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The Stock Price Effects of Bank Holding Company Securities Issuance

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This paper examines the announcement effects of bank holding company (BHC) securities issuance on their common stock prices. A key finding is that since December 1981 when objective minimum capital regulations were put into place, announcements of common stock issuance have been associated with statistically significant negative abnormal common stock returns for BHCs under regulatory pressure to boost capital. No such effects were found for highly-capitalized BHCs that were not under regulatory pressure to boost capital. These results suggest that poorly-capitalized BHCs will be reluctant to issue common stock to meet capital requirements. They also suggest that the deadweight costs associated with common stock issuance by well-capitalized banking organizations are small or nonexistent.

Bank and bank holding company (BHC) capital regulation is becoming an increasingly important tool to limit banking risk. More capital relative to assets provides a greater cushion to absorb losses. Moreover, as Furlong and Keeley (1987a, 1987b) show, more capital relative to assets reduces banks' incentives to increase asset risk. Thus, an increase in BHCs' capital-to-asset ratios should reduce the risk exposure of the deposit insurance system.[1]

Capital regulation was strengthened[2] in December 1981 when specific bank and bank holding company minimum capital standards were introduced for the first time, a departure from the previous subjective peer-group type of capital regulation. In addition, these minimum capital requirements were modified in 1983 to include the multinational bank holding companies and again in 1985 to standardize the minimum requirements for all banks and bank holding companies.[3]

In early August 1988, the Board of Governors adopted an even more stringent set of "risk-based" capital requirements for BHCs based on an international agreement among the twelve leading industrial countries. These new standards represent an important departure from the current ones in that they require different amounts of capital based on an assessment of an asset's risk class. They also require capital to be held against off-balance-sheet items. Finally, they require more capital for assets in the highest risk class than do current standards and also define capital differently than the current U.S. rules do.

To meet these new capital-to-asset ratio requirements, many banks and bank holding companies either will have to sell assets or increase capital by retaining a higher proportion of earnings and/or raising external capital. BHCs raise external capital by selling a range of different types of securities, including common stock, preferred stock, mandatory convertible debt, convertible debt, and straight subordinated debt.

Ideally, capital regulations should be designed to attain a given degree of risk exposure of the deposit insurance system while minimizing the deadweight costs imposed on the banking organizations subject to the regulations. This paper examines the stock market's reaction to BHCs' securities issuance to learn more about the effects of capital regulation on the banking firm. Specifically, the effects on BHCs' stock prices following the announcement of the issuance of different kinds of securities may reveal whether increasing capital imposes costs on banking organizations and whether increasing capital reduces the risk exposure of the deposit insurance fund.

Two novel aspects of this study are its focus on the differences between the stock price effects for BHCs under regulatory pressure to augment capital and those that raise external capital voluntarily, and its analysis of the changes in these effects after the new specific, objective minimum capital regulations were instituted in December 1981. I find statistically significant negative stock price effects associated with common stock issuance for banking organizations under regulatory pressure to augment capital and positive, but not statistically significant effects for other BHCs. Thus, unlike some studies that argue that announcement effects should be absolutely smaller for BHCs that are known to be under close regulatory scrutiny, I find just the opposite.

This study is organized as follows. Section I reviews the theory and evidence regarding the effects of securities issuance by nonbank firms and discusses the implications for BHCs' securities issuance. Section II reviews the prior studies of BHC securities issuance. Section III discusses the methodology and data employed in this study and Section IV presents the results. Section V presents a summary and conclusions.

I. Theory and Evidence from Nonbanking Firms: Implications for Bank Holding Companies

There is now an extensive literature regarding the valuation effects of securities issuance by industrial and utility firms. Modigliani and Miller (1958) have shown that in competitive markets without distortions, such as taxes, bankruptcy costs, agency costs, and asymmetric information, a firm's capital structure is irrelevant. If so, securities issuance should not affect a firm's stock price.[4]

However, as Smith (1986) points out, empirical studies have found statistically significant negative stock price effects of common stock issuance by industrial firms of approximately −3.14 percent, as well as negative significant effects associated with the issuance of preferred stock and bonds that are convertible into common stock. No statistically significant effects are found for other types of securities, although usually the point estimates are negative. Utility firms also have negative, but much smaller, announcement effects associated with common stock issuance, averaging about −.75 percent.

In an attempt to explain these empirical findings, theory has developed along two main lines. One argues that the existence of such distortions as taxes, bankruptcy costs, and agency costs means that capital structure does matter and that securities issuance will affect stock prices. The other line of reasoning relies on information asymmetries and signalling. Below, these two types of theories are discussed.

Capital Structure Theory

Although firms may indeed have optimal capital structures, it is unclear whether the existence of optimal capital structures could explain the negative stock price effects associated with common stock issuance by industrial and utility firms. The reason is that voluntary securities issuance should always represent a movement toward (and certainly not away from) a firm's optimum capital structure. As a result, the effects of a voluntary securities issuance (which affects capital structure) should be positive or zero.

Thus, while capital structure theory might be able to explain the negative effects of involuntary securities issuance, it seems unlikely that it could explain the negative effects associated with voluntary securities issuance. As a result, most of the literature has focused on signalling theories to try to explain the stock price announcement effects of securities issuance.

Signalling Theories

A variety of signalling theories have been built on the premise that management has information about the value of a firm that is not available to outside investors. Thus, the announcement of a security issuance is taken by investors as a signal that reveals at least some of management's inside information.

For example, Miller and Rock (1985) argue that net new external financing is a signal of lower earnings because internal financing would be used if earnings were sufficient. However, this argument implies that all types of external financing should have negative announcement effects and thus fails to explain the different effects of different types of securities issuance.

Myers and Majluf (1984) argue that management has an incentive to issue new stock when they believe the firm's stock is overvalued. However, investors realize that the firm's managers have such an incentive and take the information of a new stock issuance as a signal that the firm's stock is overvalued, which in turn causes the stock's price to fall.

This theory can explain why managers would be reluctant to issue new stock even to fund positive net present value opportunities. If the manager knows that the firm's stock is *undervalued*, it would not be optimal to issue securities to fund a new project that had a modest positive net present value. It also would explain a preference for internal financing as well as for the use of low-risk securities, the values of which do not strongly depend on the firm's value.

However, as Dybvig and Zender (1988) point out, investors would anticipate the tendency to pass up profitable new projects and would pay a lower initial offering price than if managers could somehow be induced to follow an optimal investment policy. (That is, if initial investors could be certain managers always would undertake positive net present value projects, they would be willing to pay more for the stock at the initial offering.) Dybvig and Zender go on to show that an optimal contract for managers can be devised to overcome the underinvestment problem.

Nonetheless, Dybvig and Zender show that even with optimal managerial contracts, the existence of information asymmetries between a firm's managers and its investors will cause investors to treat securities issuance as a signal. When the manager has good news about both the new and old projects, internal financing can and will be used to undertake new projects so that lack of need for external financing will be viewed as a positive signal. Similarly, when a manager has good news about a new project and bad news about an old project, debt is issued, which has a minimal effect on stock prices. However, when the manager has bad news about both the new and old projects, equity will be issued, providing a negative signal, which causes the stock's price to fall.

In sum, even though the Myers and Majluf story may be incomplete, securities issuance probably conveys information about the performance of the existing assets of the firm as well as the prospects for new projects. Thus, it seems most likely that stock issuance is some sort of signal.[5]

Implications for Bank Holding Companies

It seems likely that signalling theory would apply to securities issued by BHCs, as well as by nonbanking firms. However, effects for BHCs might differ from those of industrial firms because BHCs are so highly regulated.

The most common argument regarding the effects of regulation is that the market's knowledge of regulatory policy reduces the information that otherwise would be revealed by a security issuance. For example, the stock price effects associated with announcements of utility firms' common stock offerings are on average absolutely smaller than those for industrial firms. Utility firms' tendency to make repeated stock offerings (due to regulation) and the fact that utilities' stock offerings often require prior regulatory commission approval appear to diminish the information content of actual announcements and thus may explain why utilities have smaller absolute stock price announcement effects than industrial firms.

Likewise, the information content and stock price announcement effects of BHCs' securities issuance might be smaller (in absolute value) than those for nonregulated firms, even though a BHC need not obtain prior regulatory approval to issue new securities. The market's knowledge of the BHC regulatory process might well dilute the information content associated with a BHC's security offering, particularly for organizations known to be under regulatory pressure to boost capital. Moreover, since BHC capital regulation shifted to objective, minimum standards beginning in 1981, one would expect smaller absolute stock price effects during the post-1981 period.

On the other hand, there are several reasons why the stock price announcement effects associated with BHCs' securities issuance might be *more negative* than those of industrial firms. First, if the value of the deposit insurance guarantee is capitalized in a BHC's common stock value, a security issuance that is forced on a BHC by its regulator in an effort to diminish the risk exposure of the deposit insurance fund could lead to a larger negative effect because such an issuance would diminish the (option) value of the deposit insurance guarantee. In particular, one would expect BHCs with low capital positions to experience larger negative announcement effects than would highly-capitalized BHCs.[6] Similarly, a regulatory-induced increase in capital could result in larger negative announcement effects because distortions such as taxes or agency costs could make a forced change in capital structure away from the BHC's private optimum costly.

One final reason that the announcement effects for BHCs may be more negative than those for industrial and utility firms is that regulators may have inside information obtained during bank and bank holding company examinations. Thus, a securities issuance by a BHC known by the market to be under regulatory pressure to augment capital might convey information about the firm's earning prospects.

II. Previous Empirical Research on BHC Securities Issuance

Since there are theoretical arguments both for larger and for smaller announcement effects for BHCs' securities issuance than for industrials', the question regarding which forces dominate is basically an empirical one. Thus, in this section the available empirical studies are reviewed. There are several unpublished papers dealing with the effects of bank holding companies' securities issuance. These are papers by Isberg and Brown (1987), Wansley and Dhillon (1987), Wall and Peterson (1988), and Polonchek, Slovin, and Sushka (1987).

Isberg and Brown

Isberg and Brown (1987) argue that for the 1981 to 1985 period,[7] new common stock issues were the only type of security issuance associated with statistically significant negative common stock returns for BHCs both above and below the contemporaneous capital standards. Although two-day cumulative average prediction errors and Z statistics are not reported, it appears that they found a −1.1 percent effect for BHCs meeting the capital standards and a −2.0 percent effect for BHCs below the standards. However, since many BHCs issued capital *prior* to the implementation of new capital standards to be in compliance, it appears that many of the events characterized by this study as common stock issues by BHCs above the *current* standards were really issues intended to bring the holding company into compliance with expected future standards.

Wansley and Dhillon

Wansley and Dhillon (1987) examine the valuation effects of six types of securities issuance by BHCs between 1978 and 1985: common stock, preferred stock, convertible preferred stock, straight debt—non-shelf, straight debt—shelf, and debt-for-equity swaps. They find statistically significant abnormal returns for common stock of −1.5 percent, significant positive returns for preferred stock of 0.8 percent and no significant abnormal returns for other types of securities issuance. Since their estimate of the size of the announcement effect associated with common stock issuance is much smaller than that found for industrial firms, they argue that banking regulation, like utility regulation, reduces the uncertainty and information content of new securities issuance and therefore reduces the absolute size of the stock price announcement effect.[8]

Wall and Peterson

Wall and Peterson (1988) examine the valuation effects of common stock, preferred stock, convertible debt, mandatory convertible debt, and subordinated debt issuance by BHCs from 1982 through 1986. One innovation of their study is that they obtain the announcement day from the Dow Jones News Service instead of the *Wall Street Journal Index* as the other studies do. They argue that this allows them to pinpoint the actual first trading day that would be affected by the announcement. (Thus, they use only a one-day event period.) They find a statistically significant −1.5 percent abnormal return for common stock issuance, but no significant effects for other types of securities issuance.

Polonchek, et al.

Finally, Polonchek, Slovin, and Sushka (1987) follow a methodology that is closest to that of this paper. They examine the valuation effects of various types of securities issuance for the 1975 to 1985 period and distinguish the pre-1981 period from the post-1981 period. They also distinguish the effects for multinational BHCs from those for other BHCs.[9]

They find statistically significant negative abnormal returns for common stock issuance prior to December 1981 (−1.7 percent) but not for any other types of securities.[10] After December 1981 abnormal returns also are negative (−1.1 percent) but are not statistically significant. Even though the absolute decline in abnormal returns appears not to be statistically significant, they argue that the explanation for the decline is that during the post-1981 period, capital decisions were determined more by regulatory factors and thus contained a smaller (negative) information component.

They also find larger negative point estimates for multinational BHCs' issuance of common stock during the 1982–1984 period than for those of other BHCs (−1.9 percent for multinationals versus −0.8 percent for others), but it appears that the difference is not statistically significant. They argue that this apparent pattern arises because the multinationals were not subject to capital requirements until 1983. However, the main reason that the multinationals were not subject to capital requirements until 1983 is that none of these banking organizations would have met the 1981 requirements in December 1981 (see Keeley [1988]). That is, they were given time to raise capital and bring themselves into compliance. This suggests that the multinationals were, in fact, under regulatory pressure to boost their capital by a large amount. Since the multinationals actually were under severe regulatory pressure to raise capital to meet the 1983 and 1985 standards before those standards took effect, the evidence that their abnormal returns were larger (in absolute value) than those of other BHCs actually contradicts the hypothesis that regulation would cause abnormal returns to decline in absolute value.

Summary

On the whole, these studies support the hypothesis that there are negative announcement effects associated with common stock issuance by BHCs. The absolute values of the effects for BHCs appear to be smaller than those found for industrial firms but larger than those found for utilities. Although these results are broadly consistent with the hypothesis that BHC regulation dilutes the information content of securities offerings (since the absolute sizes of the BHC effects are smaller than those for industrial firms), they are not inconsistent with a number of other hypotheses. Moreover, these results tell us little about which aspects of the regulatory process may account for the smaller stock price effects.

With the exception of Polonchek, Slovin, and Sushka, none of the papers tries to distinguish the announcement effects before and after the December 1981 change in capital regulation. Similarly, none of the papers tries to distinguish the announcement effects for BHCs that had to issue capital to meet the guidelines from those that did not, although Isberg and Brown do compare the results based (apparently) on *contemporaneous* compliance with capital guidelines. Moreover, none of the papers distinguishes the effects before and after December 1981 for BHCs that would have met the guidelines from those that would not have.[11] In the analysis below, I address these issues.

III. Methodology and Data

This paper employs the market model to estimate the abnormal stock price returns associated with BHCs' securities issuance. The model is estimated with data on each BHC's daily stock returns for a 60-day period beginning 80 trading days before and ending 20 trading days before the announcement of each security issuance in order to provide a forecast of what the stock's returns would have been absent the announcement of a security issuance. (A stock's rate of return is defined as the change in the stock's price plus dividend payments, if any, divided by the original stock price.) Then estimates of abnormal stock price returns around the announcement date of securities issuance are computed as the difference between the actual and predicted value.

The market model is:

$$R_{jt} = a_j + b_j R_{mt} + e_{jt} \tag{1}$$

where:

R_{jt} = rate of return on BHC j's common stock over period t,

R_{mt} = rate of return on the CRSP value-weighted market index over period t,

a_j, b_j are coefficients for BHC j,

e_{jt} = the error term for BHC j at time t, and

t is a time index in event time, that is, $t = 81$ is the announcement date.

The prediction error for firm j on event day t is defined as:

$$PE_{jt} = R_{jt} - (\hat{a}_j + \hat{b}_j R_{mt}), \tag{2}$$

where the symbol "ˆ" denotes an estimated value.

The daily prediction errors can be averaged over events of a particular type (for example, common stock issuance) to produce daily average prediction errors:

$$APE_t = (1/N) \Sigma_j PE_{jt}, \tag{3}$$

where N is the number of events in the sample category. Tests of statistical significance are based on standardized prediction errors (see Mikkelson and Partch [1986]). Each standardized prediction error (SPE_{jt}) is defined as

$$SPE_{jt} = PE_{jt} / S_{jt} \tag{4}$$

where

$$S_{jt} = \{V_j^2[1 + 1/M + (R_{mt} - \bar{R}_m)^2 / \Sigma_i (R_{mi} - \bar{R}_m)^2]\}^{1/2} \tag{5}$$

V_j^2 is the residual variance of firm j's market-model regression, M is the number of days in the period used to estimate the market model (60 days), the summation over index i indicates summation over the period used to estimate the market model, and $\bar{R}_m$ is the mean market return over the estimation period. The average standardized prediction error is:

$$ASPE_t = (1/N) \Sigma_j SPE_{jt}. \tag{6}$$

Assuming the individual daily prediction errors are normally distributed, each SPE_{jt} is distributed Student t. If the individual prediction errors are cross-sectionally independent, the following Z statistic is asymptotically distributed unit normal under the hypothesis that the average standardized prediction error equals zero:

$$Z = \sqrt{N}\,(ASPE_t). \tag{7}$$

The empirical analysis focuses on abnormal returns associated with the announcement of a security issuance. Abnormal returns are defined as the sum of the prediction errors for the day preceding and the day the announcement is reported. This procedure allows for the possibility that the announcement may have been made during trading hours the previous day and then reported the next day.

To test the hypothesis that the two-day prediction error averaged over N events (in a given category) is zero, I compute the average two-day standardized prediction error:

$$AISPE_{t_{-1},t_0} = (1/N)\,\Sigma_j\,\Sigma^{0}_{t=-1}\,SPE_{jt}\,/\sqrt{2} \tag{8}$$

and thus the Z statistic is:

$$Z = \sqrt{N}\,(AISPE_{t_{-1},t_0}). \tag{9}$$

Data on the returns of each BHC's security and the overall market's returns are from the Center for Research on Securities Prices (CRSP) daily returns tapes. Data on securities issuance are from Irving Trust's *Capital Securities Issued: Commercial Banking* for the 1977 through 1986 period. Data from Compustat also are used to identify the quarters when major securities issues took place.

The announcement date is defined as the date of the first report of a security issuance in the *Wall Street Journal* or the SEC registration date, whichever was first. Announcement dates were obtained by searching the *Wall Street Journal Index* for the year of and the year before the actual issuance. The assumption is that the market generally only becomes aware of a security issuance after it is formally announced or that the probability of a security issuance increases upon a formal announcement. Security issues not reported in the *Wall Street Journal* were not included in the sample.

IV. Results

Dollar Volume of Securities Issuance

Charts 1, 2, and 3 plot the dollar value of debt, common stock, and preferred stock issued by all BHCs included in Irving Trust's publication. Since this publication includes many very small issues, including those of small holding companies, it appears to be a fairly complete account of publicly-traded BHCs' securities issuance. Charts 1, 2, and 3 generally show increased security issuance in response to capital regulation.

Chart 1 shows that the dollar volume of debt issued increased greatly following the change in capital regulation in December 1981. Since subordinated debt counts as total capital and mandatory convertible debt counts as primary capital, the large rise in debt issuance is not surprising.

Chart 1
Dollar Value of Public Debt Issued

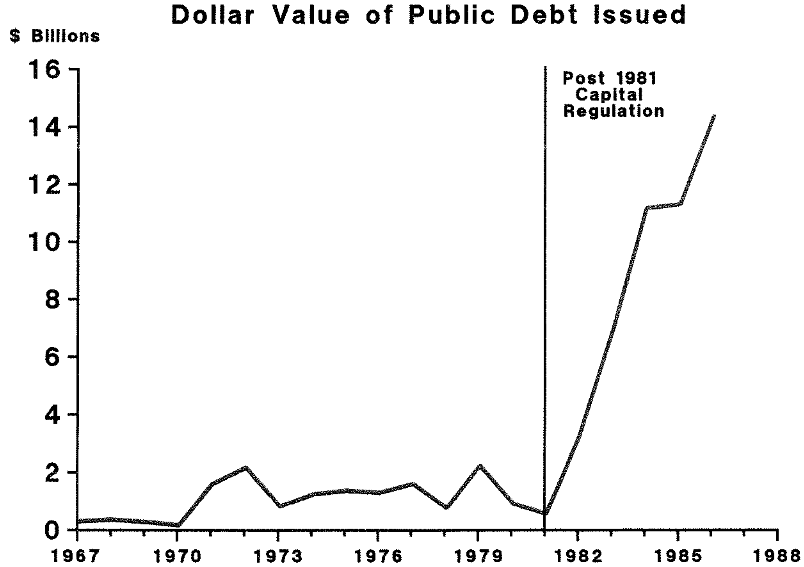


Chart 2 shows a rise in the dollar volume of common stock issued in 1981 and also an even larger rise in 1986. Some of the increased issuance in 1981 could be in anticipation of the new capital guidelines. However, it is unclear whether the even larger increase in 1986 can be explained by capital regulation unless it was in anticipation of the risk-based guidelines which were very much in public view at the time.

Finally, Chart 3 shows a large rise in preferred stock issuance in 1982, apparently in response to the new capital guidelines.

Chart 2
Dollar Value of Common Stock Issued

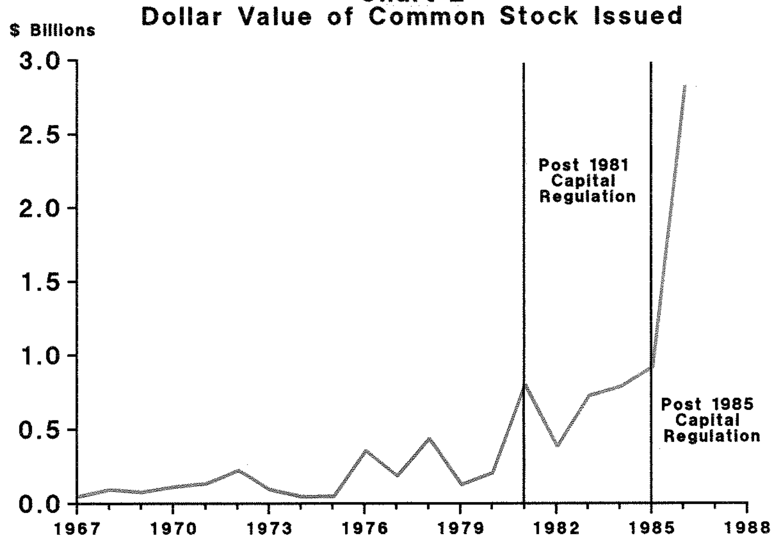
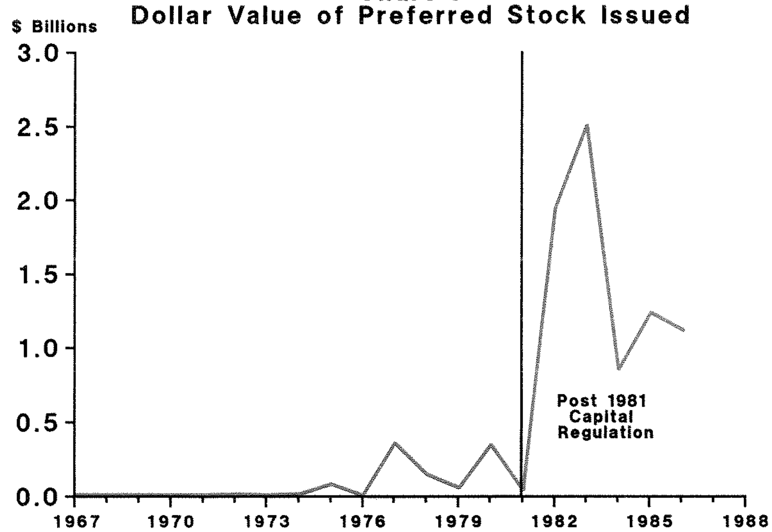


Chart 3
Dollar Value of Preferred Stock Issued



Sample Characteristics

Table 1 displays the distribution of the sample of securities announcement events analyzed in this study by type and by year. All of these securities met the regulatory definition of either primary or total capital used between 1981 and 1986. Consistent with the evidence in Charts 1 through 3, there are more security offerings per year during the 1982–1986 period than during the 1975–1981 period. Also, debt issues were the most common, followed by preferred stock, with common stock the least frequent type of offering.

Table 2 shows the distribution of the sample of securities offerings by BHC and type of issue. It shows that 34 bank holding companies were responsible for the 155 security offerings studied here. It also shows that most of the holding companies issued several different types of securities over the 1975 to 1986 period.

Prediction Errors 1975–1986

Abnormal returns—that is, two-day (cumulative) prediction errors—averaged over the entire 1975–1986 period separately for each of seven classes of securities and associated Z statistics are presented in Table 3. In addition, average abnormal returns and Z statistics are presented for simultaneous issues of debt and common stock and debt and preferred stock.

This disaggregation of security type is based on the regulatory definition of primary and total capital that was used throughout the 1982–1986 period. All of the debt issues analyzed meet the maturity requirement for inclusion in the definition of total capital. All holding company debt legally is subordinated to deposits. Nonetheless, I also examined separately debt that was explicitly called subordinated from that not explicitly called subordinated. No significant differences were found, however.[12]

The results in Table 3 indicate that, on average, there are negative abnormal returns associated with the issuance of common stock and mandatory convertible debt (which eventually will be converted into common stock). The estimated magnitude of the announcement effect for common stock is -1.5 percent, a similar magnitude to that found in the Wansley and Dhillon (1987) and Polonchek et al. (1987) studies, both of which cover similar time periods. Simultaneous issues of common stock and debt also have significant negative announcement effects, as might be expected due to the negative effect of the common stock issuance.[13]

Significant positive abnormal returns of 1.1 percent are found for perpetual preferred stock, a result similar to that of Wansley and Dhillon (1987), who find an abnormal return of 0.8 percent, and Polonchek et al., who find an abnormal return of 1.57 percent for non-multinational BHCs during the 1982–1984 period.

These results are somewhat surprising, since, in terms of

Table 1
Distribution of Number of Issues by Type of Security

	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	All Years
Common		2		3		3	1	2	4	4	3	2	24
Preferred			1	1		3	1	8	12	3	3	1	33
Convertible Debt	1				2		1				1	1	6
Mandatory Convertible Debt								3		12	6	1	22
Straight Debt	2		4	3	1	3	2	7	7	16	11	7	63
Multiple Issue			4					2		1			7
Total	3	2	9	7	3	9	5	22	23	36	24	12	155

risk characteristics, perpetual preferred is most like common stock. However, there are two important differences. First, the market may have viewed preferred stock as implicitly insured in light of the FDIC's resolution of the Continental Illinois failure in 1984. (The FDIC implicitly insured preferred stock holders as well as debt holders since the BHC was never declared insolvent.) Thus, preferred stock would have risk characteristics more similar to bank deposits than to common stock. Second, an issuance of preferred stock may contain information about the ability of the organization to meet preferred stock dividends, which the market would view favorably.

I also find significant (at the 10 percent level) negative prediction errors (-0.74 percent) for mandatory convertible debt. None of the other studies find such an effect, but, except for Wall and Peterson (1988), neither do they distinguish between mandatory convertible debt and convertible debt. Since convertible debt is usually convertible at the issuer's option, it is much more like straight debt, whereas mandatory convertible debt has risk characteris-

Table 2

Distribution of Sample by BHC and Security Type

	BHC	Common Stock	Convertible Debt	Mandatory Convertible Debt	Multiple Issue	Preferred Stock	Straight Debt	Total
1	Bank of America	1		1		2	3	7
2	Barnett Banks	1		1		1	1	4
3	Bank of NY					1		1
4	Bank of Boston		1	1			4	6
5	Bankers Trust	2		2	1		2	7
6	Citicorp	1				6	8	15
7	Citizens First	2						2
8	Chemical NY Corp	2	1	1	1	1	2	8
9	Chase Manhattan		1	1		2	4	8
10	Equimark					1		1
11	First Bank System			1				1
12	First City Bancorp	1		1		2	1	5
13	First Fed. Bancorp	1						1
14	First Chicago	1		1		2	3	7
15	First Penn. Corp	1				1		2
16	First Wisconsin					1		1
17	First Interstate	1		1			4	6
18	J.P. Morgan	1				1	4	6
19	Key Corp	1				1		2
20	MCorp	1						1
21	Mellon						1	1
22	Manuf. Hanover		1	3	1	2	3	10
23	Marine Midland			2		1	1	4
24	NBD Bancorp		1				1	2
25	Norwest Corp					2	1	3
26	Norstar Bancorp	1				1		2
27	Banc One	1						1
28	Republic NY Corp	3			2	3	4	12
29	Signet						1	1
30	Security Pacific		1	2		1	4	8
31	Texas Commerce	1					1	2
32	United Jersey					1		1
33	Irving Bancorp			1	1			2
34	Wells Fargo	1		3	1		10	15
		24	6	22	7	33	63	155

tics similar to those of common stock and should have similar announcement effects. However, aside from their selection of a different sample of events, it is unclear why Wall and Peterson's results differ.

As in all of the studies reviewed, I do not find significant abnormal returns associated with straight debt. This finding is similar to that for industrial and utility firms and thus may reflect the low-risk nature of this security. Moreover, the market may have regarded straight debt issued by BHCs during this period as having a high probability of being FDIC-insured following the Continental episode.

I also examined cumulative prediction errors for the 18-day period between the estimation period and the (2-day) announcement period and for the 18-day period after the announcement period averaged over each type of security, but none of the average cumulative prediction errors were statistically significantly different from zero. This suggests that the stock price announcement effects are permanent. Moreover, the market model was estimated over two other sample periods, one beginning 20 days after the announcement period (days 100–158) and another including both the pre- and post-announcement period samples (days 1–60 plus days 100–158) to test the robustness of the results. The results were remarkably similar for all three estimation periods.

In sum, these results strongly suggest negative announcement effects for issues of common stock and securities with risk characteristics similar to common stock, such as mandatory convertible debt.[14] In the next sections, I test for possible differences in effects over time and between groups to determine whether deposit insurance effects are important and through what avenues capital regulation may affect the size of the announcement effects.

Table 3

Average Two-Day Prediction Errors (APE) 1975–1986, By Type of Security[a]

Type of Event (Public Offerings for Cash)	APE	Z	Number of Events (155 Total)	Percent Negative
Common Stock	−.015***	−4.10	24	75%
Convertible Debt	−.0021	−.10	6	43%
Mandatory Conv. Debt	−.0074*	−1.67	22	73%
Multiple Simultaneous Issue				
Debt/Common Stock	−.031***	−2.70	2	100%
Debt/Preferred Stock	−.0072	−1.06	5	80%
Preferred Stock				
Limited Life	−.00081	−.91	9	44%
Perpetual	.011**	2.29	21	43%
Convertible	−.020	−1.31	3	67%
Straight Debt[b]	.00012	−.02	63	50%

*** Significantly different from zero at the 1% level
** Significantly different from zero at the 5% level
* Significantly different from zero at the 10% level

a Average two-day prediction errors for the day preceding and the day of the announcement. Prediction errors are actual residual returns, not percentage returns.

b Includes both shelf and non-shelf registration and subordinated and unsubordinated debt. However, none of the APEs is statistically significant nor are there any significant differences among these categories.

Differences over Time

Table 4 presents two-day average prediction errors for the period prior to the new capital regulations, January 1, 1975 through November 30, 1981, and for the period after the institution of the regulations, December 1, 1981 through December 31, 1986. There is a striking decline in the absolute size of the announcement effect associated with common stock issuance from -2.6 percent to -0.79 percent, which is statistically significant at the one percent level. No other significant differences are found. Thus, it appears that the institution of capital regulation did have a major effect on the stock price effect associated with common stock issuance.

Polonchek et al. also find an absolute decline in the (negative) effect of common stock issuance, although it is half as large and not statistically significant. These differences in results may be due to the more powerful statistical techniques and/or the longer sample period used in this paper. My results for the 1982–1986 period, however, differ in magnitude from those of Wall and Peterson, who find negative statistically-significant effects for common stock issuance of -1.5 percent for this period, possibly because of the somewhat different sample they employ.

The standard explanation for the apparent decline is that increased capital regulation made equity offerings more predictable and thus diminished their information content, especially compared to the information conveyed by offerings made during the pre-December 1981 period, when they were more likely to be voluntary. To test this explanation, I examine differences in announcement effects between the group of BHCs voluntarily issuing capital and those under regulatory pressure to do so. If the information content argument were correct, BHCs under regulatory pressure to boost capital would experience less negative announcement effects associated with stock issuance after the new regulations took effect than would the other group of BHCs.

Differences Between Groups Over Time

Although objective minimum capital regulations were phased in over the 1981 to 1985 period, I would argue that the 1985 standards were the ultimate goal even as early as

Table 4

Average Two-Day Prediction Errors (APE), Before and After the December 1981 Change in Bank Capital Regulation

	Jan. 1, 1975–Nov. 30, 1981		Dec. 1, 1981–Dec. 31, 1986		Absolute Difference	
Type of Event	APE	Z	APE	Z	APE	Z
Common Stock	−.026***	−4.60	−.0079	−1.63	.018***	2.97
Convertible Debt	−.0047	−.53	.0032	.22	.0079	.75
Mandatory Conv. Debt	—	—	−.0074*	−1.67	—	—
Multiple Issues						
Debt/Common Stock	−.031***	−2.70	—	—	—	—
Debt/Preferred Stock	−.015	−1.02	−.0023	−.18	.0043	.84
Preferred Stock						
Limited Life	−.000059	−.0033	−.0014	−.20	.0013	.20
Perpetual	—	—	.011**	2.29	—	—
Convertible	.0015	−.25	−.064	−1.89	.065	1.64
Straight Debt	−.0062	−.71	.0016	.58	.0076	1.29

*** Significantly different from zero at the 1% level
** Significantly different from zero at the 5% level
* Significantly different from zero at the 10% level

1981. The main reason the 1985 standards were not immediately imposed was to give institutions time to raise the necessary capital to bring them into compliance. In keeping with this interpretation, this paper distinguishes those banking organizations that would have met the 1985 primary capital requirements in 1981 from those that would not have. (See Keeley [1988].) Throughout the paper I refer to the former as "capital sufficient" and the latter as "capital deficient" banking organizations. As shown in Keeley (1988), capital deficient banking organizations did in fact increase capital both absolutely and relative to capital sufficient organizations.[15]

Table 5 presents separate estimates of the effects of common stock issuance for capital deficient and sufficient organizations both before and after the 1981 change in capital regulation. I also examined the announcement effects for each of the other types of securities issuance analyzed in Table 4, but no significant differences between the time periods or between capital sufficient and deficient groups were found.

The results in Table 5 suggest that the stock price effects for capital sufficient BHCs changed from -1.2 percent in the pre-1981 period to positive 1.5 percent in the post-1981 period. This change is statistically significant. Moreover, although capital deficient BHCs' estimated effect declined in absolute value, the change was not statistically significant.

If increased capital regulation reduced the signal content of common stock issuance, one would expect the announcement effects to be less negative during the post-1981 period. While Table 5 does show such a pattern for each group separately, the change is not statistically significant for the capital deficient organizations. Moreover, simple signalling theory also would predict that capital deficient BHCs' returns should be less negative than capital sufficient BHCs' returns, which would be less predictable and thus should contain more information. These results thus cast doubt on this simple signalling hypothesis since the pattern of results is opposite to that which it would predict.

An alternative interpretation of these results is that securities issuance diminishes the value of the deposit insurance guarantee. The larger negative stock price effects for capital deficient banking organizations, especially during the post-1981 period, are consistent with the view that the value of (underpriced) deposit insurance is capitalized in the share prices of capital deficient banking organizations and that increases in their capital diminished the value of that asset.

A second, but not mutually exclusive hypothesis is that regulators have inside information which is revealed to investors by the nature of a security issuance.[16] Below, I explore these two hypotheses further.

Capital Structure Effects

If the results in Table 5 primarily reflect a diminution of the value of deposit insurance, in theory, issues that have greater proportional effects on the capital-to-asset

Table 5

Average Two-Day Prediction Errors Associated with Announcement of Common Stock Issuance by Capital Deficient and Sufficient BHCs

	Capital Deficient		Capital Sufficient		Absolute Difference	
Time Period of Event	APE	Z	APE	Z	APE	Z
Pre-1981 Capital Reg. Change	−.033***	−4.43	−.012*	−1.70	.021***	2.73
Post-1981 Capital Reg. Change	−.020***	−2.90	.015	1.27	.035***	4.17
Difference	.013	1.53	.027***	2.97	.014	1.44

*** Significantly different from zero at the 1% level
** Significantly different from zero at the 5% level
* Significantly different from zero at the 10% level

ratio should have more negative abnormal returns. Consequently, I regress abnormal returns on the size of the issue, measured by the percentage change in the capital-to-asset ratio caused by the common stock issue.[17]

The results of such regressions, estimated using generalized least squares with individual error variances calculated using Equation 5, are reported in Table 6. Separate estimates are presented for capital deficient and sufficient banking organizations for three time periods: the entire sample period, 1975–1986; the period prior to the new capital regulations, December 1, 1975 through November 30, 1981; and the period after the new regulations were introduced, December 1, 1981 through December 31, 1988.

Although the results of these regressions should be viewed with caution because of the very small sample sizes, they nevertheless do suggest a marked change in the relationship between capital deficient organizations' abnormal returns and the percentage effect of the common stock issuance on the market value capital-to-asset ratio.[18] During the early period before explicit capital guidelines were in place, issues that had larger effects on the capital-to-asset ratio had less negative abnormal returns. This suggests that issues during this period were voluntary, even by banking organizations with low capital-to-asset ratios. However, during the post-December 1981 period, the point estimate suggests a negative relationship, although it is not statistically significant. In theory, if the negative mean abnormal returns were due to a diminution of the value of the deposit insurance guarantee, the relationship between the size of the issue and abnormal returns should be negative. Thus, these results are not inconsistent with this hypothesis. However, given the small sample sizes and the lack of statistical significance, neither do these results provide strong support for this hypothesis.

The results for the capital sufficient banking organizations are more striking. They show, during the post-December 1981 period, a statistically significant positive relationship between abnormal returns and the size of the issue. Thus, large issues (relative to capital) by organizations already meeting the capital requirements appear to be taken by the market as positive signals. Since such issues are voluntary,[19] presumably they would not reflect a diminution in the value of the deposit insurance guarantee or an implicit regulatory tax.

In sum, the results of these regressions provide some support for the capital structure theory, which predicts that issue size relative to capital is important and that stock price effects should become more negative for capital deficient organizations as the size of the issue increases. They also suggest that the deadweight costs of common stock issuance for well-capitalized banking organizations are small or nonexistent since, on average, stock price announcement effects are not negative and even become more positive as the relative size of the issue increases.

Table 6

Relationship Between Abnormal Returns, Capital Adequacy and the Percent Change in the Capital-to-Asset Ratio Due to Common Stock Issuance[a]

	1975–1986	1975–1981	1981–1986
Capital Deficient			
n	16	6	10
$\bar{R}^2$	−.067	.58	.018
Intercept	−.025**	−.069***	−.0079
	(.010)	(0.15)	(.0098)
Percent Change in Capital to Asset Ratio	.029	.48**	−.12
	(.12)	(.17)	(.11)
Capital Sufficient			
n	8	3	5
$\bar{R}^2$	.37	.44	.87
Intercept	−.051*	−.057	−.063**
	(.022)	(.028)	(.014)
Percent Change in Capital to Asset Ratio	.42*	.34	.66**
	(.19)	(.21)	(.13)

[a] GLS estimates

*** Significantly different from zero at the 1% level
** Significantly different from zero at the 5% level
* Significantly different from zero at the 10% level

Inside Information

These results also are consistent with the second hypothesis that the type of securities issued conveys inside information about earning prospects obtained by regulators during bank and bank holding company examinations. Since a banking organization's balance sheet is available to outside investors, the market can readily determine whether the BHC is under regulatory pressure to increase its capital ratio. However, the market does not necessarily know the future prospects of the BHC or the method the BHC will use to augment capital.

It seems likely that investors would look for information about a BHC's prospects in the type of securities it issues. Capital deficient BHCs that issue common stock may be viewed by investors as needing to do so because they are under regulatory pressure not to issue securities that require increased payouts from earnings, such as debt or preferred stock. Thus, a common stock issuance by a capital deficient BHC may be a signal of management and regulator skepticism about the BHC's ability to generate sufficient future earnings to meet the cash flow requirements of additional debt or preferred stock or to generate cash flow sufficient to permit the accumulation of retained earnings to meet the new capital requirements. On the other hand, if regulators and bank management believe that the banking organization's future earnings prospects are very good, retained earnings rather than a security issuance can be used to meet higher future capital requirements. Moreover, a voluntary issue of common stock by a capital sufficient BHC would not provide a negative signal and might even signal the availability of a positive net present value project.

The positive effects of issue size on the abnormal returns associated with securities issuance by capital sufficient BHCs also might be explained by this hypothesis. Prior to the institution of specific minimum capital guidelines, market participants would have been unsure whether a banking organization's common stock issuance was due to regulatory pressure. Since there was some chance that it was, there was a small mean negative announcement effect even for capital sufficient organizations. However, after specific capital guidelines were introduced, market participants could be confident that a common stock issue by a capital sufficient BHC was not a signal that regulators viewed the organization's earning prospects unfavorably. As a result, in the post-1981 regulatory period, the estimated mean abnormal returns associated with capital sufficient BHCs' common stock issuance were positive and were positively related to the size of the issue.

V. Conclusions

The results of this paper yield some important conclusions regarding the stock price effects of BHCs' securities issuance, especially securities issued by weakly-capitalized banks under regulatory pressure to boost capital. These findings are particularly important in light of the new risk-based capital requirements, which will require many banking organizations to increase their capital-to-asset ratios.

First, common stock issuance appears to have negative and statistically significant announcement effects for weakly-capitalized banking organizations under regulatory pressure to raise capital. Moreover, the effects are fairly large, implying a mean abnormal return of -2 percent (which represents a dilution effect of about -30 percent) for capital deficient banking organizations during the post-1981 period. Thus, contrary to the implication of some previous studies, one cannot be sanguine that the more objective capital regulation in place since December 1981 has significantly reduced the announcement effects associated with common stock issuance for those BHCs under regulatory pressure to augment capital. However, no evidence of negative announcement effects is found for BHCs that are meeting or exceeding regulatory capital guidelines.

Second, common equity (and debt that will be converted into common equity) might appear to be the most costly form of capital from the banking organization's standpoint since it has the largest negative announcement effects. Straight subordinated debt and limited life preferred have no significant stock price effects, and perpetual preferred actually appears to have positive effects. However, it is difficult to draw any strong policy conclusions from these results. One reason is that market participants may have viewed subordinated debt and preferred stock as being at least partially implicitly insured throughout this period.[20] Another reason is that the estimated announcement effects presumably result from optimizing decisions at the banking organization level. Thus, if alternatives to common stock issuance were used instead, it is unclear whether they would have lower costs. Finally, these results may reflect a decrease in the risk exposure of the deposit insurance fund (and a corresponding reduction of the capitalized value of the deposit insurance guarantee), which was the objective of the capital regulations in the first place.

Third, the data do not permit us to determine whether the negative announcement effects associated with common stock issuance simply reflect a negative signal about institutions' values or whether they are the result of a diminution of the capitalized value of the deposit insurance guarantee. While the estimated announcement effects of common stock issuance for capital deficient banking organizations appear to be negatively related to the relative size of the issue, as the deposit insurance hypothesis predicts, the relationship is not statistically significant. Nonetheless, regulators may wish to pursue a policy of requiring more capital since neither explanation for the negative abnormal returns implies that there are social costs associated with more stringent capital regulation and more stringent capital regulation does reduce the risk exposure of the deposit insurance system.

Finally, these results suggest that banking organizations with weak capital positions will attempt to resist regulatory pressure to issue common stock in order to meet capital requirements because of the negative effects on the value of their stock. However, since there is no evidence of negative effects for strongly-capitalized BHCs, they may not be reluctant to issue stock to finance new, positive net present value projects.[21] Moreover, the absence of negative stock price announcement effects for strongly-capitalized banking organizations suggests that the deadweight costs associated with common stock issuance are small or nonexistent for such firms.

ENDNOTES

1. Deposit insurance can be viewed as a put option on the bank's assets at a striking price equal to the promised maturity value of the insured deposits (see Merton [1977]). The value of the put increases as capital relative to assets decreases or as asset risk increases since both factors increase default risk.

2. Keeley (1988) finds that even though bank holding companies partially circumvented the more stringent capital regulations promulgated in the early 1980s, they did nevertheless boost capital-to-asset ratios in response to the regulations. Thus, it appears that BHCs do respond to more stringent capital regulations.

3. In December 1981, minimum primary capital was set at six percent of assets for banks and bank holding companies with assets less than $1 billion and five percent for organizations with assets of $1 billion or more except for "multinational" bank holding companies which were exempted. In June 1983, the five percent requirement was extended to the multinationals. Finally, in June 1985, a uniform 5.5 percent minimum primary capital-to-asset ratio was set for all banking organizations regardless of size.

4. Many financial executives argue that issuing shares at a price below book value depresses the stock's price because it represents a "dilution" of share value. While such a stock issuance does decrease book value per share, it should not depress the market value of the stock as long as the proceeds from the stock issuance can be invested in assets that are at least as profitable as the firm's current assets.

5. In contrast, Scholes (1972), following a different line of reasoning, argues that the demand for a stock is downward sloping due to heterogeneous expectations. Although it is difficult to reconcile heterogeneous expectations with market equilibrium (since people who think the stock is undervalued should buy, thereby driving up the stock's price, and vice versa) differential taxation might explain heterogeneous demand for a stock. Downward sloping demand, in turn, would cause price pressure when there is a new issue. This would explain why only risky securities have negative stock price effects associated with their issuance (since there would not be heterogeneous expectations for riskless securities).

6. As Furlong and Keeley (1987a, 1987b) show, the diminution in the value of the deposit insurance option associated with a given capital infusion is greatest for banking organizations with the lowest capital ratios.

7. It is unclear why they included the year 1981 since the first capital requirements did not go into effect until December 1981.

8. There are two important limitations of the Wansley and Dhillon study. First, they do not allow for potential changes in abnormal returns due to the changed capital regulatory regime beginning in 1981. Second, their announcement period is the day of and the day *after* the announcement, unlike the standard practice of using the day before and the day of the announcement. Thus, they may have underestimated the announcement effects. (I find that the largest negative residual is on the day before the announcement.)

9. Abnormal returns are calculated using the mean returns method, a procedure Brown and Warner (1985) show is not very powerful if the events are clustered in calendar time.

10. They also find significant negative abnormal returns associated with the announcement of dividend reductions both before and after December 1981, which is consistent with the negative abnormal returns associated with common stock issuance.

11. Moreover, several of these papers use statistical techniques with low power. Dhillon and Wansley use an unconventional event period, Isberg and Brown use a one-day instead of two-day event period, and Polonchek, Slovin, and Sushka use the mean adjusted return model instead of the preferable market model and use an unconventional three-day event period. Several of the studies do not adequately disaggregate different types of debt and preferred stock securities (that is, convertible versus mandatory convertible debt, limited life versus perpetual preferred stock).

12. I also tested for the possibility that shelf-registered debt would have different abnormal returns and found no significant differences.

13. Although the point estimate of the effect of the announcement of a simultaneous common/debt issue is

larger than that for common alone, the difference is not statistically significant.

14. I also found a positive announcement effect associated with perpetual preferred stock.

15. Moreover, the probability of security issuance increased for capital deficient organizations relative to capital sufficient organizations with statistically significant increases for preferred stock and debt issuance. See Keeley (1988b).

16. Another possible explanation for this pattern of returns is that the size of the offerings relative to the initial value of the firms in the two groups differs systematically. I tested for this by examining the relative dilution effects of the two groups' common stock offerings. Dilution is defined as the ratio of the change in the aggregate equity value of the outstanding shares (percent change in share price, times share price, times number of shares, divided by 100) to the total dollar proceeds of the issue. A dilution ratio of zero percent means that the announcement of a new offering does not affect the share price of existing shares, and a dilution ratio of -100 percent means that the decline in existing share value equals the value of the new capital raised by the issue. Dilution is interesting to examine because it could be that firms with the smallest abnormal returns also had very small dollar value issues and thus large dilutions.

For all BHCs I found a mean dilution effect of -27 percent, about the same as the -31 percent dilution effect found by Asquith and Mullins (1986) for industrial firms. Thus, even though the percentage stock price effect for bank holding companies is much smaller than that found for industrial firms, the dilution effect is about the same, presumably because banking organizations' stock issues typically raise far less funds in proportion to their pre-issue value than do industrial firms.

More importantly, the pattern of dilution effects is basically the same as the stock-price announcement effects. Capital deficient BHCs have more negative dilution effects than capital sufficient BHCs and both groups show less negative effects during the post-1981 period. Thus, systematic differences in issue size do not appear to explain the pattern of abnormal returns across capital deficient and sufficient organizations.

17. This variable is equal to the value of the issue divided by the pre-issue market value of the firm's equity minus the value of the issue divided by the pre-issue market value of the firm's assets.

18. Two periods were pooled and a model was estimated which allowed the intercept and the coefficient to differ in the two periods. For capital deficient organizations, the change in both the intercept and the coefficient was statistically significant at the 5 percent level. For capital sufficient banking organizations, neither parameter was significantly different.

19. Keeley (1988c) argues that insured banks voluntarily would issue capital in order to protect their valuable charters, which would be forfeited in the event of bankruptcy.

20. As long as subordinated debt and preferred stock are not implicitly insured, there is no apparent theoretical reason to restrict their use as a type of banking capital. They provide the same protection to the deposit insurance fund as common equity and they may have lower costs. See Furlong and Keeley (1987c).

21. These results are consistent with several empirical studies (Marcus and Shaked [1984], Ronn and Verma [1986], and Pennacchi [1987]) which find that for many large banking organizations, the fair value of deposit insurance appears to be less than its price. However, since the value of deposit insurance need not be capitalized into the value of the banking organization and instead may benefit bank depositors and/or borrowers, one cannot conclude from these results that more capital would not significantly reduce the risk exposure of the deposit insurance system.

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Commodity Prices as a Guide for Monetary Policy

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This paper evaluates the usefulness of a commodity price index as an indicator variable for monetary policy purposes. Commodity prices are found to be statistically significant in explaining changes in monetary policy goal variables and to improve somewhat out-of-sample forecast errors of policy variables. However, commodity prices do not by themselves fill the void left by the loss of M1 as an intermediate target, nor do the findings support using a commodity price index in place of M2 as a guide to monetary policy.

The U.S. shed the last vestige of the gold standard in 1971 with the official decision to suspend gold convertibility. This decision actually was just the final step in a long transition, starting at the time of World War I, from a commodity-based monetary system to a pure fiat monetary system. Aside from the brief official consideration of returning to the gold standard in the early 1980s, little serious thought has been given to abandoning the current fiat system in favor of a commodity standard.[1]

Even though the current fiat system is firmly in place, there are important proposals to link the monetary system to commodity prices. These new proposals generally call for using a commodity index to guide monetary policy, either as an intermediate target or as a more general indicator variable. For the most part, these proposals rely on a basket of commodities, rather than on a single commodity price such as that of gold.[2]

To be used as a guide for monetary policy, a commodity price index should have a reliable relationship with the ultimate variables of concern to monetary policy, such as inflation, economic growth, and unemployment. Movements in the commodity index also ought to precede those of the ultimate policy variables to provide policymakers with an early indication of the impact of their policies.

Accordingly, the purpose of this paper is to examine the relationships between selected commodity price indexes and policy variables to evaluate the usefulness of such indexes in the conduct of monetary policy. The paper begins with a discussion of the prerequisites for a commodity price index to serve as a guide to policy and considers the criticisms raised concerning the use of such an index. The empirical analysis then focuses on two composite indexes, those of the Commodity Research Bureau (CRB) and the Journal of Commerce (JOC). The commodities included in those indexes are listed in the Box in the Appendix. The objective of the analysis is to determine whether changes in the commodity price indexes contain information useful to policy beyond that contained in the intermediate targets like M1 and M2.

I. Commodity Prices in a Fiat System

In a fiat monetary system, a commodity price index can be used to guide monetary policy in one of two ways: as an intermediate target or as an indicator variable. The use of a commodity price index as an intermediate target is referred to as a "price rule." Adherence to such a price rule means that monetary policy generally would ease when the price index for the selected basket of commodities was below the predetermined target and tighten when the index was above its target.

Any variable taking on such a vital role in the conduct of policy, first and foremost, would have to lead reliably the ultimate policy variables. That is, policymakers would have to be confident that movements in the price index were signalling future changes in inflation and the performance of the economy. But that alone is not enough. The index also would have to be responsive to changes in monetary policy. An intermediate target that was a reliable indicator of the policy variables but did not respond relatively quickly and predictably to the actions of policymakers would be of little use.

The one intermediate target that satisfactorily exhibited these characteristics for many years was M1. Until 1985, M1 was the primary intermediate target for the Federal Reserve. Since then, evidence of distortions in the relationship between M1 and economic activity and inflation prompted the Federal Reserve to drop M1 as an intermediate target. Targets still are set for the broader monetary aggregates, M2 and M3, but these aggregates generally are not as reliable as M1 once was.

For this reason, a number of economists have looked at commodity indexes as a possible substitute for M1. However, these studies generally have rejected the use of a commodity price index as an intermediate target. Hafer (1983), Garner (1985), and Defina (1988) reject a "price rule" in part because commodity prices cannot be expected to react to open market operations with a sufficiently short and reliable time lag. Moreover, empirical analysis presented in the Appendix suggests that commodity price indexes are affected predominantly by non-monetary shocks. A number of studies also question whether available statistical evidence concerning the relationships between commodity prices and monetary policy objectives is adequate to justify using a commodity price index as the central guide to policy.[3]

However, still open is the question whether a commodity price index would be useful in a more limited role as an indicator variable to guide monetary policy. In the absence of a reliable intermediate target, indicator variables can be useful in formulating monetary policy. Such variables can be used to help forecast movements in the ultimate policy variables.

Currently, this is done systematically through the macro-econometric models that are used at the Board of Governors of the Federal Reserve System and at some of the Reserve Banks (see Judd [1988]). These models are used to project the path for monetary policy that leads to the desired values of the ultimate variables. Indicator variables can be used in this context to improve forecasts of the ultimate policy variables, and, thus, to help determine the appropriate course for monetary policy.

Movements in the commodity price index would not in themselves prescribe a particular path for monetary policy, but would influence policy only to the extent that they affect policymakers' views on the outlook for the ultimate policy objectives. With this more limited role, the criteria for whether a commodity index could be of use in formulating monetary policy can be less stringent than those applied to an intermediate target. In general, the usefulness of an indicator variable can be measured by its marginal contribution to reducing the error in predicting policy variables.[4]

In the remainder of the paper, then, the discussion focuses on the extent to which different commodity price indexes convey information about the future values of two key policy variables, inflation and unemployment.

II. The Usefulness of Commodity Prices

The Supporting Case

One advantage of commodity prices as indicator variables is that they are reported on a more frequent and timely basis than are the data on the policy variables themselves. Data on the prices of many commodities are reported daily with no more than a one day lag. Data on the CPI and employment, on the other hand, are available monthly with about a one month lag. Other measures of economic activity and inflation, such as GNP and the GNP deflator, are available only quarterly.

More fundamentally, some argue that commodity prices ought to lead movements in the ultimate policy variables because they are more flexible and adjust more quickly than do overall prices and labor markets. Their greater flexibility and speedier adjustment stem from the depth and sophistication of international spot markets. Moreover, because commodities generally are basic inputs that enter at the beginning of the production process, it is reasonable to expect shifts in aggregate demand to be reflected in the prices of commodities before those of finished goods.[5]

Supply shocks are a third factor that makes commodity price indexes potentially attractive as indicator variables. Examples of recent supply shocks include the reduction in agricultural production due to the drought in the U.S. and the increased supply of oil associated with a breakdown in the cohesion of OPEC. Such shocks affect aggregate supply and tend to be inversely related to output and employment. The effect on output, holding money constant, in turn affects the overall level of prices. Movements in a commodity price index containing the commodities subject to a shock would be expected to precede changes in the general price level.[6]

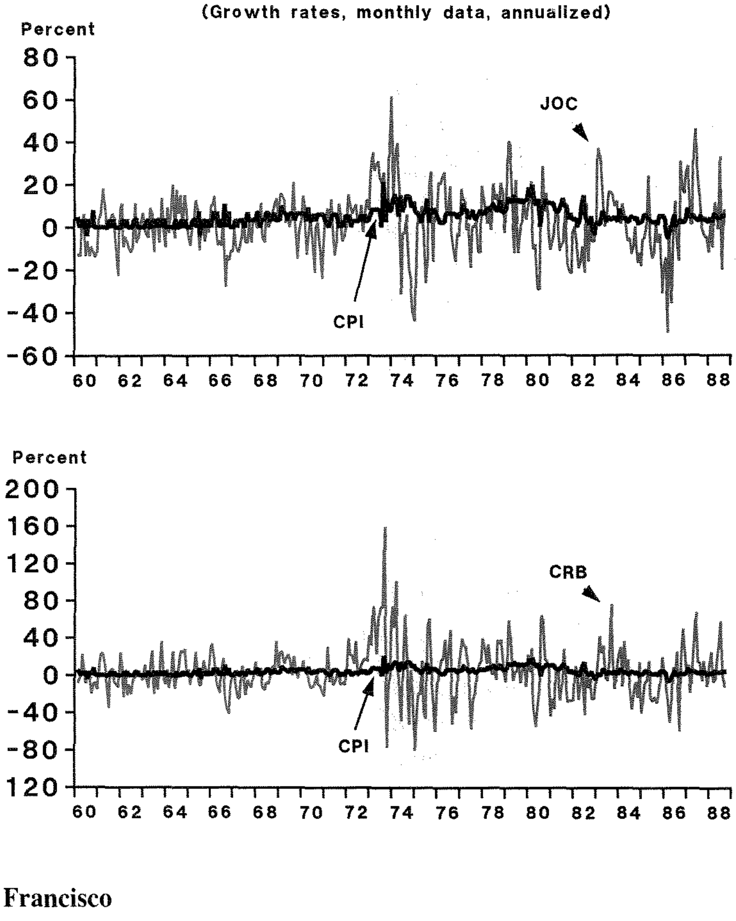
A fourth reason sometimes given for using commodity prices as an indicator variable, at least for overall inflation, is that the movement in the prices of certain commodities may provide information concerning market participants' inflation expectations. According to this argument, the current price of storable commodities, particularly metals, will rise or fall to reflect higher or lower inflation expectations. However, since nominal interest rates also should reflect inflation expectations and nominal interest rates are a principal carrying cost for commodities, movements in commodities prices may not be a perfect yardstick.[7]

Criticisms and Qualifications

The first argument in favor of considering a commodity price index as an indicator variable—that is, the ready availability of data—is not very compelling since data on other potentially useful economic variables are also readily available. For example, data on interest rates and exchange rates also could provide information to policymakers, and are available on as frequent and as timely a basis as are commodity prices.[8]

Second, the features of commodity price indexes that make them potentially beneficial for monetary policy purpose actually may limit their usefulness. For example, the flexibility and rapid adjustment of commodity prices may increase the "noise-to-information" content of a commodity price index. Indeed, commodity prices tend to be quite volatile compared with prices generally. This can be seen in Chart 1, which traces the annualized monthly

Chart 1
CPI and Commodity Indexes
(Growth rates, monthly data, annualized)



growth rates for the CPI relative to those for the two widely cited composite indexes from the CRB and the JOC. Over the period covered by the chart, growth rates for the CRB and the JOC indexes are four to seven times more volatile than the growth rate for the CPI.

Moreover, Table 1 shows that high volatility is not unique to the two indexes examined in this paper. Over the past eight years, the annualized growth rates of other commodity price indexes were 3½ to 20 times more volatile than was the growth in the CPI. The commodity price indexes were also much more volatile than were either M1 or M2.

This volatility makes it difficult to discern from short-run movements in commodity prices the implications for overall inflation. Indeed, as shown in the last column of Table 1, simple correlations for monthly data on CPI inflation and changes in commodity prices are quite low. Thus, month-to-month changes in commodity prices provide little information about overall inflation. On the other hand, short-run movements in the monetary aggregates do not provide much information, either. The correlations for CPI inflation and the month-to-month changes in M1 and M2 are only slightly higher than those for the commodity price indexes, and those for the monetary aggregates have the wrong sign.

The more critical question is whether movements in commodity prices over longer periods precede movements in overall prices in a reliable and predictable manner. The

Table 1

Volatility of Growth in Price Indexes and Monetary Aggregates

(monthly data, 1979:02 to 1988:04)

Series	Mean growth rate (annualized)	Standard deviation of growth rates	Correlation of CPI and series lagged one quarter
CPI	5.82%	4.71	—
CRB	0.30	26.41	.03[1]
JOC	0.82	17.02	.11
Other commodity indexes			
Gold	6.25	88.55	.13
IMF	1.53	27.34	.00
Economist	3.74	35.37	−.02
Dow Jones	9.61	46.98	−.21
Monetary aggregates			
M1	8.24	8.25	−.23
M2	8.24	4.51	−.12

[1]The correlation coefficient for the CRB Futures Index for the period 1981:09 to 1988:04 is .05.

Table 2

Peaks and Troughs in CPI inflation*

Peaks in CPI inflation			Troughs in CPI inflation		
	Months led by the commodity price index			Months led by the commodity price index	
Date	JOC	CRB	Date	JOC	CRB
1966:10	22	5	1967:10	6	3
1970:01	5	5	1972:07	17	18
1974:12	9	16	1976:12	17	17
1980:03	11	15	1983:07	11	20
1984:03	3	2	1986:12	7	8
Mean	10	8.6		11.6	13.2

*Changes in the CPI and the commodity price indexes are 12-month averages.

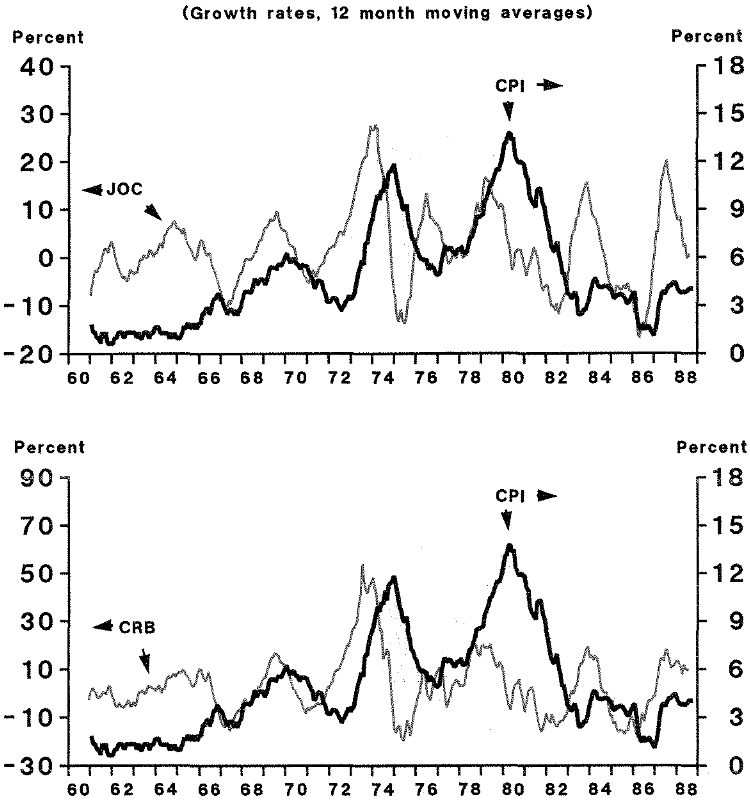
panels in Chart 2, which plot 12-month moving average growth rates, provide some perspective on this question. From the chart, a number of observations can be made. In support of the usefulness of commodity indexes, movements in the commodity indexes did tend to precede movements in overall inflation. Peaks and troughs in the moving average of CPI inflation were preceded by turning points in both of the commodity indexes.

However, the usefulness of the commodity indexes is limited for several reasons. To start with, the number of months by which changes in the commodity price indexes preceded turning points in CPI inflation varied unpredictably over the 28 years covered in the chart. Table 2 shows that the range for each of the two indexes is wide. The range for the JOC index, at three to 22 months, is only slightly wider than is the range for the CRB index.

Another weakness of the commodity price indexes, as Chart 2 shows, is that even when the series are smoothed, growth rates for the commodity price indexes still are more variable. Not only are the general swings in growth rates for commodity prices indexes more pronounced, they are more frequently interrupted by relatively sharp reversals. Had these indexes been used to guide policy at the time, such reversals would have given false signals regarding overall inflation. The signals are false in the sense that they would have been assumed to represent true turning points to contemporaneous observers.

The last observation is that the rise and fall in CPI inflation relative to the rise and fall in the growth rates for the composite commodity price indexes was much larger in the 1980s than in the 1970s. This by itself does not necessarily mean that the relationship between the CPI and the commodity price indexes is unstable; the difference in the magnitudes of the swings in CPI inflation may have been due to the influence of other variables. However, it does suggest that policymakers should not rely on a simple relationship between a commodity price index and inflation.[9]

Chart 2
CPI and Commodity Indexes
(Growth rates, 12 month moving averages)



III. Empirical Evidence on Commodity Prices

In this section, the potential contributions of the CRB and the JOC indexes as indicator variables for monetary policy are examined empirically. The analysis employs Vector Autoregressions (VARs). The basic model comprises four equations. The dependent variables are a monetary aggregate (either M1 or M2), a commodity price index, and two policy variables. One policy variable is represented by the CPI. The other policy variable, NUR, measures the strength of economic activity relative to potential. NUR is the difference between the actual civilian unemployment rate and the Congressional Budget Office's measure of full employment unemployment.

With the exception of NUR, the variables in the VARs are log-first differences. In the VARs, the explanatory variables in the equations are lagged values of all the dependent variables in the system and a constant. Separate lag lengths were selected for each variable in each of the equations to minimize the one-period-ahead prediction errors, allowing for a maximum lag of eight quarters for each variable.

The analysis is directed first at whether the CRB index or the JOC index might have been useful for monetary policy purposes had either index been used in conjunction with M1. This analysis uses quarterly data for the period cover-

Table 3
Results for CPI Equations using M1
(1965:1 to 1982:3)

Explanatory Variables	No CI	CI = JOC	CI = CRB
	F-ratio (M.L.S.)[1]	F-ratio (M.L.S.)[1]	F-ratio (M.L.S.)[1]
M1	F = 3.58 (.002)	F = 3.15 (.007)	F = 3.33 (.004)
Commodity Index (CI)		F = 2.39 (.038)	F = 0.99 (.377)
CPI	F = 20.19 (.000)	F = 15.46 (.000)	F = 18.62 (.000)
NUR	F = 3.31 (.011)	F = 4.83 (.001)	F = 2.42 (.049)
	R^2 = .87	R^2 = .89	R^2 = .87

Variance Decomposition

Quarters Ahead	% of variation in forecast errors for the level of CPI due to:			
	M1		**CPI**	**NUR**
1	8.5		91.5	0.0
4	29.7		56.0	14.3
8	41.7		35.8	22.6
12	56.0		25.3	18.7
	M1	**JOC**	**CPI**	**NUR**
1	4.9	3.6	91.5	0.0
4	27.3	26.7	36.6	9.4
8	35.3	33.9	19.5	11.3
12	48.3	28.9	14.3	8.5
	M1	**CRB**	**CPI**	**NUR**
1	12.7	0.7	86.6	0.0
4	37.0	13.4	43.0	6.6
8	46.1	17.5	26.2	10.2
12	58.3	14.5	19.4	7.8

[1]Marginal level of significance.

ing the early 1960s to the early 1980s. This is a period for which M1 was a reasonably good indicator of monetary policy. As Judd and Trehan (1987) show, the usefulness of M1 as a guide to policy deteriorated in the early 1980s as that aggregate became highly interest sensitive and subject to greater portfolio substitution.[10]

The discussion then turns to the performance of the CRB index and the JOC index when they are used with M2. For the most part, the sample period used for the systems that include M2 covers the early 1960s to the end of 1987.

In the VARs, dummy variables are included in the CPI (inflation) equations to control for the effects of the imposition and subsequent lifting of wage and price controls in the early 1970s. Dummy variables also are included in the monetary aggregate equations to control for the effects of Credit Controls in 1980.

M1 and Commodity Price Indexes

Tables 3–5 present evidence on the statistical and economic importance of the commodity price indexes in predicting inflation and NUR from the mid-1960s to the early 1980s. Table 3 reports the results from three different VAR models used to explain changes in the CPI: one without a commodity price index, one that includes JOC, and one that includes CRB. Table 3 shows that for the period considered, the JOC index did better than the CRB

Table 4

Results for NUR Equations using M1

(1965:1 to 1982:3)

Explanatory Variables	No CI	CI = JOC	CI = CRB
	F-/t-ratio (M.L.S.)[1]	F-/t-ratio (M.L.S.)[1]	F-/t-ratio (M.L.S.)[1]
M1	t = −1.44 (.150)	t = −0.99 (.363)	t = −1.02 (.312)
Commodity Index (CI)		F = 3.59 (.043)	F = 1.57 (.214)
CPI	F = 9.45 (.000)	F = 8.70 (.000)	F = 9.97 (.000)
NUR	F = 692.6 (.000)	F = 720.8 (.000)	F = 640.9 (.000)
	R^2 = .97	R^2 = .98	R^2 = .98

Variance Decomposition

Quarters Ahead	% of variation in forecast error due to:			
	M1		**CPI**	**NUR**
1	3.0		2.2	97.9
4	5.4		3.2	91.3
8	5.0		5.7	89.3
12	20.4		10.9	68.8
	M1	**JOC**	**CPI**	**NUR**
1	2.1	6.5	0.6	90.8
4	6.9	25.7	2.7	64.8
8	7.2	20.8	10.6	61.5
12	18.8	19.5	14.7	46.9
	M1	**CRB**	**CPI**	**NUR**
1	3.9	13.2	0.9	82.0
4	5.2	17.9	0.8	76.2
8	5.2	15.3	8.4	72.1
12	21.9	13.5	10.1	54.4

[1]Marginal level of significance.

index in predicting inflation. The F-statistics on the lagged values of the JOC index are statistically significant, but not on those of the CRB index.

The variance decompositions in the bottom portion of Table 3 also favor the JOC index. Variance decomposition is used to assess the relative importance of the variables in "explaining" movements in the dependent variable. This technique permits us to decompose the variation in the forecast errors of the equations into proportions associated with "shocks" to the various explanatory variables in the system. (Shocks are defined as movements in the explanatory variables that are not predicted by the system of equations.) The higher is the proportion of the error variance that is attributed to a particular variable, the greater is that variable's influence on inflation.[11] The variance decomposition in Table 3 shows that shocks to the JOC index account for about twice as much of the variance in the forecast error as does the CRB index. The variances are for the errors in forecasting the level of CPI.

The variance decompositions in Table 3 also indicate that the percent of the variance of the forecast error attributable to lagged values of the policy variables drops noticeably when the commodity price indexes are included in the model. This suggests that the commodity price indexes are affected by non-M1 shocks that also affect the policy variables. But it does not necessarily mean that the shocks affect the commodity price indexes sooner than the policy variables: when the commodity price indexes are ordered last, the percent of the variance of the policy variable forecast errors attributed to the commodity price indexes becomes quite small. However, since the information on the commodity prices becomes *available* before that on the policy variables, there may have been at least a small advantage to considering commodity prices along with M1 in the past. Such an advantage does not argue in favor of placing much weight on the commodity price indexes, though.

Perhaps more important than the variance decomposition for evaluating the role of the commodity price indexes as indicator variables is the extent to which they reduce the errors in predicting the CPI. The upper portion of Table 5 presents the standard errors of the forecasts for CPI. In contrast to the picture presented by the variance decompositions, the standard errors for the equation with the JOC index included are not appreciably lower than are those for the equation with the CRB index. In addition, neither commodity index appreciably reduces the standard errors compared with the system with only M1 and the policy variables.

The results relating to NUR are slightly more favorable to the inclusion of a commodity price index, if only because M1 itself performs rather poorly. In the top panel of Table 4, the lagged values of the JOC index are significant, but not those for M1 and the CRB index. From the variance decompositions, the JOC index is somewhat more important in predicting NUR than is M1 over the short to intermediate term. By far the most important variable is NUR itself. Taken together, the lagged values of the policy variables are much more important than those of either M1 or the commodity indexes in explaining NUR. Still, the inclusion of the JOC index does reduce somewhat the standard deviation of the forecast errors for NUR over the intermediate term, as shown in Table 5. Once again, these results point to a possible role for commodity prices, but not a particularly prominent one.

Table 5

Standard Errors of In-sample Forecasts

	CPI forecast errors[1]			
		(1965:1 to 1982:3)		
		Quarters ahead		
System	**1**	**4**	**8**	**12**
M1, CPI, NUR	.25	1.04	2.53	4.02
M1, JOC, CPI, NUR	.21	.98	2.49	4.03
M1, CRB, CPI, NUR	.25	1.04	2.52	3.98
M2, CPI, NUR	.29	1.17	3.04	4.94
		1965:1 to 1987:4		
M1, CPI, NUR	.30	1.32	3.29	5.32
M2, CPI, NUR	.32	1.24	2.87	4.77
	NUR forecast errors			
		1965:1 to 1982:3		
	1	**4**	**8**	**12**
M1, CPI, NUR	.25	.70	.87	.99
M1, JOC, CPI, NUR	.21	.65	.79	.90
M1, CRB, CPI, NUR	.24	.69	.85	.98
M2, CPI, NUR	.24	.71	.89	1.00
		1965:1 to 1987:4		
M1, CPI, NUR	.25	.72	.94	1.10
M2, CPI, NUR	.24	.71	.96	1.10

[1]The forecast errors are for the level of CPI and are expressed as a percent of the level of CPI at the end of the estimation period.

M2 and Commodity Price Indexes

Distortions to M1 in the 1980s induced the Federal Reserve to cease targeting that aggregate. As noted earlier, the Federal Reserve continues to set targets for M2. Thus, it would be helpful to know whether a commodity price index might augment the information contained in M2.

Indeed, when considered in conjunction with M2, a commodity price index may be more important on the margin since M2 is not as good an intermediate target variable as M1 once was. As shown in Table 5, for the sample period ending 1982:3, the standard errors of the forecasts from a VAR that includes M2 and the policy variables are larger than those from the model with M1. Over the longer period ending 1987:4, the model with M2 performs somewhat better relative to M1. However, that is mainly because of the deterioration in the relationship between M1 and the policy variables.

The results from the systems with M2 in Tables 6–8 are a bit more favorable to the CRB index than those from the systems with M1. From Table 6, the lagged values of the CRB index in the CPI equation are statistically significant. The F-statistic for the lagged values of the JOC index, on the other hand, is relatively low. Nevertheless, from the variance decompositions in Table 6, it is clear that the JOC

Table 6
Results for CPI Equation using M2
(1965:1 to 1987:4)

Explanatory Variables	No CI	CI = JOC	CI = CRB
	F-ratio (M.L.S.)[1]	F-ratio (M.L.S.)[1]	F-ratio (M.L.S.)[1]
M2	F = 2.00 (.059)	F = 1.90 (.075)	F = 2.44 (.020)
Commodity Index (CI)		F = 1.58 (.156)	F = 2.53 (.048)
CPI	F = 63.17 (.000)	F = 34.2 (.000)	F = 42.7 (.000)
NUR	F = 4.77 (.025)	F = 3.64 (.010)	F = 2.45 (.054)
	R^2 = .80	R^2 = .82	R^2 = .82

Variance Decomposition

Quarters Ahead	% of variation in forecast errors for the level of CPI due to:			
	M2	____	**CPI**	**NUR**
1	0.7		99.2	0.0
4	4.9		81.1	9.6
8	18.3		70.0	11.7
12	35.3		57.9	9.8
	M2	**JOC**	**CPI**	**NUR**
1	0.1	8.1	91.8	0.0
4	10.2	33.5	53.2	3.0
8	24.9	37.7	34.8	2.8
12	40.0	31.5	26.0	2.5
	M2	**CRB**	**CPI**	**NUR**
1	0.0	7.6	92.4	0.0
4	13.8	30.2	54.3	1.7
8	29.5	32.4	36.7	1.4
12	41.5	27.7	29.2	1.5

[1]Marginal level of significance.

and CRB indexes are about equally important in predicting the level of CPI. Both indexes are more important than M2 up to eight quarters ahead, but less important over a longer horizon.

The statistics on the CPI prediction errors in Table 8 for the systems that include the JOC and the CRB index are not much different, and do not provide a basis for choosing one index over the other. Moreover, a comparison of the prediction errors for the CPI from the system that *does not include* commodity prices with the errors from systems that *do include* the JOC or the CRB reveals that the inclusion of either commodity price index would mean only modest improvement in the near-term forecast. In the longer term, the standard errors are somewhat larger for the systems that include commodity prices. The forecast errors are uniformly larger for the systems that include a commodity price index but not M2.

On balance, the inclusion of the commodity price indexes does not improve inflation predictions very much. Once again, because the commodity price indexes apparently are affected by non-M2 (as well as non-M1) shocks that also affect policy variables, the principal advantage of incorporating a commodity price index appears to be that the data for commodity prices are available sooner than those for the policy variables.

The results from the NUR equations including M2 and

Table 7
Results for NUR Equation using M2
(1965:1 to 1987:4)

Explanatory Variables	No CI	CI = JOC	CI = CRB
	F-ratio (M.L.S.)[1]	F-ratio (M.L.S.)[1]	F-ratio (M.L.S.)[1]
M2	F = 4.66 (.012)	F = 2.87 (.062)	F = 4.38 (.002)
Commodity Index (CI)		F = 2.26 (.046)	F = 2.34 (.040)
CPI	F = 10.04 (.000)	F = 10.67 (.000)	F = 10.95 (.000)
NUR	F = 1430 (.000)	F = 1419 (.000)	F = 1341 (.000)
	R^2 = .98	R^2 = .98	R^2 = .98

Variance Decomposition

Quarters Ahead	% of variation in forecast error due to:			
	M2		**CPI**	**NUR**
1	1.1		4.7	94.1
4	9.2		1.7	90.6
8	18.3		8.5	73.2
12	16.8		21.0	62.2
	M2	**JOC**	**CPI**	**NUR**
1	3.6	7.5	0.4	88.5
4	11.2	17.5	3.3	67.9
8	19.0	12.0	11.3	57.7
12	17.5	12.3	20.9	48.3
	M2	**CRB**	**CPI**	**NUR**
1	2.2	20.9	0.3	76.5
4	13.7	15.9	0.4	70.0
8	22.5	10.3	2.5	64.5
12	19.8	12.0	8.1	60.1

[1]Marginal level of significance.

the commodity indexes parallel those derived earlier with M1. The one exception is that the lagged values of M2 are significant, as shown in Table 7. Otherwise, the commodity price indexes are significant but tend to account for only a small proportion of the variance in the forecast error for NUR. The policy variables are far more important than M2 or the commodity indexes. And, from Table 8, we see that standard errors tend to be slightly lower with the JOC index and slightly higher with the CRB index, compared with the model that has only M2 and the policy variables.[12]

Table 8

Standard Errors of In-sample Forecasts

1965:1 to 1987:4

	CPI[1]				NUR			
	Quarters ahead				Quarters ahead			
System	**1**	**4**	**8**	**12**	**1**	**4**	**8**	**12**
1. M2, CPI, NUR	.32	1.24	2.87	4.77	.24	.72	.98	1.07
2. M2, JOC, CPI, NUR	.30	1.16	2.98	5.08	.22	.70	.95	1.06
3. M2, CRB, CPI, NUR	.30	1.23	3.08	5.13	.25	.76	.99	1.09
4. JOC, CPI, NUR	.33	1.35	3.09	5.40	.23	.71	.93	1.07
5. CRB, CPI, NUR	.34	1.35	3.10	4.79	.25	.72	.96	1.10

[1]The forecast errors are for the level of CPI and are expressed as a percent of the level of CPI at the end of the estimation period.

Table 9

Errors for Out-of-Sample Forecasts

(1981:4 to 1988:2)

	CPI				**NUR**			
	Quarters ahead							
	4		**8**		**4**		**8**	
System	**ME[1]**	**AME[1]**	**ME[1]**	**AME[1]**	**ME**	**AME**	**ME**	**AME**
	Estimation starting 1965:1							
1. M2, CPI, NUR	.10	1.87	1.68	4.00	−.5	−1.0	.8	1.0
2. M2, JOC, CPI, NUR	−.25	.54	1.51	3.55	−.5	−1.1	.9	1.2
3. M2, CRB, CPI, NUR	−.06	1.56	1.25	3.38	−.5	−1.1	.8	1.1
4. JOC, CPI, NUR	.86	3.61	1.93	5.51	−.6	−1.1	1.0	1.3
5. CRB, CPI, NUR	.67	3.02	1.64	3.82	−.8	−1.2	.9	1.2

ME = Mean forecast error. AME = Absolute mean forecast error.

[1]The forecast errors are for the level of CPI and are expressed as a percent of the level of CPI at the end of the estimation period.

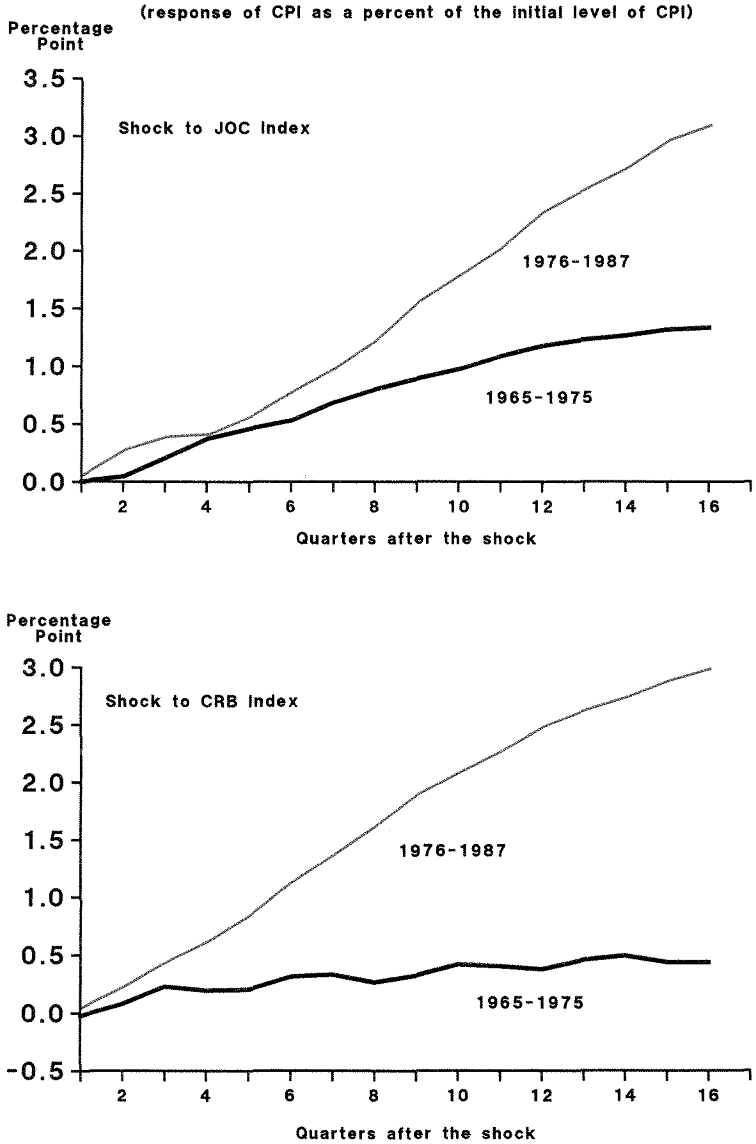
IV. Problems with Stability

A question concerning the stability of the relationships between the commodity price indexes and the policy variables was raised in connection with the discussion of Chart 2. This issue can be examined using the VARs from the previous section. From the VARs, it is possible to derive the reactions of the policy variables to shocks to the commodity price indexes. The relationship between the policy variables and the indexes would be considered stable if the reactions, or impulse responses, of the policy variables are similar when the models are estimated over different time periods.

To test for stability, then, the VAR models with M2, a commodity index (JOC or CRB), and the two policy variables were estimated for the periods 1965:1 to 1975:4 and 1976:1 to 1987:4. Chart 3 plots the responses of the CPI, expressed as a percent of the initial level of CPI, to one standard deviation shocks to the growth rates of the JOC and the CRB indexes. Both panels point to instability. The response of the CPI is more pronounced when the model is estimated over the second period. The differences in the responses tend to widen as the number of quarters after the shock increases.

The responses of NUR to shocks to the commodity price indexes also show instability. In Chart 4, the response of NUR to a shock to JOC 16 quarters later is about the same in both time periods. However, for shocks to both the JOC

Chart 3
Impulse Response Functions for CPI
(response of CPI as a percent of the initial level of CPI)

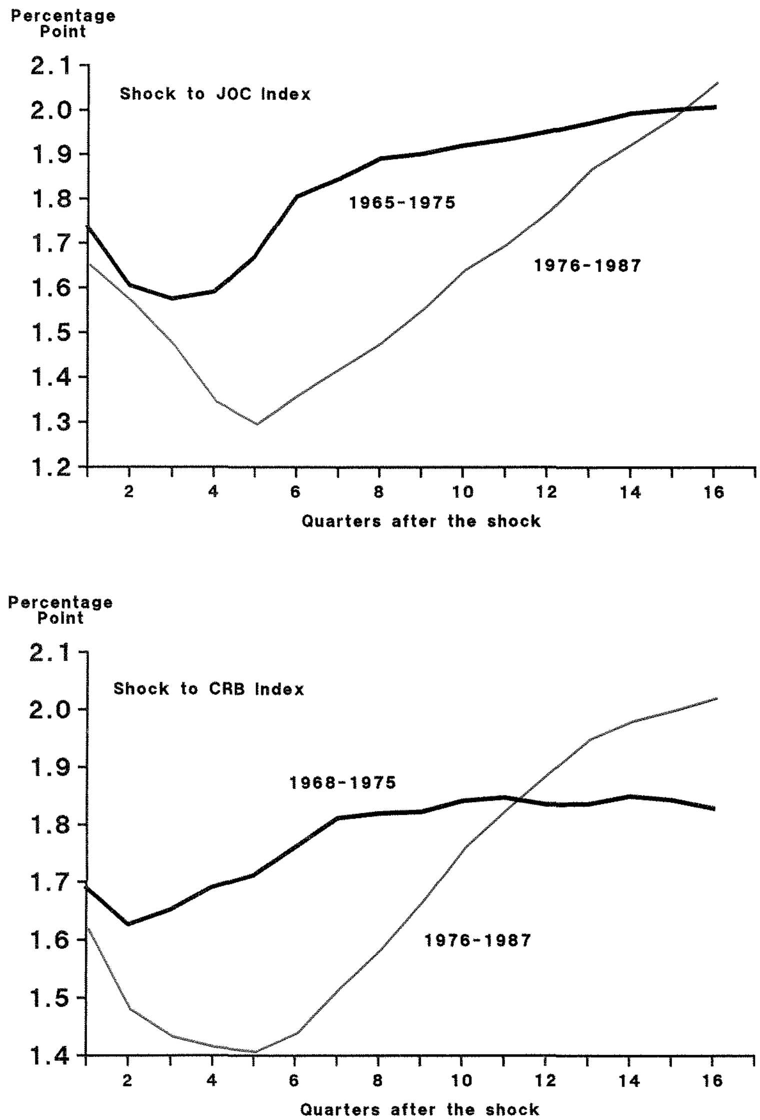


and CRB indexes, the paths of the interim responses differ for the two time periods.

Given that the relationships between changes in the commodity price indexes and the policy variables are subject to structural shifts, the earlier VAR results could understate the statistical significance of commodity prices and misrepresent their economic importance. It is possible that estimating the models over separate time periods would control for structural shifts and would result in stronger relationships between the commodity price indexes and the policy variables in each period. However, better in-sample fits for the two subperiods would not necessarily tell us whether those relationships provide useful guidance for monetary policy in the future because they do not eliminate problems caused by subsequent structural shifts.

In an effort to gauge how instability might affect the usefulness of the commodity price indexes for policy purposes, I generated a series of out-of-sample forecasts using the VARs identified in Table 8. The forecasts were for four and eight quarters ahead. The sample period for the first set of forecasts ends on 1980:4. The first four-quarter ahead forecast, then, is for 1981:4 and the first eight-quarter ahead forecast is for 1982:4. The estimation

Chart 4
Impulse Response Functions for NUR



period is extended one quarter each time for successive forecasts. The last forecast is for 1988:2. The mean forecast errors and the absolute values of the mean forecast errors from this exercise are reported in Table 9.

The benchmark for gauging the contribution of the commodity price indexes is the three variable VAR with M2, CPI, and NUR. In setting this benchmark, it is recognized that the relationship between M2 and the policy variables likely also shifted to some extent in the 1980s.[13] Thus, the three variable VAR with M2 does not necessarily give the best (lowest prediction error) forecast of the policy variables—CPI inflation and NUR. Nonetheless, this VAR is the appropriate benchmark since the Federal Reserve still sets targets for M2 and the aggregate still affects policy considerations.

The first observation is that, among the three variable systems, the models with the commodity price indexes tend to perform the worst in predicting CPI. The out-of-sample forecasts support the earlier conclusion that the commodity price indexes should not be used to replace M2 as a target variable.

Second, despite the problems with structural shifts raised earlier, including the composite commodity price indexes along with M2 does tend to improve the prediction errors for CPI eight quarters ahead. However, judging from the mean absolute errors, sizable mistakes remain in predicting CPI.

Regarding NUR, it is evident in Table 9 that including the commodity price indexes does not improve the out-of-sample forecasts for NUR, and in most cases, the price indexes make the forecasts worse. The three variable model with M2 and the policy objective variables generally does the best.

V. Conclusion

The evidence on commodity price indexes suggests that these series would not fill the void left by M1 when it was dropped as an intermediate target. Nor is there a case for using these commodity price indexes in place of M2, even though the performance of M2 falls short of that observed for M1 when that aggregate was the primary intermediate target. And certainly, neither the in-sample nor the out-of-sample evidence supports according any special status to commodity prices.

The findings in this paper do provide some support for considering one of the two composite commodity indexes along with other information variables. One advantage is the more timely availability of commodity price data relative to those on overall prices. Moreover, despite some concerns about stability, the inclusion of the JOC or the CRB index tended to improve the near-to-intermediate-term out-of-sample forecasts of the CPI, though not for the forecasts of unemployment.

APPENDIX
Commodity Prices and Money

There is evidence that the prices of at least some commodities do respond to monetary developments. Frankel and Hardouvelis (1985) find a positive and statistically significant response of the prices of several commodities to surprises in weekly money supply announcements. (See also Kitchum and Denbaly [1982].) The adjustments occur within the trading day following the money supply announcement.

Frankel and Hardouvelis also find, however, that money supply developments account for only a small fraction of the variation in commodity prices. In the short run, movements in commodity prices appear to be dominated by developments affecting their relative prices rather than by more general macroeconomic developments, including monetary policy.

To measure the relative importance of monetary influences on commodity prices, I used a system of VARs that includes a commodity index, M1, the CPI, and a measure of slack in the labor market, NUR, which is defined in the text. The data are quarterly and cover the mid-1960s to the early 1980s. With the exception of NUR, all the variables are log-first differences.

As stated in the text, separate lag lengths were selected for each variable in each of the four equations of the models to minimize the one period ahead prediction error, allowing for a maximum lag of eight quarters for each variable. Under this procedure, M1 enters the commodity price equations with a one-quarter lag and an eight-quarter lag in the CPI equation. This finding is consistent with the argument that commodity prices adjust more quickly to monetary developments than do overall prices.

The results from estimating the two models, one with the CRB index and one with the JOC index, are presented in the Table. In the commodity price index equations for both models, the lagged values of the changes in the commodity indexes themselves are highly significant, while CPI inflation is marginally significant. In the model with the JOC index, M1 is statistically significant in explaining the commodity price index, but M1 is not significant in the equation for the CRB index. Experimenting with longer lags did not alter these results.

One possible explanation for the insignificant coefficient on M1 in the CRB equation is that the CRB index reacts so quickly to M1 that the lagged values of the aggregate in the VARs do not provide additional explanatory power. However, rapid adjustment to changes in M1 should imply that lagged values of CRB beyond one

Components of the Commodity Price Indexes

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Textiles

Cotton
Burlap
Polyester
Print Cloth

Metals

Scrap steel
Copper scrap
Zinc
Tin
Lead
Aluminum

Other

Rubber
Hides
Tallow
Plywood
Old Corrugated Boxes
Red Oak
Benzine
Crude oil

Commodity Research Bureau Index, 23 Spot Prices

Textiles

Cotton
Burlap
Print cloth

Metal

Scrap steel
Copper scrap
Zinc
Tin
Lead

Other

Rubber
Hides
Tallow
Sugar
Steers
Soybean oil
Wool tops
Butter
Corn
Hogs
Lard
Rosin
Wheat (MPLS)
Wheat (KC)

quarter do not contain much information either. Yet the coefficients on both the two- and three-quarter lags for the CRB index are statistically significant and of the same magnitude as the coefficient on CRB lagged one quarter.

The variance decompositions presented in the bottom panel of the Table indicate that shocks to M1 accounted for a relatively small portion of variance in the forecast errors for levels of the CRB and the JOC indexes. In contrast, the commodity indexes themselves account for an extremely high proportion of the variance in the forecast error. This suggests that they could be treated as more or less exogenous with respect to the other variables in the system. Thus, it appears that both indexes are subject to non-monetary shocks primarily and it is not likely that either commodity price index can be used as a substitute for an aggregate like M1.

Table

Results for Commodity Price Index Equations

(1965:1 to 1982:3)

Explanatory Variables	JOC equation	CRB equation
	F-/t-ratio (M.L.S.)[1]	F-/t-ratio (M.L.S.)[1]
M1	t = 2.14 (.032)	t = 0.77 (.441)
Commodity Index (CI)	t = 6.46 (.000)	F = 4.64 (.001)
CPI	F = 4.35 (.075)	F = 2.76 (.071)
NUR	t = 1.12 (.263)	t = 0.32 (.749)
	$R^2 = .47$	$R^2 = .22$

Variance Decomposition

Quarters Ahead	% of variation in forecast errors for the level of JOC due to:			
	M1	JOC	CPI	NUR
1	2.9	97.1	0.0	0.0
4	10.1	89.6	0.1	0.2
8	12.2	85.7	0.1	1.9
12	11.1	83.4	0.1	5.4

Quarters Ahead	% of variation in forecast errors for the level of CRB due to:			
	M1	CRB	CPI	NUR
1	6.0	94.0	0.0	0.0
4	8.7	90.6	0.6	0.1
8	8.6	90.3	0.9	0.2
12	7.2	91.4	1.1	0.3

[1] Marginal level of significance.

ENDNOTES

1. The Presidential Gold Commission, established in June 1981, was charged with researching the virtues of reinstating the gold standard. The Commission's report in March 1982 rejected the idea of adopting a formal monetary role for gold. (See Cooper [1982].)

2. Two recent proposals for incorporating a composite commodity index into the conduct of monetary policy were made by then Treasury Secretary James Baker III (*Wall Street Journal* [1987]) and Governor Wayne Angell of the Board of Governors of the Federal Reserve System (Angell [1987]).

3. Other empirical evidence on the relationships of commodity prices and possible policy variables is reported in Yeats (1973), Neftci (1979), and Melton and Smith (1987).

4. The extent to which a commodity price index is responsive to monetary policy is not irrelevant, however.

5. Frankel (1986) shows commodity prices can be expected to overshoot in response to monetary shocks. This result stems from the effects of monetary shocks to real interest rates, which in turn is related to the sluggish adjustment of the general level of prices.

Another complication is that real shocks to aggregate demand also should affect the real rate of interest, and real interest rates affect the price of durable commodities. A positive shock that raises the real rate of interest would tend to reduce the price of durable (storable) commodities, but not necessarily the prices of goods and services more generally.

6. Other studies have found that oil prices do help explain inflation. For example, Throop (1988) includes the change in the real price of oil in an augmented Phillips Curve equation in the structural forecasting model of the Federal Reserve Bank of San Francisco.

7. It is true that an expected change in the relative price of a commodity can affect current decisions to hold inventories and thereby affect current spot prices. It is also the case that prices of commodities can be expected to rise as part of an increase in overall prices. That is one reason many durable commodities such as gold have been used as inflation hedges (see Bird [1984]), particularly when tax rates on capital gains and ordinary income differ. Nevertheless, because nominal interest rates also tend to rise, expectations of a general rise in prices will not necessarily mean higher current commodity prices.

8. For an analysis of exchange rates and inflation, see Kahn (1987).

9. From a policy perspective, it is useful to know whether commodity prices respond more to monetary or nonmonetary shocks. If monetary influences are more important, then a commodity price index would be a useful guide to policy even though these indexes exhibit great volatility. On the other hand, if nonmonetary shocks dominate, it would be difficult to determine whether movements in a commodity price index were signalling that monetary policy was off course.

This distinction is important since the loss of M1 as an intermediate target makes the need for reliable indicator variables more pressing. In this regard, the contribution of a commodity price index would be stronger if it were able to capture information formerly captured by M1. As discussed in the Appendix, this does appear to be the case.

10. These changes in the behavior of M1 are attributed to deposit deregulation, particularly the introduction of money market deposit accounts and interest-bearing NOW accounts.

11. The variance decompositions were derived by ordering the variables as indicated in Table 3. The order in which the variables are placed can affect the results of the variance decomposition. The ordering chosen here permits us to develop an upper bound estimate of the impact of M1.

12. In addition to M2 and the commodity indexes, there are a number of other variables that policymakers might consider as guides to monetary policy. Two possibilities are interest rates and exchange rates. However, the inclusion of the one-year Treasury note rate and the trade weighted exchange value of the dollar does not weaken materially the case for limited reliance on the commodity price indexes.

13. Indeed, impulse responses for the policy variables to shocks to M2 show that these responses for both the measure for inflation and for unemployment were different for the VARs estimated over the two subperiods used for Charts 3 and 4.

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Forecasting Growth in Current Quarter Real GNP

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Early work on this project was done by Rose McElhattan. The author wishes to thank Steve Morus and especially Sherry Brennan for extremely helpful research assistance. Editorial committee members were Brian Motley and Reuven Glick.

This paper presents a simple model for obtaining estimates of current quarter real GNP growth using data on series that are available on a monthly basis. The variables used to "forecast" GNP growth are industrial production, real retail sales, and nonfarm payroll employment. The model's forecasts compare well with the Blue Chip consensus forecast and contain information about final GNP beyond what is contained in the advance GNP estimates.

Policy actions taken today rarely have an immediate impact on the economy, and several quarters may elapse before the effects of these actions begin to show up. Consequently, policymakers must rely on forecasts of future economic activity to formulate current policy. The task of forecasting the course of the economy is complicated by the fact that the relevant data on current activity are available only with a delay. Thus, an important first step in this process is to obtain reliable *estimates* of current activity. Such information should enable policymakers to take more timely action by responding to emerging trends.

This paper presents a method of obtaining "forecasts" of current quarter real GNP growth early in the quarter, in order to improve upon forecasts of output growth obtained from econometric models that are estimated using quarterly data only. The method presented here is a statistical one; it involves forecasting current quarter output using a small number of variables. It is thus to be contrasted to techniques that require knowledge of the contemporaneous values of a large number of series constituting the various components of GNP. The hope is that an inexpensive technique that does not require keeping track of a large number of variables will provide a reasonable estimate of current quarter output.

The objective of obtaining reliable estimates of GNP early in the quarter effectively determines the nature of the exercise carried out here. First, the data series used to predict GNP must be available on a more frequent basis than the quarterly GNP data themselves. Fortunately, there are many monthly data series that, ostensibly at least, should provide some indication of emerging trends in economic activity. Second, these monthly series should be available relatively soon after the end of the month they cover. Obviously, series that are published with a lag of several months are not useful for our purposes. A number of monthly series meet this requirement as well. Finally, these series themselves should be easy to forecast (over horizons of one to three months), since we would like to predict current quarter GNP even before data on all three months of the quarter are received. Series that can be forecast reasonably accurately will lead to better estimates of current quarter output early in the quarter.

From these criteria we were able to choose a small number of series, called indicator variables, with which to construct a model for forecasting current quarter GNP. The equation that is presented here uses contemporaneous values of nonfarm payroll employment, industrial production, and retail sales, as well as lagged values of real GNP, to predict current quarter real GNP growth. We present an analysis of its forecasting performance at different points in the quarter, when varying amounts of information are available on the indicator variables. The model is not very useful in the beginning of the quarter, when we have no information about the indicator variables. The forecasting accuracy of the model improves noticeably when information on the first month of the quarter becomes available. While there is some further improvement when data on the second and third months of the quarter become available, this improvement is not large. The model's forecasts compare favorably with the Blue Chip consensus forecast. The model's forecasts also contain information about final GNP over and above that contained in the Commerce Department's advance[1] GNP release.

The paper is organized as follows. Section I discusses issues of estimation strategy and variable selection. Section II presents the estimated model, called the monthly indicators model. It provides details on the forecasting performance of the system used to predict the indicator variables and presents the equation used to predict real GNP. The next section presents the results on the model's forecasting performance over the period from 1978.3–1988.2, and a comparison of the model's forecast with the consensus Blue Chip forecast. Section IV considers the issue of combining forecasts, in order to determine whether the model forecast provides information about the final value of real GNP beyond that contained in the advance estimate of GNP released by the Commerce Department, as well as that contained in the Blue Chip forecast. This section also evaluates how the model performs relative to the Blue Chip forecast in predicting the advance GNP estimates. Section V concludes.

I. Strategy and Variable Selection

The central issue of this project is which variables to use to predict real GNP. There are several approaches to this problem. Traditional, structural macroeconomic models, for instance, focus on the product side of the National Income and Product Accounts. An alternative approach is to obtain GNP estimates from information about factor inputs—utilize Okun's Law, for example. In contrast to these two approaches, this paper uses purely statistical criteria to determine whether a given variable should be used to forecast GNP. Specifically, a variable is included in the model if it helps to reduce the "ex-ante" errors in predicting real GNP and is statistically significant in the GNP equation. (As mentioned above, only those variables for which the relevant data are available relatively early are candidates for inclusion.)

However, minimizing GNP forecast errors is not a single criterion, since we wish to make forecasts of current quarter output several times during the quarter as new information on the indicator variables becomes available. A variable that is useful in predicting GNP when all three months of information are available may not be included in the model, since we are also concerned with the variable's usefulness when we have less than three months of information on it. Thus, our ideal variable is one that minimizes GNP forecast errors whether we have one, two or three months of information for the current quarter. What this means is that the variable we choose to predict real GNP should itself be easy to forecast.

This means that the process of choosing the appropriate set of variables for forecasting real GNP can become extremely cumbersome, since each time a new variable is considered for inclusion in the GNP equation it is also necessary to respecify the equations for forecasting all the indicator variables. Selecting variables according to the criteria of minimizing forecast errors also complicates matters, since we are faced with a rather large list of potential indicator variables.

In all, more than a dozen monthly series satisfied the criterion of being available early in the quarter and were considered for inclusion in the monthly indicators model. These are listed in the Appendix. Variables that did relatively well when no information on the current quarter was available but did relatively badly otherwise were dropped from consideration early in the specification search.[2] In addition, early work also revealed that variables that did reasonably well in predicting real GNP when all three months of data were available also tended to do well when only one or two months of data were available. (The reasons for this are discussed below.)

As a consequence, the latter part of the specification search was carried out in two separate stages. In the first stage, the focus was on the usefulness of the indicator variables in predicting real GNP when information on all three months of the quarter was available. This allowed elimination of more than half the variables in the original list. The second stage involved specifying equations for forecasting the indicator variables themselves, and then using these forecasts to obtain forecasts of real GNP.

Bayesian Vector Autoregressions (BVARs) were estimated to obtain forecasts of the monthly values of the indicator variables. This is an inexpensive forecasting technique pioneered by Robert Litterman that has been shown to produce macroeconomic forecasts comparable to those obtained from large, commercial forecasting services. (See Litterman [1986] and McNees [1986] for a comparison.) The technique uses the forecaster's prior beliefs about the behavior of the variables in question to modify the coefficients that would be obtained from unrestricted estimation of a vector autoregression.[3] The use of priors reduces the probability of picking up spurious correlations in the data. Unrestricted vector autoregressions tend to pick up such correlations and consequently explain in-sample observations relatively well but tend to forecast rather badly.

The general form of the prior employed here has come to be known as the "Minnesota prior," which postulates that most economic time series behave like random walks with drift.[4] Consequently, the estimated coefficients are pushed towards this specification. Specifically, for each variable, the coefficient on its own first lag is pushed towards one, while the coefficients on all other right-hand-side variables are pushed towards zero. How much the coefficients are pushed towards this prior is determined by examining the forecasting performance of alternative specifications and choosing the one that does the best. Considerations of space preclude a complete description of this prior and the technique here. The interested reader is referred to Todd (1984) for a clear, nontechnical discussion. Roberds (1988) provides a more technical and complete description of how to set up such a model.

Different BVARs were estimated for each combination of variables included in the equation used to forecast GNP. The indicator variable forecasts obtained from each of these BVARs were then used to obtain forecasts of real GNP at different points in the quarter. The final model was selected on the basis of these GNP forecast errors.

II. The Monthly Indicators Model

This section presents the model that was obtained through this process. Choosing variables on the basis of forecasting criteria leads to an eclectic set of indicator variables. The model's GNP equation contains a measure of production, industrial production (denoted IP); a measure of factor inputs, nonfarm payroll employment (denoted EMP); and a measure of consumption, real retail sales (denoted RRS). An important advantage of the set of variables used in the model is that all data for a particular month are available by the middle of the following month.[5]

The producer price index for finished commodities (PPI) has been used to deflate retail sales. At first glance, it might seem more appropriate to use a consumption deflator. However, the deflator for personal consumption expenditures becomes available more than a month after the PPI. Another alternative is the consumer price index (CPI). It turns out that the forecasting performance of the GNP equation is not very sensitive to whether the PPI or the CPI is used to deflate retail sales. A benefit of using the PPI is that it is released about two weeks before the CPI.

Intuition also suggests that a measure of labor hours may be preferable to a measure of aggregate employment, because average worker hours can be changed (within limits) to vary production without changing employment. However, using aggregate hours instead of employment leads to no appreciable difference in the GNP forecasts when all three months of data are available. In addition, forecasting labor hours turns out to be somewhat harder than forecasting employment. As a result, GNP forecasts based on one or two months of information are somewhat worse when hours are used to predict GNP than when employment is used. Experiments with specifications including various measures of average weekly labor hours in addition to employment were similarly unsuccessful.

Another potential problem has to do with the retail sales variable. In the last few years, sales incentives offered by automobile dealers have led to wide swings in quarter-to-quarter automobile sales, distorting quarterly retail sales data. To correct for these distortions one could omit automobile sales from consideration altogether and use retail sales net of autos in the GNP equation. This alternative specification led to poorer forecasting performance than did the specification that included auto sales. Another approach would be to include automobile sales as a separate variable in the GNP equation. Although this approach does lead to a statistically significant impact of changes in the growth rate of auto sales on real GNP growth, the estimated coefficient is quite small. Furthermore, there is no appreciable difference between the forecasting accuracy of the version of the model that contains automobile sales separately and that which lumps them together with non-auto retail sales. Consequently, automobile sales were not included separately in the final version of the model.

Obviously, the small set of indicators used here omits everybody's favorite variable. Two variables that might seem particularly important are the merchandise trade balance and inventories. The merchandise trade balance was not included primarily because of the lack of a continuous series over a period long enough to allow reliable estimation. In addition, including this variable in the model is not likely to add much information to "real-time" forecasts, since data on the merchandise trade balance for a particular month do not become available until approximately two months later.

Similarly, it seems that incorporating inventory data should help, since inventory swings are a significant component of quarterly variation in real GNP growth. However, trials with several alternative measures of nominal inventories failed to turn up a measure that either was significant in the real GNP equation or did not worsen its forecasting performance. Series on real inventories were significant in the real GNP equation, but these were not included in the final specification because they become available with more than a one quarter lag. Attempts to deflate the nominal inventory data with various price level measures and so create a useful measure of real inventories were also unsuccessful.

Predicting the Indicator Variables

The BVAR used to predict the monthly values of the three indicator variables contains five variables: the indicator variables themselves plus average weekly hours of production workers on private, non-agricultural payrolls, and the six-month commercial paper rate. The last two

Table 1

Forecasts of Indicator Variables: July 1978 to June 1988
(Annualized Growth Rates)

(A) Nonfarm payroll employment

	Univariate AR Forecast				BVAR Forecast			
Months ahead	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic
1	0.06	1.60	2.46	0.81	0.01	1.51	2.23	0.73
2	0.07	1.69	2.46	0.82	0.05	1.59	2.23	0.73
3	0.12	1.82	2.66	0.87	0.09	1.67	2.33	0.77

(B) Industrial production

	Univariate AR Forecast				BVAR Forecast			
Months ahead	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic
1	1.04	7.21	9.62	0.90	0.06	6.50	8.78	0.82
2	1.61	7.95	10.40	0.86	0.13	6.74	9.17	0.76
3	1.98	7.92	10.68	0.84	0.24	6.75	9.26	0.73

(C) Real Retail Sales

	Univariate AR Forecast				BVAR Forecast			
Months ahead	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic
1	0.84	16.16	24.16	0.75	3.50	13.43	18.93	0.59
2	0.98	16.30	23.82.	0.86	3.29	13.90	19.88	0.72
3	1.06	16.15	23.39	0.91	2.97	13.82	19.80	0.77

variables increase the precision of the forecasts of the indicator variables, but are not useful in predicting GNP. Each equation contains 12 lags of each of the variables. Given the nature of the exercise, presenting the estimated coefficients does not appear to be particularly useful.[6] (The computer program used to estimate the BVAR is available from the author on request.) Instead, Table 1 presents forecast error statistics for the one-month ahead to the three-month ahead horizons over a 10-year period extending from July 1978 to June 1988 (a total of 120 forecasts). Each forecast was obtained by estimating the BVAR up to the period prior to the first month being forecast.[7] For comparison purposes, the Table also includes error statistics on forecasts obtained from univariate autoregressions.

Although both the BVAR and the univariate autoregressions predict the log levels of the indicator variables, the forecasts have been converted to annualized growth rates in order to facilitate interpretation of the various error statistics shown in the table. Four different measures of forecast accuracy are presented there: the Mean Error, the Mean Absolute Error (MAE), the Root Mean Square Error (RMSE), and Theil's U-statistic. A MAE close to the Mean Error implies that the errors are generally of the same sign, meaning that the forecasts are generally either too low or too high. A comparison between the RMSE and the MAE provides information about the relative size of the errors: if the errors are roughly of the same size, the two measures will be close. A mixture of large and small errors will lead to a RMSE above the MAE. Theil's U-statistic is unit free and provides a comparison of the model's forecast with the naive forecast of no change in growth rates. Values larger than one imply that the model's forecast is worse than the naive forecast.

As shown in Table 1, there is a substantial difference in the size of the errors made in predicting the three indicator variables. For instance, the MAEs and the RMSEs of the real retail sales forecasts are about eight times larger than the MAEs and the RMSEs of the employment forecasts. This is largely because the industrial production and real retail sales series are much more volatile than the employment series. Over the forecast period, the standard error of the growth rate of real retail sales is nearly seven times larger than the standard error of the growth rate of employment, while that for industrial production is more than three times as large as that for employment.[8]

A comparison of the MAEs and the RMSEs of the BVAR and the univariate autoregressions shows that the BVAR forecasts are better for all three variables. A similar conclusion holds for the U-statistics shown there. The Mean Errors from the BVAR are smaller than those for the univariate autoregressions for both employment and industrial production but are larger in the case of real retail sales. While it is possible to respecify the BVAR's prior to get smaller mean errors for retail sales, doing so raises the RMSEs of all three variables.

Predicting Contemporaneous GNP Growth

The equation used to predict current quarter GNP is

$$\begin{aligned} RGNP_t = {} & \underset{(2.16)}{0.81} + \underset{(2.81)}{0.17}\, IP_t + \underset{(3.77)}{0.14}\, RRS_t + \underset{(4.95)}{1.13}\, EMP_t \\ & - \underset{(-3.01)}{0.21}\, RGNP_{t-1} - \underset{(-1.41)}{0.09}\, RGNP_{t-2} - \underset{(-3.95)}{.26}\, RGNP_{t-3} \end{aligned}$$

Adjusted $R^2 = 0.74$, S.E.E. $= 2.17$
Estimation Period: 1968.2 to 1988.2.
t statistics are shown in parentheses

All variables are in (annualized) growth rates. The starting date was determined by the availability of the retail sales data. The number of lags was determined by using the FPE criterion.[9] The Lagrange Multiplier test for first order serial correlation produced a Chi-Square(1) statistic of 0.2, with a marginal significance level of 0.6. Hence, first order serial correlation is not a problem here. (The conventional Durbin-Watson statistic cannot be used because of the presence of lagged values of real GNP on the right hand side. See Pagan [1984] for a discussion of the Lagrange Multiplier test.) Omitting lagged values of real GNP leads to serially-correlated residuals and worsens the forecasting performance of the equation. Experimentation with different priors to restrict the coefficients on the lagged values of GNP did not lead to an improvement in forecasting performance.

III. Forecasting Performance

Table 2 presents the error statistics for the GNP forecasts. For each forecast, the equation was estimated up to the previous quarter and the resulting coefficients used, together with the current quarter values of the indicator variables, to predict real GNP growth in that quarter. I present results for two sample periods. The first one extends from 1983.3 to 1988.2, a total of 20 forecasts. The intent is to focus upon the most recent period. However, it is likely that a sample of 20 forecasts is not large enough to provide a reliable test of the model's performance. Accordingly, Table 2 also presents summary statistics on the model's forecasting performance over the period from 1978.3 to 1988.2, a total of 40 forecasts.

Four different exercises were performed for each sample period to duplicate the varying amounts of information available over the course of the quarter. The first one tests the forecasting capabilities of the model during the first month of each quarter, when no information is available on the indicator variables. In this case, the BVAR forecasts the values of the indicator variables for all three months of the quarter and these values are used in the GNP equation to forecast GNP growth. The second assumes that we are in the second month of the quarter, when data for one month are available on the indicator variables, and the BVAR is used to forecast the values of the indicator variables for the remaining two months of the quarter. Similarly, the third set of GNP forecasts is based on two months of data for the indicator variables, and the BVAR is used to forecast the values of the indicator variables in the third month of the quarter. Finally, the fourth set is based on all three months of actual data for the indicator variables, so that no BVAR forecast is required to forecast GNP growth.

Table 2
Comparison of Real GNP Forecast Errors
(Annualized Growth Rates)

(A) Forecasts over 83.3–88.2 (20 forecasts)

	Monthly Indicators Model Forecasts				Blue Chip Forecasts			
Month of forecast*	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic
1	0.13	2.41	3.29	1.14	0.72	2.18	2.62	0.91
2	0.29	1.62	1.96	0.68	0.96	2.19	2.62	0.91
3	0.38	1.58	1.95	0.67	0.92	1.91	2.31	0.80
4	0.40	1.39	1.86	0.64	0.80	1.80	2.14	0.74

(B) Forecasts over 78.3–88.2 (40 forecasts)

	Monthly Indicators Model Forecasts				Blue Chip Forecasts			
Month of forecast*	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic	Mean Error	Mean Abs. Error	Root Mean Sq. Error	Theil's U-Statistic
1	0.17	2.45	3.34	0.70	0.61	3.00	3.77	0.82**
2	0.20	1.45	1.81	0.38	0.71	2.69	3.30	0.69
3	0.22	1.41	1.73	0.36	0.74	2.16	2.64	0.55
4	0.22	1.30	1.69	0.35	0.64	1.80	2.16	0.45

*These dates refer to the month of the quarter in which the forecast becomes available. The 4th month is the month after the quarter ends. This dating convention implies that the model forecast may be based on as much as 1 month of additional information compared to the Blue Chip forecast. See text for details.

**Based on 39 forecasts only. (The first published Blue Chip survey is dated August 1978.)

An important issue in evaluating the forecasting performance of the model has to do with the use of real-time versus final data. Ideally one would like to duplicate the data sets that were actually in use at each point of the sample period to compare the model's forecasts with those available from other sources. Unfortunately, while it is possible (with considerable effort) to obtain preliminary data, it appears virtually impossible to find out the dates at which subsequent revisions were made for each of the series in the model. Consequently, it is not possible to duplicate the data sets that were used for the real-time forecasts made over this period. Therefore, all the statistics presented below have been computed on the basis of currently available (August 1988) data.[10]

Table 2 reveals that the real GNP forecasts obtained when the BVAR forecasts the indicator variables for all three months of the current quarter (that is, forecasts made in the first month of the quarter) are not very good, with a RMSE above 3.25 percent (at an annual rate) in both sample periods. In fact, for the short sample period Theil's U-statistic is greater than one, implying that a naive forecast of no change in growth rates would have been better than the monthly indicators model's forecast over this period. This is not a major shortcoming, however, since the purpose of the model is to forecast real GNP using contemporaneous information on the indicator variables.

The performance of the model improves noticeably when information on the first month of the quarter becomes available (that is, for forecasts made in the second month of the quarter), with the RMSE falling below two percent. Over the shorter sample period, forecasts made in the third month of the quarter (shown in the third row) are no more accurate than those made in the second month, although they are slightly more accurate for the full sample period. Similarly, forecasts made one month after the quarter has ended (that is, forecasts that use actual data on all three months of the quarter) are not much better than forecasts made in the third month of the quarter. In fact, the RMSE of the forecast made in the month after the end of the quarter being forecast is only around 0.1 percentage points smaller than the RMSE of the forecast made two months earlier.

The relatively small impact of the second and third months' data on the model's forecast accuracy reflects the fact that quarterly growth rates are a weighted average of monthly growth rates. For example, in computing the growth rate for the second quarter from monthly data, the growth rates for February and June get a weight of 1/9 each, those for March and May get a weight of 2/9 each and that for April gets a weight of 3/9. Thus, the arrival of information on the first month of the quarter doubles the amount of information we have on the quarterly growth rate (from one-third to two-thirds). By contrast, information on the third month of the quarter gives us only one-ninth of the information required for the quarterly growth rate. That is why the model's forecasts will not change significantly when data on the second and third months of the quarter become available.

Notice that the RMSEs of the real GNP forecasts made on the basis of three months of information are smaller than the standard error of the estimated GNP equation. This implies that the variables that are used to forecast real GNP are doing more than picking up random movements. Finally, while the error statistics for the shorter sample period tend to be somewhat larger than those for the full sample, the difference is not large enough to suggest that the forecasting ability of the model has changed over time.

Comparison with the Blue Chip Consensus

Table 2 also includes forecast error statistics for the consensus real GNP forecast from the Blue Chip survey. This survey is based on a panel of 51 forecasts and is contained in a newsletter titled, *Blue Chip Economic Indicators*, published by Capitol Publications. The consensus forecast is the average of the 51 individual forecasts. For the Blue Chip forecasts I have chosen a dating convention based on when the forecasts are released. The official release date of the survey is the 10th of the month, but the survey itself is conducted over the first week of the month. I have dated the forecast released on the 10th of the month as the forecast for that month. For example, the first quarter Blue Chip forecast released on the 10th of April is the forecast that is compared to the model forecast available on the 15th of April. From a policymaker's perspective, this comparison is the relevant one, since the two forecasts are lined up according to the dates when they actually become available.

However, if we want to assess the relative accuracy of the two forecasts, it would be better to compare the model forecast errors in one row with the Blue Chip forecast in the following row, since the Blue Chip average in any row will be based on less information about the economy than the model forecast in the same row. For example, the error statistics on the model's forecasts available in the second month of the quarter should be compared to the error statistics on the Blue Chip forecasts available in the third month of the quarter. Note, however, that this comparison will overcompensate in those months where employment data for a given month are released in the first few days of the following month, because the Blue Chip survey respondents are likely to have incorporated this information into their forecasts by the time of the survey.[11]

Table 2 reveals that over both the 83.3–88.2 sample period and the 78:3–88:2 sample period, the Mean Error, MAE and the RMSE of the model forecasts available in the second month of the quarter are all smaller than the corresponding error statistics for the Blue Chip consensus forecast available in each of the following two months. The model forecast does worse than the Blue Chip forecast only for forecasts made when no information on the current quarter is available. Thus, once information about the first month of the current quarter becomes available, the monthly indicators model performs better than the Blue Chip forecast.

Needless to say, this comparison exaggerates the relative advantage of the monthly indicators model, since it was estimated with the benefit of hindsight and it uses more accurate data than was available to individuals making real time forecasts over this period. Nevertheless, it does provide some reassuring evidence on the forecasting capabilities of the model. In addition, early versions of the model have been used to make real-time forecasts of output growth since the third quarter of 1987. These forecasts are presented in Table 3, along with the Blue Chip consensus forecast. Over this period (87.3–88.2), the mean error of the model's real time forecasts made using three months of data on the indicator variables is 0.7 percent, the MAE is 1.1 percent and the RMSE is 1.5 percent. Over the same period the mean error of the comparable Blue Chip forecast is 2.0 percent, the MAE also is 2.0 percent and the RMSE is 2.4 percent. For the model forecasts based on one month of information, the mean error is 0.9 percent, the MAE is 1 percent and the RMSE is 1.4 percent. While this sample of four observations is much too small for the results to be considered proof of the model's real-time forecasting capabilities, these results are at least consistent with the statistics presented in Table 2.

Finally, Chart 1 compares real GNP growth and the forecasts from the model for the period from 1983.3 to 1988.2. Two different forecasts are shown: first, forecasts made on the basis of one month of data on the current quarter and second, forecasts made on the basis of three months of data on the current quarter. The two forecasts are similar, as the RMSEs reported in Table 2 would suggest.

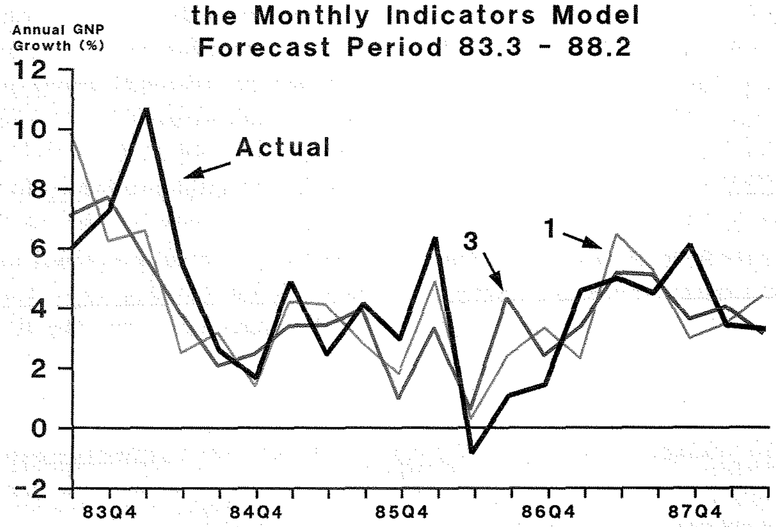
To summarize the results of this section, the forecast errors reveal that the monthly indicators model is not very useful when no information is available on the current quarter. The forecasting ability of the model increases noticeably once the first month of information becomes available, although the improvement is likely to be smaller when the model makes real-time forecasts because only preliminary data will be available at first. The model's forecasts should be much more reliable once data on the second month are available, especially because data for the first month of the quarter are often revised at this time and hence are likely to be more accurate.

Table 3
Real time forecasts of Real GNP Growth

Quarter being forecast	Monthly Indicators Model Forecast available in the 2nd month	3rd month	4th month	Blue Chip Consensus forecast available in the 2nd month	3rd month	4th month	Final GNP estimate*
87.3	3.5	3.9	3.8	2.4	2.7	2.9	4.5
87.4	3.6	3.0	3.2	1.5	1.9	2.1	6.1
88.1	3.0	4.0	4.1	0.4	0.7	1.4	3.4
88.2	3.2	2.9	3.0	2.0	2.3	2.5	3.0
Mean Error:	0.9	0.8	0.7	2.7	2.3	2.0	
Mean Absolute Error:	1.0	1.1	1.1	2.7	2.3	2.0	
Root Mean Square Error:	1.4	1.6	1.5	3.0	2.7	2.4	

*Real GNP data as of September 30, 1988.

Chart 1
Real GNP Forecasts from
the Monthly Indicators Model
Forecast Period 83.3 - 88.2



Numbers refer to months of current-quarter data used in making forecast.

IV. Combining Forecasts

The results presented above reveal that the model's forecasts are reasonably accurate. However, we have not yet examined the issue of optimality. In other words, are the model's forecasts the best available, or can they be improved by combining them with information from some other source? Although it is not possible to determine what is *the* best forecast overall, this section considers the possibility of combining the model's forecast with the advance GNP estimate and the Blue Chip consensus forecast to determine whether the model's forecasts can be improved.

The Model Forecast and the Advance GNP Estimate

We begin by looking at what happens when the advance GNP estimate (which is released by the Commerce Department about three to four weeks after the end of the quarter) is combined with the model forecast to predict final GNP. The first part of Table 4 presents regressions of final GNP on the advance GNP estimate and on the model forecast obtained by using all three months of current quarter data. Once again, results are presented for two different sample periods. The first two columns of the Table show that over 1983.2–1988.2 both estimates are unbiased (that is, the hypotheses that the constant term is zero and that the coefficient on the forecast is one cannot be rejected at conventional significance levels in either equation). Also, both equations explain about the same share of the total variation in final GNP.

The third column presents a regression including both variables. When forecasts are pooled using regression analysis it is common practice to exclude the constant term and constrain the coefficients on the two forecasts to sum to one. This procedure has the advantage that if the two individual forecasts are unbiased, the combination forecast will be unbiased as well. However, Granger (1984) points out that the forecast error obtained from such a procedure is not necessarily uncorrelated with the individual forecasts. Thus, it is possible that the forecast error itself can be forecast from one of the individual forecasts, implying that the combination procedure is not optimal. To avoid this, Granger recommends that the estimated equations include a constant and not place any restrictions on the coefficients. Accordingly, column (3) of Table 4 presents results from unrestricted regressions.

The unrestricted regressions produce coefficients on the model forecast and on the advance GNP estimate that are about the same size. The standard error of this equation is about 10 percent smaller than the equation containing the advance GNP estimate alone, suggesting that the monthly indicators model does contain information over and above that contained in the advance GNP data. Unfortunately, the coefficients in equation 3 are not estimated very precisely. Thus, the 70% confidence interval for the coefficient on the model forecast extends from .31 to .83, while the 70% confidence interval for the coefficient on advance GNP extends from .29 to .88.

Columns (4)–(6) present the same regressions over the entire sample period. A comparison of these results with those in columns (1)–(3) reveals that there is not much difference between the coefficients of either variable across the two sample periods. However, the adjusted R^2 for the full sample period is noticeably higher.

The bottom half of the Table presents the error statistics obtained when the advance GNP estimate and the model's forecast are combined to predict the final GNP number. The sample period extends from 83.3 to 88.2. (The entire sample period cannot be used since the model's forecasts over the 78.3–83.2 period are used to estimate the prediction equation.) The procedure is the same as in Table 2, that is, the forecast for each quarter is obtained by estimating the underlying equation up to the previous quarter.

Combining the two forecasts leads to a MAE of 1.26 percent and a RMSE of 1.65 percent over this period. A comparison with the results in Table 2 reveals that the MAE obtained from the combination forecast is about 10 percent less than the MAE of the model's forecast. A similar reduction is obtained for the RMSE. This combination forecast is also an improvement on the results obtained when advance GNP is itself treated as a forecast of real GNP. If the advance GNP estimate is used by itself to forecast real GNP over the 83.3–88.2 period, the mean error is 0.54 percent, the MAE is 1.52 percent and the

Table 4
Combining the Advance GNP Estimate and the Model Forecast

(A) Real GNP Regressions

	Dependent Variable: Final Estimate of Real GNP Growth					
	83.3–88.2			78.3–88.2		
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Coefficients*:						
Constant	0.96 (1.1)	0.47 (0.5)	−0.13 (−0.1)	0.38 (1.1)	0.21 (0.6)	0.13 (0.5)
Advance GNP	0.88 (4.4)	—	0.59 (2.0)	0.97 (12.4)	—	0.47 (3.5)
Model Forecast	—	0.99 (4.0)	0.57 (2.2)	—	1.01 (13.2)	0.58 (4.2)
(B) Adjusted-R^2	.49	.44	.58	.80	.82	.86
(C) S.E.E.	1.82	1.91	1.66	1.81	1.71	1.50
(D) Durbin-Watson Statistic	2.15	1.76	2.01	2.24	2.31	2.31

(B) Error statistics for Combination Forecasts

Forecast period: 1983.3–1988.2

Mean Error	Mean Abs. Error	Root Mean Sq. Error
0.21	1.26	1.65

*t-statistics are shown in parentheses.

RMSE is 1.82 percent. These errors are roughly the same size as the errors of the monthly indicators model forecast based on three months of information (see Table 2). Thus, pooling the model's forecast and the advance GNP estimate leads to forecasts that are an improvement on either one considered by itself.

The Model Forecast and the Blue Chip Forecast

Table 5 presents the results of combining the model forecast and the Blue Chip consensus forecast. The regressions shown in the first part of the table are based on forecasts that become available in the first month after the end of the quarter. Column (1) shows that the Blue Chip forecast is an unbiased estimator of final GNP, a result that is not too surprising because the forecast itself is an average. A comparison of this equation with equation (2) of Table 4 reveals that the model forecast explains a somewhat greater share of the in-sample variation of real GNP than does the Blue Chip forecast. Regressing real GNP on both the Blue Chip and the model forecast (column 2 of Table 5) improves the explanatory power of the equation, although this equation does not do quite as

Table 5
Combining the Blue Chip and Model Forecasts

(A) Real GNP Regressions

Dependent Variable: Final Estimate of Real GNP Growth

	83.3–88.2		78.3–88.2	
	(1)	(2)	(3)	(4)
(A) Coefficients*:				
Constant	0.90 (0.8)	−0.60 (−0.6)	0.43 (1.1)	0.12 (0.4)
Blue Chip Forecast	0.97 (3.2)	0.59 (2.1)	1.11 (10.3)	0.41 (2.6)
Model Forecast	—	0.74 (2.9)	—	0.71 (5.2)
(B) Adjusted-R^2	.33	.53	.73	.84
(C) S.E.E.	2.09	1.76	2.09	1.60
(D) Durbin-Watson Statistic	1.61	1.75	2.18	2.33

(B) Error statistics for Combination Forecasts

Forecast period: 1983.3–1988.2

Month of Forecast	Mean Error	Mean Abs. Error	Root Mean Sq. Error
1	0.44	2.21	3.00
2	0.25	1.71	2.04
3	0.10	1.55	1.90
4	0.06	1.33	1.76

*t-statistics are shown in parentheses.

well as the one that contains the model forecast and the advance GNP estimate. The coefficients are not estimated very precisely here either. A 70% confidence interval for the coefficient on the Blue Chip forecast extends from .30 to .87, while that for the coefficient on the model forecast extends from .48 to .99. Roughly the same sort of results are obtained for the full sample period. These are shown in columns (3) and (4) of the table.

The second part of the table presents the error statistics obtained when the two forecasts are combined to predict final GNP. The forecasts are generated in the same way as they were in Table 4. However, four sets of forecasts are presented here, to allow for the possibility that the relative weights on the two forecasts may not be the same at different points in the quarter. Unfortunately, these results do not suggest that the two forecasts can be combined very profitably in the early parts of the quarter. The Blue Chip forecast made in the first month of the quarter is generally better than the forecast obtained by combining the model and the Blue Chip forecast. By contrast, the model forecast made in the second month of the quarter is better than the combination forecast. And while the combination forecast made in the third month of the quarter is an improvement over the model forecast, the difference between the two is not striking (for instance, the RMSE falls from 1.95 to 1.90). There is a somewhat larger gain for combination forecasts made in the first month following the end of the quarter (the RMSE falls from 1.86 to 1.76); however, these forecasts are worse than those obtained by combining the model forecast and the advance GNP estimate.

Finally, it is worth asking if the error in predicting final GNP can be reduced by combining all three measures: the model forecast, the Blue Chip forecast, and the advance GNP estimate. Unfortunately, this does not lead to any improvement in the GNP forecast. The equation that contains all three variables turns out to be no better than the one that contains only the model forecast and the advance GNP estimate over either sample period. Further, an equation that contains the advance GNP estimate and the Blue Chip forecast does no better than an equation that contains only advance GNP. (For the 83.3–88.2 period, the coefficient on the Blue Chip forecast is 0.01 while that on advance GNP is 0.87.)

Table 6
Predicting Advance GNP

Dependent Variable: Advance Estimate of Real GNP Growth

	83.3–88.2			78.3–88.2		
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Coefficients*:						
Constant	1.01 (1.1)	−0.07 (−0.1)	−0.69 (−1.0)	0.15 (0.4)	0.11 (0.4)	−0.02 (−0.1)
Blue Chip Forecast	—	1.10 (6.4)	0.94 (5.2)	—	1.11 (15.7)	0.81 (6.5)
Model Forecast	0.70 (3.1)	—	0.31 (1.9)	0.90 (11.1)	—	0.31 (2.9)
(B) Adjusted-R^2	.31	.68	.72	.76	.86	.88
(C) S.E.E.	1.73	1.18	1.10	1.82	1.37	1.25
(D) Durbin-Watson Statistic	1.74	1.53	1.84	2.16	1.80	1.94

*t-statistics are shown in parentheses.

Predicting the Advance GNP Estimate

The results presented above suggest that the Blue Chip consensus forecast is closely related to the advance GNP estimate. Table 6 provides direct evidence on this issue, in the form of regressions of the advance estimates of real GNP on both the model forecast and the Blue Chip forecast. A comparison of columns (1) and (2) reveals that while both estimates are unbiased predictors of the advance GNP estimate, the Blue Chip forecast is much more closely related to advance GNP than is the model forecast over the 83.2–88.2 period. Column (3) shows that if both variables are used to forecast advance GNP, the coefficient on the Blue Chip forecast is three times that on the model forecast. A comparison of the adjusted R^2s and the standard errors of equations (2) and (3) shows relatively little difference between the two. Thus, the model forecast provides very little information about advance GNP once the information available in the Blue Chip forecast has been taken into account. The results over the entire sample period are similar, though the model forecast does noticeably better by itself.

The fact that the Blue Chip consensus is so much better at predicting advance real GNP than the model probably reflects the way that the underlying forecasts have been constructed. Private sector forecasters follow methods that are very similar to those used in constructing the advance GNP release. Since markets react to the advance release, it seems plausible that market participants will focus their efforts on obtaining forecasts of this number. In contrast, estimation of the monthly indicators model has used final GNP data and no attempt has been made to predict the advance numbers, since policymakers presumably are concerned about the actual level of economic activity, and not its first estimate.

V. Conclusions

This paper has presented a simple model to obtain estimates of current quarter real GNP growth based on a small number of variables. Information on the set of variables that is used to forecast GNP becomes available relatively early. In addition, these variables are relatively easy to predict, so that by the middle of the second month of the quarter being forecast we have a forecast of final GNP growth with a Root Mean Square Error that is less than 2 percent at an annual rate. Nor is it very difficult to generate the GNP forecasts. Obtaining these forecasts requires keeping track of a small number of monthly series, and the forecast itself can be generated very quickly on a personal computer.

The results presented here reveal that the model's forecasts based on one month of data for the current quarter are about as good as those based on all three months of data. Further, these forecasts compare well to the consensus Blue Chip forecast. Finally, the monthly indicators' forecast provides useful information on final GNP even after the advance GNP estimate is released.

APPENDIX

The set of variables over which I searched to find the best specification for the monthly indicators model contained:

- Nominal Manufacturing Shipments
- Nominal Manufacturing Inventories
- Book Value of Manufacturing and Trade Inventories
- Housing Starts
- Six Month Commercial Paper Rate
- Ten Year Bond Rate
- Producer Price Index—Finished Goods
- Consumer Price Index
- Aggregate Labor Hours Index
- Average Nonfarm Hours
- Average Manufacturing Hours
- Automobile Sales
- Retail Sales Net of Autos
- Industrial Production
- Nonfarm Payroll Employment
- Retail Sales

ENDNOTES

1. This release was known as the preliminary GNP estimate prior to 1988.3.

2. In general, these are variables that are relatively easy to forecast but are not as closely correlated to contemporaneous GNP as the variables that were finally included in the model. It is worth pointing out that this strategy is part of the reason that the monthly indicators model does relatively badly when no information on the indicator variables is available (see Table 2).

3. In a vector autoregression each variable is regressed on past values of itself and the other variables included in the vector.

4. A series y_t is said to be a random walk with drift if its behavior over time can be described as

$$y_t = a + y_{t-1} + e_t,$$

where e_t is a serially uncorrelated error term. In this case, our best guess of the value of y tomorrow is its value today plus the constant term a (the drift).

5. Employment data for a given month are generally released on the first Friday of the following month. Data on industrial production, retail sales, and the producer price index become available around the 15th.

6. Since there is no straightforward conceptual relationship between the variables included in the BVAR, it is not clear what interpretation can be placed upon the estimated coefficients. Even if this were possible, it would be difficult to analyze the 60-odd coefficients contained in each of the equations.

7. This is to avoid using coefficient estimates based on information obtained after the forecast was made. This exercise still exaggerates the degree of precision we would obtain in real time because we use revised data. This issue is discussed in the next section.

8. A secondary reason for the large difference in the forecast errors of the different variables is that I tended to favor priors that improved the forecast accuracy of the employment number at the expense of the others. This is because the employment number has a much greater weight in the equation for predicting real GNP than the other variables.

9. See Judge, et. al [1984] for a description of this criterion.

10. Braun (1987) provides an estimate of the effect of data revisions in a study that uses labor market data to predict contemporaneous output. He reports that using preliminary instead of currently available data raises the Root Mean Square Error (RMSE) of the real GNP forecasts by between 0.2 to 0.4 percentage points.

11. It is also worth pointing out that a Blue Chip forecast made in the second month following the end of the quarter being forecast is not available because no forecasts are compiled once the advance estimate of real GNP is released.

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Undocumented Workers and Regional Differences in Apparel Labor Markets

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The Immigration Reform and Control Act of 1986 (IRCA), which requires employers to verify that the workers they hire can work legally in the United States, would be expected to reduce the supply of undocumented workers. California's apparel industry appears to be particularly vulnerable to these changes, since it relies heavily on undocumented workers, but employment growth in California's apparel industry has continued to outpace that of the nation by a wide margin since employer sanctions went into effect. Empirical examination reveals little relationship between undocumented workers and employment in the apparel industry, suggesting that other factors are more important causes of growth in California's apparel industry.

In November 1986, Congress passed the Immigration Reform and Control Act of 1986, requiring employers to verify that the workers they hire can work legally in the United States. Stringent enforcement of this law should reduce the supply of undocumented workers, causing employment to fall and wages to rise in sectors and regions where undocumented workers have comprised a significant proportion of the labor force.

California's apparel industry appears to be particularly vulnerable to these changes. A number of analysts attribute its rapid growth, particularly when compared with the decline of the apparel industry nationally, to the ready supply of low-wage undocumented workers available in the state (Maram, 1980; UCLA Forecast, 1987). Industry participants note that in Southern California, which dominates apparel production in California,[1] most workers are Mexican, and by most accounts a large proportion of these are undocumented. Maram's study suggests that in 1980 about 60 percent of all garment workers in Los Angeles were undocumented Hispanics.[2] Moreover, because the apparel industry is highly competitive, with many small producers in a large number of countries, easy entry and exit, and relatively low profit margins, it is especially vulnerable to any change that increases production costs. Thus, a law that limits the supply of undocumented workers might be expected to retard growth in California's apparel industry.

In fact, however, the growth in California's apparel industry has continued to outpace that of the national industry by a wide margin since the ban on employing undocumented workers went into effect on June 1, 1987. Between July 1987 and July 1988, employment in California's apparel industry posted healthy growth of 3.1 percent, compared with a 2.4 percent decline in U.S. apparel employment during the same period.

Thus, despite its apparent vulnerability to the new law, California's apparel industry does not yet appear to have been affected by it. Why has the law not had the anticipated effects? This paper examines the provisions of the law and the characteristics of the apparel industry to evaluate the impact of the law and to determine whether the law is likely to affect the apparel industry in the future.

The paper is organized as follows. Section I discusses the implementation of the law. Section II describes the structure of the apparel industry. Section III sets out an economic theory of the effects of undocumented workers on regional labor markets. Section IV tests and interprets the hypotheses generated in Section III. Section V summarizes and draws conclusions.

I. The Immigration Reform and Control Act of 1986

The Immigration Reform and Control Act of 1986 (IRCA) became law in November 1986, but its key provision regarding undocumented workers (UWs) did not go into effect until June 1, 1987, when it became illegal for employers to hire UWs, and employers were required to verify the work status of all new employees. Even then, these provisions initially were not enforced with the full sanctions available under IRCA. Instead, on June 1, 1987, the Immigration and Naturalization Service (INS) began issuing citations to employers who violated these provisions of the law. Only the most egregious and repeated violations resulted in fines, and these fines were heavily publicized to discourage other employers from ignoring the law. After a twelve-month "first citation" period, the employer penalties became much more severe, with employers subject to fines of as much as $2,000 per violation for a first instance of knowingly hiring UWs. Under the law, even larger civil fines can be imposed for subsequent violations, and criminal penalties, including jail terms, can be imposed on employers who establish a "pattern or practice" of illegal hiring.

As a result of this phase-in period for employer sanctions, the full force of the law did not take effect until June 1988. Thus, it is not surprising that the law appears to have had no effect through July 1988. However, one cannot necessarily infer from this that IRCA will have no effect over the long term. The employer sanctions now in effect ultimately may deter the hiring of UWs, causing the inflow of migrants to slow substantially, and forcing significant adjustments in affected labor markets.

But there also is reason to believe that the employer sanctions may *not* deter employers from hiring UWs. The law requires employers to *check* documents that indicate workers' citizenship and residency status, but does not require employers to *verify* the authenticity of those documents. Moreover, the law explicitly bans employment discrimination on the basis of national origin or citizenship status. As a consequence, UWs who obtain false documents still would be able to find work and so would not be deterred from crossing the border.[3] In fact, in June 1988, the *New York Times* reported that illegal entries into the U.S. continued to rise, despite IRCA's sanctions. Moreover, if enforcement at the borders increases, the stock of UWs in the U.S. could rise, since Mexican workers who otherwise might return to Mexico for part of the year may stay in this country in order to minimize the number of border crossings.

II. The Structure of the Apparel Industry

Unlike most manufacturing industries, the apparel industry approximates a textbook case of perfect competition. It consists of a large number of relatively small firms. Four-firm concentration ratios[4] in eight 4-digit SIC categories of apparel[5] range from eight to 25 (Parsons, 1988). A ninth 4-digit category has a four-firm ratio of 49, which also is low by the norms for most other industries. Moreover, firms in the apparel industry tend to be small. In 1985, California apparel firms averaged 26 employees per establishment, compared with 44 for all manufacturing. Finally, ease of entry and exit characterize the industry because of its low capital-to-labor ratio. The value of capital averages only $4000 per employee, compared with $31,100 for all manufacturing industries (ILGWU 1985).

The Role of Labor

This low capital-to-labor ratio suggests that wages comprise a significant share of the cost of producing garments. In fact, labor compensation accounts for 53 percent of the value added by apparel manufacturers, and 27 percent of the value of finished apparel products,[6] according to the Annual Survey of Manufacturers. As a result, wage levels are an important determinant of the profitability of apparel manufacturing.

As important as the *cost* of labor is its *productivity*. Employers look for workers who are willing to work at the low level of wages offered by apparel manufacturers.

While employers prefer workers who are skilled and experienced garment makers, most garment workers have little formal education, often know little English, and tend to have few employment options outside the apparel industry. Cities with large immigrant populations frequently provide such workers.

Technology

Although garment manufacturing continues to be a labor-intensive process, some technological improvements have been made in recent years. Most of these improvements have been in the areas of fabric cutting and pattern making, where laser cutters and computerized sizing and pattern layouts are now in use. In addition, some products are particularly suited to the development of specialized machinery. For example, specialized machines are available for sewing pockets, zippers, or belt loops on blue jeans. This more sophisticated machinery is widely available, but only the larger plants can afford the substantial investment it represents. As a result, its use is somewhat limited, and many smaller shops continue to produce garments using less specialized technology.

Heterogeneity of Apparel Products

It is important to recognize that the apparel industry is far from homogeneous. Some of the differences are obvious. For example, some producers specialize in women's sportswear while others produce men's suits. These differences have important implications for the production processes and the plant's location relative to factor and product markets.

For one thing, production of some items can be automated more easily than can production of others. For example, as mentioned above, production of standard blue jeans can be automated or subcontracted to other locations. In contrast, tailored clothing requires considerably more hand work and closer supervision.

Production of high-fashion apparel also is difficult to standardize. Most garments for which demand can be predicted many months in advance, and for which designs are well established, can be produced almost anywhere. The manufacturer can subcontract the production to plants in other states or other countries. However, garments for which demand is less predictable need to be produced in a shorter time frame. For these more fashion-oriented items, the short lead time means that designers need to be close to the production facility in order to be able to check samples as they are made, make last-minute decisions regarding trims, and monitor the quality of production. Consequently, the cost of production labor is less important for these more fashion-oriented producers than it is for more standard garments, and they are more likely to locate in fashion design centers such as New York or Los Angeles.

Trends in International Trade

For all types of producers, pressures on profit margins have grown in recent years, leading to increased use of overseas production facilities. Estimates of import penetration indicate that imports have become significantly more important during the past twenty years.[7] As a result, patterns of international trade in garments play an increasingly important role in explaining the condition of the industry.

Overseas production offers the major advantage of lower labor costs. However, longer lead times and higher transportation costs make it inappropriate for some types of garments, particularly high-fashion garments. Although some foreign producers seem to be more responsive than their American counterparts are (Lardner, 1988), others have lax production standards and quality control procedures that make relying on them risky (Jacobs, 1988).

Frequent changes in quota and tariff restrictions further complicate life for overseas "sourcers." The Multi-Fiber Agreement (MFA) establishes a series of bilateral quotas for particular apparel items. Thus, most countries have limits on the number of items (skirts, jackets, etc.) that they can export to the U.S. These quotas are based on the country's past exports of each item. Thus, U.S. distributors cannot buy unlimited quantities of apparel items from the lowest-cost or highest-quality producers. Indeed, there is a strong incentive for countries to start producing apparel items they never have produced before, in order to supply as much as possible to the U.S. before quotas for that item from that country are imposed.

Another legal arrangement that affects international trade patterns is "Item 807," which permits U.S. firms to export cut fabric to Caribbean and Latin American countries (including Mexico) for assembly. The finished product, when returned to the U.S., is subject to tariff only on the value added in the foreign country—which, given prevailing wage rates in Item 807 countries, usually is a relatively small fraction of the finished price of the garment. Because materials and garments can be trucked to and from Mexico at low transportation costs, producers in California and Texas tend to be heavy users of Item 807.

III. Regional Labor Market Theory and Undocumented Workers

The role that the presence of UWs plays in California's apparel industry can be examined by considering labor markets in different regions, where a region is defined as a state. To take the simplest possible case, assume that each region produces an identical, homogeneous apparel product, and that the cost of living and the productivity of workers are identical across regions.

Two such regions, A and B, are illustrated in Chart 1. Initially, the supply of and demand for labor are S^0 and D^0 (with appropriate subscripts). Wages in the two regions are equal, at W^0, so neither workers nor firms have an incentive to move from one region to another. Employment initially stands at L_a^0 in region A and at L_b^0 in region B.

Now assume that region A experiences a sudden influx of UWs.[8] This shifts the labor supply curve in Chart 1a to the right, to S_a^1, initially reducing wages in region A to W_a^1, and increasing employment to L_a^1.

At this point, the system is in disequilibrium. The wage in region A, W_a^1, is lower than the wage in region B, which remains W^0. Consequently, firms seeking lower wages have an incentive to shift production from region B to region A. At the same time, workers seeking higher wages have an incentive to move from region A to region B. Migration of labor and firms would continue until the wage rates in the two regions are equalized.

In terms of Chart 1, migration of firms from region B to region A causes the labor demand curve to shift to the right in region A and to the left in region B. Migration of workers from region A to region B causes the labor supply curve to shift to the left (from S_a^1) in region A and to the right in region B. Migration stops, and the curves stop shifting, when wages have risen in region A and fallen in region B, to the point where they are equal in the two regions, at W^*. This equilibrium occurs when labor demand and supply reach D^2 and S^2 (with appropriate subscripts) in the graphs. At this point, wages are lower than they were initially (W^0), but they also are higher than they were in region A immediately after it received the influx of UWs (W_a^1). Employment in region A settles at L_a^*, higher than its initial level of L^0, and either higher or lower than the employment level after the initial influx of immigrants, L_a^1. Likewise, in region B, the direction of change in employment between L^0 and L_b^* is indeterminate, and depends on the relative magnitudes of the shifts in supply and demand curves.

If workers' productivity levels differ from one region to another, or if the cost of living differs, then *nominal* wages would not be expected to be equal in the two regions. Nevertheless, if workers and firms migrate freely from one region to another, the cost of labor, adjusted for differences in productivity and the cost of living, still should be equalized across regions.

However, this scenario assumes that both workers and firms can move freely among regions, an assumption that is not likely to be realized in practice. Workers as a group tend to move slowly in response to changing economic conditions. Apparel industry workers, who tend to have little formal education, tend to be particularly closely tied to their regions by strong cultural bonds. Thus, perhaps paradoxically, apparel workers may be more mobile be-

Chart 1A
Region A

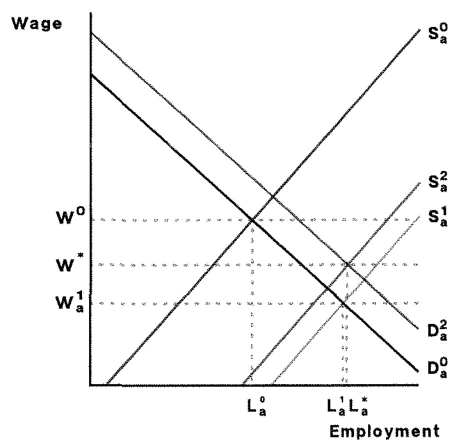
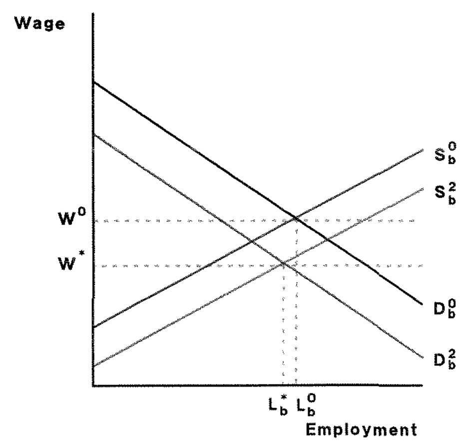


Chart 1B
Region B



tween Mexico and such centers of the Mexican community in the U.S. as Los Angeles than they are between Los Angeles and New York. Similarly, apparel workers in the Southeast may be unwilling to move to the Northeast for cultural or family reasons, despite higher pay in the Northeast.

Likewise, there may be reasons why firms do not respond to real wage differentials. For example, as mentioned earlier, proximity to designers can be important for products that are new on the market or for which demand is uncertain. These limits on firms' mobility could result in real wage differences among regions.

Thus, disequilibrium in real, quality-adjusted wages could persist because some workers and firms may be unwilling or unable to move in response to wage differentials among regions. In this case, an increase in a particular region's population of UWs would cause wages to fall more than they would in other regions that do not experience a similar influx of UWs. In such a disequilibrium world, wages (appropriately measured) could be persistently lower in regions that have large UW populations.

These observations lead to two empirically testable conjectures:

(1) Regions that receive undocumented workers from other countries should have a higher proportion of their *employment* in labor-intensive industries such as apparel than they would have if their populations included no UWs. This assumes that the initial influx of UWs is localized, but does not depend on whether migration of individuals and firms leads the system to approach equilibrium.

(2) If workers and firms are not perfectly mobile, *wage differentials* can persist, and wages will be lower in regions that receive UWs. However, if factors are perfectly mobile, there should be no significant regional differences in wage rates, and a regression that attempted to explain those differences might perform poorly.

IV. Testing the Undocumented Worker Hypothesis

The model presented in Section III can be formalized. To do so, consider the factors that determine the supply of and demand for labor in the apparel industry. The model of Section III and the information about the industry presented in Section II suggest that the number of workers available to apparel manufacturers in a particular region should rise if apparel wages rise, if the number of UWs is greater, and if a large proportion of the region's population has few alternatives to apparel industry employment. Education is used to proxy the general job skills that would allow workers a wide range of employment alternatives. Moreover, the demand for labor among apparel manufacturers would be greater if wages are lower, and if the state's production activity is more closely tied to design activity.

These factors suggest the following structural model:

$$S_L = f(\text{UW, UNED, WAGE}) \tag{1}$$

$$D_L = f(\text{DESIGN, WAGE}) \tag{2}$$

WAGE = apparel industry wage
UW = undocumented workers as a proportion of population
UNED = proportion of population without a high school education
DESIGN = importance of design to the state's apparel industry

If labor demand and supply curves are linear, demand and supply take the following form:

$$S_L = t + u\,\text{UW} + v\,\text{UNED} + w\,\text{WAGE} \tag{1'}$$

$$D_L = x + y\,\text{DESIGN} + z\,\text{WAGE} \tag{2'}$$

The theory suggests that u, v, w, and y should be positive, and z should be negative. The region's labor market clears when the wage is such that labor supply equals labor demand.[9] Using these conditions along with equations (1′) and (2′), one can solve for equilibrium employment, EMP, and wages:

$$\text{EMP} = [(zt-xw) + uz\,\text{UW} + vz\,\text{UNED} - yw\,\text{DESIGN}][1/(z-w)] \tag{3}$$

$$\text{WAGE} = [(x-t) - u\,\text{UW} - v\,\text{UNED} + y\,\text{DESIGN}][1/(w-z)] \tag{4}$$

To simplify the expression of the reduced form, define the following variables:

$$a = \frac{zt - xw}{z - w} \qquad e = \frac{x - t}{w - z}$$

$$b = \frac{uz}{z - w} > 0 \qquad f = -\frac{u}{w - z} < 0$$

$$c = \frac{vz}{z - w} > 0 \qquad g = -\frac{v}{w - z} < 0$$

$$d = -\frac{yw}{z - w} > 0 \qquad h = \frac{y}{w - z} > 0$$

Thus, the model is estimated in the following form:

$$EMP = a + b\,UW + c\,UNED + d\,DESIGN \quad (3')$$
$$WAGE = e + f\,UW + g\,UNED + h\,DESIGN \quad (4')$$

In the employment equation (3′), the coefficients b, c, and d are expected to be positive. That is, apparel employment should be more important in states where undocumented workers, less educated workers, and the design function, all are more prevalent.

In the wage equation (4′), f and g should be negative, since wages should be lower in regions that have greater supplies of potential apparel workers, as measured by the population's education level and undocumented workers. The coefficient h should be positive, since a more important design function would increase the demand for workers, and hence raise wages, *ceteris paribus*.

Based on the theoretical discussion in section III, the coefficient on UWs in equation (3′), b, should be positive, since an influx of UWs in a particular region should lead to a higher level of apparel employment than would exist otherwise. If factors are not perfectly mobile, there also may be systematic differences in wages, and so a higher UW population would be associated with lower wages. Thus, the coefficient f in equation (4′) might be expected to be negative. However, if factors are quite mobile, there may be little interregional wage variation, and so equation (4′) may have little predictive power.

The Data

The empirical work focuses on the states that have apparel industries of significant size, where "significant" is defined as having more than ten thousand workers in either 1975 or 1985. Table 1 lists total employment for each of the fifty states plus the District of Columbia for these two years. The states that meet this criterion comprise the nineteen most important apparel-producing states for both years, and account for about 95 percent of total U.S. apparel employment in both 1975 and 1985.

Table 2 lists each state's measure of each variable used in the regressions, along with the variables' means and standard deviations. The data sources and precise definitions of the variables are explained below.

EMP

EMP is defined as apparel industry employment, divided by the state's total payroll employment, to control for state size. These figures are computed using data for SIC 23[10] (Apparel and Other Textile Products) from the Employment and Earnings data base for 1980.[11] These data are compiled from a survey of all employers who file

Table 1
Apparel Employment by State

State	1975	Rank 1975	1985	Rank 1985
New York	185513	1	136968	1
California	85735	3	113568	2
Pennsylvania	126045	2	99608	3
North Carolina	53682	4	61582	4
New Jersey	52687	5	44899	5
Georgia	43083	7	43736	6
Texas	50553	6	42567	7
Tennessee	40246	8	39887	8
South Carolina	35288	10	38116	9
Alabama	28736	11	37488	10
Massachusetts	38683	9	35762	11
Florida	24034	12	27285	12
Mississippi	18291	13	20055	13
Virginia	15218	15	16653	14
Illinois	15842	14	11313	15
Ohio	14482	18	11258	16
Missouri	15087	16	10455	17
Maryland	14525	17	8271	18
Connecticut	11018	19	7060	19
Kentucky	3945	22	5578	20
Washington	3411	26	4715	21
Utah	4124	21	4373	22
Arizona	3778	24	4137	23
Indiana	3929	23	3798	24
Hawaii	3281	27	3496	25
Louisiana	5027	20	2885	26
Rhode Island	2284	31	2822	27
Oregon	2477	28	2632	28
New Hampshire	1523	33	2085	29
Colorado	2299	30	1976	30
Minnesota	3600	25	1701	31
Oklahoma	2476	29	1680	32
Wisconsin	471	39	1675	33
Delaware	1608	32	1130	34
Arkansas	945	34	953	35
Kansas	310	41	560	36
Michigan	731	35	557	37
West Virginia	0	50	511	38
Maine	460	40	369	39
Nebraska	681	36	340	40
Iowa	669	37	308	41
Nevada	91	43	210	42
Idaho	0	51	77	43
Vermont	648	38	73	44
District of Columbia	112	42	71	45
New Mexico	0	46	62	46
Wyoming	16	44	28	47
Alaska	13	45	21	48
Montana	0	47	16	49
South Dakota	0	49	0	50
North Dakota	0	48	0	51
Top 19/U.S.	0.9467		0.9429	

reports with the Treasury Department. Cornelius' survey (1988b) suggests that very few employers of UWs operate completely "underground," so the vast majority would be included in the survey. Employers who do not comply with labor laws, including the minimum wage and overtime provisions, may report wage and employment levels inaccurately in order to avoid detection.[12] For example, employers may report their total wage bills correctly, but under-report the number of workers if they are violating minimum wage laws or violating overtime provisions. This would lead EMP to be underestimated in states where UWs are important, which would bias the results toward finding no significant effect of UWs on apparel employment.

WAGE

The variable *WAGENOM* is defined as nominal average hourly earnings for production workers in the apparel industry (SIC 23). The wage data also are subject to potential biases from misreporting by employers who are violating minimum wage and overtime laws. In addition, nominal wages may not be strictly comparable across states because costs of living differ and workers' productivity may differ systematically by state.

An adjusted measure, *WAGEADJ*, can be constructed by dividing the average hourly wage in apparel by the average hourly wage in all manufacturing. Since states with the highest costs of living are likely to have the highest manufacturing wages, a high ratio of apparel to manufacturing wages would imply that "real" apparel wages in that state are higher than are real wages in a state with a lower ratio of apparel to manufacturing wages.

Normalizing by manufacturing wages also may adjust for productivity differences, *if* interstate differences in apparel workers' productivity are highly correlated with interstate differences in manufacturing workers' productivity. Of course, the skills required for apparel production are quite different from those required for other types of manufacturing, and the populations of workers also are quite different. Consequently, apparel workers' skill levels may not be highly correlated with the skill levels of workers in other manufacturing industries. However, the available data do not permit a better approximation of regional differences in apparel workers' skill levels.

Table 2
Summary Statistics 1980

STATE	EMP	WAGENOM	WAGEADJ	UW	UNED
Alabama	0.040	4.234	0.652	0.007	0.250
California	0.011	4.833	0.627	0.217	0.142
Connecticut	0.008	4.575	0.646	0.006	0.163
Florida	0.010	4.294	0.718	0.044	0.176
Georgia	0.033	4.032	0.699	0.011	0.237
Illinois	0.005	4.599	0.573	0.061	0.185
Maryland	0.009	5.002	0.658	0.040	0.165
Massachusetts	0.015	4.960	0.762	0.013	0.144
Mississippi	0.049	3.927	0.722	0.010	0.270
Missouri	0.016	N.A.	N.A.	0.007	0.217
New Jersey	0.018	5.035	0.689	0.028	0.177
New York	0.023	5.313	0.740	0.069	0.183
North Carolina	0.037	4.102	0.763	0.008	0.246
Ohio	0.004	5.113	0.596	0.005	0.154
Pennsylvania	0.026	4.773	0.629	0.003	0.184
South Carolina	0.039	4.132	0.739	0.007	0.257
Tennessee	0.040	4.322	0.710	0.007	0.277
Texas	0.013	4.080	0.571	0.065	0.207
Virginia	0.016	4.119	0.663	0.033	0.216
Mean	0.022	4.525	0.675	0.034	0.203
Std. Dev.	0.014	0.427	0.059	0.048	0.042

UW

In principle, constructing the *UW* variable is straightforward. To measure the importance of UWs in the labor force, one can divide the number of UWs by the working population. Here, the working population of a given state is defined as the number of respondents to the 1980 census who listed that state as their place of work.

However, reliable data on the presence of UWs is, for obvious reasons, both scarce and based on incomplete information. For example, the 1980 Census included detailed questions about nationality, birthplace, and language use. It did not include questions specifically about residency status, although several researchers (for example, Hill and Pearce, 1987; McCarthy and Valdez, 1986; Pearce and Gunther, 1985) have argued that the number of UWs in a given locality is highly correlated with the number of aliens who speak a language other than English at home. Defining UWs in this way has obvious problems, since many legal immigrants speak their native language at home.

More sophisticated estimates of the number of undocumented aliens residing in each state were calculated by two staff members at the Census Department, Passel and Woodrow (1984). They used 1980 Census data on the total alien population and INS data on the legally resident alien population, and estimated the number of undocumented aliens residing in each state by calculating the residual and making adjustments to account for known biases in the data. *UW* is the number of undocumented residents in each state, as estimated by Passel and Woodrow, divided by the state's total working population.

Even this measure has clear limitations. For one thing, it provides estimates of the stock of UWs in the U.S. during 1980, but does not permit analysis of the changes in that stock over time.[13] A more fundamental problem is that it relies on official data regarding a segment of the population with a strong incentive to hide its existence. Nevertheless, these estimates do represent a serious attempt to construct consistent data across states, using all available information regarding the presence of undocumented aliens.

UNED

The apparel industry, which depends heavily on workers with few employment alternatives, would be expected to be more important in states with relatively uneducated populations. *UNED* is defined as the proportion of the state's population without a high school education. Presumably, a higher value of *UNED* indicates that a relatively large proportion of the state's workers have few employment options.

DESIGN

The design variable is an attempt to account for differences in the fashion content of apparel production in various states by measuring the importance of the design community to the state's apparel industry. There are two alternative specifications of the design dummy. In one, separate dummies represent New York (*DESIGNNY*) and California (*DESIGNCA*). In the alternative specification, *DESIGND* is a dummy variable which equals 1 for New York and California, and 0 for all other states.[14]

Employment Regressions

Results of employment regressions using various combinations of explanatory variables are listed in the top panel of Table 3. The *UW* variable is expected to have a positive coefficient in all of the employment regressions, but the coefficients are negative when the regressions include the design variable. Moreover, the statistical significance is higher in the regressions that have negative coefficients. Thus, the presence of undocumented workers does not explain the variation in the ratio of apparel to manufacturing employment.

Failure to confirm the hypothesis could be because the presence of UWs does not have a significant effect on the supply of labor to apparel manufacturers, or because the *UW* variable is mismeasured. Alternatively, it could be because the employment variable does not capture the importance of apparel employment very well. As discussed earlier, if firms that do not comply with minimum wage and overtime laws under-report employment to hide their activities, estimated coefficients would be less likely to show a positive relationship between UWs and apparel employment. However, the fact that the education and design variables do have the expected signs suggests that EMP provides some information about the importance of the apparel industry.

One way to get around these problems would be to run regressions using rates of change in apparel employment

Table 3
Regression Results
(absolute values of T-statistics in parentheses)

Intercept	UW	UNED	DESIGN CA	DESIGN NY	$\bar{R}^2$
Dependent Variable: EMP					
−0.036	0.005	0.283			0.715
(3.577)	(0.115)	(6.227)			
−0.034	−0.084	0.281	0.018		0.772
(3.828)	(1.586)	(6.924)	(2.245)		
−0.029	−0.215	0.271	0.049	0.018	0.818
(3.548)	(2.814)	(7.410)	(3.081)	(2.572)	
Dependent Variable: WAGENOM					
6.174	−0.833	−8.025			0.544
(15.559)	(0.528)	(4.4570)			
6.250	−5.357	−8.112	0.907		0.727
(20.318)	(2.936)	(5.827)	(3.332)		
6.241	−5.131	−8.096	0.855	0.907	0.706
(18.823)	(1.661)	(5.562)	(1.350)	(3.207)	
Dependent Variable: WAGEADJ					
0.611	−0.221	0.355			0.026
(7.591)	(0.692)	(0.972)			
0.622	−0.870	0.343	0.130		0.187
(8.431)	(1.986)	(1.026)	(1.989)		
0.636	−1.227	0.317	0.213	0.131	0.149
(8.102)	(1.680)	(0.919)	(1.423)	(1.964)	

and UWs rather than proportions of the total populations at a single point in time. However, the data on UWs exist only for the census year 1980. In principle, data on other demographic variables, such as the Hispanic population, could be used to proxy for UWs.[15] However, only four of the states included in the empirical work had data on both wages and Hispanic population for 1975, so regressions using rates of change for the UW proxy include little information. Nevertheless, it is worth noting that among those four states, between 1975 and 1985 Florida and California had faster rates of growth in Hispanic population (79 and 63 percent, respectively) and growing apparel employment (16 and 17 percent). In contrast, Illinois and New York had slower rates of growth in Hispanic population (58 and 46 percent) and shrinking apparel employment (at rates of 38 and 25 percent, respectively). Although these figures are inadequate to substantiate the claim that the presence of UWs affects the apparel industry's health, they do support the possibility that the failure to confirm that hypothesis may be due to measurement problems rather than an inadequate theory.

Wage Regressions

The results of regressions using nominal apparel wages and the ratio of apparel to manufacturing wages are listed in the lower two panels of Table 3. The coefficients on UW consistently are negative in all six regressions, as the theory predicts, although the statistical significance of the coefficients varies among the regressions. The design dummies also have the expected signs but varying levels of statistical significance.

The performance of the education variable depends crucially on the specification. In the nominal wage equations, it is negative, as expected, and highly significant. However, in the adjusted wage equations it is positive but insignificant.

Overall, the wage equations suggest that there are significant differences in real, quality-adjusted wages among regions which are related systematically to the presence of UWs. Thus, immobilities of firms and/or workers appear to be significant in preventing labor markets from reaching interregional equilibrium.

Summary of Empirical Work

The effect of UWs on the apparel industry is unclear. Although the data provide little support for the contention that the presence of UWs stimulates employment (and, presumably, production) in the apparel industry, the evidence also is not strong enough to dismiss the possibility. Nevertheless, other factors, such as the employment alternatives of the legal population (including legal aliens) and the nature of the region's apparel industry, seem to be more important factors.

The regressions do suggest that the presence of UWs may be associated with lower wages, although UWs do not appear to be the most important factor affecting apparel wages. In all of the wage regressions, the coefficient on UWs is of the expected sign and is at least marginally significant. Thus, factor immobilities appear to prevent an interregional labor market equilibrium in which wages (however measured) are equalized across states.

V. Conclusions and Implications

This paper started by asking whether the new immigration law, IRCA, would stifle growth in California's apparel industry. The analysis presented here suggests that the impact of IRCA on the industry should be modest, for two reasons.

First, it is unlikely that the sanctions the law imposes on employers of undocumented aliens will effectively reduce employment of undocumented workers. Employers can comply with the law simply by requiring workers to provide documentation of their work status. Employers are not required to verify those documents, and are specifically forbidden from discriminating on the basis of national origin or citizenship status. As a result, employers are likely to continue to provide jobs to UWs. As long as jobs exist on this side of the border, there is an incentive for illegal immigration, and UWs likely will continue to comprise an important share of the U.S. labor supply.

Second, even if IRCA does reduce the supply of undocumented workers in the United States, such a reduction probably would not have a major effect on labor markets in the apparel industry. The empirical relationship between undocumented workers and employment is inconclusive. Data problems may be partially responsible, but the contrast between the inconclusiveness of the undocumented worker results and the conclusiveness of the results regarding the education and design variables suggests that the presence of undocumented workers probably was not the most important factor determining regional employment patterns within the apparel industry. The empirical work does not address the possibility that the presence of immigrants (including documented workers) is an important determinant of apparel industry health, but previous studies (such as Waldinger 1986) suggest that this may be the case.

Since undocumented workers apparently have not been the most important cause of the observed rapid growth in California's apparel industry during recent years, even if IRCA does effectively reduce the supply of undocumented workers to California's apparel industry, California should continue to be an attractive location for U.S. apparel manufacturers. Some firms may encounter problems finding sufficient labor at prevailing wages, and some marginally profitable firms may be driven out of business. Nevertheless, California's growing role as a design center, and its large populations of Hispanic and Asian immigrants as well, suggest that California's apparel industry could survive a reduction in the number of undocumented workers available to it.

ENDNOTES

1. In 1985, 74 percent of California's apparel workers were in Los Angeles County alone.

2. Although it is commonly believed that agriculture is the most important employer of undocumented workers, Cornelius (1988b) estimates that less than 15 percent of undocumented workers currently work in agriculture. Nonagricultural industries that account for large shares of undocumented workers include food processing, hotels, and manufacturing (including apparel).

3. UWs who are found to be carrying false documents are subject to deportation, but as the experience of the past several years indicates, the threat of deportation does not deter most would-be UWs.

4. The four-firm concentration ratio, defined as the percentage of the market covered by the industry's four largest firms, is a standard measure of the concentration and, by implication, the competitiveness of an industry.

5. In 1985, these eight categories accounted for 48 percent of U.S. apparel employment.

6. By way of comparison, among all manufacturing industries, labor compensation accounts for only 41 percent of value added and 17 percent of the value of shipments.

7. Specific estimates differ, however. Whereas Cline (1987) calculated that the import penetration ratio for apparel rose from 4 percent during the 1961–65 period to 31 percent in 1986, the ILGWU (1988) calculates that it rose from 9 percent in 1967 to 58 percent in 1987.

8. If agents were motivated only by economic incentives, the initial influx of immigrants would be expected to be spread evenly among the regions. Nevertheless, available evidence overwhelmingly supports the contention that immigrants arrive in only a few regions, due to cultural, language, social, and geographic factors.

9. This market clearing simply implies that a region's wages are determined by that region's labor demand and labor supply schedules, and should not be confused with the interregional labor market *equilibrium* which implies equal real wages across regions.

10. These data include the three-digit SIC category 239, which includes nonapparel textile items such as carpets, drapes, and automobile upholstery.

11. More recent employment data are available, but 1980 data are used because 1980 is the only year for which data are available on UWs.

12. Researchers disagree about whether these problems are important. According to Maram's 1980 study of Los Angeles apparel workers, 39 percent of the UWs reported making less than the minimum wage, and 82 percent reported violations of overtime regulations. In sharp contrast, Cornelius' broader 1984 worker survey (reported in (1988b)) reveals that only 2 of 177 firms paid their workers the minimum wage, and "virtually all workers who worked overtime were compensated for it."

13. The 1980 Census was the first that was designed with the problem of undercounting minority and undocumented residents in mind. However, most observers agree that the stock of UWs has been growing more or less continuously at least for the past fifteen years.

14. A third alternative would be to construct a variable that reflects the proportion of apparel employment in nonproduction jobs. However, because there are nonproduction jobs other than design, and because the ratio of nonproduction to production jobs varies with the type of apparel produced, this variable does not reflect accurately the relative importance of the design function across states.

15. The Hispanic population obviously is a very crude proxy for the population of UWs, both because many Hispanics are in the U.S. legally and because many UWs are not Hispanic.

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