Bank Capital Requirements for Market Risk: The Internal Models Approach

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The increased prominence of trading activities at many large banking companies has highlighted bank exposure to market risk—the risk of loss from adverse movements in financial market rates and prices. Recognizing the importance of trading operations, banks have sought ways to measure and to manage the associated risks. At the same time, bank supervisors in the United States and abroad have taken steps to ensure that banks have adequate internal controls and capital resources to address these risks.

Prominent among the steps taken by supervisors is the development of formal capital requirements for the market risk exposures arising from banks' trading activities. These market risk capital requirements, which will take full effect in January 1998, depart from earlier capital rules in two notable ways. First, the capital charge is based on the output of a bank's internal risk measurement model rather than on an externally imposed supervisory measure. Second, the capital requirements incorporate qualitative standards for a bank's risk measurement system.

This paper presents an overview of the new capital requirements. In the first section, we describe the structure of the requirements and the considerations that went into their design. In addition, we address some of the concerns that have been raised about the methods of calculating capital charges under the new rules. The paper's second section considers the probable impact of the market risk capital requirements. After performing a set of rough calculations to show that the effect of the internal models approach on required capital levels and capital ratios will probably be modest, we identify some significant benefits of the new approach. Most notably, the approach will lead to regulatory capital charges that conform more closely to banks' true risk exposures. Moreover, the information generated by the models will allow supervisors and financial market participants to compare risk exposures over time and across institutions.

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THE STRUCTURE OF THE MARKET RISK CAPITAL REQUIREMENTS

The new capital requirements for market risk have been put forward as an amendment to existing capital rules. In late 1990, banks and bank holding companies in the United States became subject to a set of regulatory capital guidelines that defined minimum amounts of capital to be held against various categories of on- and off-balance-sheet positions.[1] The guidelines also specified which debt and equity instruments on a bank's balance sheet qualified as regulatory capital. These guidelines were based on the 1988 Basle Accord adopted by the Basle Committee on Banking Supervision, a group made up of bank supervisors from the Group of Ten countries.

While the original Basle Accord and U.S. risk-based capital guidelines primarily addressed banks' exposure to credit risk, the new requirements set minimum capital standards for banks' market risk exposure.[2] Broadly speaking, market risk is the risk of loss from adverse movements in the market values of assets, liabilities, or off-balance-sheet positions. Market risk generally arises from movements in the underlying risk factors—interest rates, exchange rates, equity prices, or commodity prices—that affect the value of these on- and off-balance-sheet positions. Thus, a bank's market risk exposure is determined both by the volatility of underlying risk factors and the sensitivity of the bank's portfolio to movements in those risk factors.

Banks face market risk from the full range of positions held in their portfolios, but the capital standards focus largely on the market risks arising from banks' trading activities.[3] This focus reflects the idea that market risk is a major component of the risks arising from trading activities and, further, that market risk exposures are more visible and more easily measured within the trading portfolio because these positions are marked to market daily. Thus, under the amended capital standards, positions in a bank's trading book are subject to the market risk capital requirements but are exempt from the original risk-based capital charges for credit risk exposure.[4] In addition, commodity and foreign exchange positions held throughout the institution (both inside and outside the trading account) are subject to the market risk capital requirements.

Because the capital standards principally address the market risk arising from trading activities, only those U.S. banks and bank holding companies with significant amounts of trading activity are subject to the market risk requirements. In particular, the U.S. standards apply to banks and bank holding companies with trading account positions (assets plus liabilities) exceeding $1 billion or 10 percent of total assets. The institutions meeting these criteria, while relatively few in number, account for the vast majority of trading positions held by U.S. banks.[5] Supervisors also have the discretion to impose the standards on institutions that do not meet these criteria if such a step appears necessary for safety and soundness reasons. The rules become effective as of January 1998, although the U.S. regulation also permits banks to elect early adoption during 1997.

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INNOVATIVE FEATURES

The market risk capital standards have drawn considerable attention because they differ significantly in approach from the risk-based capital rules for credit risk. The market risk standards impose a quantitative minimum capital charge that is calculated for each bank using the output of that bank's internal risk measurement model; they also establish a set of qualitative standards for the measurement and management of market risk. In both regards, the capital standards break new ground. By substituting banks' internal risk measurement models for broad, uniform regulatory measures of risk exposure, this approach should lead to capital charges that more accurately reflect individual banks' true risk exposures. And by including qualitative standards,

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the approach is consistent with the shift in supervisory interest from a focus on risk measurement to a more comprehensive evaluation of banks' overall risk management.

The qualitative standards are designed to incorporate basic principles of sound risk management in the capital requirements. Any bank or bank holding company subject to the market risk capital requirements must be able to demonstrate that it has a conceptually sound risk measurement system that is implemented with integrity. The risk estimates produced must be closely integrated with the risk management process: for example, management could rely on daily reports from the system to assess current strategy or could base its limit structure on the risk estimates. In addition, the bank must conduct periodic stress tests of its portfolio to gauge the impact of extreme market conditions. Further, the bank must have a risk control unit that is fully independent of the business units that generate market risk exposures. Finally, internal and/or external auditors must conduct an independent review of the bank's risk management and measurement process.

The quantitative capital requirements distinguish between *general market risk* and *specific risk*. As defined in the capital standards, general market risk is the risk arising from movements in the general level of underlying risk factors such as interest rates, exchange rates, equity prices, and commodity prices. Specific risk is defined as the risk of an adverse movement in the price of an individual security resulting from factors related to the security's issuer. At one level, general and specific market risk are analogous to systematic and nonsystematic risk in a standard asset-pricing framework. Specific risk, however, is intended to cover variation both from day-to-day price fluctuations and from surprise events, such as an unexpected bond default. The following subsections provide an overview of the capital treatment of the two types of risk.

Capital Requirements for General Market Risk

The capital requirements for general market risk are based on the output of a bank's internal value-at-risk model, calibrated to a common supervisory standard. In brief, a value-at-risk model produces an estimate of the maximum amount that the bank can lose on a particular portfolio over a given holding period with a given degree of statistical confidence.[6] Although there are a variety of empirical approaches to calculating value at risk, estimates are almost always derived from the behavior of underlying risk factors (such as interest rates and exchange rates) during a recent historical observation period.

The general market risk capital requirement is based on value-at-risk estimates calibrated to a ten-day, 99th percentile standard. That is, if the ten-day, 99th percentile value-at-risk estimate is equal to $100, then the bank would expect to lose more than $100 on only 1 out of 100 ten-day periods. The common supervisory standard is imposed to ensure that the capital charge entails a consistent prudential level across banks. The value-at-risk estimates must be calculated on a daily basis using a minimum historical observation period of one year, or the equivalent of one year if observations are weighted over time. The capital charge for general market risk is equal to the average value-at-risk estimate over the previous sixty trading days (approximately one quarter of the trading year) multiplied by a "scaling factor," which is generally equal to three.[7]

Several aspects of this calculation have generated considerable discussion, and thus it is worth taking a moment to consider them further. First, the ten-day holding period has been criticized as being overly conservative, since under normal market conditions, many positions in a bank's trading portfolio could be liquidated in less than this amount of time.[8] The ten-day standard, however, also reflects the need to address the risks posed by options and other positions with nonlinear price characteristics. Because options' sensitivities to changes in market risk factors can grow at a rate that is disproportionate to the size of changes in the risk factors, a longer holding period can reveal risk exposures that might not be evident with the smaller risk factor movements associated with shorter holding periods. Thus, the choice of a ten-day holding period stems from the view that the value-at-risk estimates used in the capital calculation should incorporate the impact of instantaneous ten-day-sized price moves in the market risk factors. In the language of options, the ten-day holding period serves to calibrate the coverage of "gamma" risk.[9]

Second, the minimum historical observation period has come under question. Critics characterize the year-long minimum as intrusive and argue that longer observation periods have not been shown to result in more accurate value-at-risk estimates. In fact, however, the minimum historical observation period requirement primarily reflects concerns about the variability of the capital requirement across institutions, rather than a judgment by supervisors about the historical observation period likely to produce the most accurate value-at-risk estimates for capital or risk management purposes.[10]

The basic idea behind this requirement is that banks with similar risk exposures should face similar capital charges. In this regard, empirical evidence suggests that shorter observation periods tend to generate value-at-risk estimates that are more volatile over time (Hendricks 1996). Thus, for a set of banks with similar risk exposures, this result implies that the dispersion of value-at-risk estimates across banks will tend to be greater when some of the banks are using short observation periods. The minimum one-year historical observation period is an attempt to limit this disparity.

A third element of the new capital requirements that has proved controversial—indeed, more controversial than any other element—is the scaling factor. The scaling factor has been criticized as an ad hoc supervisory adjustment that undercuts the benefits of basing a capital charge on banks' internal models. In this view, the key advantage of using internal risk measurement models is that they

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provide more accurate measures of an individual bank's risk exposure than do broad supervisory measures. Accordingly, some have argued that a bank that can demonstrate convincingly that its model is accurate should be subject to a scaling factor of one.

In considering this argument, however, it is important to recognize that the overall purpose of the scaling factor is to produce the desired degree of coverage for the market risk capital charge. The market risk capital requirements are intended to ensure that banks hold sufficient capital to withstand the consequences of prolonged and/or severe adverse movements in the market rates and prices affecting the value of their trading portfolios. The key assumption behind the internal models approach is that a value-at-risk estimate calibrated to a ten-day, 99th percentile standard is well correlated with the degree of such risk inherent in the portfolio, and thus is a reasonable base for a minimum capital standard.

Nonetheless, by itself, even a perfectly measured ten-day, 99th percentile value-at-risk figure may not provide a sufficient degree of risk coverage to serve as a prudent capital standard. For one, such a standard implies that a bank is expected to have trading portfolio losses that exceed its required capital in one ten-day period out of a hundred, or about once every four years. An environment in which banks depleted their market risk capital so frequently could be highly unstable, particularly if such events happened to many banks at the same time (which could occur if banks adopted similar trading strategies). Further, value-at-risk estimates based only on recent historical market data may not incorporate the possibility of severe market events. Thus, a capital standard based on unadjusted value-at-risk estimates might not provide sufficient capital for a bank to withstand the effects of market breaks or unanticipated regime shifts.

The role of the scaling factor is to translate the value-at-risk estimates into an appropriate minimum capital requirement, reflecting considerations both about the accuracy of a bank's value-at-risk model and about prudent capital coverage. The capital cushion should cover possible losses due to market risk over a reasonable capital planning horizon—which is generally seen to reflect a period between one quarter and one year—while at the same time reflecting the fact that banks' trading positions change rapidly over time. As an alternative to the scaling factor, supervisors could have based the capital charge on value-at-risk estimates calibrated to a very stringent prudential standard (for example, a one-year holding period or a 99.999th percentile standard). In practice, however, it is very difficult to derive reliable and verifiable value-at-risk estimates for such extreme parameter values. Actual observations of such "tail events" are few, greatly complicating the task of verifying that any model is accurately measuring the probability of these occurrences. Thus, instead of representing a more "scientific" alternative to the scaling factor, a requirement of this kind would simply introduce a false sense of precision into the capital standards.

By contrast, the scaling factor has the advantage of being simple and easy to implement. It does not require banks to make (or supervisors to evaluate) complex calculations intended to model rare or as yet unobserved events, such as regime shifts or market breaks. At the same time, however, it does seek to provide a capital cushion against such incidents. In addition, it is similar to the techniques used by some banks for internal capital allocation, in which one-day value-at-risk estimates are extrapolated to a much longer holding period (for example, six months or one year) by multiplying by the square root of time (in the case of ten-day value-at-risk estimates, this calculation for a one-year holding period implies a multiplication factor of five). Moreover, comparisons of ten-day, 99th percentile value-at-risk estimates with banks' actual daily trading results suggest that the scaling factor of three provides an adequate level of capital coverage. The results of bank stress-testing programs were also a key input in the decision to use a scaling factor of three.

For additional protection, the market risk capital requirements incorporate a feature intended to ensure that models that systematically underestimate risk exposures are subject to a higher multiplication factor. This feature is the so-called backtesting requirement. Backtesting is a process of confirming the accuracy of value-at-risk models by comparing value-at-risk estimates with subsequent trading outcomes. For instance, an accurate model will produce one-day, 99th percentile value-at-risk estimates that are exceeded by actual trading losses only 1 percent of the time.

The backtesting procedures in the market risk capital requirements use a very simple statistical test based on the number of times during a year that trading losses exceed value-at-risk estimates. For purposes of the backtest, banks will compare daily end-of-day value-at-risk estimates calibrated to a one-day, 99th percentile standard with the next day's trading outcome. Each instance in which a trading loss exceeds the value-at-risk estimate is termed an exception. Since it is unlikely that an accurate model would produce a large number of exceptions, banks with five or more exceptions over a one-year period are subject to a higher scaling factor. The increase in the scaling factor is as large as 33 percent (from three to four) for banks with a very large number of exceptions.

The introduction of the higher scaling factor for banks experiencing five or more exceptions is based on a simple statistical technique that calculates the probability that an accurate value-at-risk model would generate a given number of exceptions during a year of trading days. In theory, these probabilities are independent of the design of any particular model, so the same number of exceptions is used as the starting point for the higher scaling factor across all banks. Overall, the backtest is calibrated to ensure that a bank with an accurate value-at-risk model is very unlikely to face an increased scaling factor. The relationship between the number of exceptions and the scaling factor is reported in Table 1.[11]

For technical reasons, the backtests conducted by banks may deviate from the ideal conditions assumed in the statistical derivation. For one, the trading gains and losses used in the backtest calculation may be based on the actual trading outcomes booked by the bank, and in that case will include fee income and the profits and losses from intraday trading. This means that the profit and loss figures used in the backtest could reflect influences not incorporated into the value-at-risk model, potentially introducing bias into the backtest results. The direction of the bias is not clear, however. On the one hand, including fee income in the profit and loss figures will tend to reduce the number of exceptions identified. On the other hand, the impact of intraday trading will likely increase the volatility of the daily profit and loss figures relative to the value-at-risk estimates, increasing the probability of an exception.

One possible response would be to require banks to calculate hypothetical profit and loss figures by holding end-of-day positions constant and excluding fee income. This calculation could become quite burdensome, however.

Table 1
BACKTESTING AND THE SCALING FACTOR

Number of Exceptions (Out of 250 Trading Days)	Scaling Factor	Cumulative Probability (Percent)
0 to 4	3.00	10.78
5	3.40	4.12
6	3.50	1.37
7	3.65	0.40
8	3.75	0.11
9	3.85	0.03
10 or more	4.00	<0.01

Note: The "cumulative probability" column reports the probability that an accurate model would generate more than the number of exceptions reported in the first column. These figures are generated using a binomial distribution, assuming a sample size of 250 trading days. For the purpose of the backtest, an accurate model is one that produces an accurate estimate of the 99th percentile of the distribution of one-day trading gains and losses. Thus, an accurate value-at-risk model will produce more than five exceptions over a 250-day trading period 4.12 percent of the time.

For this reason, and because the use of actual profit and loss figures in the backtest does not produce a clear bias in the test, banks are allowed to use the profit and loss information already at hand.

Finally, the backtest is calibrated to a one-day standard, whereas the value-at-risk estimates used for capital purposes are calibrated to a ten-day standard. Many commentators have pointed out that this difference introduces a discrepancy between the value-at-risk estimates validated in the backtest and the estimate actually used for capital purposes. Once again, the reasoning behind this specification reflects the practical limitations of testing value-at-risk estimates calibrated to a ten-day standard: backtesting such estimates would require a significant amount of historical data to generate a series of independent ten-day profit and loss figures. With only a limited number of such observations—just twenty-six over a one-year horizon—the power of the backtest to distinguish between accurate and inaccurate models is very limited. Thus, the supervisory backtest is calibrated to a one-day standard to strike a balance between the need to have a sufficient amount of data to give the backtest statistical power and the desire to determine the accuracy of the value-at-risk model used in the capital calculations.

CAPITAL REQUIREMENTS FOR SPECIFIC RISK

As noted earlier, the capital requirements for specific risk are intended to cover the risk of adverse price movements stemming from factors related to the issuer of an individual security. Thus, debt and equity positions in bank trading portfolios are assumed to be subject to specific risk. Under the original risk-based capital guidelines put forth in 1988, long debt and equity positions in a trading portfolio were subject to capital charges ranging from 0 percent (for government securities) to 8 percent (for corporate debt and equity) of the book value of the positions. Under the amended guidelines, both long and short debt and equity positions are covered by the market risk capital requirement for specific risk.

Banks whose value-at-risk models incorporate specific risk can use the specific risk estimates generated by their models.[12] Under the most recent announcement by the Basle Committee on Banking Supervision (1997), these model-based specific risk estimates are subject to a scaling factor of four until market practice evolves and banks can demonstrate that their models of specific risk adequately address both idiosyncratic risks and "event risks"

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that might not be captured in a value-at-risk model.[13] This provision holds out the prospect of harmonizing the specific risk capital requirements fully with the general market risk requirements as market practices with respect to positions subject to significant event risks become clearer. This approach is consistent with the view that there is no compelling conceptual reason to separate market risk into a general and a specific portion in a value-at-risk model, or to apply different standards to one portion than to another.

Impact of the Capital Requirements

Effect on Capital Levels and Capital Ratios

How the market risk requirements will affect banks' required capital ratios is difficult to calculate precisely with the data currently in the public domain. Such calculations require both information on banks' value-at-risk estimates—calibrated to the ten-day, 99th percentile supervisory standard—and information about the distribution of trading assets and liabilities among various specific risk categories. Despite the lack of such data, however, it is possible to make a rough estimate of the impact of the capital charge by using information reported in banks' annual reports.

Table 2 reports 1996 average value-at-risk estimates for a sample of large bank holding companies that presented annual average value-at-risk estimates in their 1996 annual reports along with sufficient descriptive detail to identify the holding period and percentile underlying the estimate.[14] As indicated in Table 2, all of the estimates were based on a one-day holding period, with percentiles ranging from the 95th to the 99th. The divergence in these parameters, as well as in other aspects of the estimates such as correlation assumptions, makes direct comparisons of these figures across institutions difficult.

Nevertheless, these figures suggest that the impact of the market risk capital charge on required capital levels and capital ratios is likely to be quite small. Using these numbers, we calculate that the estimated increase in the level of required capital from the general market risk component of the new capital charge ranges roughly between 1.5 and 7.5 percent for these banking companies. We find that the impact on the capital ratios is also fairly modest, with an average decline of about 30 basis points and 40 basis points in the tier 1 and total capital ratios, respectively. These calculations are at best rough estimates, however, and could differ significantly from the actual impact of the capital charge at the time it becomes effective. Such differences would reflect both estimation error in translating the reported figures to the supervisory stan-

Table 2
1996 Annual Average Value-at-Risk Estimates for Selected U.S. Bank Holding Companies

Bank Holding Company	1996 Average Daily VAR (Millions of Dollars)	Percentile Basis	Holding Period
BankAmerica	42[a]	97.5	1 day
Bankers Trust	39	99.0	1 day
Chase Manhattan	24[b]	95.0	1 day
Citicorp	45[c]	2σ	1 day
J.P. Morgan	21	95.0	1 day

Note: The average 1996 value-at-risk (VAR) figures are drawn from the companies' 1996 annual reports.

[a] Figure assumes a correlation of one between broad risk categories. The comparable figure assuming a correlation of zero is $18 million.

[b] Figure is based on the volatility of actual daily trading results, as reported in the 1996 annual report.

[c] The 2σ VAR figure is equivalent to the 97.7th percentile under a normal distribution.

dards and changes in the bank holding companies' portfolios over time.

Once we account for the capital treatment of specific risk, the overall impact of the market risk capital charge is likely to be even smaller than our calculations suggest. As noted earlier, many traded debt and equity positions subject to the credit risk capital requirements under the original capital guidelines are now subject to specific risk capital requirements based on the output of banks' internal models. This "specific risk carve-out" will offset the impact of the additional general market risk capital charge, possibly to a considerable degree. Unfortunately, the data needed to make reasonably precise estimates of this effect are not currently available. However, given the significant positions that some institutions hold in instruments that will become subject to the specific risk capital requirements, this carve-out may well result in a net *reduction* in required capital levels for some institutions.

ADVANTAGES OF THE INTERNAL MODELS APPROACH

Whatever the effect of the new standards on the level of overall required capital, capital requirements based on internal models should produce minimum regulatory capital charges that more closely match banks' true risk exposures. This closer relationship is important not only for determining the risk facing an institution at a particular moment in time, but also for tracing the evolution of risk over time. That is, while the value-at-risk estimates underlying the market risk capital charge are useful for assessing the *level* of risk undertaken by a bank or bank holding company at a given moment, they are potentially even more beneficial for understanding *changes* in risk exposure over time. By extension, the key benefit of the market risk capital charge is that the required capital levels will evolve with risk exposures over time.

In addition to tightening the link between risk exposures and capital requirements, a capital charge based on internal models may provide supervisors and the financial markets with a consistent framework for making comparisons across institutions. As the information in Table 2 makes clear, the value-at-risk figures presented in the annual reports of various bank holding companies are calculated using different parameters, especially the percentile of the loss distribution. These differences make comparisons across institutions difficult without additional calculations to convert the figures to a common basis. Typically, these calculations require assumptions that may be only approximately correct, introducing additional noise in the comparisons.

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By contrast, the market risk capital charge provides a common standard for value-at-risk estimates that makes comparisons across institutions easier and more reliable. The value-at-risk estimates underlying banks' capital charges will be based on a uniform set of prudential parameters and will accurately reflect the assumptions and specifications of each bank's internal model (rather than an external approximation). Further, the financial markets may gain information about the performance and accuracy of these models over time if banks make public the results of their backtests. While disclosure of the details of these results is purely discretionary, this backtesting information is consistent with the type of disclosures about market risks advocated in several recent discussion papers (see Bank for International Settlements [1994] and Federal Reserve Bank of New York [1994] for two examples).

CHALLENGES FOR SUPERVISORS

The actual benefits to be derived from the value-at-risk estimates depend crucially on the quality and accuracy of the models on which the estimates are based. To the extent that these models are inaccurate and misstate banks' true risk exposures, then the quality of the information derived from any public disclosure will be degraded. More important, inaccurate value-at-risk models or models that do not produce consistent estimates over time will undercut the main benefit of a models-based capital requirement: the closer tie between capital requirements and true risk exposures. Thus, assessment of the accuracy of these models is a key concern for supervisors.

The discussion of value-at-risk models in this paper might suggest that supervisory evaluation of banks' internal models is a daunting task, necessitating the hiring of large numbers of new staff with the same degree of technical and market expertise as the bank personnel responsible for developing and implementing the models. This interpretation is somewhat mistaken, however. Although the task of assessing value-at-risk models requires supervisors to maintain staff with a high degree of technical skill and experience in reviewing banks' trading operations, it is largely an extension of the activities routinely performed by U.S. bank supervisors in overseeing the trading operations of major banks. These activities have typically entailed review and assessment of the accuracy and appropriateness of the models used by banks for pricing, risk management, and general ledger profit and loss calculations. Thus, the basic procedures for evaluating value-at-risk models are similar to those that have been used by U.S. supervisors for some time in reviewing banks' trading activities. The procedures followed by examiners are also quite similar in spirit to the techniques used by auditors and accountants to assess the accuracy of the books and records of a banking institution.

As a first step, supervisors can turn to the internal auditing and certification processes used by the banks to validate the accuracy and performance of their models. The qualitative standards imposed by the market risk capital guidelines require independent validation of any models used to value positions or to measure the sensitivity of portfolios to market risk. As we have seen, the standards also call for an independent risk management unit and an independent internal or external audit of a bank's risk management processes. The results of these internal reviews provide supervisors with a valuable starting point for their own evaluation. The standards also mandate that the models be used as an integral part of a bank's risk management process—for instance, as part of daily management reports or as the basis of the bank's limit system. Because the models are used for purposes that go well beyond calculating regulatory capital levels, the interests of bank management in obtaining accurate value-at-risk estimates may be more closely aligned with the interests of supervisors.

Backtesting results—both those generated as required for supervisory capital purposes and additional results generated by institutions for internal validation and calibration—also provide supervisors with important information about the accuracy of value-at-risk models. Although the backtesting procedures incorporated in the market risk capital requirements are based on relatively simple statistical tests, researchers at the banks and elsewhere are actively investigating how to use ex post trading results to draw inferences about the accuracy and performance of value-at-risk models (see Kupiec [1995], Crnkovic and Drachman [1996], and Lopez [1997]). This work may lead to better and more powerful techniques for using these data to assess the accuracy of value-at-risk models.

Although the task of assessing value-at-risk models requires supervisors to maintain staff with a high degree of technical skill and experience in reviewing banks' trading operations, it is largely an extension of the activities routinely performed by U.S. bank supervisors in overseeing the trading operations of major banks.

In addition to drawing on these resources, supervisors rely on a dialogue with risk management staff at the bank in question and on a process of evaluating key assumptions and parameters of the models. Both the dialogue with the banks and the evaluation of the model parameters depend on having supervisory staff that can assess the technical work performed by a bank's risk management and trading staff. But while developing and retaining examiners with these skills is a key challenge for supervisors, the task is likely to become easier over time. Basic information about the structure and theoretical underpinnings of value-at-risk models is spreading, and the models are quickly becoming commonplace at financial (and nonfinancial) institutions. An understanding of these models is also emerging as a standard part of the skills acquired through academic and on-the-job training in finance and risk management. Thus, value-at-risk modeling is becoming a significantly less arcane area of both risk management and supervisory oversight.

Taken together, these factors suggest that supervisors have a broad arsenal of approaches to use in evaluating value-at-risk models. While experience over time will determine whether the information generated by these models is consistent and reliable, there is good reason to believe that the market risk capital requirements will yield information that is useful to both supervisors and market participants.

Implications for the Future

Market risk capital requirements based on internal models have drawn considerable attention since the initial proposal for these requirements was released in 1995. During this time, supervisory interest in value-at-risk models has encouraged banks in the United States and abroad to direct resources and attention toward the further development of these models and their fuller integration with the risk management process.

In the coming years, some of the key issues facing banks in value-at-risk modeling—and in risk management more generally—will concern the extension of these models to cover a broader range of the risks facing banking institutions. For example, can quantitative risk models be applied to credit, operational, and legal risks? And if so, should supervisors expand the use of their internal models to derive capital charges for these exposures? Interestingly, these issues have already surfaced in banks' efforts to model specific risk. Specific risk incorporates elements of both market risk and credit risk. In measuring specific risk, banks face a number of difficult technical and conceptual problems—how to measure the probability and likely impact of events that occur infrequently and how to quantify the effects of complex events that depend on the interrelated actions of many parties. These problems, which are at the frontier of thinking about regulatory capital and banks' internal capital allocation, will need to be resolved if quantitative risk models are to be used systematically to gauge other forms of risk.

At present, banks and other financial institutions are still in the early stages of developing methods for quantifying other types of risk and for integrating these risks into a unified capital allocation framework. Understanding the ways that risk models can and cannot be used is clearly one of the most significant challenges facing financial institutions and their supervisors today. The market risk capital requirements may further this understanding by providing a test case for the supervisory use of internal models.

Endnotes

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1. See Board of Governors of the Federal Reserve System (1994) for a description of the risk-based capital standards that apply to state member banks and bank holding companies. The standards for state nonmember banks and for national banks (administered by the Federal Deposit Insurance Corporation and the Office of the Comptroller of the Currency, respectively) are essentially identical.

2. Readers interested in the details of the market risk capital requirements should see Basle Committee on Banking Supervision (1996a) and U.S. Department of the Treasury, Federal Reserve System, and Federal Deposit Insurance Corporation (1996). The amended Basle Accord contains a second method for calculating market risk capital requirements that is not included in the U.S. guidelines. This second approach—the "standardized approach"—requires an institution to apply certain uniform techniques to calculate the capital charge for market risk. It is also important to distinguish the internal models approach contained in the U.S. guidelines from the so-called precommitment approach, which has been released for discussion by the Board of Governors of the Federal Reserve System and is being explored in a pilot project by the New York Clearing House (see Board of Governors of the Federal Reserve System [1995]). Under the precommitment approach, banks would have latitude to specify the amount of capital they wished to allocate to market risk, subject to penalties if subsequent trading losses exceeded this precommitted amount. This approach is one of several alternative methods that have been suggested for determining banks' capital requirements. For another, see Estrella (1995), who proposes capital supervision based on banks' internally determined "optimal" capital levels, in combination with a simple supervisory minimum.

3. The U.S. capital standards have recently been amended to require that a bank's capital be adequate to cover its overall exposure to interest rate risk. This determination is made as part of a bank's supervisory examination, rather than through a formal minimum capital requirement.

4. The exceptions are derivative positions, which continue to be subject to counterparty credit risk capital requirements.

5. As of the end of 1996, seventeen commercial banks met these criteria. These seventeen banks held nearly 98 percent of the trading positions (assets plus liabilities) held by all U.S. commercial banks. In addition, seventeen bank holding companies met the criteria, including the holding companies associated with fourteen of the seventeen banks. The actual number of institutions that are ultimately subject to the market risk capital requirements may differ from these figures, for two reasons: supervisors can, at their own discretion, include or exclude particular institutions, and institutions have the option to become subject to the capital requirements with supervisory approval.

6. See Jorion (1996) for a more detailed discussion of value-at-risk models. Hendricks (1996) compares the performance of several types of value-at-risk models.

7. To be precise, the capital charge for general market risk is equal to the greater of the sixty-day average value-at-risk estimate times the scaling factor *or* the previous day's value-at-risk estimate. As a practical matter, the previous day's value-at-risk estimate should rarely, if ever, exceed the sixty-day average times three.

8. Of course, some positions could take longer than ten days to liquidate. The extent to which a ten-day holding period is a suitable average would obviously depend on the characteristics of an individual portfolio.

9. Gamma risk arises from the fact that the sensitivity of an option's value to changes in the value of the option's underlying instrument will vary as the value of the underlying instrument changes.

10. Note, however, that the existing empirical evidence does not suggest substantial differences in the performance of value-at-risk models with varying observations periods.

11. For a full discussion of the use of backtesting in the market risk capital requirements, see Basle Committee on Banking Supervision (1996b). For a discussion of the statistical properties of backtesting and other methods of evaluating the accuracy of value-at-risk models, see Kupiec (1995) and Lopez (1997).

12. For banks whose value-at-risk models do not adequately incorporate specific risk, debt and equity positions in the trading portfolio are subject to a set of standardized specific risk charges, which apply to both long and short positions. These charges are added to the value-at-risk-based general market risk charge. The standardized charges are in many cases significantly lower than the original credit risk capital charges. For instance, an investment-grade corporate bond, which would have been subject to an 8 percent credit risk capital charge under the earlier guidelines, is now subject to a 1.6 percent specific risk charge.

13. There is a concern that measures of recent price variability may not provide a complete guide to the potential risk inherent in some positions—for example, illiquid positions that trade infrequently. This concern, together with the existence of differing market practices in this regard, has been a factor in shaping the interim approach to specific risk.

14. The institutions cited in Table 2 are used for illustrative purposes only. They do not represent an exhaustive list of the bank holding companies that reported value-at-risk estimates in their 1996 annual reports.

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The Benefits of Branching Deregulation

*Jith Jayaratne and Philip E. Strahan**

The Riegle-Neal Interstate Banking and Branching Efficiency Act, implemented in June 1997, enables banks to establish branches and buy other banks across the country. This legislation is the final stage of a quarter-century-long effort to relax geographic limits on banks. As recently as 1975, no state allowed out-of-state bank holding companies (BHCs) to buy in-state banks, and only fourteen states permitted statewide branching. By 1990, all states but Hawaii allowed out-of-state BHCs to buy in-state banks, and all but three states allowed statewide branching. The Riegle-Neal Act removes the remaining restrictions by permitting banks and BHCs to cross state lines freely.[1]

Although the effects of the recent federal legislation will be known only over time, we can study the impact of geographic restrictions on the banking industry by examining an earlier stage of the deregulatory process. The states were most active in removing geographic limits on banks in the fifteen years from 1978 to 1992. By observing the changes in banking that followed the state initiatives, we can learn much about the impact of these limits.[2] Previous research has suggested that geographic restrictions destabilized the banking system by creating small, poorly diversified banks that were vulnerable to bank runs and portfolio shocks (Calomiris 1993). In this article, we focus instead on the effect of the restrictions on the efficiency of the banking system.

We find that bank efficiency improved greatly once branching restrictions were lifted. Loan losses and operating costs fell sharply, and the reduction in banks' costs was largely passed along to bank borrowers in the form of lower loan rates. The relaxation of state limits on interstate banking was also followed by improvements in bank performance, but the gains were smaller and the evidence of a causal relationship less robust.

Our analysis suggests that much of the efficiency improvement brought about by branching was attributable

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to a selection process whereby better performing banks expanded at the expense of poorer performers. It appears that the branching restrictions acted as a ceiling on the size of well-managed banks, preventing their expansion and retarding a process of industry evolution in which less efficient firms routinely lose ground to more efficient ones.

While the improvements to the banking system following deregulation helped bank customers directly, we also find important benefits to the rest of the economy. In particular, state economies grew significantly faster once branching was allowed—in part, we suggest, because deregulation permitted the expansion of those banks that were best able to route savings to the most productive uses. Although it is uncertain whether the observed acceleration in economic growth will last beyond ten years, the stimulative effect of branching deregulation on the economy has been considerable.

A Brief History of Geographic Restrictions on Banking

States began imposing limits on branch office locations in the nineteenth century. Such limits were intended in part to prevent unscrupulous bankers from "choosing inaccessible office sites to deter customers from redeeming . . . circulating banknotes" (Kane 1996, p. 142). Geographic limits were also justified by the political argument that allowing banks to expand their operations freely could lead to an excessive concentration of financial power. Appearing before Congress in 1939, the Secretary of the Independent Bankers Association warned that branch banking would "destroy a banking system that is distinctively American and replace it with a foreign system . . . a system that is monopolistic, undemocratic and with tinges of fascism" (Chapman and Westerfield 1942, p. 238).

As late as 1975 only fourteen states allowed statewide branching. Twelve states prohibited branching altogether, and the remainder imposed restrictions of varying severity.

Inefficient banks probably supported these restrictions because they prevented competition from other banks. Economides, Hubbard, and Palia (1995) show that states with many weakly capitalized small banks favored the 1927 McFadden Act, which gave states the authority to regulate national banks' branching powers. The states themselves often benefited from exercising control over the supply of bank charters and the expansion of branch banking. Massachusetts and Delaware, for instance, received a majority of their state revenues from bank regulation in the early nineteenth century (Sylla, Legler, and Wallis 1987).

Geographic restrictions may not have seriously constrained the banking industry before the appearance of large corporations that required large-scale, multistate banking services. Rapid industrialization and the growth of transcontinental railroads after the Civil War, however, created firms whose need for comprehensive corporate financial services could not be met adequately by the existing system of fragmented unit banks. In response, banks formed "chain banks"—an alliance of several banks whose principal ownership rested with the same group of investors—after 1890. A few years later, "banking groups"—banks owned directly by a holding company—were created in an effort to get around branching restrictions (Calomiris 1993).

Nevertheless, branching restrictions persisted, and as late as 1975 only fourteen states allowed statewide branching. Twelve states prohibited branching altogether, and the remainder imposed restrictions of varying severity. Pennsylvania was representative of a partially restrictive state. Until 1982, Pennsylvania banks were allowed to branch only in the county where their head offices were located and in contiguous counties.

In addition to facing restrictions on in-state branching, banks have traditionally been limited in their ability to cross state lines. The Douglas Amendment to the 1956 Bank Holding Company Act prohibited a BHC from acquiring banks outside the state where it was headquartered unless the target bank's state permitted such acquisitions. Since no state allowed such transactions in 1956, the amendment effectively barred interstate banking organizations. Although states had the option to allow out-of-state BHCs to enter, none exercised that right until 1978, when Maine permitted such transactions. Even then, however, little changed: the Maine statute allowed an out-of-state BHC to buy a Maine bank only if the home state of the acquiring BHC permitted Maine-based BHCs the reciprocal right to buy banks there; since no other state allowed such entry, interstate bank organizations could not be formed. Banks could not in fact cross state borders until 1982, when Alaska, Massachusetts, and New York permitted out-of-state BHCs to enter.

MOVES TOWARD DEREGULATION

Maine's 1978 move to permit entry by out-of-state BHCs marked the beginning of a fifteen-year period in which the states relaxed barriers to bank expansion.[3] By the end of 1992, the state-level deregulatory process was essentially completed: all states but Arkansas, Iowa, and Minnesota allowed statewide branching, and all states but Hawaii permitted out-of-state BHCs to enter.

Table 1 chronicles the steps taken by individual states to eliminate geographic restrictions.[4] The first column presents the year in which each state authorized branching by means of merger and acquisition.[5] The second column reports the year in which each state first permitted interstate banking. In some cases, choosing a date for the authorization of branching was difficult, because the states often deregulated only gradually. In most cases, the date selected reflects the time at which the state finished the branching deregulation process.[6] In four cases, however, we chose dates earlier than the literal end of the process of deregulation because the remaining restrictions did not appear to impose a meaningful constraint on branching.[7]

FORCES OF CHANGE

Several developments contributed to the removal of the geographic barriers to bank expansion. In the mid-1980s, the Office of the Comptroller of the Currency took advantage of a clause in the 1864 National Bank Act to allow nationally chartered banks to branch freely in those states where thrifts did not face branching restrictions. The Comptroller's action was instrumental in introducing statewide branching in

Table 1
THE STATES REMOVE RESTRICTIONS ON GEOGRAPHIC EXPANSION

State	Intrastate Branching Deregulated	Interstate Banking Deregulated
Alabama	1981	1987
Alaska	Before 1970	1982
Arizona	Before 1970	1986
Arkansas	1994	1989
California	Before 1970	1987
Colorado	1991	1988
Connecticut	1980	1983
Delaware	Before 1970	1988
District of Columbia	Before 1970	1985
Florida	1988	1985
Georgia	1983	1985
Hawaii	1986	—
Idaho	Before 1970	1985
Illinois	1988	1986
Indiana	1989	1986
Iowa	—	1991
Kansas	1987	1992
Kentucky	1990	1984
Louisiana	1988	1987
Maine	1975	1978
Maryland	Before 1970	1985
Massachusetts	1984	1983
Michigan	1987	1986
Minnesota	1993	1986
Mississippi	1986	1988
Missouri	1990	1986
Montana	1990	1993
Nebraska	1985	1990
Nevada	Before 1970	1985
New Hampshire	1987	1987
New Jersey	1977	1986
New Mexico	1991	1989
New York	1976	1982
North Carolina	Before 1970	1985
North Dakota	1987	1991
Ohio	1979	1985
Oklahoma	1988	1987
Oregon	1985	1986
Pennsylvania	1982	1986
Rhode Island	Before 1970	1984
South Carolina	Before 1970	1986
South Dakota	Before 1970	1983
Tennessee	1985	1985
Texas	1988	1987
Utah	1981	1984
Vermont	1970	1988
Virginia	1978	1985
Washington	1985	1987
West Virginia	1987	1988
Wisconsin	1990	1987
Wyoming	1988	1987

Source: Chronology is based on information in Amel (1993).

Note: Before the passage of the Riegle-Neal Act, Iowa had not deregulated intrastate branching and Hawaii had not deregulated interstate banking.

several southern states. Another impetus behind deregulation may have been the rash of bank and thrift failures in the 1980s, which increased public awareness of the advantages of large, well-diversified banks (Kane 1996).

Kroszner and Strahan (1997) suggest that the emergence of new technologies in both deposit taking and lending encouraged the elimination of geographic barriers by changing the nature of banking markets. For instance, the introduction of the automated teller machine in the late 1970s and the development of money market mutual funds increased competitiveness in deposit markets. As a result, branching and interstate banking restrictions could no longer offer the same degree of protection from competition, making it less likely that banks would lobby for the preservation of these rules. At the same time, new information technologies diminished the value of the specialized knowledge that long-established local bankers might have had about the risks of borrowers in the community. This change enhanced the ability of banks to lend in more distant markets. Thus, a situation developed in which protected banks' incentive to defend restrictions on branching and interstate banking diminished over time, while expansion-minded banks' desire to see the restrictions fall increased.

The initiative to relax restrictions on interstate banking came primarily from larger banking organizations that were well equipped to pursue lower funding costs and better lending opportunities in neighboring states.

The initiative to relax restrictions on interstate banking came primarily from larger banking organizations that were well equipped to pursue lower funding costs and better lending opportunities in neighboring states. Their efforts may have succeeded in the 1980s because it became apparent that banks and nonbanks were already practicing interstate banking. As Savage (1993) argues, "the proliferation of loan production offices, nonbank subsidiaries of bank holding companies, nonbank banks, and interstate thrift institutions, the widespread use of credit cards, and the provision of financial services by nonfinancial firms not subject to geographic limitations all made the traditional restrictions on the geographic expansion of banks more difficult to explain and justify. If so many financial services could be provided across state lines by these various means, why shouldn't deposit-taking institutions be allowed to expand as well?"

The breakdown of the geographic constraints on banks over the last twenty years has had a significant impact on the industry. Branching deregulation has prompted banks to enter new markets (Amel and Liang 1992), persuaded BHCs to consolidate their subsidiaries into branches (McLaughlin 1995), and forced smaller institutions to exit banking (Calem 1994). Interstate banking activity has increased dramatically, boosting the percentage of deposits held by out-of-state BHCs in the typical state from 2 percent to 28 percent between 1979 and 1994 (Berger, Kashyap, and Scalise 1995). Interstate banking has also intensified the demands placed on bank management: the compensation of managers is now tied more closely to bank performance, and the turnover rate among banks' chief executive officers has increased (Hubbard and Palia 1995).

In addition to prompting changes in the organization of the industry and the behavior of individual banks, deregulation has had profound effects on the overall performance of the banking system. The next section looks at the impact of deregulation on two components of bank performance: the costs of providing services and the prices charged customers for those services.

Deregulation, Cost Efficiency, and Prices

Did banks perform better when they were permitted to operate statewide branch networks and to build multistate bank holding companies? We investigate this question by examining whether bank costs—as measured by loan losses (net loan charge-offs divided by total loans) and noninterest costs (noninterest expenses divided by total assets)—declined after deregulation, creating a more efficient system. We also examine changes in loan prices (interest income on loans and leases divided by total loans and leases) to determine whether bank customers are better off following deregulation. We look at state-level data for the 1978-92 period to summarize the impact of deregulation on the overall performance of the banking system.

To understand how we arrive at our measures of the cost efficiency of the banking system, consider New York in 1978. We construct the charge-offs ratio by dividing the sum of loans charged off by all banks operating in New York in 1978 by the sum of all loans held by New York banks in 1978. We construct similar aggregates for the noninterest expense and loan price variables in each state and year in the sample.[8] The data for these performance measures are derived from the year-end Reports of Condition and Income, filed by all banks with the federal banking agencies.

We use regression techniques to estimate the impact of deregulation on bank costs and loan prices. (For a detailed discussion of these calculations, see Box 1.) The regression methods allow us to control for other factors that might influence our measures of bank cost and loan prices—most notably, the health of the state's economy. Bank costs, particularly those related to loan defaults, generally move with the business cycle: borrowers tend to pay off loans during boom times but are less able to do so during recessions. If states deregulated branching and interstate banking during hard times, average measures of costs could improve after deregulation as states' economies recovered from recession. A simple before-and-after comparison of bank performance would show an improvement in bank loan portfolios and profitability after deregulation, but these advances would largely reflect the timing of deregulation. We address this possibility by controlling

BOX 1: AN EMPIRICAL MODEL OF BANK PERFORMANCE

Using the dates of deregulation reported in Table 1, we construct two indicator variables equal to 1 for states permitting branching and interstate banking. We then use these indicator variables to estimate the effects of the policy changes in the following regression model:

$$y_{t,i} = \alpha_t + \beta_i + \gamma_1 branch_{t,i} + \gamma_2 bank_{t,i} + \varepsilon_{t,i} \ ,$$

where $y_{t,i}$ equals one of our two cost measures or our measure of loan prices in the ith state in year t, $branch_{t,i}$ is an indicator equal to 1 for states without restrictions on branching, and $bank_{t,i}$ is an indicator equal to 1 for states that have entered into an interstate banking agreement.

In this specification, β_i measures the state-specific component of banking performance, α_t measures the effects of the national business cycle at time t, and γ_1 and γ_2 measure the changes in performance stemming from the two types of deregulation. In constructing the deregulation indicators, we drop the year in which the deregulation went into effect. We also drop Delaware and South Dakota from the analysis entirely. These two states experienced a dramatic expansion in their banking sectors during the 1980s when credit card operations relocated there to take advantage of liberal usury laws. As a result, performance measures for banks in these two states do not reflect their branching laws, but rather the health and profitability of the credit card business.

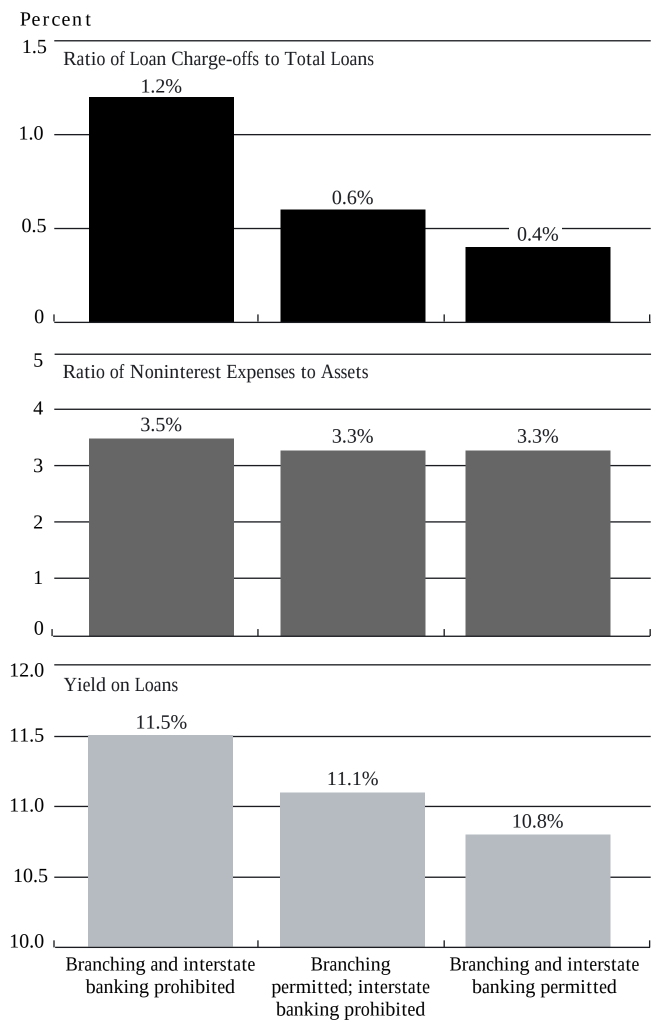
We then use the regression model to construct average predicted values for our two cost measures and our measure of loan prices in different regulatory environments. Consider charge-offs. We estimate the predicted value of this variable for each state and year for each of three regulatory configurations: one in which both branching and interstate banking are fully regulated ($branch_{t,i}$ = 0 and $bank_{t,i}$ = 0), one in which branching is permitted but interstate banking is not ($branch_{t,i}$ = 1 and $bank_{t,i}$ = 0), and one in which both branching and interstate banking are permitted ($branch_{t,i}$ = 1 and $bank_{t,i}$ = 1). This gives us a panel of predicted values for each state and year in each of the three regulatory environments. We then compute the simple average predicted charge-off ratio (across states and years) for each regulatory configuration and report each of those three averages in Chart 1 in the text. The statistical significance reported in the text is derived by testing the hypothesis that γ_1 and γ_2 estimated from the above regression equal zero.

for the national business cycle in our regressions.[9]

Our analysis suggests that loan losses, noninterest expenses, and loan rates decreased significantly once statewide branching was allowed—even after we adjust for the influence of the business cycle on bank performance and for persistent cross-state differences in bank performance.[10] Chart 1 reports the average levels of the cost and price measures that would have been observed during the 1978-92 sample period under three alternative regulatory regimes: (1) restrictions in place on both branching and interstate banking, (2) branching permitted but interstate banking prohibited, and (3) both branching and interstate banking permitted. The top panel suggests that if no state had allowed either statewide branching or interstate banking between 1978 and 1992, the ratio of charge-offs to total loans in the typical state in a typical year would have been 1.2 percent. Had all states allowed statewide branching but prohibited interstate banking in our sample period, average charge-offs in the typical state would have fallen by half, to 0.6 percent.[11] The ratio of noninterest expenses to assets would have fallen from 3.5 percent to 3.3 percent if branching had been permitted throughout the period (middle panel). It appears that most of these reduced costs were passed along to bank borrowers in the form of lower loan rates, which in our estimates declined from 11.5 percent to 11.1 percent on average (bottom panel).[12] Each of these improvements is statistically significant at the 5 percent level.[13]

Chart 1

Costs and Interest Rates Are Lower in Deregulated Environments



Source: Authors' calculations, based on data from Federal Financial Institutions Examination Council, Reports of Condition and Income.

Note: Chart shows the average level of price and performance measures that would have been observed in the 1978-92 period had all states been subject to the regulatory regimes identified along the x-axis.

Foes of bank deregulation and consolidation have argued that the increasing concentration in the banking industry could enhance market power. While measures of concentration at both the state and national levels have increased in recent years following deregulation, concentration at local levels has remained remarkably constant (Rhoades 1996). If enhanced market power were a problem, we would see both increased concentration and higher prices at the local level following deregulation, neither of which has occurred. It is true that our estimates indicate that bank costs have fallen more than revenues, suggesting an increase in industry profitability. Similarly, estimates of the impact of deregulation on banks' return on equity and return on assets in another study (Jayaratne and Strahan forthcoming) showed small increases in profitability that were sometimes statistically significant (at the 10 percent level) and sometimes not. Nevertheless, it appears that most, or perhaps all, of the cost reductions from deregulation are passed along to customers. There is little evidence that deregulation has increased market power.

Our regression analysis also shows that some modest improvements in bank performance have followed the introduction of interstate banking. Although operating costs do not decline at all (Chart 1, middle panel), charge-offs fall from 0.6 to 0.4 percent of total loans when interstate banking is allowed in addition to statewide branching (top panel), and the average interest rate falls from 11.1 percent to 10.8 percent (bottom panel).

The evidence of gains following interstate banking deregulation, however, is much less robust than the evidence of improvements following branching deregulation. When we control for state business cycles (by including lags of state-level personal income growth) as well as national business cycles, we see no statistically significant improvements following interstate banking. This finding suggests that the observed gains might stem from favorable banking conditions at the time of deregulation rather than from deregulation itself. Alternatively, robust evidence of performance improvements following interstate banking may be lacking because most states entered interstate banking agreements around the same time, making it difficult to distinguish the effects of deregulation from the effects of other changes. Because of this statistical problem, we cannot determine whether interstate banking had a significant impact on bank performance. Consequently, we focus on branching deregulation in the remainder of the article.

Robustness of the Performance Improvements

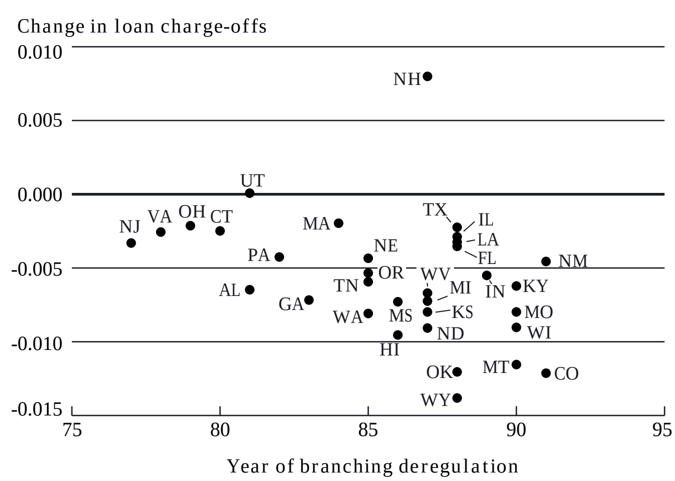
A possible explanation for the observed reduction in loan losses and loan rates is that banks made fewer risky loans following branching deregulation. If the output mix of banks changed from riskier to safer loans following deregulation, then we might expect to observe declines in both loan losses and loan rates. Changes in banks' output could also explain declines in noninterest expenses if, for instance, banks provided fewer checking accounts (which are relatively costly for banks to maintain) following deregulation. To investigate this possibility, we estimate the effects of deregulation on noninterest expenses, loan losses, and loan prices while controlling for banks' output mix. In each case, we find that the improvements in costs and the reductions in loan losses and loan prices after branching deregulation remain statistically significant even after controlling for the output mix. We also find no decrease in two risky loan categories—credit cards and commercial loans—following branch deregulation, suggesting that banks did not shift to safer loans after deregulation.[14]

It is possible, however, that within each loan category banks are making safer loans after deregulation than they did before. So, even though the volume of credit card loans and commercial loans has remained fairly constant, after deregulation the loans themselves may be less risky. This is unlikely for two reasons. First, evidence suggests that, if anything, banks *increased* their risk taking after geographic deregulation because eliminating entry barriers reduced banks' franchise value (Keeley 1990). Second, as we indicate below, banks with higher profits and fewer loan losses grew faster than banks with lower profits and more loan losses once branching was permitted. Declines in loan losses seem to reflect not a change in the inherent riskiness of the pool of borrowers but better screening and monitoring of borrowers by the banking system.

We have established that bank performance in the average state improved following statewide branching. But did banks in only a few states experience improvements, or was the phenomenon widespread? To answer this question, we look at the changes in bank cost efficiency in individual states (Chart 2). Specifically, we plot the change in banks' ratio of charge-offs to total loans before and after deregulation relative to the corresponding change for the group of states that did not deregulate their branching laws during the period. This "control group" of states is used to remove the

Chart 2

Loan Charge-offs Fall after Branching Deregulation in All but Two States



Source: Authors' calculations, based on data from the Federal Financial Institutions Examination Council, Reports of Condition and Income.

effects of nationwide shocks to bank performance. The control group consists of the eleven states that are identified in Table 1 as having deregulated in or before 1970 and the three that are identified as not having deregulated as of 1992.[15]

Reductions in loan losses following branching deregulation are widespread; in all states but New Hampshire and Utah, charge-offs decline after deregulation relative to the change in charge-offs experienced by states that did not deregulate branching during the period.

The change in loan charge-offs for each of the thirty-three deregulating states appears as a single point plotted above the year of deregulation for that state; multiple points appear above a year when more than one state deregulated in that year. Consider the example of Pennsylvania, represented by the single point plotted in 1982. This state's mean charge-off ratio rose by about 0.3 percentage point after deregulation in 1982, while all states that did not change policy in 1982 experienced a 0.7 percentage point increase in charge-offs after 1982. We therefore report a relative decline in charge-offs of 0.4 percentage point for Pennsylvania.

As the chart shows, reductions in loan losses following branching deregulation are widespread; in all states but New Hampshire and Utah, charge-offs decline after deregulation relative to the change in charge-offs experienced by states that did not deregulate branching during the period. Similar pictures emerge for both loan prices and noninterest expenses. For loan prices, we find declines following branching deregulation in twenty-five cases out of thirty-three. Again, New Hampshire is a significant outlier.[16] We find that noninterest expenses fall in nineteen out of the twenty-four deregulating states available for this analysis, again relative to the control group of states.

WHY DEREGULATION IMPROVES BANK EFFICIENCY

Limits on bank expansion could have had adverse effects on efficiency in banking for at least three reasons. First, prohibitions on branching and interstate banking may have limited the opportunity for the best run banks to grow. In unregulated markets, more efficient firms have a natural tendency to gain market share over their less productive competitors, an outcome that will increase average efficiency as the industry evolves over time. By preventing better run banks from establishing branches, and by preventing BHCs from expanding across state lines, these regulations may have retarded this natural evolution. After the geographical constraints were lifted, the more efficient banks may have expanded, thereby improving the performance of the average banking asset. We call this the *selection hypothesis*.

Second, limited restrictions on geographic expansion may have weakened the discipline that markets usually place on managers of corporations. When interstate banking is prohibited, managers worry less about takeovers. Because their jobs are more secure, they may also be less motivated to increase shareholder value, maximize efficiency, and minimize costs. According to this *disciplining hypothesis*, efficiency in banking improves after deregulation because managers are forced to increase shareholder value in order to preserve their jobs. Note that the disciplining hypothesis predicts that all

Prohibitions on branching and interstate banking may have limited the opportunity for the best run banks to grow.

banks will improve their performance following deregulation, since managers at all banks will come under greater pressure. By contrast, the selection hypothesis predicts that the more efficient banks will gain market share, not that the efficiency of all individual banks will improve.

A third possible reason why efficiency might improve following deregulation is that barriers to geographic expansion prevent banks from operating at the most efficient size. There is some evidence, for instance, that small banks can reduce average costs by expanding up to about $500 million in total assets (Berger, Hunter, and Timme 1993). According to the *economies of scale hypothesis*, the efficiency of the banking system will improve after deregulation as small banks grow and reduce costs. Of course, according to this view, all of the benefits come from changes occurring at the lower end of the bank size distribution. Since small banks hold a relatively small share of total banking assets, these benefits would likely be small.

Which of these three explanations best accounts for the efficiency gains observed following deregulation? We can rule out the economies of scale explanation on two grounds. First, there is scant evidence of scale economies in banking beyond about $500 million in total assets (Berger, Hunter, and Timme 1993). The large improvements that we have found in the state-level aggregates cannot plausibly be attributed to the fact that small banks are moving closer to the optimal scale. In 1980, for instance, banks with under $500 million in assets (in 1994 dollars) held less than 30 percent of total assets in the banking system. Second, we have estimated the change in our performance measures following branching deregulation for small banks (those with assets under $100 million) and large banks separately. We find that the improvements are greater for large banks than for small, a finding inconsistent with the economies of scale explanation.[17]

More difficult to evaluate is the hypothesis that management discipline accounts for the beneficial effects of branching deregulation. Because we lack good measures of the degree of managerial effort at banks, we cannot test this hypothesis directly. Nevertheless, we cannot reject the possibility that disciplining played some role in the improved efficiency of banks. Hubbard and Palia (1995) find evidence of greater managerial discipline following interstate banking: the turnover rate for banks' chief executive officers rises and the pay-performance relation tightens once states allow interstate banking. Hubbard and Palia contend that these changes result from a more active market for corporate control after deregulation. Such changes may well have disciplined management to improve bank performance, although neither this article nor the Hubbard and Palia study establishes this point.

The remaining explanation for bank efficiency gains, the selection hypothesis, can readily be tested. To do so, we examine whether better run banking companies grow faster than their less efficient rivals following branching deregulation. First, we classify banks on the basis of their profitability just before deregulation. We then observe the change in the market share after deregulation for the high-profit banking companies. If the selection hypothesis is correct, we should find that profitable banks increase their market share at the expense of unprofitable banks following deregulation.

Specifically, for each state, we first rank banking companies from highest to lowest according to their return on equity at the end of the year prior to the year of deregulation. Next, we go down that ranking until we reach a bank that, together with all previous banks, accounts for 50 percent of the state's bank assets. The banking companies in this group constitute our high-profit firms.[18] We then calculate the group's share of state bank assets five years after branching deregulation.[19] As implied by the selection hypothesis, we find that the high-profit banking companies grow faster after branching deregulation (Table 2, row 1); their share of banking assets increases, on average, by 8.5 percentage points (from 51.3 percent to 59.8 percent)—a statistically significant increase.[20]

Table 2

BETTER BANKS INCREASE THEIR MARKET SHARE AFTER BRANCHING DEREGULATION

	Initial Market Share of High-Profit Banks (Percent)	Market Share of High-Profit Banks Six Years Later (Percent)	Increase in Share (Percentage Point Change)
Post-deregulation period	51.3	59.8	8.5 (3.91)**
Pre-deregulation period	49.9	51.7	1.8 (0.99)

Source: Authors' calculations, based on data from Federal Financial Institutions Examination Council, Reports of Condition and Income.

Notes: The table reports the change in the share of total bank assets held by that half of the banking companies with the highest return on equity at the beginning of the specified six-year period. The post-deregulation period begins the year before the year of deregulation; the pre-deregulation period begins seven years before the year of deregulation. The t-statistic reported below the market share change for each period tests the hypothesis that the change equals zero.

*Statistically significant at the 10 percent level.

**Statistically significant at the 5 percent level.

Of course, we would expect banks enjoying high profits and good loan portfolios to grow relatively faster at all times, even when branching restrictions are in place. In other words, the fact that banks with good balance sheets grow faster than less profitable banks need not indicate that deregulation caused the weaker banks to lose ground. To isolate the effects of deregulation on selection, we compare the differential growth rates of high- and low-profit banks in a deregulated environment with the same differential growth rates in a regulated environment.[21]

A striking contrast is evident in the growth rates achieved in regulated and deregulated environments (Table 2). High-profit banks increase their market share by only 1.8 percentage points (from 49.9 to 51.7 percent) in the average state over the pre-deregulation period (Table 2, row 2). This change is so small that we cannot reject the possibility that high-profit banks do not increase their market share at all over the six-year period before deregulation (that is, 1.8 percent is not a statistically significant change). In the post-deregulation period, by contrast, the market share of the high-profit banks rises sharply. In sum, the evidence in Table 2 strongly supports the hypothesis that branching deregulation forced a process of selection whereby weaker banks lost ground to better run banks.[22]

High-profit banking companies grow faster after branching deregulation; their share of banking assets increases, on average, by 8.5 percentage points—a statistically significant increase.

Deregulation and Economic Growth

Thus far we have argued that relaxation of geographic restrictions improved the performance of the banking system, enhancing the efficiency of the average bank asset and improving bank lending. How did these changes affect the rest of the economy? Earlier research has shown that countries with better developed banking systems grow faster because savings are channeled into the highest-return investments (King and Levine 1993). Banks can help to route savings to the most productive uses in two ways. First, they provide information about the profit potential of different businesses, channeling savings toward good projects and away from bad. Second, banks monitor those firms with which they have lending relationships to ensure that bank funds are put to proper use (Diamond 1984).[23]

Branching deregulation should enhance the ability of banks to direct savings to the best projects and to oversee the successful execution of those projects. As we have seen, banks function better after branching deregulation, and their loan losses decrease sharply. The selection hypothesis suggests that these improvements occur because banks that are better able to screen and monitor loans are able to expand their operations at the expense of less effectively managed banks after deregulation. As a result, the economy can grow faster because savings flow more consistently into profitable investment opportunities.

The Effect on State Economies

To investigate whether state-level rates of economic growth did in fact increase following branching deregulation,[24] we estimate the change in the average growth rate of two measures of economic activity: real per capita personal income and real per capita gross state product.[25] These two measures differ somewhat in concept: Personal income reflects the income of a state's residents, providing a measure of residents' welfare. Gross state product, by contrast, measures the total incomes of factors of production located within the state, allowing us to assess the economic activity that actually occurs there.[26] As in our estimates of the effects of branching deregulation on bank performance, we control for both business cycle effects and the effects of differences in the long-run growth rate across states.[27] Our tests of the effects of branching deregulation on the state economies show a significant acceleration in growth: annual personal income grows about 0.51 percentage point faster after branching deregulation, and gross state product, about 0.69 percentage point faster (Table 3, row 1). This acceleration is not only statistically significant at the 5 percent level but is also economically "large" relative to the 1.6 percent annual average growth rate of real per capita personal income over the sample period.

Of course, there is uncertainty associated with this estimate—with a 5 percent probability of error, we can only be confident that personal income growth increased somewhere between 0.06 and 0.97 percentage point. Moreover, these figures are estimated under the assumption that the growth pickup persists indefinitely. One possibility is that the economy benefits for a few years as the banking system becomes more efficient, then growth returns to the level that prevailed before the policy change.

We disentangle the short- and long-run effects of deregulation on growth by assessing the average growth rate following deregulation during three distinct time periods (Table 3, rows 2-4). We measure the change in the growth rate during the first five years after branching deregulation, the change in growth relative to the years before deregulation during years five to ten, and the change from years eleven and beyond. We find that the beneficial effects of the policy change are greatest during the first ten years. Personal income growth accelerates by 0.35 percentage point in the first five years and by 0.37 percentage point in the next five years. But after ten years, our estimate of the growth effect falls to 0.17 percentage point and is no longer statistically significant. In the gross state product series, however, the increases in growth appear to last beyond ten years. (See Box 2 for a detailed discussion of the growth regressions used to generate these results.)

Table 3
STATES' ECONOMIC GROWTH ACCELERATES AFTER BRANCHING DEREGULATION

	Change in Personal Income Growth (Percentage Point)	Change in Gross State Product Growth (Percentage Point)
(1) Overall increase in growth	0.51	0.69
	(2.22)**	(2.09)**
(2) Increase in growth, years 1-5	0.35	0.60
	(1.75)*	(2.07)**
(3) Increase in growth, years 5-10	0.37	0.65
	(1.85)*	(2.41)**
(4) Increase in growth, years 10+	0.17	0.67
	(0.89)	(2.48)**

Source: Jayaratne and Strahan (1996), Tables 2 and 5, rows 3 and 7.

Note: The t-statistics are given in parentheses.

*Statistically significant at the 10 percent level.

**Statistically significant at the 5 percent level.

Annual personal income grows about 0.51 percentage point faster after branching deregulation, and gross state product, about 0.69 percentage point faster.

Overall, we lack conclusive evidence on whether the growth effects persist beyond ten years. This limitation is not surprising, however, since we observe only about ten years of growth experience after deregulation for most states. Nevertheless, even if the observed increases in growth do not continue indefinitely, the short-run effects appear to be large.[28]

ROBUSTNESS OF THE GROWTH ACCELERATION

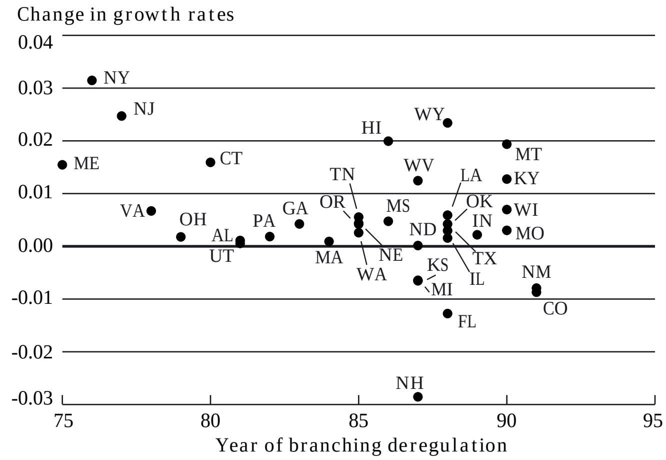
Did many states experience a growth pickup in the wake of branching deregulation or was the change concentrated among a few? To evaluate whether the effects were widespread, we offer a state-by-state assessment of the growth in personal income. Chart 3 plots the average change in growth for each of the thirty-five states that deregulated their branching restrictions relative to the average change in growth for the nonderegulating states. (The latter group of states, as in Chart 2, is used to control for nationwide changes in growth.) Like Chart 2, Chart 3 plots these growth changes by the year of deregulation.

The growth acceleration following deregulation is clearly a general phenomenon. Twenty-nine of the thirty-five states that deregulated performed better than the nonderegulators. (The exceptions are New Hampshire, Florida, Michigan, Kansas, Colorado, and New Mexico.) Even when the deregulating states experienced growth declines following branching, the nonderegulators generally fared even worse. This pattern suggests that when a downturn was occurring in the national business cycle at the time of branching deregulation, the downturn was at least partly offset by the positive effects of statewide branching.

We have shown that rates of economic growth increased following branching deregulation. The increase is both statistically large, which suggests that we can be confident that it is not the result of chance, and economically large, which suggests that over time economic welfare would be raised dramatically as a consequence of the accelerated growth. The growth acceleration is also widespread, benefiting twenty-nine of the thirty-five deregulating states. The remaining question, however, is whether deregulation actually caused the growth pickup. Establishing causal relationships is always difficult in empirical economics because researchers cannot run controlled

Chart 3

Personal Income Growth Rates Accelerate after Branching Deregulation in All but Six States



Source: Authors' calculations.

BOX 2: AN EMPIRICAL MODEL OF GROWTH

To estimate the effects of branching deregulation on growth, we use the following model:

$$\begin{aligned} Y_{t,i} / Y_{t-1,i} &= \alpha_t + \gamma^5 D^5_{t,i} + \gamma^{10} D^{10}_{t,i} + \gamma^{10^+} D^{10^+}_{t,i} \\ &+ \mu_1 [Y_{t-1,i} / Y_{t-2,i}] + \mu_2 [Y_{t-2,i} / Y_{t-3,i}] \\ &+ \mu_3 [Y_{t-3,i} / Y_{t-4,i}] + \delta Y_{t-1,i} + \varepsilon_{t,i} , \end{aligned}$$

where $Y_{t,i}$ is a measure of real per capita income (output), $D^5_{t,i}$ is a branching indicator equal to 1 for states that allowed statewide branching at most five years ago, $D^{10}_{t,i}$ is a branching indicator equal to 1 for states that allowed statewide branching six to ten years ago, and $D^{10^+}_{t,i}$ is a branching indicator equal to 1 for states that allowed statewide branching more than ten years ago.

In this specification, the γ coefficients measure the increase in per capita economic growth stemming from branching deregulation at different time periods. The α_t terms measure the common, economy-wide shocks to growth such as the national business cycle. The μ terms capture the effects of the state-specific business cycle, and δ reflects the extent to which poorer states grow faster (the "convergence effect" observed in Barro and Sala-I-Martin [1992]).

We estimate the model with a variety of different specifications. The simplest uses ordinary least squares (OLS). The model is also estimated by weighted least squares (WLS), with weights proportional to the size of the state economy at the beginning of the period. We use WLS because measurement error in state economic data—particularly in data relating to interstate commerce—is likely to be greater for smaller states. Smaller states are also more likely to depend on a limited number of industries, leading to greater susceptibility to industry-specific shocks. In all cases we report heteroskedasticity-consistent standard errors (White 1980).

While there is no a priori reason to suspect that regional business cycles will introduce a bias, we also present estimates from an augmented version of the above model allowing the time effects (that is, the business cycle effects) to vary across four broad regions of the United States. This specification is included mainly as a robustness check. Table 1 in the text shows that many states in the South and Midwest deregulated around the same time, leading to the possibility that regional business cycle effects drive the estimate of the growth effect coefficients. To control for the regional business

Box 2: An Empirical Model of Growth *(Continued)*

cycle, we modified the above model slightly by interacting the year-fixed effect with four regional indicator variables (for the Northeast, South, West, and Midwest).

The table below presents the results of estimating these models. Almost all specifications show that the increase in growth after branching deregulation lasts up to ten years, but only half the models show a growth increase beyond ten years.

State Economies Grow More Rapidly after Branching Deregulation

	Growth Effect: Years 1-5 (1)	Growth Effect: Years 6-10 (2)	Growth Effect: Years 10+ (3)	$Growth_{t-1}$ (4)	$Growth_{t-2}$ (5)	$Growth_{t-3}$ (6)	Lag of Per Capita Income (7)	Adjusted R^2 (8)
GROWTH BASED ON PERSONAL INCOME								
Basic model, OLS	0.59**	0.86**	0.34	0.14*	-0.03	-0.04	-0.38**	0.52%
	(0.23)	(0.23)	(0.22)	(0.08)	(0.06)	(0.08)	(0.13)	(1,015)
Basic model, WLS	0.61**	0.86**	0.34**	0.20**	0.06	0.04	-0.29**	0.73%
	(0.21)	(0.22)	(0.16)	(0.05)	(0.04)	(0.04)	(0.08)	(1,015)
Regional effects, OLS	0.35	0.37*	0.17	0.08	-0.03	0.02	-0.29**	0.64%
	(0.20)	(0.20)	(0.19)	(0.08)	(0.07)	(0.08)	(0.11)	(974)
Regional effects,WLS	0.31**	0.38**	0.21	0.16**	0.04	0.07	-0.28**	0.79%
	(0.16)	(0.19)	(0.13)	(0.05)	(0.04)	(0.05)	(0.09)	(974)
GROWTH BASED ON GROSS STATE PRODUCT								
Basic model, OLS	0.77**	0.94**	0.63**	0.21**	0.09*	0.03	-0.07**	0.41%
	(0.30)	(0.30)	(0.27)	(0.06)	(0.05)	(0.07)	(0.03)	(521)
Basic model, WLS	0.64**	0.83**	0.48*	0.21**	0.13**	0.06	-0.09**	0.62%
	(0.26)	(0.33)	(0.26)	(0.05)	(0.06)	(0.07)	(0.03)	(521)
Regional effects, OLS	0.60**	0.65**	0.67**	0.15**	0.06	0.07	-0.04*	0.50%
	(0.29)	(0.27)	(0.27)	(0.06)	(0.05)	(0.07)	(0.02)	(500)
Regional effects, WLS	0.43**	0.57**	0.59**	0.23**	0.11**	0.08	-0.08**	0.69%
	(0.21)	(0.24)	(0.24)	(0.04)	(0.04)	(0.07)	(0.03)	(500)

Source: Jayaratne and Strahan (1996), Table 5.

Notes: The table presents estimates of the increase in state economic growth following relaxation of intrastate branching restrictions. Delaware is dropped from all regressions used to produce these estimates while Alaska and Hawaii are dropped from the regressions with regional effects. In addition, the year in which each state deregulated was dropped. Growth data for personal income are from 1972-92 and for state product from 1981-91 (three years are lost with the addition of the lagged dependent variables). In column 8, the number of observations appears in parentheses below the R^2. In columns 1-7, standard errors appear in parentheses below the coefficients. Reported standard errors are heteroskedasticity-consistent (White 1980). The coefficients on the branching indicators and the lag of income are multiplied by 100.

*Statistically significant at the 10 percent level.

**Statistically significant at the 5 percent level.

experiments. Nevertheless, we must consider other factors that could explain our finding. One possibility is that state governments instituted a variety of new policies at the same time that they deregulated their banking systems. If so, these policy changes could be responsible for the improved growth performance.

We find no evidence of such coincident policy changes. The political control of state governments did not change significantly around the time of branching deregulation. In only two cases out of thirty-five did control of both houses of the state legislature and the governorship pass from one political party to the other during the four-year election cycle leading up to branching deregulation. The political affiliation of both houses of the state legislature changed only six times out of thirty-five during the four-year window before branching deregulation.

Moreover, even after controlling for two measures of state fiscal policy—the ratio of public investment by the state government to total income and the ratio of tax receipts by the state government to total income—we continue to find a significant growth acceleration after branching deregulation. Our tests suggest that there were no changes in states' tax and other fiscal policies that coincided with branching deregulation and that could explain the observed increase in state economic growth following statewide branching.

Another possible explanation for our finding is that state legislatures relaxed branching restrictions in anticipation of faster growth and the need to finance attractive projects. Why might this be the case? Perhaps when a state has strong growth prospects, potential bank borrowers pressure state governments to deregulate their banking systems. But if states deregulated branching rules because they anticipated the need to finance a future economic boom, then we should see a sharp rise in bank lending following deregulation. Jayaratne and Strahan (1996) demonstrate, however, that no increase in lending occurred. Moreover, the growth effects of branching deregulation remain largely unchanged even after we control for loan growth.

Finally, we consider the possibility that some unobserved set of technological changes led to branching deregulation, improved bank performance, and increased economic growth. For example, increased competition from nonbank financial institutions clearly helped to spur the removal of barriers to branching. Perhaps such financial innovations also forced banks to improve their performance and boosted states' economic growth. Two considerations, however, lead us to discount this possibility. First, if this explanation were true, we would see an improvement in bank performance and increased economic growth immediately before, as well as after, deregulation. Our data show no such pattern.[29] Second, any technological changes that occurred around the time of deregulation should have affected all states. In that case, we should not see any improvement in bank performance nor any increase in economic growth in deregulating states relative to nonderegulating states. Our data, of course, provided clear evidence of such differences in the experiences of the states.

To summarize, the large increase in bank loan quality in conjunction with little or no change in loan growth suggests that the increase in states' economic growth was at least partly due to statewide branching. The improvements in banking stemming from selection (and possibly disciplining) appear to have had important beneficial effects on the economy.

Conclusion

Restrictions on bank branching have proved to be very costly. By preventing the more efficient banks from expanding at the expense of their less efficient rivals, these restrictions retarded the "natural" evolution of the industry. As our analysis has shown, once state branching restrictions were lifted, the efficiency of the banking system improved as the better banks expanded into new markets. Bank borrowers benefited from lower loan rates, while the overall economy grew faster as banks did a better job separating the good projects from the bad and monitoring firms after lending relationships had been established. State restrictions on interstate banking may have created similar constraints, although our statistical procedure has a harder time identifying such effects.

The Riegle-Neal Act removes the remaining geographic barriers to bank expansion and permits the creation of multistate banking franchises. This federal legislation may produce benefits similar to those achieved through state deregulation—reduced bank costs, lower loan rates, and accelerated economic growth. Nevertheless, it is possible that the latitude given banks to create branches and buy out-of-state banks over the last two decades may have already weeded out weaker institutions and exhausted the benefits of geographic deregulation. Whether there is additional room for improved efficiency through the process of selection remains to be seen.

ENDNOTES

1. Although the act gives each state the right to prevent out-of-state banks from owning branches there, only Texas and Montana have chosen to do so.

2. Several types of geographic restrictions have been imposed over the years on banks, but this article focuses on limits on banks' ability to establish branches within their home states and on limits on BHCs' ability to acquire banks outside their home states. We do not consider other restrictions, such as those prohibiting the formation of multibank BHCs, primarily because we lack the necessary data.

3. Although some states removed barriers to branching before 1978 (see Table l), most of the state deregulatory activity was concentrated in the 1978-92 period. The focus on this period also enables us to take advantage of the greater availability of bank data after 1978.

4. We include Delaware and South Dakota in Table 1, but we exclude them from our analysis (see Box 1).

5. Many states also permitted de novo branching after permitting banks to branch through mergers and acquisitions. We do not emphasize de novo branching powers because bank expansion into new markets generally occurs through the purchase of whole banks or branches of banks located in those new markets, not through the opening of new branches.

6. Information on the timing of states' deregulatory initiatives is taken from Amel (1993).

7. For instance, in 1982 Pennsylvania passed a law permitting banks to branch in the home office county, in a contiguous county, in a bicontiguous county, or in the counties of Allegheny, Delaware, Montgomery, and Philadelphia. In 1990, Pennsylvania permitted unrestricted branching statewide. In the results presented below, we assume that by 1982 Pennsylvania permitted intrastate branching (despite the fact that the process was not finished until eight years later) because the effect of the 1982 law brought Pennsylvania so close to complete intrastate branch freedom. We follow a similar practice for Ohio, Virginia, and Washington. Our results are not sensitive to the alternative dating of deregulation in these four states.

8. The noninterest expense variable equals total noninterest expenses incurred by all banks in a state divided by total banking assets held by banks in that state. The loan price variable equals interest earned on all loans and leases in a state divided by total loans plus leases held on bank balance sheets in that state.

9. When we control for the state business cycle, the estimated effects of statewide branching decrease but are still both statistically significant and economically important.

10. The long-run average level of bank loan losses may differ across states because banks operating in states dominated by particularly high-risk industries will exhibit higher loan losses. Oil states such as Texas, Alaska, and Louisiana, for instance, exhibited loan losses that exceeded the national average during our sample period. Improvements in loan quality after deregulation could therefore reflect a tendency for states dominated by high-risk industries to deregulate their branching and interstate banking restrictions later than the typical state. We accounted for this possibility by controlling for persistent cross-state differences in bank performance.

11. We find declines in loan loss provisions and nonperforming loans of similar magnitude following branching deregulation. See Jayaratne and Strahan (forthcoming).

12. We find no change in deposit interest rates following deregulation, however. All of the cost declines seem to be passed along to bank borrowers rather than depositors.

13. The estimates of the effects of deregulation on our performance measures are based on a regression model that assumes that the changes occur immediately following deregulation and are permanent. Because we have only five to ten years of experience after deregulation for most states, we cannot be sure that these effects will continue indefinitely. Nevertheless, we find that the observed improvements in bank performance persist more than five years after branching deregulation.

14. These results are reported in Jayaratne and Strahan (forthcoming).

15. New York and Maine are dropped from this analysis because they deregulated before loan charge-off data became available. As noted earlier, Delaware and South Dakota are dropped throughout the analysis.

16. New Hampshire eliminated its branching restrictions in 1987, just before the beginning of the New England banking crisis. This sequence of events might explain why bank performance is observed to deteriorate after deregulation.

17. These results are available on request.

18. When we substitute loan charge-offs for return on equity as a measure of bank quality, we obtain similar results. To conserve space, however, we do not include these results in this article. In addition, we do not include noninterest expenses in this analysis, because the data are available beginning only in 1984. The lack of earlier data means that we can conduct the exercise in Table 2 for only three deregulating states using noninterest expense data.

19. We chose this window length because most of the observed changes in bank structure occurred within five years after branching deregulation.

Note 19 continued
For example, nearly two-thirds of the 30 percent increase in the state-level bank asset concentration occurred within five years after branching deregulation. Similar results are reported in Berger, Kashyap, and Scalise (1995), who find that most changes to bank structure occur within five years after geographic deregulation. (Some states entered interstate banking agreements during the five-year window. For these states, we use the year just prior to the year in which the state entered the interstate banking agreement as the end of the window. We dropped four states—West Virginia, Tennessee, Oregon, and New Hampshire—that entered interstate banking agreements in the same year or one year after branching was deregulated.)

20. Although high-profit banks are defined to have 50 percent of a state's bank assets at the beginning of the deregulation period, we can only approximate this target because no group of banks in a state will contain exactly one half of that state's total bank assets. Thus, in Table 2, high-profit banks are shown to have 51.3 percent of the average state's bank assets, not 50 percent.

21. We define high-profit banking companies before deregulation in much the same way we defined high-profit banking companies after deregulation. Banking companies are identified as high-profit on the basis of their return on equity at the end of the year seven years before the year of deregulation. We then measure their change in market share over the next six years.

22. Recall that we found only weak evidence that overall bank profits increased after branching deregulation. This earlier finding does not conflict with the fact that high-profit banks grew faster than low-profit banks. Two forces are operating. Because the high-profit banks tend to grow at the expense of their less efficient competitors after deregulation, aggregate profits should increase, all else being equal. At the same time, however, because the high-profit banks are likely to have achieved their superior growth rates in part by charging customers less, aggregate profits should drop. These two forces are approximately offsetting: thus, overall profits changed little following deregulation.

23. For instance, banks write loan covenants that restrict firms' ability to engage in certain activities during periods of financial distress. The writing and exercising of such covenants allow banks to monitor their borrowers effectively (Morgan 1995).

24. We focus here on branching deregulation, rather than interstate banking, because once we controlled for the business cycle, we found sharp improvements in bank performance associated with statewide branching but not with interstate banking. Although we looked for evidence of changes in economic growth associated with interstate banking, we found none.

25. Statistics on personal income and gross state product are published annually by the U.S. Department of Commerce. Annual state population figures are from the U.S. Bureau of the Census. We convert nominal personal income to constant dollars using a national price deflator, the consumer price index.

26. The difference between personal income and gross state product is apparent in how the two measures treat capital income. Capital income is allocated to personal income according to the state of residence of the owner of capital, while for gross state product, capital income is allocated according to the physical location of the capital itself. Real per capita personal income grew 1.6 percent per year during our analysis period (1972-92), while gross state product grew 1.4 percent per year between 1978 and 1992. (Because the Commerce Department changed the base year for the industry price deflators in 1977, we could not construct a consistent growth series prior to 1978 using gross state product.)

27. To control for regional business cycle effects, we include a set of time dummy variables that vary across four broad regions. For details, see Jayaratne and Strahan (1996), Table 2.

28. Note that there are theoretical reasons to believe that reductions in financial market frictions can increase the steady-state growth rate of the economy. For a survey of the relevant models, see Galetovic (1994) and Pagano (1993).

29. These results are available from the authors upon request.

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What Moves the Bond Market?

*Michael J. Fleming and Eli M. Remolona**

To what extent can movements in the financial markets be attributed to the arrival of new information? In a landmark 1989 study of the stock market, David Cutler, James Poterba, and Lawrence Summers found that it was surprisingly difficult to identify information that could account for the largest price movements. No similar effort has been made, however, to explain the largest price movements in the bond market, although both theory and a large literature on announcement effects suggest that the results for this market should be more promising.

In this article, we take a close look at a single year in the U.S. Treasury securities market (which we refer to as the bond market) and attempt to identify information that may account for the sharpest price changes and the most active trading episodes. Sharp price moves may be attributed to changes in expectations shared by investors, and surges in trading activity to a lack of consensus on prices.[1] To explain the price changes and trading surges, we examine how closely these events correlate with the release times of macroeconomic announcements.

We also investigate whether the bond market's behavior is related to factors affecting the informational value of the announcements—specifically, the type of announcement and the magnitude of the surprise in the data released. While other studies have examined announcement effects in the bond market, our use of high-frequency market data and precise announcement release times allows us to identify such effects more precisely than most earlier studies. In addition, our analysis of the role of uncertainty in assessing the impact of macroeconomic announcements goes beyond the scope of earlier bond market studies. To represent the bond market in our analysis, we focus on the five-year U.S. Treasury note, one of the most actively traded U.S. Treasury securities.

For the period examined—August 23, 1993, to August 19, 1994—we find that each of the twenty-five

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sharpest price changes and each of the twenty-five greatest trading surges can be associated with a just-released announcement. We also show that the market differentiates among announcements containing different information, with the employment, producer price index (PPI), federal (fed) funds target rate, and consumer price index (CPI) announcements eliciting the most pronounced responses in terms of both price movements and trading activity. In addition, our precise data allow us to document for the first time a significant market impact from U.S. Treasury security auction results. Finally, we demonstrate that the market's reactions depend on the surprise component of a given announcement and on conditions of market uncertainty.

PREVIOUS STUDIES

The literature on announcement effects in the stock and bond markets is quite extensive. Our review of this literature serves two purposes: it pulls many of the different strands of the literature together for the first time and it suggests the extent to which our empirical results—based on a one-year sample—can be generalized to other periods.

STOCK MARKET STUDIES

Theory says that movements in financial asset prices should reflect new information about fundamental asset values. In the case of the stock market, however, such theory has been difficult to confirm. Most notably, in an analysis of the fifty largest one-day price moves in the Standard and Poor's Composite Stock Index since 1946, Cutler, Poterba, and Summers (1989) find that in most cases the information cited by the press as causing the market move "is not particularly important." In earlier studies, Schwert (1981), Pearce and Roley (1985), and Hardouvelis (1987) find little evidence that the stock market responds to macroeconomic news other than monetary information (such as money supply and discount rate announcements). More recently, McQueen and Roley (1993) find a stronger relationship between stock prices and news after controlling for different stages of the business cycle. Even with their best effort, however, McQueen and Roley are able to explain only 3.9 percent of the daily variation in the S&P 500 Index.

The apparently weak informational effects found in the stock market are not entirely surprising. Much of the observable information likely to be relevant to the stock market as a whole takes the form of macroeconomic announcements. The theoretical effects of such announcements are often ambiguous for stocks, but not for bonds. The reason is that stock prices depend on both cash flows and the discount rate, while bond prices—for which cash flows are fixed in nominal terms—depend *only* on the discount rate. An upward revision of expected real activity, for example, raises the discount rate for both stocks and bonds, which would reduce prices. At the same time, however, the revision raises expected cash flows for stocks, an outcome that increases stock prices. The net effect on bond prices of such an announcement is clearly negative, but the net effect on stock prices will depend on whether the cash flow effect or the discount rate effect dominates.

Theory says that movements in financial asset prices should reflect new information about fundamental asset values. In the case of the stock market, however, such theory has been difficult to confirm.

BOND MARKET STUDIES

Earlier findings on announcement effects in the bond market suggest that it will be easier to relate this market's movements to information arrival.[2] Indeed, studies over the years have documented a significant bond market impact from numerous macroeconomic announcements, including money supply, industrial production, PPI, CPI, unemployment rate, and nonfarm payroll employment numbers (Table 1). Market movements in these studies are typically based on daily interest rates, and announcements are measured by the extent of the surprise each entails—that is, the difference between the forecast and the actual number released. Forecasts are either derived by the studies' authors from the time series of the variables or generated by the market analysis firm MMS International Inc. from surveys conducted a few days before the announcements.

Table 1
STUDIES FINDING THAT MACROECONOMIC ANNOUNCEMENTS SIGNIFICANTLY AFFECT INTEREST RATES

Announcement	Study Author	Sample Period
Money supply	Berkman (1978)	Jul. 1975 - Jun. 1977
	Grossman (1981)	Sep. 1977 - Sep. 1979
	Urich and Wachtel (1981)	Jan. 1974 - Dec. 1977 Jan. 1979 - Sep. 1979
	Cornell (1982, 1983)	Oct. 1979 - Dec. 1981
	Roley (1982)	Sep. 1977 - Nov. 1981
	Roley (1983)	Sep. 1977 - Oct. 1982
	Roley and Troll (1983)	Sep. 1977 - Oct. 1982
	Urich and Wachtel (1984)	Nov. 1977 - Jul. 1982
	Roley and Walsh (1985)	Oct. 1979 - Oct. 1982
	Hardouvelis (1988)	Oct. 1979 - Aug. 1984
	Dwyer and Hafer (1989)	Feb. 1980 - Dec. 1981 Jan. 1983 - Dec. 1983
	Thornton (1989)	Jan. 1978 - Jan. 1984
	Strongin and Tarhan (1990)	May 1980 - Jan. 1984
	McQueen and Roley (1993)	Sep. 1977 - May 1988
Industrial production	Roley and Troll (1983)	Sep. 1977 - Oct. 1979
	Harvey and Huang (1993)	Dec. 1981 - Apr. 1988
	McQueen and Roley (1993)	Sep. 1977 - May 1988
	Edison (1996)	Feb. 1980 - Feb. 1995
Producer price index	Urich and Wachtel (1984)	Oct. 1979 - Jul. 1982
	Smirlock (1986)	Oct. 1979 - Dec. 1983
	Hardouvelis (1988)	Oct. 1979 - Aug. 1984
	Dwyer and Hafer (1989)	Feb. 1980 - Dec. 1980
	McQueen and Roley (1993)	Sep. 1977 - May 1988
	Edison (1996)	Feb. 1980 - Feb. 1995
Consumer price index	Smirlock (1986)	Oct. 1979 - Dec. 1983
	Hardouvelis (1988)	Oct. 1982 - Aug. 1984
	McQueen and Roley (1993)	Sep. 1977 - May 1988
	Edison (1996)	Feb. 1980 - Feb. 1995
Durable goods orders	Hardouvelis (1988)	Oct. 1982 - Aug. 1984
Retail sales	Hardouvelis (1988)	Oct. 1982 - Aug. 1984
	Edison (1996)	Feb. 1980 - Feb. 1995
Unemployment rate	Hardouvelis (1988)	Oct. 1982 - Aug. 1984
	Cook and Korn (1991)	Feb. 1985 - Apr. 1991
	McQueen and Roley (1993)	Sep. 1977 - May 1988
	Prag (1994)	Jan. 1980 - Jun. 1991
	Edison (1996)	Feb. 1980 - Feb. 1995
Nonfarm payroll employment	Cook and Korn (1991)	Feb. 1985 - Apr. 1991
	McQueen and Roley (1993)	Sep. 1977 - May 1988
	Edison (1996)	Feb. 1980 - Feb. 1995
	Krueger (1996)	Feb. 1979 - Apr. 1996

Notes: The table lists those studies that have found a statistically significant relationship between the surprise component of an announcement and U.S. interest rates. For studies that examine the impact on several interest rates, we consider only the results for the longest maturity rate. Studies are not listed in which the impact of an announcement is found to have a sign opposite to that
predicted.

The literature provides evidence of a "flavor-of-the-month" aspect to the bond market's behavior, in which different announcements are regarded as important in different periods. Starting with Berkman (1978), studies from the late 1970s to the mid-1980s document a significant impact of money supply announcements. However, Dwyer and Hafer (1989) show a diminishing significance for such announcements in the mid-1980s. Studies in the 1980s, such as Urich and Wachtel (1984) and Smirlock (1986), begin to demonstrate the importance of the PPI, CPI, and unemployment rate announcements. More recent studies, particularly Cook and Korn (1991) and Krueger (1996), establish the ascendant importance of the nonfarm payrolls number in the Bureau of Labor Statistics' (BLS) employment report.

It is noteworthy that the bond market studies that consider several announcements tend to find that relatively few of them have significant effects on the market.[3] One possible reason for this finding is that the daily interest rate data on which these studies rely are not of sufficiently high frequency to capture the market's reaction cleanly. As Hardouvelis (1988) points out, researchers ought to measure the market change from just before to just after the announcement. Another possible reason for the lack of significance is that the effect of a given announcement surprise may vary even over short periods of time, depending on what else is going on in the economy. Prag (1994), for example, shows that the effect of unemployment rate announcements on interest rates depends on the existing level of unemployment.

Earlier findings on announcement effects in the bond market suggest that it will be easier to relate this market's movements to information arrival.

Bond Market Studies Using Intraday Data

The recent availability of high-frequency intraday price data has increased the power of researchers' efforts to estimate announcement effects. Ederington and Lee (1993), for instance, use such data on Treasury bond futures to examine the impact of monthly economic announcements. They find that nine out of sixteen announcements have significant price effects, with the greatest impact coming from the employment, PPI, CPI, and durable goods orders releases. More recently, Fleming and Remolona (1997) analyze intraday cash market Treasury securities data and find that eight out of nineteen announcements have a significant impact on price and eleven out of nineteen have a significant impact on trading volume. Instead of measuring surprise components, both studies rely on dummy variables for announcement days to isolate the announcements' effects. They therefore measure the average impact of the announcements without regard for the particular numbers released in any given report.

If an announcement's impact depends only on the unexpected part of the released information, then accounting for the sign and magnitude of the unexpected component should improve the estimates of announcement effects. Nonetheless, intraday studies relying on such surprises do not identify more significant announcements than do studies relying only on announcement dummy variables. For example, Becker, Finnerty, and Kopecky (1996) find that nonfarm payroll employment and CPI surprises affect the fifteen-minute returns on bond futures significantly, while housing starts and merchandise trade surprises do not. In addition, Balduzzi, Elton, and Green (1996) conclude that surprises from only six of twenty-three monthly announcements have a significant price impact on the ten-year U.S. Treasury note.

Studies of Trading Activity

Much of the research on trading activity has been limited to the stock market, with the early literature focusing on the difference between the effects of earnings announcements on prices and the effects on trading activity. Beaver (1968) argues, for example, that stock price movements in weeks of earnings announcements reflect "changes in the expectations of the market as a whole" while surges in trading activity reflect "a lack of consensus regarding the price." Morse (1981) provides evidence that earnings announcements affect daily trading volume, but Jain (1988) finds that macroeconomic news has no effect on hourly trading volume. Moreover, Woodruff and Senchack (1988) find that the effects of earnings announcements on prices and trading volume depend on the magnitude of the surprises.

As hypothesized by Beaver (1968), an increase in trading activity after announcements may largely reflect differences of opinion among market participants.[4] Other literature on trading activity has focused on the idea that both price changes and trading activity reflect the arrival of private information.[5] The conveyance of private information through trading is probably not that important in the bond market, however, since much of the information relevant to the market is released to the public through scheduled announcements. An explanation for changes in trading activity that is more pertinent to the bond market is that investors with duration targets or dynamic hedging strategies rebalance their portfolios after price changes.[6]

In summary, macroeconomic announcements cannot account for the largest price moves in the stock market and, in fact, are typically found to have an insignificant impact on stock prices. In contrast, numerous studies find a significant impact on bond prices, although no study prior to this one has explicitly tried to account for the largest price movements. As for the effects of announcements on trading activity, differences of opinion among traders or portfolio rebalancing might lead to a surge in trading activity after a release, but studies have been limited largely to the stock market and the results so far have been mixed.

Methodology and Data

Analytical Approaches

Our analysis of the U.S. Treasury securities market combines the different approaches offered by the literature on announcement effects. First, we follow Cutler, Poterba, and Summers (1989) in examining the largest price changes and determining the extent to which these changes coincide with the release times of announcements. Second, like Ederington and Lee (1993), we run dummy-variable regressions to measure

We employ high-frequency price and trading data from the U.S. Treasury securities market, as well as data on the dates and exact release times of various macroeconomic announcements. These data allow us to correlate market movements closely with information releases and to identify the market impact of announcements precisely.

the extent to which the market systematically differentiates among the different types of announcements to reflect the inherent differences in the information released. Third, we follow Becker, Finnerty, and Kopecky (1996) and other studies in investigating whether measured surprises in the announcements help explain the market's responses. Finally, following McQueen and Roley (1993), we analyze the possible effects of market conditions on the impact of a given announcement surprise.

In applying each of these approaches, we employ high-frequency price and trading data from the U.S. Treasury securities market, as well as data on the dates and exact release times of various macroeconomic announcements. These data allow us to correlate market movements closely with information releases and to identify the market impact of announcements precisely. In addition, we utilize data on the market's expectations for each announcement in our analyses of the effects of announcement surprises. Finally, we depend on quantitative measures of uncertainty for our analysis of the impact of market conditions. The specific data we use are described in detail in the rest of this section.

U.S. Treasury Securities Data

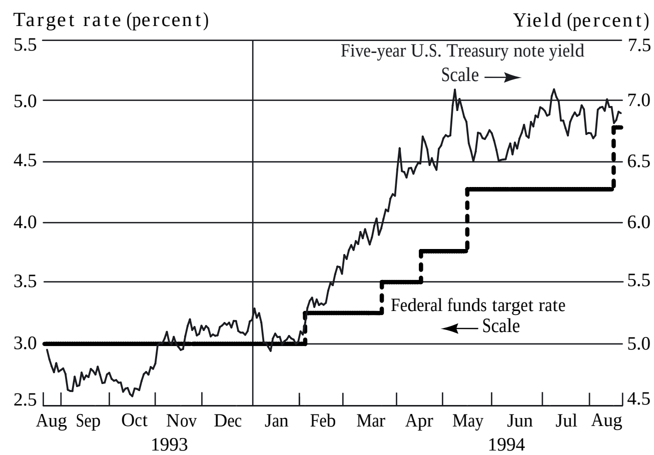
Our U.S. Treasury securities data cover one year of tick-by-tick trading activity in the interdealer broker market. Our data source is GovPX, Inc., a joint venture set up by the primary dealers and interdealer brokers in 1991 to improve the public's access to U.S. Treasury securities prices (*Wall Street Journal* 1991). GovPX consolidates and posts real-time quote and transaction data from five of the six major interdealer brokers, which together account for roughly two-thirds of the interdealer broker market. Posted data include the best bids and offers, trade prices and sizes, and the aggregate volume of trading for all Treasury bills, notes, and bonds. GovPX data are distributed electronically to the public through several on-line vendors.

Our sample period runs from August 23, 1993, to August 19, 1994, giving us a year with 250 trading days after excluding ten holidays. The period is somewhat unusual in that it covers a time when the Federal Reserve was particularly active in monetary tightening, raising its fed funds target rate five times (Chart 1). We choose the on-the-run five-year U.S. Treasury note to represent the U.S. Treasury securities market in our analysis. On-the-run

Chart 1

Federal Funds Target Rate and Five-Year U.S. Treasury Note Yield

August 23, 1993, to August 19, 1994



Sources: Federal Reserve Bank of New York; GovPX, Inc.

Note: Federal Open Market Committee meeting dates are indicated by the blue vertical lines.

securities are the most recently issued securities of a given maturity and account for the majority of interdealer trading volume.[7] Fleming (1997) reports that among the on-the-run issues, the five-year note is the most actively traded security among the brokers reporting to GovPX. During our sample period, GovPX posted a daily average of 2,167 bid-ask quotations and 659 trades for this note.[8]

Announcement Dates and Release Times

We also collected data on the dates and release times of twenty-one different macroeconomic announcements (Table 2). These include the nineteen monthly announcements that regularly appear in "The Week Ahead" section of *Business Week*, as well as fed funds target rate announcements and announcements of U.S. Treasury security auction results.[9] Nineteen of the announcements come from government agencies and two come from the private sector. Eighteen of the nineteen monthly announcements are released at regularly scheduled times of the day, with ten released at 8:30 a.m. eastern time, one at 9:15 a.m., six at 10 a.m., and one at 2 p.m.[10] Announcement times vary for one monthly announcement (consumer credit), for the fed funds target rate announcements, and for the Treasury security auction results announcements. We rely on Bloomberg for the precise release times of these announcements.

As for release dates, consumer confidence is the first report to be released with information about a given month and is actually released at the end of the same month it is covering (Chart 2). The NAPM survey, the other private-sector report in our sample, is typically the next report released—on the first business day of the month following the month covered. The employment report, usually released on the first Friday of the month, is the first government report to be announced with informa-

Table 2
MACROECONOMIC ANNOUNCEMENTS

Time	Short Title	Full Title	Reporting Entity
8:30 a.m.	Consumer price index (CPI)	Consumer Price Index	Bureau of Labor Statistics
8:30 a.m.	Durable goods orders	Advance Report on Durable Goods Manufacturers' Shipments and Orders	Bureau of the Census
8:30 a.m.	Employment	The Employment Situation	Bureau of Labor Statistics
8:30 a.m.	Gross domestic product (GDP)	Gross Domestic Product	Bureau of Economic Analysis
8:30 a.m.	Housing starts	Housing Starts and Building Permits	Bureau of the Census
8:30 a.m.	Leading indicators	Composite Indexes of Leading, Coincident, and Lagging Indicators	Bureau of Economic Analysis
8:30 a.m.[a]	Personal income	Personal Income and Outlays	Bureau of Economic Analysis
8:30 a.m.	Producer price index (PPI)	Producer Price Indexes	Bureau of Labor Statistics
8:30 a.m.	Retail sales	Advance Retail Sales	Bureau of the Census
8:30 a.m.	Trade balance[b]	U.S. International Trade in Goods and Services	Bureau of the Census, Bureau of Economic Analysis
9:15 a.m.	Industrial production and capacity utilization	Industrial Production and Capacity Utilization	Federal Reserve Board
10 a.m.	Business inventories	Manufacturing and Trade: Inventories and Sales	Bureau of the Census
10 a.m.	Construction spending	Value of New Construction Put in Place	Bureau of the Census
10 a.m.	Consumer confidence	Consumer Confidence Index	Conference Board
10 a.m.	Factory inventories	Manufacturers' Shipments, Inventories, and Orders	Bureau of the Census
10 a.m.	NAPM survey	National Association of Purchasing Management Report on Business	National Association of Purchasing Management
10 a.m.	New single-family home sales	New One-Family Houses Sold and For Sale	Bureau of the Census
2 p.m.	Federal budget	Treasury Statement (the Monthly "Budget")	Department of the Treasury
Varies[c]	Consumer credit	Consumer Installment Credit	Federal Reserve Board
Varies[d]	Federal funds target rate	N.A.	Federal Reserve Board
Varies[e]	Treasury security auction results	Treasury Security Auction Results	Department of the Treasury

Notes: The table reports the announcement time, title, and reporting entity for eighteen regularly scheduled announcements and three announcements with varying release times. All times are eastern.

[a]Personal income was reported at 10 a.m. for the first three announcements in the period of analysis and at 8:30 a.m. thereafter.

[b]This report replaced the Census Bureau's Report of U.S. Merchandise Trade in March 1994.

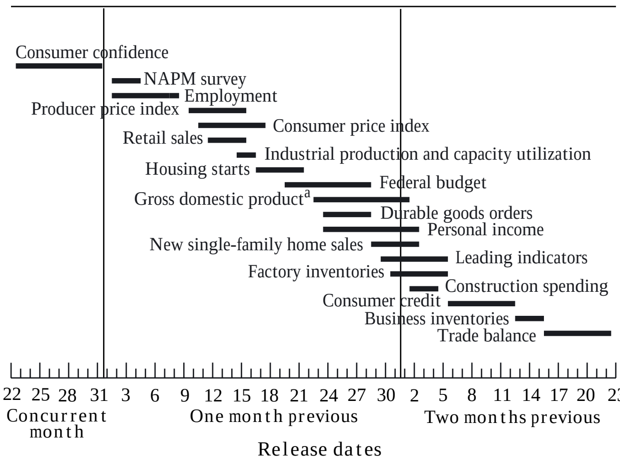
[c]Eight of the twelve announcements in our sample were made at 4 p.m. The others were made at 2:12 p.m., 2:45 p.m., 3:14 p.m., and 3:55 p.m.

[d]The six announcements in our sample were made at 10:06 a.m., 11:05 a.m., 1:17 p.m., 2:18 p.m., 2:20 p.m., and 2:26 p.m.

[e]All of the auction results in our sample were announced between 12:30 p.m. and 2:15 p.m., with most reported between 1:30 p.m. and 2 p.m.

Chart 2

Macroeconomic Announcement Release Dates
August 23, 1993, to August 19, 1994



Sources: *Business Week*; Office of Management and Budget, *Schedule of Release Dates for Principal Federal Economic Indicators* (1993, 1994); Bloomberg L.P.

Note: The chart shows the range of release dates for scheduled monthly economic announcements and indicates the month of economic data included in the report.

[a]Although gross domestic product is a quarterly statistic, the advance, preliminary, and final estimates are released in successive months. The advance statistic is released roughly one month after the end of the quarter.

tion about a given month.[11] It is followed by releases of the PPI, the CPI, retail sales, and industrial production and capacity utilization. The remaining twelve monthly reports are released in the second half of the month following the month covered, or in the month after that.

Our year of data contains twelve releases for each of the nineteen monthly announcements. In 1994, the Federal Reserve began making fed funds target rate announcements, the first one at its February 1994 Federal Open Market Committee (FOMC) meeting. This study provides the first intraday analysis of the fed funds target rate announcements, of which there are six in our sample.[12] The impact of the Treasury security auction results announcements, which are scheduled at regular intervals, are considered separately for each coupon security of a given maturity. Our year of data contains results of two thirty-year-bond auctions, four ten-year-note auctions, twelve five-year-note auctions, four three-year-note auctions, and twelve two-year-note auctions. In total, our sample contains 268 announcement releases on 173 separate days, leaving 77 days with no announcement.

Expectations and Announcements

Market expectations for the nineteen monthly announcements are obtained from the *Wall Street Journal*. Every Monday, the *Journal* publishes consensus forecasts provided by Technical Data, a market analysis firm, for the coming week's announcements. Technical Data produces the forecasts from a survey of twenty-five economists conducted the Friday before.[13] We refer to *Barron's* (which also relies on Technical Data) for forecasts unavailable in the *Wall Street Journal* and to *Business Week* (which relies on MMS International) for forecasts that we could not get from the first two sources. We obtained a complete set of forecasts for eighteen of our nineteen monthly announcements and a partial set (eight out of twelve) for the remaining one (factory inventories). Actual announcement data are retrieved from these same three sources and are supplemented by data from Bloomberg when necessary.

Expectations for the fed funds target rate are calculated using the rates on fed funds futures contracts. Since the settlement price of a fed funds futures contract is based on the average effective overnight fed funds rate over an entire month, the rate at any point during a month i^f is a weighted average of the actual fed funds rate to date i^a and the rate expected to prevail for the rest of the month, i^m. Specifically, $i^f = \frac{T}{N} \times i^a + \frac{N-T}{N} \times i^m$, where T is the number of days passed to date and N is the number of days in the month. The fed funds target rate expected to prevail after an FOMC meeting is then calculated by solving for i^m using the daily rate data up to each FOMC announcement.[14]

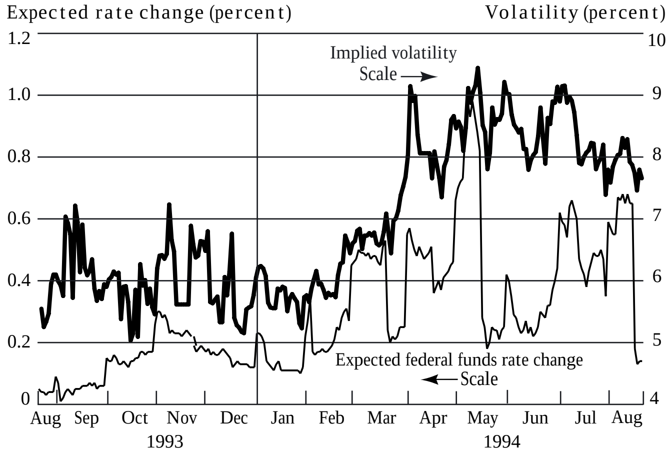
We can measure expectations for the Treasury security auction results much more precisely than other expectations. Our measure is the yield in the when-issued market (extracted from the GovPX data set) at the time of the auction. Actual results are then measured by the auction yield as reported in the next day's *Wall Street Journal*.[15]

Market Uncertainty

Our analysis of market conditions relies on two measures of market uncertainty (Chart 3). One is the implied volatility derived from options on U.S. Treasury futures traded on the Chicago Board of Trade. Specifi-

Chart 3

Measures of Market Uncertainty
August 23, 1993, to August 19, 1994



Source: Authors' calculations, based on data from the Chicago Board of Trade and the Federal Reserve Bank of New York.

Notes: The expected federal funds rate change is defined as the difference between the federal funds futures rate (drawn from the contract expiring at the end of the month two months ahead) and the federal funds target rate. Implied volatility is an annualized measure derived from futures options on ten-year U.S. Treasury notes.

cally, the volatility measure equals the average of six individual implied volatilities calculated using the nearest-to-the-money calls and puts on futures contracts on ten-year U.S. Treasury notes. The second measure is the expected change in the fed funds rate—defined as the difference between the fed funds futures rate (drawn from the contract expiring at the end of the month two months ahead) and the fed funds target rate. The expected fed funds rate change is positive for our entire sample year because the question during this period was largely whether the Federal Reserve was going to raise rates, and if so, by how much.

The Largest Market Moves

To account for the sharpest price changes and the greatest surges in trading activity in the bond market, we selected the twenty-five largest price changes and the twenty-five most active trading episodes from every five-minute interval across the global trading day from August 23, 1993, to August 19, 1994 (Tables 3 and 4).[16]

Price Shocks

It is striking that the twenty-five sharpest price changes in the bond market all occurred on announcement days.[17]

Table 3

Sharpest Price Changes for the Five-Year U.S. Treasury Note

Price Change (Percent)	Date	Time	Announcement (Time)
-0.590	August 5, 1994	8:30-8:35 a.m.	Employment (8:30 a.m.)
-0.536	May 6, 1994	8:30-8:35 a.m.	Employment (8:30 a.m.)
-0.440	July 8, 1994	8:30-8:35 a.m.	Employment (8:30 a.m.)
-0.412	April 1, 1994	8:30-8:35 a.m.	Employment, personal income (8:30 a.m.)
0.407	July 29, 1994	8:30-8:35 a.m.	Gross domestic product (8:30 a.m.)
0.406	September 3, 1993	8:30-8:35 a.m.	Employment, leading indicators (8:30 a.m.)
0.384	May 12, 1994	8:30-8:35 a.m.	Producer price index, retail sales (8:30 a.m.)
-0.343	May 27, 1994	8:35-8:40 a.m.	Gross domestic product (8:30 a.m.)
0.332	November 9, 1993	8:30-8:35 a.m.	Producer price index (8:30 a.m.)
0.315	February 4, 1994	8:30-8:35 a.m.	Employment (8:30 a.m.)
0.313	September 10, 1993	8:30-8:35 a.m.	Producer price index (8:30 a.m.)
0.282	January 7, 1994	8:30-8:35 a.m.	Employment (8:30 a.m.)
-0.266	August 16, 1994	1:45-1:50 p.m.	Federal funds target rate (1:17 p.m.)
-0.265	June 3, 1994	8:40-8:45 a.m.	Employment (8:30 a.m.)
-0.259	February 4, 1994	11:05-11:10 a.m.	Federal funds target rate (11:05 a.m.)
-0.255	April 1, 1994	8:40-8:45 a.m.	Employment, personal income (8:30 a.m.)
0.253	July 14, 1994	8:30-8:35 a.m.	Retail sales (8:30 a.m.)
-0.249	September 14, 1993	8:30-8:35 a.m.	Consumer price index, retail sales (8:30 a.m.)
0.224	April 13, 1994	8:30-8:35 a.m.	Consumer price index, retail sales (8:30 a.m.)
-0.223	May 11, 1994	1:40-1:45 p.m.	Ten-year-note auction results (1:42 p.m.)
-0.223	April 1, 1994	8:35-8:40 a.m.	Employment, personal income (8:35 a.m.)
0.223	February 11, 1994	8:30-8:35 a.m.	Producer price index, retail sales (8:30 a.m.)
0.222	July 12, 1994	8:30-8:35 a.m.	Producer price index (8:30 a.m.)
0.221	May 17, 1994	2:35-2:40 p.m.	Federal funds target rate (2:26 p.m.)
-0.218	December 9, 1993	8:30-8:35 a.m.	Producer price index (8:30 a.m.)

Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The table reports the largest percentage price changes by five-minute interval for the five-year U.S. Treasury note along with associated announcements (and announcement times). The largest price changes are chosen from all
five-minute intervals across the global trading day for the period August 23, 1993, to August 19, 1994. All times are eastern.

Moreover, all but one came within fifteen minutes of an announcement's release. The largest shock was a price decline of 0.59 percent (a yield increase of 14 basis points) immediately upon the release of the August 5, 1994, employment report. Nine other shocks were found to follow an employment report, six a PPI report, five a retail sales report, three a personal income report, two a CPI report, and two a GDP report. In eight instances, the shocks came after the concurrent release of two reports.

It is striking that the twenty-five sharpest price changes in the bond market all occurred on announcement days. Moreover, all but one came within fifteen minutes of an announcement's release.

Three other shocks followed a fed funds target rate announcement and one trailed a release of auction results for the ten-year U.S. Treasury note.

The fact that price shocks in the bond market are so explainable stands in contrast to the difficulty of explaining them in the stock market. It is true that we attempt to explain only a year in the bond market, while Cutler, Poterba, and Summers (1989) seek to explain more than forty years in the stock market. However, it is important to note that our explanations are based on an *ex ante* list of announcements, thus reducing the bias of hindsight in the analysis. Cutler, Poterba, and Summers rely on explanations offered by the *New York Times* after the events.[18] Because these are *ex post* explanations, the authors focus on whether the explanations are convincing. Although our analysis is limited to a single year, it is a year for which we are able to verify precise release times for announcements that we have reason to believe a priori contain information relevant to the market.

Table 4
MOST ACTIVE TRADING INTERVALS FOR THE FIVE-YEAR U.S. TREASURY NOTE

Number of Trades	Date	Time	Announcement (Time)
35	July 29, 1994	8:50-8:55 a.m.	Gross domestic product (8:30 a.m.)
30	September 14, 1993	8:40-8:45 a.m.	Consumer price index, retail sales (8:30 a.m.)
29	July 20, 1994	8:35-8:40 a.m.	Housing starts (8:30 a.m.)
28	January 7, 1994	8:45-8:50 a.m.	Employment (8:30 a.m.)
28	February 11, 1994	8:35-8:40 a.m.	Producer price index, retail sales (8:30 a.m.)
28	February 11, 1994	9:00-9:05 a.m.	Producer price index, retail sales (8:30 a.m.)
27	May 27, 1994	8:45-8:50 a.m.	Gross domestic product (8:30 a.m.)
27	July 14, 1994	8:35-8:40 a.m.	Retail sales (8:30 a.m.)
26	May 6, 1994	9:20-9:25 a.m.	Employment (8:30 a.m.)
26	May 13, 1994	8:50-8:55 a.m.	Consumer price index (8:30 a.m.)
25	November 5, 1993	8:35-8:40 a.m.	Employment (8:30 a.m.)
25	January 7, 1994	8:35-8:40 a.m.	Employment (8:30 a.m.)
25	January 28, 1994	8:40-8:45 a.m.	Gross domestic product (8:30 a.m.)
25	March 1, 1994	10:50-10:55 a.m.	NAPM survey, construction spending (10:00 a.m.)
25	March 15, 1994	8:35-8:40 a.m.	Producer price index (8:30 a.m.)
25	April 20, 1994	8:45-8:50 a.m.	Housing starts (8:30 a.m.)
25	June 3, 1994	8:35-8:40 a.m.	Employment (8:30 a.m.)
25	June 10, 1994	9:00-9:05 a.m.	Producer price index (8:30 a.m.)
25	July 8, 1994	8:40-8:45 a.m.	Employment (8:30 a.m.)
24[a]	March 4, 1994	8:45-8:50 a.m.	Employment, leading indicators (8:30 a.m.)
24[a]	April 20, 1994	9:40-9:45 a.m.	Housing starts (8:30 a.m.)
24[a]	June 29, 1994	9:15-9:20 a.m.	Gross domestic product (8:30 a.m.)
24[a]	July 8, 1994	8:45-8:50 a.m.	Employment (8:30 a.m.)
24[a]	July 12, 1994	8:35-8:40 a.m.	Producer price index (8:30 a.m.)
24[a]	July 12, 1994	8:40-8:45 a.m.	Producer price index (8:30 a.m.)

Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The table reports the highest number of trades by five-minute interval for the five-year U.S. Treasury note along with associated announcements (and announcement times). The most active intervals are chosen from all five-minute intervals across the global trading day for the period August 23, 1993, to August 19, 1994. All times are eastern.

[a]Eight intervals with twenty-four trades are in the sample; we report the six with the largest number of bid-ask quotations.

TRADING SURGES

It is similarly striking that the twenty-five greatest surges in trading activity all occurred on announcement days. The evidence linking each surge to an announcement may seem less compelling than the corresponding evidence for price shocks because a longer lag separates these surges from the time of announcement. Nonetheless, all of the surges in activity came within seventy minutes of an announcement's release, nineteen of them within half an hour.[19] The greatest surge consisted of thirty-five transactions worth a total of $240 million (in face value) in a five-minute interval twenty minutes after the July 29, 1994, GDP report.[20] Eight of the other surges followed an employment report, six a PPI report, four a GDP report, four a retail sales report, three a housing starts report, and two a CPI report. In five instances, the surges followed the concurrent release of two reports.

It is similarly striking that the twenty-five greatest surges in trading activity all occurred on announcement days. . . . All of the surges in activity came within seventy minutes of an announcement's release, nineteen of them within half an hour.

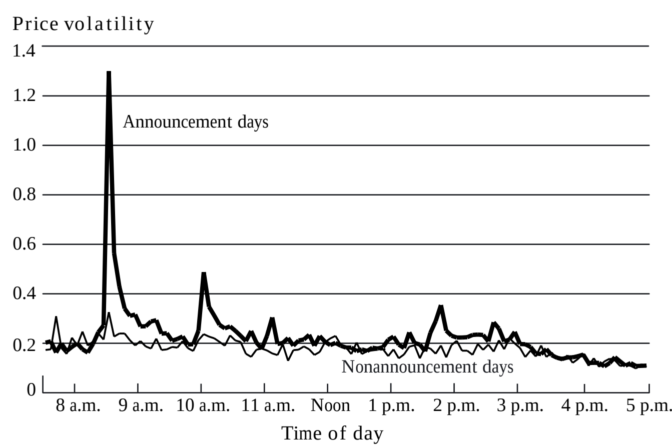
INTRADAY ANNOUNCEMENT PATTERNS

The largest movements in prices and surges in trading activity exhibit certain regularities. First, we account for all these movements with only twelve announcements. Among these, the employment, PPI, and retail sales announcements appear to be consistently important for both price shocks and trading surges, fed funds target rate actions for price shocks, and housing starts announcements for trading surges. Second, the large movements tend to be concentrated in the second half of the period: sixteen of the twenty-five price shocks and eighteen of the twenty-five trading surges. Federal Reserve target rate changes and market uncertainty over those changes may explain this pattern, a hypothesis we explore later.

The association between announcement release times and the largest price shocks and trading surges reflects a more general intraday pattern seen on most announcement days. In general, pronounced market movements follow announcement releases. On an average announcement day, we find that price volatility spikes just after the release times and that these spikes are absent on nonannouncement days (Chart 4).[21] This pattern has also been documented by Ederington and Lee (1993) and Fleming and Remolona (1997). In addition, we find that the average number of trades following release times on announcement days exceeds the average on nonannouncement days (Chart 5). Trading volume, which accounts for the size as well as the number of trades, follows a similar pattern, as documented in Fleming and Remolona (1997).

Chart 4

Intraday Price Volatility on Announcement and Nonannouncement Days

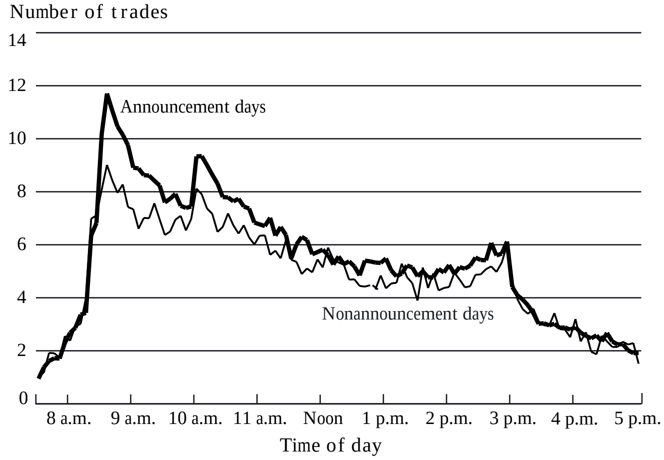


Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The chart shows the standard deviation of log price changes by five-minute interval for the five-year U.S. Treasury note for days with at least one of the twenty-one announcements listed in Table 2 and days with none of these announcements. The standard deviation equals the actual standard deviation times 10^3. The period of analysis is August 23, 1993, to August 19, 1994. Times shown are interval starting times (eastern).

Chart 5

Intraday Trading Activity on Announcement and Nonannouncement Days



Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The chart shows the mean number of interdealer trades by five-minute interval for the five-year U.S. Treasury note for days with at least one of the twenty-one announcements listed in Table 2 and days with none of these announcements. The period of analysis is August 23, 1993, to August 19, 1994. Times shown are interval starting times (eastern).

Which Announcements Have the Most Relevance?

If the market's movements represent a reaction to new information, some types of announcements should induce a stronger reaction than others because of inherent differences in the information contained about the economy. We now test whether the market's price movements and trading activity serve to differentiate among the various announcements, and to the degree they do, which announcements matter the most. While differences from expectations in a given announcement may be an important determinant of the market's response—an issue we explore in the next section of the article—our first step is simply to determine which announcements consistently affect the market and to what extent.

Estimation of Announcement Importance

To establish the importance of the various announcements, we run regressions of price volatility and trading activity on dummy variables representing each of the announcements listed in Table 2. We measure price volatility by the absolute value of the change in log prices in the five-minute interval following an announcement, with prices defined as the midpoints between bid and ask quotes.[22] We measure trading activity as the number of transactions during the one-hour interval following the announcement. The longer interval for trading activity is consistent with Fleming and Remolona's (1997) results suggesting that prices adjust rapidly while high trading activity persists for an extended period after an announcement.

For our explanatory variables, we define announcement dummy variables D_{knt}, where $D_{knt} = 1$ if announcement k is made on day n just before interval t and $D_{knt} = 0$ otherwise.[23] We rely on an additional set of dummy variables D_t to control for intraday patterns of price volatility and trading activity. We denote the dependent variables by Y_{nt}^{j}, where the superscript j indicates whether the variable is price volatility or trading activity. Our regression equation is then $Y_{nt}^{j} = a_0^j + \Sigma_{t=1}^{T-1}\, a_t^j\, D_t + \Sigma_{k=1}^{K}\, b_k^j\, D_{knt} + e_{nt}^j$, where T=22 (the number of different intervals corresponding to the release times of the different announcements) and K=25 (the number of announcements we analyze). The coefficient of interest is b_k^j, which measures the impact of announcement k.

Announcements Affecting Price

Our results suggest that the bond market differentiates among the various types of announcements through the magnitude of its price movements. Nine of the twenty-five announcements examined are found to have a significant impact on price, six showing significant effects at the 1 percent level and three at the 5 percent level (Table 5). In order of importance, the significant announcements with the greatest effects on price are: (1) employment, (2) PPI, (3) fed funds target rate, (4) retail sales, (5) CPI, (6) NAPM survey, (7) five-year-note auction results, (8) industrial production and capacity utilization, and (9) consumer confidence. This list of significant announcements is longer than any such list in previous studies.

Our regression results are noteworthy for several other reasons. First, we document for the first time a significant market impact from U.S. Treasury security auction results. Second, bond prices react so consistently to four announcements—the NAPM survey, five-year-note auction results, industrial production and capacity utilization, and consumer confidence—that these announcements are significant even when absent from the twenty-five largest price shocks. Third, although GDP releases account for two of our twenty-five largest price shocks, such releases fail to induce a price reaction consistently and hence are not found to be significant in our regressions.[24]

Our results, in conjunction with those of earlier researchers, also provide evidence of stability in the announcements that have relevance to bond prices. In their analysis of bond futures prices from November 1988 to November 1991, Ederington and Lee (1993) find the employment, PPI, CPI, and durable goods orders reports to be the most important regularly scheduled announcements. The continued significance of the employment report may be explained by the fact that it still offers the market the first comprehensive look at the economy's strength, with data on nonfarm payroll employment, the unemployment rate, and average hourly earnings.[25] The PPI and CPI reports also continue to be significant. Of Ederington and Lee's most important announcements, only the durable goods orders report has lost its significance.[26]

Table 5
IMPACT OF ANNOUNCEMENTS ON PRICE

Rank	Announcement	Coefficient
1	Employment	26.10**
2	Producer price index	13.71**
3	Federal funds target rate	11.00**
4	Gross domestic product	7.19
5	Retail sales	7.04*
6	Consumer price index	6.75**
7	Thirty-year-bond auction results	6.48
8	Ten-year-note auction results	5.84
9	NAPM survey	4.12*
10	Five-year-note auction results	3.62**
11	Industrial production and capacity utilization	3.42**
12	Consumer confidence	3.09*
13	New single-family home sales	2.58
14	Durable goods orders	1.78
15	Construction spending	1.78
16	Three-year-note auction results	1.76
17	Trade balance	1.68
18	Housing starts	1.34
19	Personal income	1.15
20	Business inventories	1.14
21	Consumer credit	0.86
22	Factory inventories	0.70
23	Two-year-note auction results	0.26
24	Federal budget	0.03
25	Leading indicators	-3.32

Memo:	
Adjusted R^2	0.40
χ^2 statistic[a]	362**
Number of observations	5,323

Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The table presents the regression coefficients indicating the average difference in price volatility for the five-year U.S. Treasury note for the five-minute period after an announcement as compared with the same period on nonannouncement days. Volatility is defined as the absolute value of the log price change times 10^4. Coefficient significance is based on two-sided t-tests using heteroskedasticity-consistent (White) standard errors. The period of analysis is August 23, 1993, to August 19, 1994.

[a]The χ^2 statistic tests whether all model coefficients equal zero and is computed using the heteroskedasticity-consistent covariance matrix.

*Significant at the .05 level.

**Significant at the .01 level.

ANNOUNCEMENTS AFFECTING TRADING ACTIVITY

Our results, in conjunction with those of earlier researchers, also suggest that the bond market differentiates among announcements through the extent of trading activity elicited. Fourteen of the announcements have a significant positive impact on trading activity, twelve at the 1 percent level and two at the 5 percent level (Table 6). In order of importance, the announcements that generate significant trading activity are: (1) employment, (2) fed funds target rate, (3) thirty-year-bond auction results, (4) PPI, (5) ten-year-note auction results, (6) CPI, (7) NAPM survey, (8) GDP, (9) retail sales, (10) three-year-note auction results, (11) new single-family home sales, (12) factory inventories, (13) business inventories, and (14) industrial production and capacity utilization.

We note that, first, the announcements that matter for price also tend to matter for trading activity. The employment report, for example, has the greatest impact on both price and trading activity. Second, housing starts releases account for three of the twenty-five greatest trading surges but do not consistently produce a rise in trading activity. Third, eight announcements consistently lead to additional trading activity even when they do not account for any of the twenty-five greatest trading surges: fed funds target rate, thirty-year-bond auction results, ten-year-note auction results, three-year-note auction results, new single-family home sales, factory inventories, business inventories, and industrial production and capacity utilization.

Table 6
IMPACT OF ANNOUNCEMENTS ON TRADING ACTIVITY

Rank	Announcement	Coefficient
1	Employment	87.93**
2	Federal funds target rate	72.14**
3	Thirty-year-bond auction results	63.55**
4	Producer price index	58.29**
5	Ten-year-note auction results	46.50**
6	Consumer price index	45.92**
7	NAPM survey	39.72**
8	Gross domestic product	39.47**
9	Retail sales	38.21**
10	Three-year-note auction results	36.24**
11	New single-family home sales	30.05**
12	Factory inventories	26.14**
13	Business inventories	23.53*
14	Industrial production and capacity utilization	23.02*
15	Housing starts	15.37
16	Trade balance	13.54
17	Leading indicators	6.46
18	Consumer confidence	5.35
19	Personal income	3.72
20	Two-year-note auction results	0.72
21	Durable goods orders	-0.32
22	Consumer credit	-0.35
23	Construction spending	-1.21
24	Federal budget	-7.03
25	Five-year-note auction results	-10.42*

Memo:	
Adjusted R^2	0.38
χ^2 statistic[a]	6,721**
Number of observations	5,386

Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The table presents the regression coefficients indicating the average difference in trading activity for the five-year U.S. Treasury note for the one-hour period after an announcement as compared with the same period on non-announcement days. Trading activity is defined as the number of interdealer broker transactions reported by GovPX. Coefficient significance is based on two-sided t-tests using heteroskedasticity-consistent (White) standard errors. The period of analysis is August 23, 1993, to August 19, 1994.

[a]The χ^2 statistic tests whether all model coefficients equal zero and is computed using the heteroskedasticity-consistent covariance matrix.

*Significant at the .05 level.

**Significant at the .01 level.

TIMELINESS

The timeliness of an announcement—that is, how soon data are released after the period covered ends—helps to explain its impact on prices and trading activity. Of the government reports, the most timely are employment, PPI, CPI, and retail sales, in that order (Chart 2). This order of timeliness is nearly matched by the reports' order of importance for both price shocks and trading activity. Timeliness, however, is not the sole determinant of market impact. The two private sector reports—consumer confidence and the NAPM survey—are even more timely than the employment report. Although both reports significantly affect the market, the bond market evidently regards their information about the economy as somewhat less important than the information in the government's employment, PPI, CPI, and retail sales reports. As we will demonstrate, the degree of surprise in a given announcement and conditions of market uncertainty also influence an announcement's importance.

ANNOUNCEMENT SURPRISES AND MARKET CONDITIONS

DOES THE MAGNITUDE OF SURPRISE MATTER?

A bond market that truly responds to the arrival of information should not only differentiate among the various types of announcements but also react more sharply to larger surprises in a given announcement.[27] Many bond market announcement studies focus on the surprise component because information is believed to have value only to the extent that it is unexpected. For example, an unexpectedly strong nonfarm payrolls number should cause a fall in bond prices, with a greater surprise causing a greater fall. The effect on trading activity is less clear, however, because a larger surprise would not necessarily lead to wider disagreement among traders about the appropriate price adjustment, although we might expect it to lead to greater portfolio rebalancing if the larger surprise is accompanied by a greater price change.

To measure the impact of unexpected information, we regress five-year U.S. Treasury note price changes and trading activity on the surprise components of announcements. We define surprises $S_{knt} \equiv A_{knt} - F_{knt}$, where A_{knt} is the actual number released in announcement k on day n in interval t and F_{knt} is the corresponding forecast number (S_{knt}=0 on days and in intervals without a release of announcement k). Although each announcement typically reveals several pieces of information, we limit our analysis to surprises in the headline number. For the employment report, we therefore focus on nonfarm payroll employment surprises; for industrial production and capacity utilization, we focus on industrial production surprises. To facilitate a comparison of announcement effects and to ensure that our estimated coefficients are representative of a typical announcement, we scale the surprises by the mean absolute surprise $\overline{S_k} = \frac{1}{N_k}\Sigma_n|S_{knt}|$, where N_k is the number of releases of announcement k in our sample.

Hence, our regression equation for bond prices is given by $Z^P_{nt} = a^P_0 + \Sigma^K_{k=1}\, c^P_k \frac{S_{knt}}{\overline{S_k}} + u^P_{nt}$, where Z^P_{nt} is the signed price change. In the case of trading activity, our equation is $Z^Q_{nt} = a^Q_0 + \Sigma^{T-1}_{t=1}\, a^Q_t D_t + \Sigma^K_{k=1}\, c^Q_k \frac{|S_{knt}|}{\overline{S_k}} + u^Q_{nt}$, where Z^Q_{nt} is trading activity, D_t are dummy variables to control for intraday patterns of trading activity, and $|S_{knt}|$ are the absolute surprises.[28] The coefficients c^P_k and c^Q_k, which measure the effects of announcement surprises on prices and trading activity, respectively, are reported in Table 7 along with the mean absolute surprise for each announcement.

In general, the surprise components provide more precise estimates of announcement effects on bond prices, indicating a market that is indeed reacting to the arrival of information. Taking account of the magnitude and sign of the surprise lends significance to six announcements not found to be significant in the regressions with announcement dummy variables, adding to an already long list of significant announcements. The six additional announcements are the auction results for the ten-year U.S Treasury note and thirty-year U.S. Treasury bond, new single-family home sales, housing starts, the trade balance, and consumer credit. The fed funds target rate and retail sales announcements, however, lose their significance because their price effects do not bear a consistent sign. Increases in the fed funds target rate, in particular, often had a strong effect on bond prices during the period, but the effects were at times positive and at times negative.[29]

In the case of trading activity, it is much less clear that taking account of the magnitude of the surprise helps explain the bond market's response to announcements. A comparison of Tables 6 and 7 shows that the absolute surprises add significance to the effects of the business inventories releases but reduce significance for the new single-family home sales releases. Unlike the effects on prices, the significance of fed funds target rate actions for trading activity remains the same. On the whole, these results suggest that larger announcement surprises do not systematically widen the divergence in traders' views or lead to greater portfolio rebalancing.

Table 7
IMPACT OF ANNOUNCEMENT SURPRISES

Announcement	Mean Absolute Surprise	Price Coefficient	Trading Activity Coefficient
Employment (nonfarm payrolls)	92,000 jobs	-23.10**	60.52**
Producer price index	0.23%	-8.59**	27.87**
Ten-year-note auction results	0.02%	-8.05**	34.21**
Thirty-year-bond auction results	0.03%	-7.71**	40.41**
Retail sales	0.46%	-6.51	39.03**
Consumer price index	0.10%	-6.48**	24.56**
New single-family home sales	63,000 homes[a]	-5.08**	23.97*
Federal funds target rate	0.13%	-4.61	60.80**
Consumer confidence	3.92	-4.42**	9.62
Five-year-note auction results	0.01%	-4.20**	-7.86*
NAPM survey	0.93%	-4.17**	35.83**
Industrial production	0.18%	-3.87**	17.81*
Housing starts	62,000 homes[a]	-3.42**	12.05
Gross domestic product	0.36%	-3.20	29.04**
Trade balance	$1.04 billion	-2.50**	4.94
Construction spending	0.94%	-1.79	-5.35
Consumer credit	$2.10 billion	-1.70**	2.24
Durable goods orders	1.03%	-1.41	-5.10
Two-year-note auction results	0.01%	-1.25	6.37
Leading indicators	0.09%	-0.46	2.28
Federal budget	$1.33 billion	-0.29	-1.86
Business inventories	0.22%	0.05	24.88**
Personal income	0.19%	0.19	-1.66
Three-year-note auction results	0.02%	1.06	27.40**
Factory inventories	0.14%	1.61*	27.55**
Memo:			
Adjusted R^2		0.27	0.37
χ^2 statistic[b]		996**	5,655**
Number of observations		5,319	5,382

Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The table presents the regression coefficients indicating the impact of announcement surprises on price and trading activity for the five-year U.S. Treasury note. Announcement surprises are the actual numbers announced minus the forecast numbers divided by the mean absolute surprise for each announcement type. The impact on price is examined with signed surprises while surprise magnitudes are used for trading activity. Price is defined as the log price change times 10^4 for the five-minute period immediately after announcement; trading activity is defined as the number of transactions in the one-hour period after an announcement. Coefficient significance is based on two-sided t-tests using heteroskedasticity-consistent (White) standard errors. The period of analysis is August 23, 1993, to August 19, 1994.

[a]Figure reported is at an annual rate.

[b]The χ^2 statistic tests whether all model coefficients equal zero and is computed using the heteroskedasticity-consistent covariance matrix.

*Significant at the .05 level.

**Significant at the .01 level.

Do Market Conditions Matter?

The largest price shock in our sample followed an employment report that contained relatively little surprise. Specifically, on August 5, 1994, the price of the five-year U.S. Treasury note fell 0.59 percent within five minutes of the release of a nonfarm payrolls number that exceeded the forecast by only 54,000 jobs.[30] The period seems to have been a time of great uncertainty, with previous announcements giving mixed signals about the strength of the economy and bond market participants trying to guess whether the Federal Reserve was about to raise rates for the fifth time in six months. Hence, the issue we examine is whether market participants attach more significance to the same information during times of greater uncertainty.

To analyze the impact of market uncertainty, we run regressions that allow the surprise variables to interact with our uncertainty variables. As described earlier, our measures of uncertainty are the implied volatility from Treasury futures options and the expected change in the fed funds rate. We specify the announcement surprise coefficients to depend on uncertainty, $c_k^P = g_k^P + h_k^{Pi} V_n^i$ and $c_k^Q = g_k^Q + h_k^{Qi} V_n^i$, where V_n^i is one of our two measures of uncertainty and the coefficients h_k^{Pi} and h_k^{Qi} measure the influence of uncertainty on announcement effects. The regression equation for bond prices then becomes

$$Z_{nt}^P = a_0^P + \Sigma_{k=1}^K g_k^P \frac{S_{knt}}{\hat{S}_k} + \Sigma_{k=1}^K h_k^{Pi} V_n^i \frac{S_{knt}}{\hat{S}_k} + u_{nt}^P$$

and the equation for trading activity becomes

$$Z_{nt}^Q = a_0^Q + \Sigma_{t=1}^{T-1} a_t^Q D_t + \Sigma_{k=1}^K g_k^Q \frac{S_{knt}}{\hat{S}_k}$$

$$+ \Sigma_{k=1}^K h_k^{Qi} V_n^i \frac{|S_{knt}|}{\hat{S}_k} + u_{nt}^Q .$$

Table 8 presents the results of these regressions for the 8:30 a.m. announcements, identifying the announcement surprises for which h_k^{Pi} and h_k^{Qi} are significant. Because the two measures of uncertainty are highly correlated, we analyze them in separate regressions.[31]

Our results show that the price response to a given announcement surprise is frequently greater under conditions of increased uncertainty. Uncertainty in the form of implied volatility from Treasury futures options helps explain the bond market's price reaction to durable goods orders, GDP, and housing starts surprises, while uncertainty in the form of an expected fed funds rate

Table 8
Impact of Market Conditions on Announcement Responses

Model	Dependent Variable	Interaction Terms	Interaction χ^2 [a]	Significant Interaction Coefficients[b]	Model χ^2 [c]	Model R^2	Number of Observations
1	Price	None	N.A.	N.A.	219**	0.42	250
2	Price	Implied volatility	45**	Durable goods orders**, gross domestic product*, housing starts**	1,050**	0.44	250
3	Price	Expected federal funds rate change	22*	Durable goods orders**, employment (nonfarm payrolls)*	248**	0.44	250
4	Trading activity	None	N.A.	N.A.	158**	0.29	250
5	Trading activity	Implied volatility	82**	Consumer price index**, producer price index**, trade balance**	671**	0.34	250
6	Trading activity	Expected federal funds rate change	60**	Consumer price index**, durable goods orders*, employment (nonfarm payrolls)*, personal income**, producer price index**	514**	0.32	250

Source: Authors' calculations, based on data from GovPX, Inc.

Notes: The table presents the results from regressions of price and trading activity on announcement surprises and two variables interacted with announcement surprises for the five-year U.S. Treasury note. All results are derived from analyses of the 8:30 a.m. monthly announcements. The price regressions are run with signed announcement surprises and with signed price changes for the 8:30 a.m.-8:35 a.m. interval. The trading activity regressions are run with absolute announcement surprises and with trading activity measured as the number of trades in the 8:30 a.m.-9:30 a.m. interval. Coefficient significance is based on two-sided t-tests using heteroskedasticity-consistent (White) standard errors. The period of analysis is August 23, 1993, to August 19, 1994.

[a]This χ^2 statistic tests whether all interaction terms equal zero and is computed using the heteroskedasticity-consistent covariance matrix. The statistic is calculated excluding any significant interaction terms that have a sign opposite to that predicted.

[b]The list of coefficients excludes significant interaction terms that have a sign opposite to that predicted.

[c]This χ^2 statistic tests whether all model coefficients equal zero and is computed using the heteroskedasticity-consistent covariance matrix.

*Significant at the .05 level.

**Significant at the .01 level.

change helps explain the reaction to durable goods orders and employment surprises.

For trading activity, market uncertainty often heightens the trading surge that follows announcement surprises. Uncertainty as measured by implied volatility helps explain the rise in trading activity in the wake of CPI, PPI, and trade balance surprises, while uncertainty as measured by the expected fed funds rate change helps explain the increase in activity after CPI, durable goods orders, employment, personal income, and PPI surprises. These results suggest that uncertain market conditions contribute to the divergence in traders' interpretations of announcement surprises.

CONCLUSION

Our finding that the largest price shocks and the greatest surges in trading activity in the bond market stem from the arrival of public information is reassuring. Over the August 23, 1993, to August 19, 1994, sample period, each of the twenty-five sharpest price changes and each of the twenty-five greatest surges in trading activity can be associated with a just-released announcement. These results suggest that U.S. Treasury securities prices react largely to the arrival of public information about the economy. The surge in trading activity following the price shocks suggests a lack of consensus among market participants over whether the initial price change is precisely the appropriate adjustment to the new information, although portfolio rebalancing may also be important.

It is also reassuring to find that various measures of the information content of the different announcements generally help explain such market responses. In particular, the market distinguishes among announcements with inherently different information, reacting most dramatically—through both price movements and trading activity—to the employment, PPI, fed funds target rate, and CPI announcements. U.S. Treasury security auction results are also found to have significant effects on both price and trading activity.

Moreover, we find that the bond market's reactions depend on the unexpected component of a given announcement and on conditions of uncertainty. Taking account of the surprise component in a report's announced numbers extends our list of announcements that significantly affect bond prices from nine to thirteen, longer than any such list in previous studies. Greater market uncertainty also leads to a stronger market response, particularly in the form of increased trading activity. These results suggest that the bond market's price and trading reactions reflect differences of informational content in and among the varying announcements under changing market conditions.

ENDNOTES

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1. See Beaver (1968).

2. However, in an analysis similar to the Cutler, Poterba, and Summers (1989) study of the U.S. stock market, Elmendorf, Hirshfeld, and Weil (1996) find it difficult to relate the largest movements in U.K. bond prices from 1900 to 1920 to news arrival.

3. Roley and Troll (1983), for example, find no significant announcement effects from the CPI, the unemployment rate, and the PPI; Hardouvelis (1988) finds none from consumer credit, housing starts, industrial production, leading indicators, merchandise trade, or personal income; and Dwyer and Hafer (1989) find none from the CPI, industrial production, the unemployment rate, or merchandise trade.

4. Kim and Verrechia (1991) and He and Wang (1995) show theoretically how heterogeneity of views among investors can generate speculative trading activity.

5. French and Roll (1986), for example, attribute the fact that stock return volatilities are higher when the exchanges are open than when they are closed to the effect of private information conveyed through trading.

6. This is the argument used by Fleming and Remolona (1997) to explain the persistence of trading volume beyond price volatility in the Treasury market after an announcement. That study, as well as earlier stock market studies by Jain and Joh (1988) and Gallant, Rossi, and Tauchen (1992), suggests that price volatility causes trading activity.

7. Fleming (1997) finds that 64 percent of interdealer trading is in on-the-run issues, 24 percent is in off-the-run issues, and 12 percent is in when-issued securities. Off-the-run securities are issued securities that are no longer active; when-issued securities are securities that have been announced for auction but not yet issued.

8. Appendix B of Fleming and Remolona (1997) details the data cleaning and processing.

9. We count the announcement of gross domestic product (GDP) as a monthly release. Although GDP is a quarterly measure, the Bureau of Economic Analysis issues advance, preliminary, and final estimates in successive months.

10. Included in the 8:30 a.m. count is the personal income announcement, which was released at 10 a.m. for the first three announcements in our sample but at 8:30 a.m. thereafter.

11. The employment report was released on the second Friday in October 1993 and in July 1994.

12. Five announcements occurred after the regularly scheduled February, March, May, July, and August 1994 FOMC meetings. The other announcement occurred in April 1994, when the fed funds target rate was increased without an FOMC meeting. Cook and Hahn (1989), Pakko and Wheelock (1996), and Roley and Sellon (1996) use daily data to examine the impact of fed funds target rate changes.

13. Ideally, we would like to use forecasts that are based on expectations right before each announcement since expectations can change over the course of a week. Our use of weekly forecasts may bias the coefficients of our estimates toward zero in those regressions that depend on announcement surprises.

14. Krueger and Kuttner (1996) show that the fed funds futures rate is effective at identifying changes in the fed funds rate. Our methodology follows that of Pakko and Wheelock (1996), using effective fed funds rate data from the Federal Reserve Bank of New York and fed funds futures data from the Chicago Board of Trade.

15. The three-, ten-, and thirty-year securities are issued at price-discriminating auctions, so for these securities the yield corresponding to the lowest accepted price is used. The two- and five-year securities are issued at uniform-price auctions.

16. Andersen and Bollerslev (forthcoming) perform a similar exercise with Deutsche mark–dollar exchange rates and find that fifteen of the twenty-five largest five-minute absolute returns from October 1992 to September 1993 are directly associated with the release of economic news.

17. Note that there are seventy-seven nonannouncement days on which purely random shocks could have taken place. With a sample of 250 days, the probability that all 25 of the shocks occur on an announcement day purely by chance is 0.01 percent.

18. The explanation for the 20 percent decline on October 19, 1987, for example, is "worry over dollar decline and trade deficit, fear of U.S. not supporting dollar."

19. Fleming and Remolona (1997) analyze the adjustment patterns of trading volume after major announcements. They find an appreciable lag in the surge in trading volume after the initial price shock and a persistence of high volume for a few hours afterward.

20. We use the number of transactions as our measure of trading activity instead of the face value of securities traded. We base this decision on Jones, Kaul, and Lipson's (1994) finding that transaction size has no information content beyond that contained in the frequency of trades.

21. On the days with 8:30 a.m. announcements, the price change in the first five minutes after the announcement explains 31 percent of the whole day's (7:30 a.m. to 5 p.m.) price change.

22. We could also use transaction prices, but using the bid-ask midpoints allows us to avoid complications associated with the "bid-ask bounce," in addition to providing us with more observations.

23. For announcements released in the final minute of an interval, we begin the analysis at the start of the next interval. For all other announcements, the analysis begins in the same interval. For example, a 1:34 p.m. release time implies an analysis based on the 1:35 p.m.-1:40 p.m. interval for price and the 1:35 p.m.-2:35 p.m. interval for trading activity, while a 1:33 p.m. release time implies an analysis based on the 1:30 p.m.-1:35 p.m. interval for price and the 1:30 p.m.-2:30 p.m. interval for trading activity.

24. As noted earlier, the releases consist of advance, preliminary, and final estimates of quarterly GDP announced in successive months. An advance estimate accounted for one of the two largest price shocks associated with GDP releases; a preliminary estimate accounted for the other.

25. As Krueger (1996) notes, the BLS now collects the nonfarm payroll employment data from a sample of more than 200,000 establishments that offers wide geographic and industry coverage. We document the employment report's importance for the bond market; Harris and Zabka (1995) and Andersen and Bollerslev (forthcoming) show its importance for the foreign exchange market.

26. The decreased significance of durable goods orders may reflect their declining reliability as an indicator of future manufacturing activity. Because an increasing share of durable goods are now shipped almost immediately, much of the lag time that existed between order receipt and shipment has been eliminated. In the past, that lag time enabled analysts to use durable goods orders to predict future manufacturing activity. Now, however, the reduction of that lag time has made such projections difficult. Compounding the problem, orders have increased for goods whose prices are changing rapidly, particularly computers; this price volatility has made it harder for the durable goods report to assess the quantity of goods ordered, since the report measures orders only in dollar terms.

27. We do not address issues of rationality or market efficiency in this article—that is, we do not test whether market prices properly reflect all available information, nor whether they adjust to such information in an appropriately rapid fashion.

28. Absolute surprises are used for the trading activity regression (and not the price regression) because we are testing whether the magnitudes of announcement surprises are correlated with changes in trading activity. For example, we suspect that nonfarm payroll surprises of 100,000 jobs and -100,000 jobs would have contrary effects on price, but that both would be associated with an increase in trading activity relative to smaller magnitude surprises.

29. Pakko and Wheelock (1996) discuss why the effects change in sign.

30. The average absolute nonfarm payroll employment surprise in the sample was 92,000 jobs (Table 7) and was as large as 206,000 on April 1, 1994. Other components of the employment report do not seem to explain the market's sharp August 5 response—the announced unemployment rate of 6.1 percent was expected, manufacturing overtime hours were unchanged at 4.6, average manufacturing hours actually declined to 41.9 from 42.0 the previous month, and the previous month's nonfarm payroll employment was revised down from 379,000 to 356,000. Nonfarm payroll employment was not the only sign of strength, however; average hourly earnings increased by 4¢ to $11.12.

31. The correlation between our implied volatility measure and the expected fed funds rate change is 0.73.

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Is There an Inflation Puzzle?

*Cara S. Lown and Robert W. Rich**

Historically, inflation has followed a fairly predictable course in relation to the business cycle. Inflation typically rises during an economic expansion, peaks slightly after the onset of recession, and then continues to decline through the first year or two of recovery. During the present U.S. expansion, however, inflation has taken a markedly different path. Although more than six years have passed since the 1990-91 recession, inflation in the core CPI (the consumer price index excluding its volatile food and energy components) has yet to accelerate (Chart 1). Moreover, during the last three years, inflation has remained stable despite projections of higher expected inflation from the Blue Chip Consensus forecast and contrary to traditional signals such as the run-up in commodity prices experienced from late 1993 to early 1995.

Economists and policymakers have referred to the restrained behavior of prices during this long expansion as an "inflation puzzle." In a recent interview, Robert T. Parry, president of the Federal Reserve Bank of San Francisco, commented, "I have a question mark, and it leads me to recommend vigilance with regard to inflation, but I do have to note that things have turned out well. . . . [We've] either been lucky, in which case the old relationships will reassert themselves, or [we've] got a new regime under way. And I don't think we know enough at this point to know which of those two things is operative."[1] As Parry suggests, two different types of explanations could account for the recent behavior of inflation. The failure of inflation to accelerate may reflect the effects of temporary factors unique to this expansion. Alternatively, the unexpectedly low level of inflation may indicate a permanent change in the way inflation reacts to economic growth and other related variables.

Each of these explanations holds important implications for the conduct of monetary policy. The Phillips

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curve, the principal tool used by economists to explain inflation, has been subject to systematic overprediction errors during the past few years. If these errors reflect the influence of temporary factors, then the Phillips curve relationship should ultimately regain its stability. However, if these errors reflect a permanent change in the dynamics of the inflation process, then economists could no longer view the Phillips curve as a reliable guide in forecasting inflation.

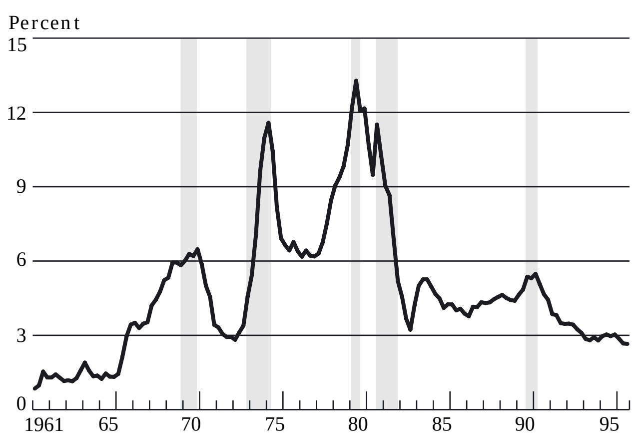
Because labor costs are an important factor in determining prices, the recent slowdown in compensation growth has been cited in both types of explanations for the inflation puzzle. Some commentators argue that this slowdown in compensation growth, attributable largely to declining benefit costs, has acted as a supply shock and has temporarily lowered inflation relative to its historical proximate determinants. Others contend that a permanent change in compensation growth, resulting from heightened job insecurity and its constrictive effect on wage growth, has led to a fundamental shift in the inflation process.

This article explores the inflation puzzle and investigates whether compensation has acted as either a temporary restraint on inflation or as the underlying source of a new inflation regime.[2] After reviewing the recent

Chart 1

Core CPI

Percentage Change from a Year Ago



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

Note: Shaded areas indicate periods designated recessions by the National Bureau of Economic Research.

behavior of inflation, we specify and estimate a traditional price-inflation Phillips curve model over the 1965-96 period. Our results show that in late 1993 the model begins to systematically overpredict inflation and appears to break down.

We then modify our traditional Phillips curve specification by incorporating compensation growth as an additional determinant of inflation. With this variable, the model's explanatory power improves significantly, and it tracks inflation much more accurately over the current

Our findings indicate that compensation growth has been weak during this expansion, especially from late 1992 through early 1995, a period that corresponds to the observed breakdown in our traditional Phillips curve specification.

expansion. The restored stability of the model appears to rule out the view that inflation's recent behavior reflects a fundamental shift in the inflation process.

Finally, we specify and estimate a wage-inflation Phillips curve model quantifying the restraint in compensation growth over the post-1991 period. Our findings indicate that compensation growth has been weak during this expansion, especially from late 1992 through early 1995, a period that corresponds to the observed breakdown in our traditional Phillips curve specification. This coincidence further supports our conclusion that compensation's slow growth has temporarily restrained inflation during this expansion.

THE EMERGENCE OF THE INFLATION PUZZLE

Contrary to expectations, inflation has not accelerated since the end of the 1990-91 recession. Yet variables commonly regarded as inflation indicators have remained at levels that usually coincide with an inflation pickup. The level of the actual unemployment rate relative to the nonaccelerating inflation rate of unemployment (NAIRU) is one such variable. The NAIRU represents the rate of unemployment that is consistent with stable inflation. Unemployment rates below (above) the NAIRU are thought to signal higher (lower) inflation in wages and prices. As the upper panel of Chart 2 shows, the unemployment rate has been below 6 percent—the consensus estimate of the NAIRU at the beginning of this expansion—since late 1994. Even if the NAIRU has declined below 6 percent during the 1990s, as some analysts argue, there is little direct evidence suggesting that it has tracked the unemployment rate or fallen low enough to be consistent with the level of inflation observed since 1995.[3]

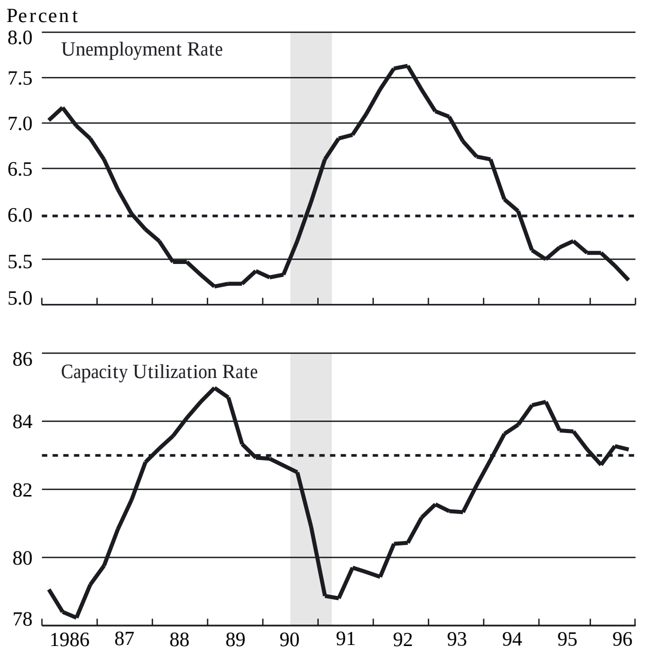
Like the NAIRU, the capacity utilization rate has stayed at levels that typically signal higher future inflation (bottom panel of Chart 2). In the past, capacity utilization rates in excess of 82 to 84 percent were associated with rising inflation because of the onset of supply shortages and bottlenecks in production (Boldin 1996). Capacity utilization has moved down from its peak of almost 85 percent; still, it has stayed above or close to 83 percent since 1994.

The Phillips curve's recent failure in forecasting price changes contrasts sharply with its long-standing reliability in predicting short-run movements in inflation.

Consistent with these two indicators, the Blue Chip Consensus forecast overpredicted inflation from 1992 to 1995 by progressively larger margins of error each year (Chart 3). Estimated price-inflation Phillips curves have also systematically overpredicted inflation in the past couple of years. The Phillips curve's recent failure in forecasting price changes contrasts sharply with its long-standing reliability in predicting short-run movements in inflation. We now turn to a discussion of the Phillips curve and its recent record in forecasting inflation.

Chart 2

Unemployment and Capacity Utilization Rates

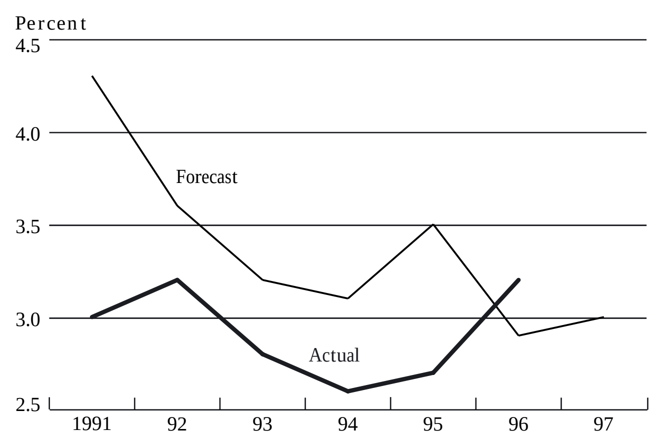


Sources: Board of Governors of the Federal Reserve System; U.S. Department of Labor, Bureau of Labor Statistics.

Notes: The dashed line marks the level at which unemployment or capacity utilization will likely begin to exert upward pressure on inflation. The period from the third quarter of 1990 to the first quarter of 1991, shaded in the chart, is designated a recession by the National Bureau of Economic Research.

Chart 3

CPI Inflation, Actual and Forecast



Sources: *Blue Chip Economic Indicators*, various December issues; U.S. Department of Labor, Bureau of Labor Statistics.

A Traditional Price-Inflation Phillips Curve

The origin of the Phillips curve can be traced back to the 1950s, when A.W. Phillips documented an inverse relationship between the rate of change of nominal wages and the level of unemployment in the United Kingdom. His findings were interpreted as establishing a wage adjustment process in which low levels of unemployment represent tight labor markets that signal, or coincide with, accelerating wage growth. Although the term "Phillips curve" still refers to the posited relationship between nominal wage or price changes and various indicators of real economic activity, the econometric modeling of this relationship has changed considerably over the years.[4]

Modern versions of the Phillips curve incorporate several features that differentiate them from earlier descriptions of the behavior of nominal wages and prices.[5] For example, in current models the output gap (the log ratio of actual to potential real GDP) and the unemployment gap (the difference between the actual rate of unemployment and the NAIRU) figure importantly as measures of excess aggregate demand pressure in the economy. In addition, current models recognize the role that expected inflation plays in wage bargaining and price setting and typically include past rates of inflation as a proxy for this expectation.[6] Finally, modern Phillips curve models include variables to control for supply shocks such as the oil price increases of the 1970s. As Fuhrer (1995) notes, many of these developments were anticipated by Phillips in his original discussion.

[Our specification for the traditional Phillips curve] embodies the "triangle" model of inflation: the set of explanatory variables is meant to capture the effects of demand, inertia, and supply considerations on inflation.

We begin our empirical analysis by specifying a traditional price-inflation Phillips curve model. The model allows for a more formal investigation of the stability of the Phillips curve relationship during the current expansion. In addition, the model will serve as a benchmark to evaluate compensation growth's role in explaining recent movements in inflation.

Our traditional Phillips curve model is given by:

$$(1)\quad INF_t = \alpha_0 + \alpha_1 GDPGAP_{t-1} + \alpha_2(\Delta GDPGAP_{t-1}) + \sum_{i=1}^{3}\alpha_{2+i} INF_{t-i} + \sum_{i=1}^{2}\alpha_{5+i} OILG^{+}_{t-i} + \varepsilon_t \,,$$

where

INF = inflation measured by the growth rate of the core CPI,

$GDPGAP$ = the output gap measured by the log ratio of actual to potential real GDP,

$\Delta\, GDPGAP$ = the first difference or change in the output gap,

$OILG^{+}$ = the net positive change in the real price of oil, and

ε = a mean zero, serially uncorrelated random disturbance term.

Equation 1 provides a general specification for the rate of change in prices and is similar to other models currently used in the Phillips curve literature.[7] In the terminology of Gordon (1996), the specification embodies the "triangle" model of inflation: the set of explanatory variables is meant to capture the effects of demand, inertia, and supply considerations on inflation.

The model uses the output gap (the percentage deviation of real GDP from potential GDP), shown in Chart 4, as a measure of excess aggregate demand pressure.[8] A positive (negative) output gap indicates that the economy is operating above (below) potential GDP and would thus generate upward (downward) inflationary pressure on prices. Following the methodology in Gordon (1977, 1996) and Fuhrer (1995), we also include the quarterly change in the output gap variable to allow for a rate-of-change effect so that the pressure on prices depends on how quickly the output gap narrows or widens.

The remaining basic determinants of inflation include its own lagged values and oil prices. To incorporate price inertia effects, we include lagged inflation terms in the model. In the past, researchers used lagged inflation rates as a proxy for expected inflation. In modern versions of the Phillips curve, however, this interpretation has been deemed overly restrictive (Gordon 1996). Instead, past inflation rates are viewed as capturing the dynamics of price adjustment related to expectations formation as well as the importance of institutional factors such as wage and price contracts and delivery lags in the economy.

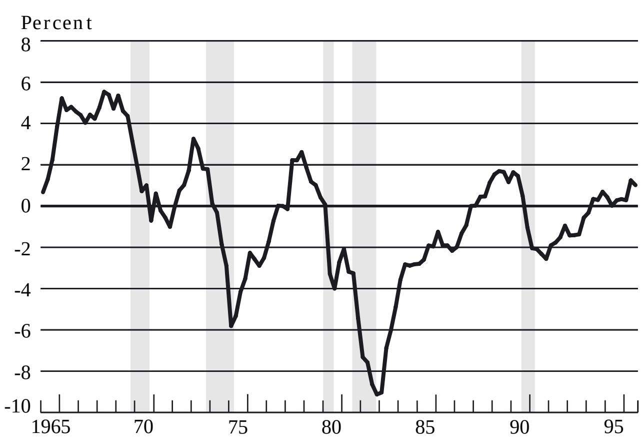
Our benchmark model also includes a measure of the net positive change in real oil prices to account for the influence of supply shocks.[9] This oil price variable is the only notable departure from other conventional Phillips curve specifications and allows for an asymmetric effect of oil price changes on inflation (Chart 5). In other words, while oil price *increases* appear to affect inflation, oil price *decreases* do not seem to be important.[10] The construction of the supply shock variable follows the approach in Hamilton (1996) and is designed not only to model the asymmetric effects of oil price changes, but also to account for the observed increase in the volatility of oil prices over the post-1986 period. Because the core CPI has no energy price component, our supply shock variable attempts to capture any indirect effect of oil price increases on inflation.

Although our traditional price-inflation Phillips curve takes real oil prices as exogenous, we include only lagged values of the output gap as regressors in order to avoid simultaneity bias arising from the endogeneity of this variable. The lag lengths in equation 1 are selected by maximizing adjusted R^2 (a measure of the model's ability to explain inflation), by searching over one to four lags of inflation and the output gap, and by searching over zero to four lags for the net positive change in the real price of oil.[11]

Chart 4

The Output Gap

Percentage Difference between Actual and Potential GDP

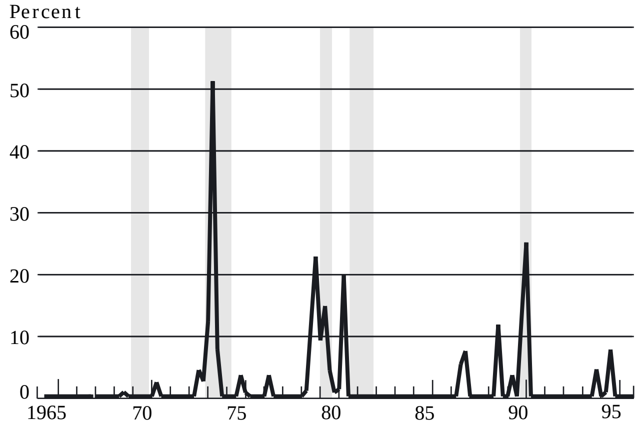


Source: Federal Reserve Bank of New York staff estimate.

Note: Shaded areas indicate periods designated recessions by the National Bureau of Economic Research.

Chart 5

Net Positive Change in Real Oil Prices



Source: Authors' calculations, based on Department of Energy, *Monthly Energy Review*.

Note: Shaded areas indicate periods designated recessions by the National Bureau of Economic Research.

Model Estimation

We estimate equation 1 using the method of ordinary least squares (OLS) for quarterly data from the first quarter of 1965 to the third quarter of 1996. Parameter estimates are presented in Table 1. For the full sample period, the value of the adjusted R^2 indicates that the model can explain a high proportion of the variation in inflation. In addition, the Ljung-Box (1978) Q-test statistic—a general test for serial correlation in the regression residuals—does not reveal any evidence of model misspecification.

The estimation results also indicate that both the level of the output gap variable and the rate-of-change effect

Table 1
TRADITIONAL AND MODIFIED PRICE-INFLATION PHILLIPS CURVE MODELS

	Traditional Model		Modified Model	
Variable	Estimate	p-Value	Estimate	p-Value
CONSTANT	0.0786 (0.0782)	0.3146	0.0532 (0.0720)	0.4601
$GDPGAP_{t-1}$	0.0339** (0.0107)	0.0016	0.0190 (0.0108)	0.0783
$\Delta\ GDPGAP_{t-1}$	0.1452** (0.0511)	0.0045	0.2620** (0.0537)	0.0000
INF_{t-1}	0.4080** (0.1209)	0.0007	0.2610* (0.1064)	0.0142
INF_{t-2}	0.1296 (0.1168)	0.2672	0.1252 (0.1046)	0.2312
INF_{t-3}	0.3487** (0.1227)	0.0045	0.2913** (0.1011)	0.0040
$OILG_{t-1}$	0.0186** (0.0056)	0.0009	0.0167** (0.0046)	0.0003
$OILG^{+}_{t-2}$	0.0242** (0.0071)	0.0007	0.0228** (0.0058)	0.0001
$UNITG^{+}_{t-1}$	—	—	0.1901** (0.0380)	0.0000
$UNITG_{t-2}$	—	—	0.0732 (0.0390)	0.0609
Memo:				
Adjusted R^2	0.776		0.815	
Q-test statistic	22.731 (0.859)		27.572 (0.643)	

Notes: Asymptotic standard errors for the parameter estimates are reported in parentheses and are computed using the procedure of White (1980). The Ljung-Box Q-test statistic for serial correlation of the regression residuals is distributed asymptotically as χ^2 with thirty-one degrees of freedom. Probability values for the test statistics are reported in parentheses.
*Significant at the 5 percent level.
**Significant at the 1 percent level.

are highly significant and have the expected positive signs. The two lagged values of the net positive change in the real price of oil are also highly significant with the anticipated positive signs. The three lags of the inflation rate are generally significant, and we are unable to reject the hypothesis that the sum of the coefficients equals unity $(\alpha_3+\alpha_4+\alpha_5=1)$ at conventional significance levels. The latter restriction follows from the natural rate hypothesis and has been previously imposed in the estimation of Phillips curves to make the level of potential output (or the unemployment rate) independent of inflation in the long run.

MODEL STABILITY OVER THE 1992-96 PERIOD

We conduct two exercises to examine the stability of the model from 1992 to 1996. First, we apply Chow (1960) split-sample tests to test the null hypothesis of constant parameters against the alternative hypothesis of a onetime shift in the parameters at some specified date. One test compares the estimates obtained using the data from one subperiod (1965-91) with the estimates using the full sample.[12] Another test employs dummy variables for the entire parameter vector for one subperiod (1992-96) and then tests the joint significance of the dummy variables.[13] As shown by the reported value of the two test statistics in Table 2, we fail to reject the null hypothesis of parameter stability for the post-1991 period at conventional significance levels.[14]

The dynamic simulation provides strong evidence of instability in the traditional price-inflation Phillips curve during the current expansion.

As a second exercise, we construct dynamic out-of-sample forecasts from the traditional price-inflation Phillips curve. This simulation provides a more stringent test of model stability by relying on lagged predicted values of inflation rather than the lagged actual values of inflation to construct the subsequent one-quarter-ahead forecasts of inflation. In addition, the Chow tests may suffer from low power because they are conducted over a relatively small part of the sample period (1992-96). For this part of the analysis, we estimate equation 1 using data from the first quarter of 1965 through the fourth quarter of 1991. We then use the estimated equation to forecast inflation over the 1992-96 period.

Table 2
TRADITIONAL AND MODIFIED PHILLIPS CURVE MODELS
Chow Test Results for 1992-96

Model	F-Statistic	Likelihood Ratio Statistic
Traditional Phillips curve	0.192 (0.999)	4.539 (0.999)
Modified Phillips curve	0.244 (0.999)	5.860 (0.998)

Note: Probability values for the test statistics are reported in parentheses.

The dynamic simulation provides strong evidence of instability in the traditional price-inflation Phillips curve during the current expansion (Chart 6). Specifically, the out-of-sample forecasts systematically overpredict inflation beginning in the third quarter of 1993. In addition, the forecasted inflation series is characterized by a rising trend and generates prediction errors that increase over time. This excerise is robust to the choice of starting dates.[15]

The results of our dynamic simulation appear to show a shift in the Phillips curve relationship and are consistent with commentators' claims that inflation has remained unexpectedly low during this expansion. We now examine the role of compensation growth in the recent behavior of inflation.

Examining the Role of Compensation Growth

Because labor costs represent about two-thirds of the total cost of production, some economists have suggested that inflation's recent behavior may be linked to movements in compensation growth and its two components, benefits and wages (Chart 7). Since the end of the 1990-91 recession, the growth rates for total compensation, benefits, and wages have not only failed to display any significant acceleration, but have generally displayed a downward trend. This downward trend is particularly apparent for benefit costs, where the four-quarter change has fallen from 6 percent to about 2 percent during the 1990s. These observed patterns support the view that labor costs may be a key factor in understanding recent movements in inflation.

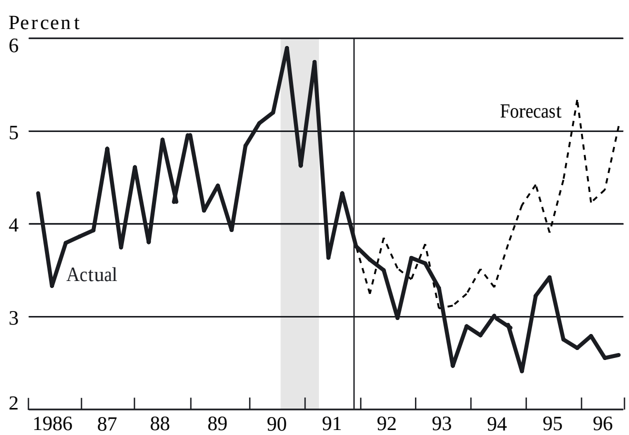
Meyer (1997), for example, poses two explanations relating compensation growth to inflation's puzzling behavior. First, he suggests that declining benefit costs have caused a temporary slowdown in compensation growth, which has acted as a supply shock. By lowering the increase in overall labor costs, this shock has reduced the pressure on firms to raise prices. Because most price-inflation Phillips curves exclude the effects of compensation growth altogether, their forecasting ability appears to break down and the models overpredict inflation.

Alternatively, Meyer suggests, the slowdown in compensation growth may reflect a long-term change in the behavior of the labor market. In particular, Meyer questions whether heightened job insecurity has permanently diminished workers' ability to obtain wage increases and has consequently altered the link between changes in com-

Chart 6

Out-of-Sample Forecast of Core CPI Inflation

Traditional Phillips Curve Model



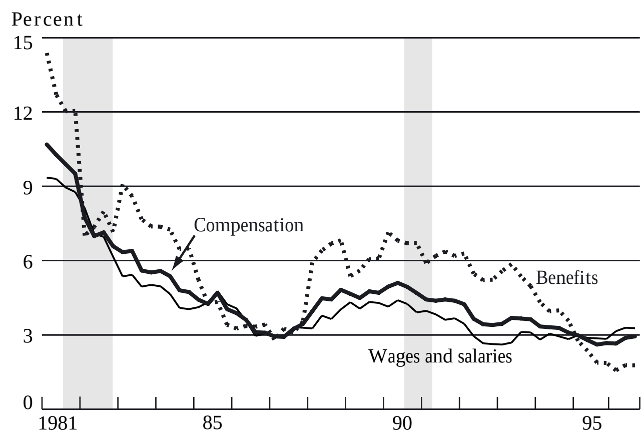
Sources: Authors' calculations; U.S. Department of Labor, Bureau of Labor Statistics.

Note: The period from the third quarter of 1990 to the first quarter of 1991, shaded in the chart, is designated a recession by the National Bureau of Economic Research.

Chart 7

Employment Cost Index for Private Industry

Percentage Change from a Year Ago



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

Note: Shaded areas indicate periods designated recessions by the National Bureau of Economic Research.

pensation (and other macroeconomic variables) and price changes. According to this view, the recent breakdown in price-inflation Phillips curves reflects a fundamental shift in the inflation process emanating from the labor market.[16]

Although we do not look at the decline in benefit costs or the behavior of wages individually, we investigate the role of total compensation growth in restraining infla-

Since the end of the 1990-91 recession, the growth rates for total compensation, benefits, and wages have not only failed to display any significant acceleration, but have generally displayed a downward trend. This downward trend is particularly apparent for benefit costs.

tion.[17] Our methodology is designed to evaluate whether this role has been temporary or permanent in nature.

If compensation growth has acted as a temporary supply shock, we would expect the forecasting performance and the stability of the Phillips curve over the current expansion to be restored by incorporating the effects of compensation growth. Moreover, because a "shock" implies an unexpected event, we would also likely observe some evidence of unusual restraint in the recent behavior of compensation growth. However, if a change in the behavior of compensation growth has permanently altered the Phillips curve relationship, we should find evidence of a breakdown, rather than stability, in the relationship between the inflation process and compensation growth during the current expansion. We now turn to our modified Phillips curve equation.

MODIFYING THE TRADITIONAL MODEL

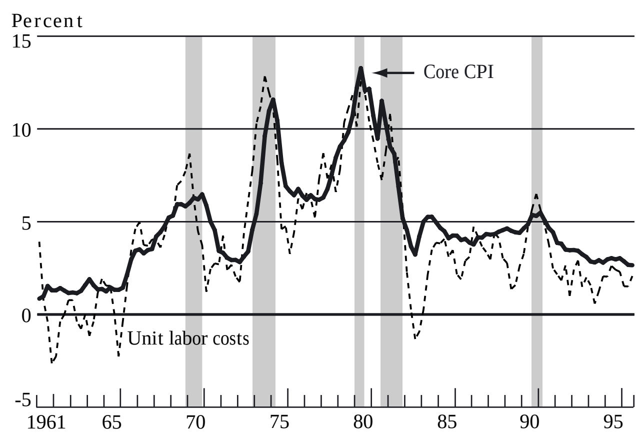
Within our Phillips curve framework, we include the growth rate of unit labor costs—compensation (benefits and wages) divided by productivity—as an additional determinant of inflation. Unit labor costs provide a measure of compensation that controls for the effects of productivity.[18]

During this expansion, growth in unit labor costs has been weak and a persistent gap has been evident between unit labor cost growth and core CPI inflation (Chart 8). The decline in unit labor cost growth could suggest either falling compensation growth or rising productivity growth. As Chart 9 shows, however, productivity growth has not been unusually strong in the current expansion. Although from late 1991 to early 1992 the series rose at roughly a 3 percent rate, contributing to weaker growth in unit labor costs, since then productivity has typically grown at rates below 1 percent.

By contrast, compensation growth fell to around 2 percent fairly early in the expansion and hovered around that rate for more than two years before showing signs of a modest pickup. This 2 percent growth rate is below any rate recorded in the past thirty-five years. Thus, we can conclude that the growth rate of unit labor costs over the post-1991 period has been primarily driven by slow compensation growth rather than high productivity growth. This finding ensures that our approach will pick up the effect of slow compensation growth, not the effect of high productivity growth, on inflation during this expansion.

Chart 8

Core CPI and Unit Labor Costs
Percentage Change from a Year Ago



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

Note: Shaded areas indicate periods designated recessions by the National Bureau of Economic Research.

Our modified price-inflation Phillips curve model is given by:

$$(2)\quad INF_t = \alpha_0 + \alpha_1 GDPGAP_{t-1} + \alpha_2(\Delta GDPGAP_{t-1}) + \sum_{i=1}^{3}\alpha_{2+i} INF_{t-i} + \sum_{i=1}^{2}\alpha_{5+i} OILG^{+}_{t-i} + \sum_{i=1}^{2}\alpha_{7+i} UNITG_{t-i} + \varepsilon_t \ ,$$

where $UNITG$ is the growth rate of unit labor costs in the nonfarm business sector. In our modified model, unit labor costs provide an explicit channel by which slow compensation growth may have acted to offset other sources of inflationary pressures over the current expansion, resulting in lower inflation rates than those predicted using the traditional model.[19]

Model Estimation

We estimate equation 2 by the method of OLS using quarterly data from the first quarter of 1965 to the third quarter of 1996. Parameter estimates are presented in Table 1. The two lagged values of unit labor cost growth enter with the anticipated positive sign. The inclusion of the unit labor cost terms improves the fit of the model over the full sample period by almost 5 percent relative to the traditional model, and the Q-test statistic does not suggest evidence of model misspecification.

The results for all other explanatory variables are broadly similar across the traditional and modified models, although the modified Phillips curve suggests that the output gap has a smaller level effect and a larger rate-of-change effect on core CPI inflation. Like the traditional model, the estimated version of the modified model does not constrain the sum of the coefficients on lagged inflation to equal unity ($\alpha_3 + \alpha_4 + \alpha_5 = 1$). As shown in the Equation Appendix, however, we can eliminate compensation growth from the system consisting of equation 2 and our estimated wage-inflation Phillips curve to yield a reduced form of a price-inflation Phillips curve. The resulting model is characterized by coefficients on lagged inflation whose sum is not statistically different from unity, and it associates an acceleration in inflation with a positive output gap and a negative unemployment gap.

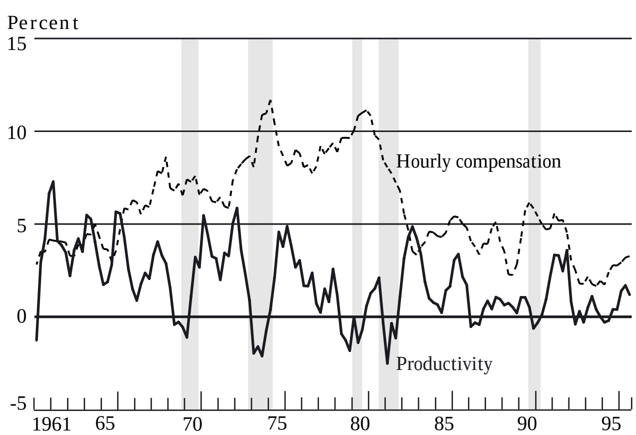
Model Stability over the 1992-96 Period

Does the inclusion of unit labor costs and the effects of compensation growth correct the instability of our benchmark model over the post-1991 period? An examination of the dynamic simulation for the modified price-inflation Phillips curve suggests that it does (Chart 10).[20] Once we incorporate the effects of unit labor costs in the model, the simulated values track inflation closely over the post-1991 period and display no significant sign of model instability. Despite a notable error in the fourth quarter of 1995, the equation regains its predictive accuracy over the next two quarters.[21] Because the dynamic simulation uses forecasted values of inflation, however, the error in the fourth quarter of 1995 continues to affect the subsequent quarters' forecasts and contributes to the error in the third quarter of 1996.

Overall, the evidence from the modified price-inflation Phillips curve is compelling. Indeed, slow compensation growth appears to be a key force in restraining inflation over the current expansion. By including unit labor costs as an additional explanatory

Chart 9

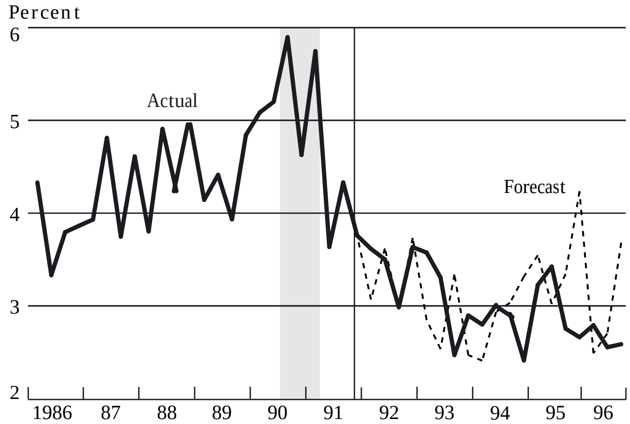
Productivity and Hourly Compensation
Percentage Change from a Year Ago



Source: U.S. Department of Labor, Bureau of Labor Statistics.
Note: Shaded areas indicate periods designated recessions by the National Bureau of Economic Research.

Chart 10

Out-of-Sample Forecast of Core CPI Inflation
Modified Phillips Curve Model



Sources: Authors' calculations; U.S. Department of Labor, Bureau of Labor Statistics.

Note: The period from the third quarter of 1990 to the first quarter of 1991, shaded in the chart, is designated a recession by the National Bureau of Economic Research.

variable, the multiperiod forecast performance of the model improves dramatically, and we seem to eliminate the sharp divergence between actual and predicted inflation. Thus, the restored stability of the model resulting from the inclusion of unit labor costs appears to rule out the view that inflation's recent behavior reflects a fundamental shift in the Phillips curve relationship. The analysis, however, has yet to provide any specific insights into compensation growth and its recent behavior. We explore these issues in the next section.

The Behavior of Compensation Growth

The results from our modified price-inflation Phillips curve reveal compensation growth's role in lowering inflation since 1991. In this section, we analyze compensation's level of restraint compared with expected levels during the present expansion. The comparison allows us to determine if the recent slowdown in compensation growth has been particularly severe. We show that while restraint in compensation growth appears to be easing, compensation growth was unexpectedly low from late 1992 to early 1995.

To analyze the behavior of compensation growth, we specify a model that represents a modified version of the wage-inflation Phillips curve proposed by Englander and Los (1983):

$$(3)\qquad LXNG_t = \beta_0 + \sum_{i=1}^{2} \beta_i LXNG_{t-i} + \beta_3 U_{t-1} + \sum_{i=1}^{3} \beta_{3+i} INF_{t-i} + \beta_7 SOC_t + \beta_8 UIR_{t-1} + \beta_9 DUM_t + \eta_t \,,$$

where

$LXNG$ = the growth rate of compensation per hour in the nonfarm business sector,

U = the unemployment rate for males aged twenty-five to fifty-four,

INF = inflation measured by the growth rate of the CPI (all items, urban consumers),

SOC = the change in employer Social Security contributions,

UIR = the income replacement ratio from unemployment insurance benefits,

DUM = dummy variable for the wage and price controls of the 1970s, and

η = a mean zero, serially uncorrelated random disturbance term.

Equation 3 principally links the movements in compensation growth to the unemployment rate and other labor market variables.[22] The unemployment rate of prime-age males is used as a measure of labor market tightness. We enter the variable in its level form and thereby abstract from any explicit discussion of the NAIRU, except to note that the specification can be viewed as implicitly assuming a constant value for the NAIRU over the sample period.[23] Equation 3 does not include a rate-of-change effect for the unemployment rate; the estimated coefficient on a second lag of the unemployment rate was found to be quantitatively and statistically insignificant and therefore was omitted from the specification.[24]

The remaining determinants of compensation growth include the change in employer Social Security tax contributions, a component of hourly compensation. The income replacement ratio from unemployment insurance benefits attempts to capture changes in compensation growth related to job search. A dummy variable accounts for the restraining effect of wage and price controls in the fourth quarter of 1971 and for the rebound effect after the relaxation of the controls in the first quarter of 1972.[25] We include lagged values of compensation growth and price inflation to incorporate wage and price inertia effects. Finally, we include only lagged values of the unemployment rate and inflation rate as regressors because of endogeneity considerations.

While restraint in compensation growth appears to be easing, compensation growth was unexpectedly low from late 1992 to early 1995.

MODEL ESTIMATION AND MODEL STABILITY OVER THE 1992-96 PERIOD

We estimate equation 3 using the method of OLS for quarterly data from the second quarter of 1967 to the third quarter of 1996. The parameter estimates are presented in Table 3. As the table indicates, the lagged values of both compensation growth and price inflation are generally significant. The unemployment rate is highly significant and has the expected negative sign. Further, the variables reflecting other labor market conditions are all significant with the expected signs. The adjusted R^2, although not quite as high as the values reported in Table 1, also indicates that the estimated equation fits the data quite well over the full sample period. In addition, the regression residuals display little evidence of serial correlation over the full sample period.

We also conduct Chow tests and a dynamic simulation. The Chow tests do not reject the null hypothesis of parameter stability at conventional significance levels (Table 4). For the dynamic simulation, we estimate equation 3 from the second quarter of 1967 to the fourth quarter of 1991; we then use the estimated equation to generate predicted values for compensation growth over the 1992-96 period.

The evidence from the dynamic simulation indicates that compensation growth has displayed unexpected restraint during this expansion. The out-of-sample forecasts consistently overpredict compensation growth beginning in the fourth quarter of 1992 (Chart 11). In addi-

Table 3
WAGE-INFLATION PHILLIPS CURVE MODEL FOR COMPENSATION GROWTH

Variable	Estimate	p-Value
CONSTANT	0.3884 (0.2155)	0.0715
$LXNG_{t-1}$	0.1359 (0.0861)	0.1144
$LXNG_{t-2}$	0.2621** (0.0689)	0.0001
U_{t-1}	-0.0672** (0.0218)	0.0021
INF_{t-1}	0.2018** (0.0692)	0.0036
INF_{t-2}	0.0175 (0.0832)	0.8332
INF_{t-3}	0.1257 (0.0698)	0.0720
SOC_t	0.0849** (0.0186)	0.0000
UIR_{t-1}	1.4288* (0.6666)	0.0321
DUM_t	-0.7442** (0.0790)	0.0000
Memo:		
Adjusted R^2	0.709	
Q-test statistic	28.109 (0.838)	

Notes: Asymptotic standard errors for the parameter estimates are computed using the procedure of White (1980) and are reported in parentheses. The Ljung-Box Q-test statistic for serial correlation of the regression residuals is distributed asymptotically as χ^2 with twenty-nine degrees of freedom. Probability values for the test statistics are reported in parentheses.
*Significant at the 5 percent level.
**Significant at the 1 percent level.

Table 4
COMPENSATION GROWTH MODEL
Chow Test Results for 1992-96

Model	F-Statistic	Likelihood Ratio Statistic
Compensation growth Phillips curve	0.879 (0.609)	20.287 (0.377)

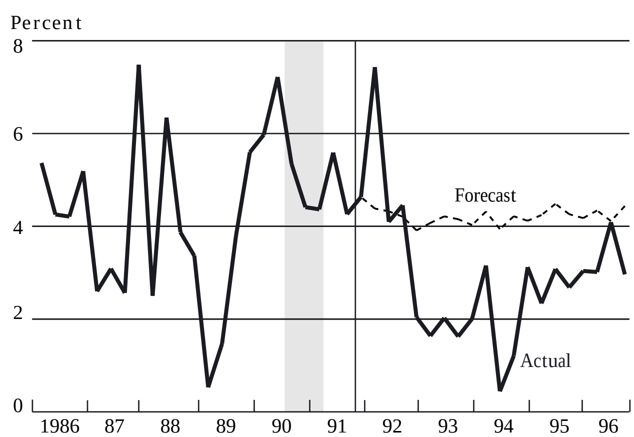
Note: Probability values for the test statistics are reported in parentheses.

tion, the size of the errors at times is quite large. For example, our dynamic simulation predicts that compensation growth should have been about 2 percent higher from the end of 1992 through the end of 1994. After 1994, however, the size of the forecast errors begins to diminish, a pattern that supports the temporary supply shock hypothesis. If a permanent change in compensation growth had occurred, we would expect the large disparity between the model's simulated values and actual growth to continue, as it did in the traditional price-inflation Phillips curve model.

Evidence from the dynamic simulation corroborates our earlier finding that the modified price-inflation Phillips curve model, which incorporates the effects of compensation growth, appears to resolve the inflation puzzle. The slowdown in compensation growth is most pronounced from the end of 1992 to early 1995, the same period during which the traditional Phillips curve starts to display evidence of model instability. Thus, not surprisingly, variables and relationships that ignore compensation growth's influence (such as the inflation indicators in Charts 2 and 3 and the traditional Phillips curve) begin to break down in late 1993 and 1994.

Chart 11

Out-of-Sample Forecast of Compensation Growth



Sources: Authors' calculations; U.S. Department of Labor, Bureau of Labor Statistics.

Note: The period from the third quarter of 1990 to the first quarter of 1991, shaded in the chart, is designated a recession by the National Bureau of Economic Research.

Conclusion

Contrary to its behavior in previous expansions, price inflation has not accelerated in the six years since the 1990-91 recession. This article focuses on compensation's role in the inflation puzzle, investigating whether a temporary slowdown in compensation growth has lowered the level of inflation or if a more permanent change in compensation growth has fundamentally altered the inflation process. We present two pieces of evidence suggesting that slow compensation growth has acted as a temporary restraining force on inflation.

We begin our investigation by estimating a traditional price-inflation Phillips curve model over the 1965-96 period. Although the model tracks inflation quite well over most of the period, it begins to break down in late 1993. We then modify the traditional Phillips curve model to include the effects of compensation growth. With this addition, the model tracks inflation much more accurately over the current expansion and displays no significant evidence of instability. This finding provides the first piece of evidence suggesting that no fundamental change in the inflation process has occurred.

To arrive at the second piece of evidence supporting the notion that the low level of inflation has resulted from a temporary slowdown in compensation growth, we look at compensation growth itself. By estimating a wage-inflation Phillips curve model, we find that compensation growth showed unusual restraint from late 1992 to early 1995. This period of restraint appears to be temporary and coincides with the observed breakdown in the traditional Phillips curve model and in other inflation indicators. Thus, taking compensation growth into account appears to explain inflation's behavior during the current expansion. Still uncertain, however, is the reason for the dramatic slowdown in compensation growth during the early 1990s. The solution to this puzzle must await further investigation.

EQUATION APPENDIX: DERIVATION OF THE ACCELERATIONIST PHILLIPS CURVE MODEL

This appendix briefly examines the derivation of the accelerationist model of the Phillips curve from equations 2 and 3. The key features of this model can be illustrated by examining the relationship between the output gap (and the unemployment gap with a constant NAIRU) and the inflation rate. Abstracting from the influence of other terms, we note that the system of equations 2 and 3 can be rewritten as

$$(4) \qquad INF_t = \alpha_1 GDPGAP_{t-1} + \sum_{i=1}^{3} \alpha_{2+i} INF_{t-i} + \sum_{i=1}^{2} \alpha_{7+i} (LXNG_{t-i}) \, ,$$

and

$$(5) \qquad LXNG_t = \frac{\beta_3 U_{t-1} + \sum_{i=1}^{3} \beta_{3+i} INF_{t-i}}{(1-\beta_1 L - \beta_2 L^2)} ,$$

where we substitute for the definition of the growth rate of unit labor costs (compensation growth less productivity growth) in equation 4, and L denotes the lag operator in equation 5 such that $L^k X_t = X_{t-k}$.

We can substitute equation 5 into equation 4 to obtain an expression relating current inflation to the output gap, the unemployment gap, and past rates of inflation. If the sum of the coefficients on lagged inflation equals unity, then there is a "natural rate" value of the output gap (and unemployment gap) of zero that is consistent with a constant rate of inflation. Alternatively, the model would associate a permanent positive value for the output gap with an ever-accelerating inflation rate. Within our system of equations, the condition that the sum of the coefficients on lagged inflation equals unity is given by

$$(6) \qquad \alpha_3 + \alpha_4 + \alpha_5 + \left[\frac{(\alpha_8 + \alpha_9)(\beta_4 + \beta_5 + \beta_6)}{(1 - \beta_1 - \beta_2)} \right] = 1 \, .$$

The hypothesis that the coefficients on lagged inflation sum to unity can be tested using the OLS estimates of equations 2 and 3 to construct estimates for the expression on the left-hand side of equation 6 and its standard error. The standard error is the standard error of a function of several estimated parameters and can be computed using the delta method approximation (Greene 1993, p. 297):

$$SE[g(\theta)] = \sqrt{\frac{\partial g}{\partial \theta'} \cdot VAR(\theta) \cdot \frac{\partial g}{\partial \theta}} \quad ,$$

where θ denotes the parameters in equation 6, $g(\theta)$ is the function of the parameters in 6, and $VAR(\theta)$ is the variance-covariance matrix of those parameters.

Because of the slight disparity in the sample periods for Tables 1 and 2, we estimate equation 2 and equation 3 from the second quarter of 1967 to the third quarter of 1996. The estimate for the expression on the left-hand side of equation 6 is 0.87, with an estimated standard error of 0.08. Thus, we are unable to reject the null hypothesis that the sum of the coefficients in equation 6 is equal to unity at the 5 percent significance level.

DATA APPENDIX

This appendix defines the variables and the data sources used to estimate our traditional Phillips curve model, modified Phillips curve model, and compensation growth model. All data in our analysis include revisions through August 12, 1997.

INFLATION EQUATION VARIABLES

INF = the growth rate of the core CPI for all urban consumers as reported by the Department of Labor, Bureau of Labor Statistics. Data are released monthly and are seasonally adjusted.

UNITG = the growth in unit labor costs for the nonfarm business sector as reported by the Department of Labor, Bureau of Labor Statistics. Data are released quarterly and are seasonally adjusted.

GDPGAP = the logarithmic ratio of *GDP* to *P0TGDP*, where *GDP* equals quarterly real gross domestic product and *P0TGDP*, quarterly potential GDP. Both variables are in 1987 dollars until the third quarter of 1987. They are in chain-weighted 1992 dollars from the fourth quarter of 1987 to the present. The GDP data are from the National Income and Product Accounts. Potential GDP is a Federal Reserve Bank of New York staff estimate.

$OILG^+$ = the net positive change in the real price of oil, calculated as the percentage change in the current real price of oil from the previous year's maximum (if that change is positive, zero otherwise). Data for the price of oil are an extension of Mork's (1989) series, which reflects corrections for the effects of price controls during the 1970s. The real price of oil is defined as the nominal oil price index deflated by the GDP deflator.

COMPENSATION EQUATION VARIABLES

LXNG = the growth rate of compensation per hour for the nonfarm business sector as reported by the Department of Labor, Bureau of Labor Statistics. Compensation comprises wages and salaries for workers plus employers' contributions for Social Security insurance and private benefit plans. The series also includes an estimate of wages, salaries, and supplemental payments for self-employed workers. Data are released quarterly and are seasonally adjusted.

INF = the growth rate of the CPI for all urban consumers as reported by the Department of Labor, Bureau of Labor Statistics. Data are released monthly and are seasonally adjusted.

U = the unemployment rate for males aged twenty-five to fifty-four as reported by the Department of Labor, Bureau of Labor Statistics. Data are released monthly and are seasonally adjusted.

UIR = unemployment insurance per job loser, normalized by the average annual earnings of a manufacturing worker. This variable can be thought of as a replacement ratio, that is, the fraction of earnings of manufacturing workers replaced by unemployment insurance payments. Manufacturing workers are the most likely workers to collect unemployment insurance. *UIR* is constructed as (*YPTU/LUJL*)/(*YPWF/LAMANU*), where

> YPTU = government unemployment insurance benefits according to the National Income and Product Accounts. Data are reported quarterly and are seasonally adjusted.
>
> LUJL = job losers and persons who have completed temporary jobs as reported by the Department of Labor, Bureau of Labor Statistics. Data are released monthly and are seasonally adjusted.

DATA APPENDIX *(Continued)*

YPWF = wage and salary disbursements in manufacturing according to the National Income and Product Accounts. Data are reported quarterly and are seasonally adjusted.

LAMANU = nonfarm payroll employees in manufacturing as reported by the Department of Labor, Bureau of Labor Statistics. Data are reported monthly.

SOC = a measure of the direct effect of changes in payroll tax rates for Social Security and Medicare. The quarterly data are Federal Reserve Bank of New York staff estimates.

DUM = 1 in the fourth quarter of 1971, -0.6 in the first quarter of 1972, and 0 elsewhere. This variable accounts for the restraining effect of the wage and price freeze in the fourth quarter of 1971 and the rebound effect after the wage and price controls were relaxed in the first quarter of 1972.

ENDNOTES

The authors are grateful to J.S. Butler, Gabriele Galati, Steve Kamin, Jonathan McCarthy, Richard Peach, Charles Steindel, and two anonymous referees for helpful comments. We also benefited from the suggestions of conference participants at the Bank for International Settlements. Beethika Khan provided excellent research assistance.

1. Dow Jones News Service, January 7, 1997.

2. Our analysis expands on results that we presented in two earlier papers. See Lown and Rich (1997a, 1997b).

3. Gordon (1996), however, obtains an estimate of 5.3 percent for the NAIRU starting in 1996.

4. Gordon's work (1970, 1975, 1977, 1982, 1990) is prominent in the literature on the estimation of the Phillips curve.

5. See King and Watson (1994), Tootell (1994), Fuhrer (1995), King, Stock, and Watson (1995), and Gordon (1996).

6. The estimation of "expectations-augmented" Phillips curves is the result of work by Phelps (1967) and Friedman (1968), who developed the natural rate hypothesis and drew the distinction between the short-run and long-run Phillips curve trade-off.

7. For detailed definitions and sources of data, see the Data Appendix.

8. The results are little affected when the unemployment rate instead of the output gap is used to measure aggregate demand pressure. Potential GDP measures the full-employment level of output or the output level at which there is no tendency for inflation to accelerate or decelerate. The level of potential GDP grows over time because of the increased availability of resources (land, labor force, capital stock, and the level of technology). Because potential GDP is not directly observable, several techniques have been developed to calculate estimates of the series. A complete review of these techniques and an evaluation of the alternative potential GDP series are beyond the scope of this paper. As noted in the Data Appendix, we employ a staff estimate of potential GDP to construct the output gap variable.

9. Commodity prices and/or an exchange rate term have been used as supply shock variables in some price-inflation Phillips curve models. We do not include these terms in our specification, however, because we found their effects to be small and statistically insignificant. The absence of a strong link between commodity prices and inflation is consistent with evidence presented by Blomberg and Harris (1995), who document a recent decline in the predictive power of commodity prices for inflation.

10. We exclude the net negative real oil price change variable from equation 1 because the variable displays quantitatively and statistically insignificant effects.

11. The compensation growth Phillips curve described later in the text includes dummy variables to capture the effects from the imposition and relaxation of wage and price controls during the 1970s. We exclude these dummy variables from the traditional price-inflation Phillips curve because they were found to be statistically insignificant. Alternative dating schemes for the dummy variables (Gordon 1982) also proved to be unimportant in explaining the dynamics of inflation during the 1971-75 period.

12. This test yields an F-statistic, which is distributed asymptotically as F with $(m, n\text{-}k)$ degrees of freedom under the null hypothesis. The values of n and $n+m$ refer to the number of observations in the first subperiod and the total sample, respectively. The value of k refers to the number of parameters in the model.

13. This test yields a likelihood ratio statistic, which is distributed asymptotically as chi-square with k degrees of freedom under the null hypothesis.

14. We also looked for evidence of parameter instability using the CUSUM and CUSUMSQ tests proposed by Brown, Durbin, and Evans (1975). The tests are based on recursive residuals, with the CUSUM test primarily used to detect gradual structural change and the CUSUMSQ test used to detect sudden structural change. The tests provided no evidence of parameter instability.

15. The dynamic simulation yielded similar results for the 1994-96 period.

16. Meyer (1997) notes that the declines in computer prices and import prices over the current expansion may also be acting as temporary supply shocks helping to restrain inflationary pressures in the economy. Moreover, as an additional explanation for the inflation puzzle, he cites firms' inability to raise prices because of increased international competitive pressures. We do not address these factors in this paper and instead restrict our attention to the two explanations that concern labor market phenomena. Further, while our analysis is not exhaustive, we nevertheless believe that it is instructive to evaluate these explanations before considering alternative hypotheses.

17. Our focus on compensation growth is also motivated by the idea that the pricing decision of a firm should be based on a consideration of its total labor costs rather than the behavior of the wage and benefit

Note 17 continued
components of these costs. In addition, the data preclude us from obtaining observations on wages and benefits separately over the full sample period. The employment cost index, which provides measures of wages and benefits, is only available beginning in 1980 for the nonfarm sector.

18. We modify the traditional price-inflation Phillips curve to include unit labor costs rather than compensation per hour because it is the behavior of compensation growth *relative* to productivity growth that is relevant for describing the dynamics of the inflation process. That is, greater productivity growth will act to offset the inflationary pressure on prices arising from an increase in compensation growth.

19. Note that our model does not allow us to examine whether a shift in the Federal Reserve's inflation fighting credibility has changed the inflation process by directly altering inflation expectations. Such an examination is beyond the scope of this paper and would involve estimating a separate equation for inflation expectations and including some measure of Federal Reserve credibility as an explanatory variable. Previous evidence, however, suggests that such a shift has not taken place. Blanchard (1984) notes that similar types of Phillips curves remained stable even after the 1979 change in Federal Reserve operating procedures.

20. As the value of the test statistics in Table 2 indicates, the Chow tests fail to reject the null hypothesis of parameter stability at conventional significance levels. However, this result is not particularly informative because the Chow tests also failed to reject the null hypothesis of model stability for the traditional Phillips curve.

21. The increase in the forecasted value for inflation primarily reflects the influence of a change in the output gap and the oil price variable.

22. For definitions of the data and their sources, see the Data Appendix.

23. For example, we could follow the approach of Fuhrer (1995), who assumes a value of 6 percent for the NAIRU, and use the unemployment gap (the difference between the actual level of unemployment and the NAIRU) instead of the unemployment rate as an explanatory variable in equation 3. This approach, however, would not affect the regression results other than to change the estimated value of the constant term.

24. Fuhrer (1995) also finds an absence of significant rate-of-change effects for the unemployment rate in wage-inflation Phillips curve models.

25. The definition of the dummy variable is from Englander and Los (1983).

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