employment after the acquisition, the value in parentheses is the p value for the hypothesis of no difference, and the bottom number is the total number of acquisitions included in the mean. Thus, the first entry under "acquiring bank" indicates that the acquiring banking organization increased its employment about 2 percent, on average, in the period two quarters before the acquisition to one quarter after (or nearly a year, if one includes the acquisition quarter). My sample includes 197 banks for this particular comparison, and the p value indicates that the hypothesis of no change in the acquiring bank's employment would be rejected at any reasonable level of significance.

The patterns suggested by table 3 run contrary to the accepted wisdom regarding acquisitions and employment. Both the acquiring bank and its target expanded their payrolls rather than trimming them. This process took two or three years, but by the end of that time, both

TABLE 3

Pre- and Post-Acquisition Comparisons

Quarters after Acquisition	Acquiring Bank	Target Bank
1	0.0228 (0.0002) 197	0.0150 (0.15) 197
2	0.0316 (0.00005) 199	0.0314 (0.01) 199
3	0.0372 (0.0002) 186	0.0475 (0.004) 186
4	0.0379 (0.000008) 174	0.0469 (0.01) 174
6	0.0520 (0.0000002) 158	0.0586 (0.005) 158
8	0.0573 (0.000007) 133	0.0586 (0.02) 133
12	0.0725 (0.00001) 113	0.0565 (0.04) 113
16	0.0696 (0.0006) 89	0.0481 (0.126) 89

NOTE: The top number is the mean difference in log employment after acquisition versus two quarters before acquisition. The number in parentheses is the *p* value for the hypothesis of no difference. The bottom number is the total number of acquisitions included in the mean.
SOURCE: Author's calculations.

the acquirer and its target were employing between 5 and 7 percent more workers than in the period just before the acquisition.

This is not meant to suggest that the acquisition caused the employment gain. Indeed, the most plausible story is that banks in growing markets tend to get bigger—in part by acquiring other banks, which can then participate in the expanding market. The employment pattern of acquiring banks prior to acquisition shows that they are in fact generally growing before takeover. (The same is not true of the targets.) However, the evidence does refute the commonly held idea that acquisitions are usually accompanied by large employment cuts. Four years after acquisition, targets average nearly 5 percent *more* workers.

The regression analyses reported in table 4 support the notion that banks involved in an acquisition are generally expanding; however, the acquisition slows their growth. The fixed-effect estimates basically reinforce the OLS estimates. Acquisitions do cause a drop in employment, but the effect is small: Three years after a takeover, a bank may see its payroll shrink about 2 to 4 percent because of the acquisition effect. (Given that an acquisition has taken place, the sample average is three years from the takeover date.) By contrast, all of the small banks in the acquisition sample experienced secular *growth* in employment of 30 percent, even after accounting for the growth of measured variables included as inputs, outputs, or prices. Size alone, measured by total assets, accounted for 10 times more of the dynamic employment effect than did the time from acquisition.

Clearly, there is room for further research. My acquisition sample is small compared to the consolidation that has occurred in the industry over the last decade. Moreover, for all of the advantages offered by studying acquisitions, much is left out by excluding mergers. There is every reason to believe that a merger, which destroys a bank's identity, will have a different employment effect than an acquisition, which allows that identity to continue. Acquisitions may occur precisely because the acquirer wants to keep offices open under the target's old name. Thus, a merger may have a larger negative effect on employment. Future research can also improve the estimates by documenting the selectivity effects caused by consolidation.

IV. Conclusion

The primary lesson of the call report data is that the decline in banking employment over the last 10 years is a large-bank phenomenon. A typical small bank experienced no employment loss when its loan portfolios and real wage were held constant, whereas the largest institutions saw their payrolls shrink by nearly 1 percent per quarter, all else equal. In 1984, the beginning of the sample period, larger banks employed more workers to service the same number of loans. By 1996, this differential had been wiped out. The effect on the

TABLE 4

Fixed-Effect Regressions

Variable	Acquiring Bank	Target Bank
Intercept	–4.453	–4.628
	(5.8)	(11.8)
Log wage	–0.192	–0.233
	(5.3)	(7.4)
Log price capital	0.070	0.080
	(7.1)	(7.1)
Log price funds	–0.011	–0.0029
	(0.28)	(0.674)
Log real estate loans	0.0007	0.0258
	(0.115)	(2.9)
Log commercial/ industrial loans	0.004	0.0222
	(0.382)	(3.2)
Log other loans	0.122	0.0983
	(4.1)	(3.3)
Log core deposits	0.039	0.135
	(0.938)	(2.1)
Spring	0.0033	0.0072
	(1.79)	(3.0)
Summer	–0.0012	–0.002
	(0.558)	(0.872)
Fall	–0.0007	0.0044
	(0.229)	(1.4)
Post-acquisition dummy	–0.0007	–0.002
	(0.095)	(0.178)
Post-acquisition * time	–0.0017	–0.0037
	(1.69)	(3.0)
Time	0.012	0.0168
	(2.3)	(2.0)
Log total assets	0.606	0.507
	(7.43)	(7.2)
Log total assets * time	–0.00103	–0.0017
	(2.5)	(2.3)
Number of observations	7,040	6,951
Number of banks	315	315

NOTE: Numbers in parentheses are *t* ratios.
SOURCE: Author's calculations.

industry as a whole has been dramatic, because large banks employ the lion's share of the banking workforce.

There are several possible reasons for the secular decline in employment within the nation's largest banks. One possibility, emphasized above, is that these institutions have been more effective at incorporating technical substitutes for labor than have small banks. A technical transition in one area of a large bank may provide important lessons for a transition in a different area. Also, the fixed costs of a transition may be amortized over a larger operation, justifying the technical transformation.

On the other hand, measurement error may supply just as cogent a reason for the secular decline in large-bank employment. Large banks may employ more-skilled workers, allowing them to hire fewer people. Some supporting evidence is offered by the fact that larger banks pay higher average wages than do smaller ones. In addition, large banks may be more able to use outside organizations to accomplish tasks that were once performed in-house. Thus, a small bank may hire a single person to do its accounting because the fixed costs of hiring an outside firm are prohibitive, whereas a larger bank may use outside consultants who do not appear on the company payroll. Clearly, further work is needed before the large-bank effect can be attributed solely to technical change.

The large-bank effect is big enough to swamp any of the other possible suspects in the employment decline. Consolidation's impact on the industry's payrolls amounts to about a tenth of the large-bank effect. Indeed, seasonal changes are responsible for more of the employment variation than is the impact of industrial structure.

It is fascinating that so little measurable effect on employment is observed for either the acquiring bank or its target. Equally intriguing is the dramatic impact of bank size in explaining the employment changes witnessed over the last decade. Given this marked empirical pattern, any research effort that attempts to properly measure scale economies in banking should have great relevance in predicting future employment trends.

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