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Table of Contents

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Predicting Contemporaneous Output

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This paper presents an update of a simple model for predicting real GDP using contemporaneous monthly data. These forecasts are based on just three variables, all of which are available early in the quarter. The earlier version of this model was used at this bank for more than four years. An analysis of the real-time forecasts made over this period shows that the forecasting errors were reasonable, and that the model's forecasts compare well to the Blue Chip consensus forecasts.

It is easy to appreciate the value of an accurate reading on the current state of the economy during times of great uncertainty. During the first quarter of this year, for example, observers were trying to determine whether an economic recovery would take hold, or whether the economy would slip back into recession. Such a determination is likely to be especially important for monetary policymakers. For instance, information that the economy was contracting over the first quarter could very well have led to a further easing of policy. Yet data on broad measures of economic activity are available only with a lag. In this paper I discuss a method of obtaining estimates of contemporaneous aggregate activity using data that are available with relatively short lags.

Earlier work at this bank showed that reasonable forecasts of real gross national product (GNP) growth in the current quarter could be obtained using a set of only three variables: nonagricultural employment, industrial production, and real retail sales.[1] This paper adds to the earlier analysis in three ways. First, I update the model so that it can be used to predict real gross domestic product (GDP) instead of real GNP. After searching over a list of about a dozen or so variables, I find that the same three variables (with one small modification) still provide reasonably accurate forecasts of real GDP.

Second, I present data on the ex ante forecast accuracy of this model. The model (which I shall refer to as the Monthly Indicators model or MI model below) has been used to predict real GNP at the Federal Reserve Bank of San Francisco for about four years now. The model's (real time) forecasts over this period have been more precise than the Blue Chip consensus forecast (which is the average of the forecasts of roughly fifty leading private sector forecasters).

Last, I look at what the MI model contributes to the accuracy of real GDP forecasts over a time horizon of one to two years. I use a quarterly Bayesian vector autoregression (BVAR) model which forecasts GDP (plus some other variables) to examine this issue. I present evidence which

[1]See Trehan (1989) for a discussion.

suggests that attempts to improve the MI model's forecast of current quarter real GDP growth are unlikely to have large payoffs in terms of forecasting real GDP growth over longer horizons.

The rest of the paper is organized as follows. Section I briefly reviews earlier work on the model and then discusses the process that was used to choose the variables to predict real GDP. Sections II and III present tests of the forecasting accuracy of the model. I present both results for the variables used to predict real GDP (the indicator variables) and the results of predicting real GDP itself. Section III also contains the comparison with the Blue Chip forecasts. Section IV takes up the issue of what the BVAR contributes to real GDP forecasts beyond the one-quarter horizon, mainly to determine the likely benefits of making the current quarter forecast more precise. Section V concludes.

I. Choosing the Indicator Variables

The Original Model

When the model was first specified, three criteria were employed to choose variables that would be used to predict real GNP. The same criteria will be used this time as well. For a variable to be included in our model, the first (and most important) test it must pass is purely statistical: variables will be ranked on the basis of their usefulness in predicting real GDP. Second, in order to limit the costs of collecting and processing data, I also impose the requirement that only a relatively small number of variables be used to predict real GDP. This rules out methods that attempt to predict each (or most) component(s) of GDP in the National Income and Product Accounts (NIPA). Finally, since I am interested in obtaining current quarter real GDP forecasts as early as possible, I impose the requirement that the monthly variables that are to be included be available relatively early.

Based largely on considerations of timeliness, a set of more than a dozen variables was chosen for statistical analysis. These included different measures of interest rates, sales, labor inputs, and so on.[2] I found that reasonable forecasts could be obtained on the basis of three variables: nonagricultural employment, industrial production, and retail sales deflated by the producer price index.

The estimated equation was

$$\underset{}{RGNP_t} = \underset{(2.2)}{0.8} + \underset{(2.8)}{0.17\, IP_t} + \underset{(3.4)}{0.14\, RSALS_t} + \underset{(5.0)}{1.13\, EMP_t}$$

$$- \underset{(3.0)}{0.21\, RGNP_{t-1}} - \underset{(1.4)}{0.09\, RGNP_{t-2}} - \underset{(4.0)}{0.26\, RGNP_{t-3}},$$

adjusted $R^2 = 0.74$, SEE $= 2.17$

where
RGNP = real GNP
IP = industrial production
RSALS = real retail sales
EMP = nonfarm payroll employment,
(all variables are included as annualized growth rates)

The estimation period was 1968.Q2 to 1988.Q2. The absolute value of the *t*-statistics are shown in parentheses.

Even though purely statistical criteria were employed to select indicator variables, the final selection consists of three of the four key variables that the NBER's Business Cycle Dating Committee used to date the beginning of the current recession.[3] Further, employment and industrial production are two of the four series included in the Commerce Department's Index of Coincident Indicators. That index also includes real personal income and real manufacturing and trade sales.[4] Note that the real retail sales variable included in the MI model is similar to the latter variable and has the advantage of being available roughly one month earlier. This similarity to the coincident indicator index suggests that the model should do reasonably well at turning points. (I will return to this issue below.)

Before going further it is also worth noting that data on the variables included in the model become available relatively quickly. Specifically, data for any month are available by the middle of the following month. For example, data for January are available by mid-February.

Updating the Model

An important reason for updating the model has to do with the benchmark revision of the National Income and Product Accounts (NIPA) that was released in early December 1991. Two of the numerous changes introduced as part of that revision are particularly relevant for the purpose of forecasting GDP. First, the Bureau of Economic Analysis announced that it was shifting from the gross

[2]In addition to the three variables included in the model, the list of variables I looked at contains manufacturing shipments and inventories, housing starts, automobile sales, retail sales net of autos, total labor hours, average weekly hours, manufacturing hours, and short-term and long-term interest rates. For a detailed description of the variable selection strategy, see Trehan (1989).

[3]The fourth variable used by the NBER is real income. See Hall (1991-92) for a discussion of how the NBER dates cycles.

[4]See U.S. Department of Commerce (1984) for a discussion.

national product (GNP) to the gross domestic product (GDP) as the primary measure of production. (GNP includes net receipts of factor income from the rest of the world while GDP excludes it.) Second, the base period of the NIPA was shifted from 1982 to 1987. We need to determine whether the MI model has to be respecified because of these changes.

I did make one small change to the original specification before carrying out this analysis. The first time around, the producer price index was used to deflate retail sales instead of the more obvious consumer price index (CPI), because producer prices typically became available more than one week earlier than consumer prices. However, the gap between the release dates of the two series has narrowed over time, and thus it is now possible to employ the CPI to deflate retail sales and produce the forecasts at around the same time as when the PPI was used.

The search for the best specification was carried out in two parts. Starting with a set of 16 variables, I first isolated variables that were useful in explaining within-sample changes in real GDP. Several alternative statistical criteria were used to help determine the best set of variables.[5] At the end of this procedure I ended up with a set of variables that included the three variables in the original monthly indicators model as well as average weekly hours worked and the 10-year Treasury bond rate. In the second part of this procedure the "out-of-sample" forecasts obtained from this set of variables were compared to the out-of-sample forecasts obtained from the set of variables originally included in the MI model. (This procedure involves estimating the real GDP equation up to a given quarter and using the indicator variables to predict real GDP the following quarter. I used a sample of more than 40 forecasts to carry out this comparison.) It turns out that this larger set of variables does not provide forecasts that are noticeably different from the three variables originally included in the equation. Consequently, I decided not to alter the specification of the original monthly indicators model.

Thus, nonfarm payroll employment, industrial production and real retail sales (which is obtained by deflating nominal retail sales by the consumer price index) turn out to provide reasonably good forecasts of real GDP as well as of real GNP.

[5]These included using the "general-to-specific" strategy recommended by David Hendry (see Hendry and Mizon 1978, for example) as well as the "Final Prediction Error" criterion (see Judge, et al. 1985 for a description) to determine which variables and lag lengths were to be included. Some judgment was also involved; for instance, a variable for which a mechanical procedure included the second lag but not the contemporaneous term was dropped.

The estimated equation is

$$\underset{}{\mathrm{RGDP}_t} = \underset{(3.9)}{1.1} + \underset{(4.1)}{0.20}\,\mathrm{IP}_t + \underset{(5.0)}{0.16}\,\mathrm{RSALS}_t + \underset{(5.5)}{0.96}\,\mathrm{EMP}_t$$

$$- \underset{(3.4)}{0.20}\,\mathrm{RGDP}_{t-1} - \underset{(1.8)}{0.10}\,\mathrm{RGDP}_{t-2} - \underset{(4.7)}{0.26}\,\mathrm{RGDP}_{t-3},$$

adjusted $R^2 = 0.79$, SEE $= 1.80$.

where
RGDP = real GDP
IP = industrial production
RSALS = real retail sales
EMP = nonfarm payroll employment,
(all variables are included as annualized growth rates)

The absolute value of the t-statistics are shown in parentheses. The equation has been estimated over the period 1968.Q2 to 1991.Q2; as before, the starting date is determined by the availability of the retail sales data. The Lagrange multiplier test for first order serial correlation leads to a test statistic of 0.5, which has a marginal significance level of 50 percent. Thus, it appears that the inclusion of the lagged real GDP terms is sufficient to eliminate serial correlation.

It is worth noting that the new equation is not very different from the original one, despite definitional changes in the dependent variable (specifically, the use of real GDP instead of real GNP, as well as the change in the base year) and in one of the explanatory variables (specifically, the use of the CPI to deflate retail sales instead of the PPI).

It also is tempting to speculate about why the lagged real GDP terms are significant in the estimated equation. One reason that comes to mind is the role played by inventories. This conjecture can be verified by subtracting changes in inventories from real GDP and re-estimating the above equation in terms of final sales:

$$\mathrm{RFSAL}_t = \underset{(4.7)}{1.2} + \underset{(0.6)}{0.03}\,\mathrm{IP}_t + \underset{(10.9)}{0.30}\,\mathrm{RSALS}_t + \underset{(3.5)}{0.55}\,\mathrm{EMP}_t$$

$$- \underset{(1.7)}{0.12}\,\mathrm{RFSAL}_{t-1} - \underset{(1.0)}{0.06}\,\mathrm{RFSAL}_{t-2} - \underset{(0.1)}{0.01}\,\mathrm{RFSAL}_{t-3},$$

adjusted $R^2 = 0.76$, SEE $= 1.52$.

where
RFSAL = real final sales.

Note that lags of the dependent variable are significantly less important than in the GDP equation; in fact, the $F(3,87)$ statistic for the null hypothesis that the three lagged RFSAL terms are zero is 1.2, so that the null hypothesis cannot be rejected at any reasonable significance level. This suggests that the lagged RGDP terms in the RGDP equation are capturing the effects of inventory adjustments.

These results suggest that including inventory data may help to make the forecast more precise. Unfortunately, inventory data are released rather late to be useful in this forecast. The lag for data on nominal magnitudes is about two months, while the lag for data on the appropriate deflators is even longer.

II. Predicting the Indicator Variables

Since the forecaster (or policymaker) is likely to be interested in obtaining real GDP forecasts even before three months of information on the indicator variables becomes available, it is necessary to have a method for predicting the monthly values of the indicator variables themselves. I estimate a Bayesian vector autoregression (BVAR) to obtain these forecasts. A vector autoregression (VAR) involves regressing each of a set of variables on lagged values of all variables in the system. Estimating a BVAR implies imposing priors so that the resulting coefficients are a mixture of the coefficients that would be obtained from an unrestricted VAR and the forecaster's prior beliefs. The prior employed here has been termed the "Minnesota prior;" it imposes the belief that most economic time series behave like random walks with drift. 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 should the estimated coefficients be pushed towards this prior? Answering this question involves estimating different versions that vary in how tightly the prior is imposed. The forecasting performance of these different versions is then compared, and the specification that leads to the best forecasts is chosen.[6]

Searching for the best specification to forecast the indicator variables led to a BVAR with five variables: the three indicator variables themselves, plus the interest rate on six-month commercial paper and the average weekly hours of production workers on private, nonagricultural payrolls. Each equation contains 12 lags of each of the variables plus a constant. Since interpreting this many coefficients would be a difficult task, the estimated equations are not presented here. Instead, Table 1 shows cumulative errors from the BVAR over horizons from one

[6]See Todd (1984) for a discussion of Bayesian vector autoregressions.

Table 1
Predicting the Indicator Variables: January 1981–June 1991

Months Ahead	Mean Error	Mean Absolute Error	Root Mean Square Error	Theil's *U*-Statistic[a]
		Nonfarm Payroll Employment		
1	−.06	1.45	2.19	.73
2	−.08	1.15	1.54	.91
3	−.10	1.12	1.43	1.03
		Industrial Production		
1	.79	6.04	8.42	.78
2	.67	4.75	6.17	.82
3	.55	3.10	4.05	.78
		Real Retail Sales		
1	.83	12.54	17.74	.57
2	−.10	7.41	9.80	.58
3	−.23	5.30	7.04	.61

Note: Growth rates are annualized. The errors shown here are cumulative. For instance, the mean error three months ahead is the error in predicting the annualized growth rate between today and three months into the future.

[a]This is the ratio of the RMSE of the model forecast to the RMSE of the naive forecast of no change in growth rates.

to three months; the errors are measured as annualized growth rates.

The sample period covers slightly more than ten years, extending from January 1981 to June 1991, a total of 126 forecasts. For each forecast, the BVAR is estimated up to the prior month and then used to forecast the next three months. For example, for the first forecast the model is estimated through December 1980 and is used to generate forecasts over the January-March period. Next time around the model is estimated through January 1981 and forecasts are generated over the February-April period. Four different measures of forecast accuracy are presented in Table 1: the mean error (ME), the mean absolute error (MAE), the root mean square error (RMSE) and Theil's *U*-statistic (which compares the RMSE of the model forecast with the RMSE of the naive forecast of no change).

Note that the errors get smaller as the forecast horizon lengthens, a result consistent with the presence of substantial negative serial correlation in the monthly errors. As may be expected, the differences in the size of the errors reflect differences in volatility among the variables; for instance, the standard deviation of the month-to-month growth rates (over the 1981.M1–1991.M6 period) of the employment variable is 2.8 percent, that of industrial production is more than three times as much, and that of real retail sales is roughly seven times as much. The Theil statistics show that the model outperforms the naive forecast by a greater margin when predicting real retail sales than when predicting either industrial production or nonfarm payroll employment.

The errors from the BVAR are smaller than those obtained from univariate autoregressive equations for the same variables, although the differences are not large. Averaging across the three variables, the errors from univariate AR equations are roughly 5 percent larger than those from the BVAR at the one-month horizon and roughly 10 percent larger at the three-month horizon.

III. Predicting Real GDP

Error statistics for the real GDP forecast are shown in Table 2. The full sample period runs from 1981.Q1 to

Table 2

Real GDP Forecast Errors from Monthly Indicators Model

Month of Forecast[a]	Mean Error	Mean Absolute Error	Root Mean Square Error	Theil's *U*-Statistic[b]
		Full Sample 1981.Q1–1991.Q2 (42 forecasts)		
1	.25	2.08	2.60	.78
2	.27	1.56	1.92	.58
3	.26	1.13	1.59	.48
4	.26	1.11	1.54	.46
		Subsample 1981.Q1–1986.Q1 (21 forecasts)		
1	.52	2.39	2.86	.70
2	.60	1.77	2.17	.53
3	.70	1.45	1.88	.46
4	.75	1.44	1.89	.46
		Subsample 1986.Q2–1991.Q2 (21 forecasts)		
1	−.02	1.77	2.32	1.02
2	−.06	1.34	1.63	.71
3	−.18	0.81	1.22	.53
4	−.23	0.78	1.06	.47

Note: Growth rates are annualized.
[a]These dates refer to the month of the quarter in which the forecast becomes available. The fourth month is the month after the quarter ends. Each forecast is based on complete data for the previous month.
[b]This is the ratio of the RMSE of the model forecast to the RMSE of the naive forecast of no change in growth rates.

1991.Q2, a total of 42 forecasts. In addition, I also show the results for the two halves of the sample period, that is, for the subperiods 1981.Q1–1986.Q1 and 1986.Q2–1991.Q2. For each forecast, the GDP equation is estimated up to the previous quarter, and the resulting coefficients are used, together with the current quarter values of the indicator variables, to predict real GDP growth in that quarter.

Four different exercises were performed for each sample period to duplicate the amount 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 GDP equation to forecast GDP growth. The second assumes that we are in the second month of the quarter, when we have one month of data 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 GDP forecasts is based on two months of data for the quarter, 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 predict GDP growth.

Table 2 reveals that the monthly indicators model does not do a very good job of predicting real GDP growth when it has no information about the current quarter. Indeed, for the 1986.Q2–1991.Q2 subsample, the RMSE of the monthly indicators model is slightly larger than the RMSE of the forecast that is based on the simple rule that the rate of real GDP growth this quarter will be the same as it was last quarter (which is why the computed *U*-statistic is slightly greater than 1).

The model's forecasts become noticeably more precise in the second month of the quarter, that is, once information about the first month of the quarter becomes available. For the full sample, both the MAE and the RMSE fall by around 25 percent. The arrival of the second month of information leads to some further improvement in the forecast.[7]

In comparing the two subsamples, note that while the RMSEs of the first half (that is, the 1981.Q1–1986.Q1 period) are larger than those for the second half (the 1986.Q2–1991.Q2 period), the reverse is true for the *U*-statistics. This finding suggests that real GDP was more volatile in the first subsample than in the second. Indeed, this conjecture is confirmed by the data, which show that the standard deviation of quarterly real GDP growth fell by more than 50 percent, from 4.1 percent over the 1981.Q1–1986.Q1 period to 1.9 percent over the 1986.Q2-1991.Q2 period.

Real-time versus Final Data

It is possible that the results presented in Table 2 exaggerate the precision of the BVAR forecast, since they are based upon better data than would be available for use in forecasts made in real time. While it is not possible to overcome this problem completely, some information on the model's performance can be obtained from the real time forecasts of real *GNP* that have been made over the past four years. Specifically, we have compiled data on the original model's forecasts since the model began forecasting in 1987.Q3, which gives us a total of 16 forecasts to analyze.

The results of this analysis are shown in Table 3. To provide some sense of the model's relative performance, the table also includes data on the forecasting performance of the consensus real GNP forecast from the Blue Chip Survey. These data are taken from a newsletter titled *Blue Chip Economic Indicators* published by Capitol Publications. This well-known consensus forecast is the average of the individual forecasts of about 50 major forecasters in the private sector.

It needs to be pointed out that it is difficult to line up the two forecasts so that the two are based upon the same amount of information. The Blue Chip forecasts have been dated on the basis of the month in which they are released. For instance, the second quarter Blue Chip forecast released on the June 10 is compared to the model forecast available on June 15. Thus, the Blue Chip forecast will be based on less information than the MI forecast. Further, while the official release date of the Blue Chip survey is the 10th of the month, the survey itself is conducted over the first week of the month. Of the three indicator variables used in the real GDP equation, the only variable likely to be available at that time is payroll employment.[8]

One way to overcome this problem is to compare the model forecast in a given row with the Blue Chip forecast in the following row. Note that such a comparison will tend to overcompensate in those months when employment data for the previous month are released before the survey is

[7]In the original version of the model the arrival of the second month of information did not lead to a reduction in the model's forecast errors. This is reflected in the results of real time forecasting shown in Table 3. The reasons behind this change are not obvious, although experimentation suggests that the change in results has to do with the change in base years and not the change from GNP to GDP.

[8]Forecasters will also know interest rates and labor hours.

Table 3

Comparison of Real GNP Forecast Errors, 1987.Q3–1991.Q2

	Monthly Indicators Model Forecasts				Blue Chip Forecasts			
Month of Forecast[a]	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
				Using Real-Time GNP				
2	0.11	0.71	0.90	0.60	0.42	1.13	1.49	0.99
3	0.21	0.86	1.09	0.72	0.36	0.99	1.30	0.86
4	0.14	0.79	1.02	0.68	0.34	0.90	1.14	0.76
				Using Revised GNP				
2	0.14	1.01	1.34	0.86	0.46	1.37	1.99	1.28
3	0.24	1.06	1.48	0.95	0.39	1.26	1.83	1.17
4	0.18	1.05	1.38	0.88	0.38	1.19	1.67	1.07

Note: Growth rates are annualized.

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

conducted. (Employment data for a particular month are usually released on the first Friday of the following month.)

The top half of the table compares both sets of forecasts with "early" GNP data. These early GNP data have been obtained from the Commerce Department's *Survey of Current Business* four months after the end of the quarter. The idea is to reproduce, as closely as possible, the GNP data as it existed when the forecasts were made. The results for the monthly indicators model show that the MAEs average around 0.8 percent, regardless of whether we have one, two, or three months of data on hand. Similarly, the RMSEs are around 1.0 percent.

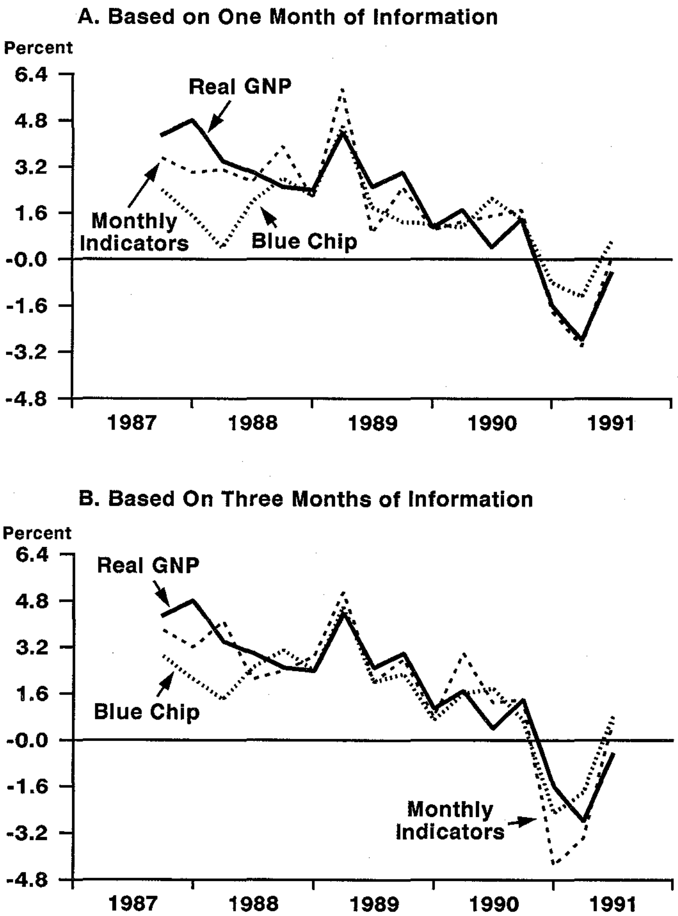
The results for the Blue Chip consensus forecast show that the MAE varies around 1 percent depending upon the amount of information available, while the RMSE falls from around 1.5 percent for the forecast made in month 2 of the quarter to approximately 1.1 percent for the forecast made in the month after the quarter has ended. While these errors are not that much larger than those of the monthly indicators model, it is worth pointing out that the MI forecasts made in the second month of the quarter (that is, forecasts that are based on one month of information) are more accurate than the Blue Chip consensus forecast made after the quarter has ended (month 4). The Theil statistics show that both sets of forecasts do better than the naive forecast of no change in growth rates.

The second half of the table compares the two forecasts to revised real GNP data. Specifically, the two forecasts are compared to real GNP data as of the fourth quarter of 1991. Note that this increases the forecast errors of both models; the deterioration is especially noticeable in the case of the Blue Chip forecast since it does worse than the simple prediction that real GNP growth this quarter will be the same as it was last quarter.

Chart 1 plots the MI and Blue Chip forecasts as well as early GNP data over this period. The top panel of the chart shows forecasts based on one month of information, while the lower panel shows forecasts based on three months of information. Note that the MI forecast tracks the recession quite well, a result that is not surprising since the forecasts are based on information about the current quarter. Recall also that the set of indicator variables is close to the set of variables included in the Index of Coincident Indicators. Finally, as the results in Table 3 would suggest, while the MI forecasts are more accurate on average than the Blue Chip forecasts, this is not always the case.

Before going further it needs to be pointed out that the Blue Chip consensus forecast has been used only as a benchmark (since it is widely available), and not because it is taken to be the most accurate forecast of real activity in the current quarter. In fact, it is not unreasonable to believe that the forecasters included in the panel were trying to minimize their forecast errors over a time span of a year or so instead of a quarter. In that context, it is useful to ask

Chart 1
Real Time Forecasts of Real GNP Growth



Note: Real GNP data have been taken from the Commerce Department's *Survey of Current Business* four months following the end of each quarter.

what the monthly indicators model contributes to the accuracy of real GDP forecasts beyond the current quarter. We examine this question in the next section.

IV. Evaluating the Usefulness of the Current Quarter GDP Forecast

Usually the forecaster (or policymaker) is interested not just in the forecast of real GDP growth this quarter, but in growth over some longer time period, such as a year or two. It is, therefore, natural to ask what the monthly indicator model's forecast contributes to predicting real GDP over somewhat longer horizons. Perhaps a more important issue for the project at hand concerns the payoff to making the MI forecast more precise. As discussed above, the MI model is a simple one; adding greater detail could improve its accuracy somewhat, especially late in the quarter when more information becomes available. However, greater detail also implies greater cost. Thus, we need to compare the benefits to greater accuracy with the costs of putting together and maintaining a more detailed model.

In the present context (where we are interested in looking at contributions to forecast accuracy over horizons of one to two years), a measure of the benefits can be obtained by examining how the accuracy of real GDP forecasts over one to two years is affected as we increase the accuracy of the current quarter forecast. Here I will make an extreme assumption about how much more accurate the current quarter forecast can be: I will assume that real GDP this quarter is known with certainty.

Forecasts over a two-year horizon will be generated using a BVAR model that is similar to one used for forecasting at the Federal Reserve Bank of San Francisco. This model is estimated on quarterly data (and I will refer to it as the quarterly BVAR). It contains a total of ten variables, including real GDP, consumption, unemployment, the dollar, a measure of money, measures of short and long-term interest rates, and inflation.

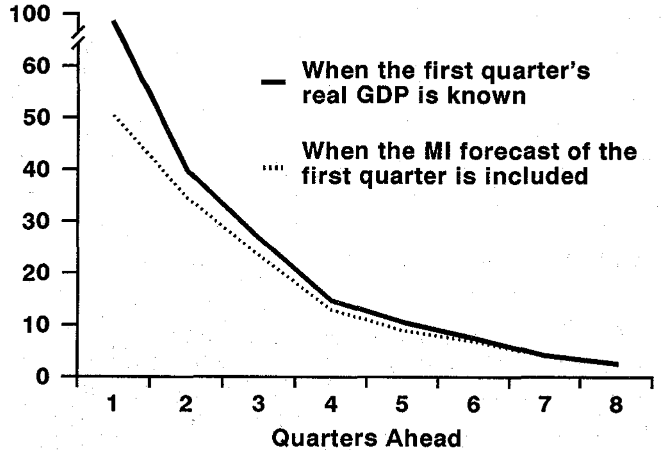
Chart 2 plots the percentage reduction in the RMSE of the GDP forecast from the quarterly BVAR when the MI forecast for the first period is included or when the actual value of GDP for the first quarter is included.[9] I show forecasts for an eight-quarter horizon over the 1981.Q1–1991.Q2 period. The errors are cumulative; that is, the RMSE of the four-quarter ahead forecast measures the errors in predicting the level of real GDP four quarters in the future.

Including the MI forecast reduces the RMSE of the one-quarter ahead forecast by about 50 percent and the two-quarter ahead forecast by 35 percent (compared to the case when the MI forecast is not included). The degree of improvement becomes smaller as the forecasting horizon lengthens, falling to less than 15 percent after four quarters and to less than 5 percent in the seventh and eighth quarters.

The degree of improvement we obtain is, of course, dependent upon the model that is being used to forecast real output over the next two years. However, the question of whether the returns to making the MI forecast more precise are worth the effort can be answered in a way that is less model-dependent. We begin by looking at how much the forecast from the quarterly BVAR can be improved

[9]The first quarter here is actually the quarter for which we already have data for the indicator variables. This was termed the contemporaneous quarter in Sections I-III. The change in terminology is necessitated by the introduction of the quarterly BVAR, which contains no contemporaneous information. Note also that the MI forecasts used here are based on three months of information.

Chart 2
Percentage Reduction in the
RMSE of the GDP Forecast



when next quarter's real GDP is assumed to be known, that is, assuming perfect information.

Perfect information implies that the first quarter value of this number is 100 by assumption. More interestingly, knowledge of the first quarter's real GDP reduces the RMSE of the two-quarter ahead forecast by 40 percent and the RMSE of the four-quarter ahead forecast by about 15 percent.

As before, the precise effects of including information about next quarter on the one-year ahead forecast are likely to depend upon the model that is being used, since models differ in their ability to process information about the next—or any other—quarter's GDP. Nevertheless, it is possible to compare the marginal benefit of moving from the no information case to the case where the MI forecast is known to the marginal benefit of moving from knowledge of the MI forecast to knowledge of next quarter's real GDP. (Recall that this is a theoretical upper bound to further improvements in the MI forecast.) Chart 2 provides a simple way of making the comparison. At each point in time, the marginal benefit of moving from the no information case to the case where MI is known is given by the vertical distance between the horizontal axis and the MI line; the marginal benefit of moving from knowledge of MI to perfect information is measured by the vertical distance between the two curves. The greater the difference between the two curves relative to the height of the MI curve, the greater the advantage to improving upon the MI forecast.

The chart indicates that at a two-quarter horizon, the relative improvement in going from the no information case to including the MI model forecast is substantially greater than the relative improvement in going from the MI model to the perfect information case. This continues to be the case at all forecast horizons; in fact, the difference between the two curves is essentially zero from the fourth quarter on. Of course, both curves are close to zero towards the end of the forecast horizon and the difference between them at that point is not very significant.

This exercise suggests that further attempts at improving the current quarter forecast of real GDP are not likely to have substantial rewards in terms of improving our ability to forecast real GDP over somewhat longer horizons. In other words, if the objective is to forecast real GDP beyond the first two quarters, then the simple MI model reaps a large proportion of the gains that would accrue in going from the case of no information about the first quarter's real GDP to the case where the first quarter's real GDP is known with certainty, and does so at relatively little cost.

V. *Summary and Conclusions*

This paper has reviewed a simple method of predicting real GDP. This method requires relatively few resources; the forecasts are cheap to produce and update. The evidence presented above demonstrates that these forecasts compare well to those obtained from major private sector forecasters.

It is possible that the forecast of current quarter real GDP growth could be made more precise by devoting additional resources to the task. However, the evidence presented above also suggests that, if the objective is to forecast real GDP beyond the current quarter, then such an endeavor is likely to lead to relatively limited returns.

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Does California Drive the West? An Econometric Investigation of Regional Spillovers

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This paper measures linkages between the California economy and its neighbors, and the extent to which economic shocks to California spill over to its neighbor states, through vector autoregression techniques. Leading and lagging relationships between California and other western states are identified through Granger causality tests. Then, under certain identifying assumptions, the economic importance of these relationships is measured. Finally, the sources of the linkages are then considered by examining the effect of California on specific sectors within a state. In general, the results suggest that the California economy does have important spillover effects on other western states—particularly those in close geographic proximity to it.

In terms of population, output, and diversity, California dwarfs its neighbors in the Twelfth Federal Reserve District —which includes Alaska, Arizona, California, Hawaii, Idaho, Nevada, Oregon, Utah, and Washington. In July 1990, the 12.9 million jobs in California accounted for almost two-thirds (63 percent) of total employment in the District. For comparison, it had five times as much employment as the next largest District state, Washington, which has 2.2 million jobs.

This paper examines the extent to which the California economy drives the western region. In particular, it attempts to measure linkages between the California economy and its neighbors, and the extent to which economic shocks to California spill over to its neighbor states.

The topic is relevant to the most recent recession, which hit California and the nation in mid-1990. Most District states, however, were not affected until much later, with employment declines becoming evident only in early 1991. To the extent that systematic spillovers from California occur with a lag of two to three quarters, this pattern of regional recession would not be surprising. Accounting for these spillovers would yield better forecasts of economic developments in western states.

A more general motivation is that information on linkages and spillovers between states adds to the understanding of how regions operate and when regional analysis is appropriate. A model of regional linkages due to trade flows, for example, results in different predictions from a model of linkages due to factor flows. Positive shocks that increase economic activity in one state may stimulate trade with other states, inducing positive spillovers. If the increased economic activity induces labor to migrate, however, a negative effect on neighbor states might result. Furthermore, if regional economies are relatively open and driven by national shocks, a broad macroeconomic perspective might be appropriate for monetary or fiscal policy analysis. If regional economies are closed to spillovers from the nation or other states, however, a region-by-region approach to policy analysis might be called for. Finally, if particular sectors (such as housing or finance) are shown to be more closed than others, policies targeted toward those sectors can be implemented on a regional rather than national basis.

This paper measures linkages through vector autoregression (VAR) techniques. Employment growth rates (used as a proxy for growth in economic output) in Twelfth District states are estimated as a function of lagged growth in own employment, lagged growth in California employment, and lagged growth in national employment. The goal is to explore the extent to which economic fluctuations in a state are driven by the state's own economy or by linkages to California or national markets.

Leading and lagging relationships between California and other western states are tested through Granger causality tests. A standard decomposition of the forecast error variance then measures the economic importance of these relationships. The sources of the linkages are then explored through examining the effect of California on specific sectors within a state.

In general, the results suggest that the California economy has important spillover effects on its neighboring states in the Twelfth District, namely, Arizona, Nevada, Oregon, Utah, and Washington, but not on Alaska, Hawaii, and Idaho. In the reverse direction, Granger causality tests suggest that only Arizona has significant spillover effects on California.

The variance decomposition results indicate that the measured spillovers from California to its neighbors is relatively large and statistically significant through three quarters. The state with the largest measured linkages is Arizona, followed by Nevada, Oregon, Washington, and Utah.

The sectoral breakdowns suggest varied sources of linkages. Shocks to California affect manufacturing in Arizona, Oregon, and Utah, while the service sectors appear to respond in Arizona, Nevada, Oregon, and Utah.[1] No spillovers are observed in finance. The observed spillovers in manufacturing are consistent with a model of linkages propagated through trade flows of manufactured products between firms, while spillovers in the service sector suggest that trade flows also exist in nonmanufacturing sectors—possibly tourism and recreation.

In sum, the results indicate that shocks to California influence its neighbor states, and suggest the magnitude of spillovers that can be expected given this historical relationship. The estimates should be treated with caution, however. In particular, the VAR modeling approach does not capture structural change or adequately measure factor flows. Moreover, it may not control adequately for shocks common to western states (perhaps due to common industries). The spillovers identified in this paper, however, indicate that these problems merit further research.

This paper is organized as follows. Section I reviews the theory of linkages between regions and considers the strengths and weaknesses of using VARs to model them. Section II presents the basic results. Section III explores which sectors are most affected by spillovers. Section IV concludes and considers areas for future research.

I. Modeling Regional Linkages with VARs

While linkages among states may exist for several reasons, this paper is concerned with measuring spillovers of economic shocks to the California economy to its neighbor states. As such our focus is on linkages that are principally economic in nature: flows of goods (trade) and factors of production.[2] What then is the nature of the economic shocks, and how are they transmitted through these linkages?

Positive economic shocks to California could come from the demand side (for example, due to jumps in national demand for California products like computers, entertainment, aerospace), or from the supply side (for example, from technological innovations that enhance productivity or result in new products). Negative shocks, of course, also have occurred and are of current concern. Falling national demand for California defense products is reducing manufacturing activity. Recent natural supply shocks include the 1989 Loma Prieta earthquake, freezes, and drought. Supply constraints induced by environmental problems, inadequate infrastructure, or regulatory burdens also may become binding.

Trade flows of goods and services between regions are an obvious mechanism for transmission of economic shocks from California to its neighbors. Increases in economic activity in California heighten the demand for imports of raw materials, intermediate inputs, and final products from other states. Raw materials could include minerals, electricity, or water. Intermediate inputs could range from lumber and wood products for housing, to electronic components for defense and aerospace. Final products could include the whole range of consumer goods. Economic growth in California also can affect the consumption of services in other states, including entertainment (skiing in Utah or casinos in Nevada).

[1]While Washington exhibits a significant overall linkage, no one sector is significantly affected.

[2]Linkages other than trade or factor flows also may exist. First, multiregional government institutions (such as Federal Reserve Districts) or multiregional firms may exist. Second, information flows may give rise to differential adaption rates of innovations across regions. Third, physical flows of pollutants such as acid rain across regional boundaries could occur.

The transmission of shocks through trade should occur relatively quickly, as California factories place orders for goods, or as consumers plan vacations. If the shocks are measured as changes in growth rates from trend, however, they should be short-run in nature. A jump in demand from California would permanently raise the *level* of economic activity in a neighbor state, but the period of higher growth would be of relatively short duration.

In general, if positive (negative) economic shocks to California spill over to other states through trade flows, they should have a positive (negative) short-run effect on growth in the state that dampens down relatively quickly. Furthermore, since transportation costs increase with distance, I expect more trade to be conducted between California and states in close geographic proximity. As such, states contiguous to California should be subject to greater spillover effects than those at greater distance.

If the linkages between states are through factor flows as well as trade, the expected spillover effects of shocks to California become less clear. Positive shocks to California that raise the demand for labor might attract workers from other states, leading to a negative effect on economic activity as the population and labor emigrates. Alternatively, a positive shock that raises demand for California products might lead firms to consider moving production facilities to other states if supply constraints in infrastructure (or environment) become binding. Negative shocks to productivity also could lead firms to relocate. Much attention is currently being given to California firms relocating production facilities to other western states due to regulatory burdens and other perceived costs of operating in California.

If the predominant mechanism for regional linkages is factor flows, then I have no clear prediction of how shocks to California will affect neighbor states. Spillovers propagated through factor flows, however, will likely occur over a longer time horizon than those propagated through trade flows. (Relocating a firm takes longer than placing orders.)

A further problem, however, is that spillovers involving factor flows entail long-run structural change in regional economies that will result in changed trade flows. The VAR model assumes that structural patterns are fixed and cannot distinguish between long-run and short-run influences in the data. This limits our ability to distinguish between trade and factor flows.

A final note is that trade and factor flows should be reciprocal. The relative size of the California economy to its neighbors, however, suggests that the neighbors' effect on California growth will be smaller than California's effects on its neighbors. Though theory predicts that a relationship exists, in practice it may be difficult to pick up a small effect in noisy data.

The VAR Approach

This paper uses a VAR approach to model linkages between states in the Twelfth District. The advantages of this method include its parsimonious use of data, allowance for top-down effects from the nation to the region, allowance for feedbacks (with a lag) from the region to the nation, and identification of leading and lagging relationships between pairs of states. The drawbacks include the lack of an explicit structural model to explore the mechanism of linkages and the need for untestable identifying restrictions to measure the economic importance of spillovers.

A vector autoregression is a relatively simple modeling approach that has become widely used by economists to gather evidence on business cycle dynamics. Typically, these models focus on a limited number of random variables at the national level, such as money, interest rates, prices, and output. Each variable is expressed as a linear function of past values of itself, past values of the other variables, and nonrandom constant terms and time trends. After estimating the model (equation by equation with ordinary least squares) the results can be used to identify leading and lagging relationships between variables and, with further identifying restrictions, to measure the economic importance of these dynamic relationships.

The identification of leading and lagging relationships is accomplished through causality tests. For example, if there are two time series m and y, the series y fails to Granger-cause m according to the Granger (1969) test if, in a regression of m on lagged m and lagged y, the latter (lagged y) takes on a zero coefficient. If y fails to Granger-cause m, that m is said to be exogenous with respect to y. Furthermore, if in addition m does Granger-cause y, m is said to be causally prior to y.[3]

While statistical leading and lagging relationships can be identified through Granger tests, measuring the economic importance of these relationships requires further identifying restrictions. The standard approach developed by Sims (1980a,b) uses the estimated VAR results to measure the dynamic interactions among variables in two different ways. First, from a moving average representation of a VAR model, each variable can be written as a function of the errors. A tabulation of the response of the ith variable to an innovation in the jth variable is called an impulse response function and shows how one variable

[3]See Cooley and Leroy (1985). In another approach presented by Sims (1972), y fails to Granger-cause m if in a regression of y on lagged y and future m, the latter takes on a zero coefficient. Jacobs, Leamer, and Ward (1979) show that the Granger and Sims tests are implications of the same null hypothesis.

responds over time to a single surprise increase in itself or another variable. Second, a forecast error variance decomposition (or innovation accounting) can be used to analyze the errors the model would make if used to forecast. It determines the proportion of each variable's forecast error that is attributable to each of the orthogonalized innovations in the VAR model.

Identification of a VAR system is achieved by assuming a recursive chain of causality among the surprises in any given period. This identification (or ordering of equations), however, is justified only under a predeterminedness assumption. If y_t is predetermined with respect to m_t, the conditional correlation between y_t and m_t is attributed to the contemporaneous effect of y_t on m_t; the contemporaneous effect of m_t on y_t is restricted to zero. This assumption, however, is untestable in the absence of prior restrictions derived from theory. In particular, since Granger noncausality (which tests for the effect of lagged as opposed to contemporaneous variables) is neither necessary nor sufficient for predeterminedness, predeterminedness is not tested by the Granger or Sims tests.[4]

Identifying Assumptions for Regional Modeling

Previous research using VARs to measure national-regional linkages by Sherwood-Call (1988) and Cargill and Morus (1988) has used the identifying assumption that growth in the (large) national economy is predetermined with respect to any particular (small) state. The observed contemporaneous correlation of errors stems from the national economy affecting the region, and not vice versa.[5]

To achieve identification between California and its neighbors, I extend this assumption as follows: The national economy is predetermined with respect to states, and the large California economy is predetermined with respect to its smaller neighbors. (The orders of magnitude involved are displayed in Table 1 which shows payroll employment figures for the nine states in the Twelfth District in July 1990, the most recent business cycle peak.) Any observed contemporaneous correlation of shocks between California and its neighbors is due to California affecting the neighbors, rather than vice versa.

Table 1

Twelfth District State Payroll Employment, July 1990

State	Payroll Employment (thousands)	As a Percent of California	As a Percent of U.S.
Alaska	239	1.9	0.2
Arizona	1,486	11.6	1.3
California	12,861	100.0	11.7
Hawaii	529	4.1	0.5
Idaho	384	3.0	0.3
Nevada	625	4.9	0.6
Oregon	1,255	9.8	1.1
Utah	725	5.6	0.7
Washington	2,157	16.8	2.0
U.S.	110,078		100.0

An alternative explanation and potentially serious objection, however, would be that the correlation of the errors represents some joint regional shock common to both California and its neighbors. For example, if California and Nevada both rely heavily on the same industry (perhaps tourism), an industry-specific shock could cause the observed error pattern. Exploring such possibilities is beyond the scope of this paper and is left for future research.

A final cautionary note to the VAR analysis is the extent to which results are robust. A criticism of the Sims analysis of monetary intervention, for example, is that the results often changed for seemingly arbitrary redefinitions of variables, time periods, and periods of observations. In this analysis I test the robustness of the results for different time periods, but because of data limitations, I cannot test for the robustness of the results across different measures of economic activity.[6]

[4]See Cooley and Leroy (1985) for a detailed review of the applications and pitfalls of vector autoregression.

[5]Sherwood-Call (1988) uses the portion of the forecast error for an individual state attributable to national innovations as her measure of linkage between the nation and state. Among Twelfth District states, she found California to be most linked to the national economy. In modeling the Nevada economy, Cargill and Morus (1988) also assume that the nation is predetermined with respect to Nevada. Furthermore, they recognize the proximity and interrelatedness of the California and Nevada economies and include California civilian employment in the system of VAR equations. VARs also have been used to generate regional forecasts, as with the VAR model of Ninth District states run by the Federal Reserve Bank of Minneapolis (Todd 1984).

[6]See Todd (1990) and Spencer (1989).

II. Model and Estimation

I examine the linkages between California and its neighbor states using a three-equation VAR model with employment growth rates for the nation (NATEMP), California (CALEMP), and neighboring states (STEMP) as the random variables. Several specifications are tested. First, I include all Twelfth District states (except California) in STEMP. Second, I include only states contiguous to California (Oregon, Nevada, and Arizona) in STEMP to examine the importance of geographic proximity. Finally, I estimate eight separate VARs (one each for the Twelfth District states other than California) to examine state-by-state spillovers from California. In all specifications, NATEMP excludes CALEMP and STEMP, and employment growth rates are taken from trend by including a constant term in the regression.

Economic activity is measured with quarterly payroll employment data. This variable is chosen as a proxy of economic activity for several reasons. First, it is measured consistently over time and across states from state-level payroll records. Second, other state-level variables (such as personal income) are in part derived from the payroll employment data. Some alternative measures of state-level economic activity (such as state gross product) are not considered reliable at present. Third, employment data are broken into sectors, allowing for the examination of the source of spillovers between states. Finally, employment fluctuations should adequately capture relative output fluctuations between states over time if relative capital-labor ratios across states change little over time.

The estimation period is from 1947.Q1 to 1991.Q4 (except for Alaska and Hawaii). To test for robustness I also break the sample period into two segments.

The basic form of the VAR is shown in equations (1) through (3). The growth rate (in log difference form signified by a dot) of each variable is estimated as a function of 6 lags of itself and the other two variables using ordinary least squares.[7]

$$
\begin{aligned}
(1)\quad \dot{\text{NATEMP}}_t = a_1 &+ \sum_{i=1}^{6} \beta_1 \dot{\text{NATEMP}}_{t-i} \\
&+ \sum_{i=1}^{6} \beta_2 \dot{\text{CALEMP}}_{t-i} + \sum_{i=1}^{6} \beta_3 \dot{\text{STEMP}}_{t-i} + e_{nt}
\end{aligned}
$$

$$
\begin{aligned}
(2)\quad \dot{\text{CALEMP}}_t = a_2 &+ \sum_{i=1}^{6} \beta_4 \dot{\text{NATEMP}}_{t-i} \\
&+ \sum_{i=1}^{6} \beta_5 \dot{\text{CALEMP}}_{t-i} + \sum_{i=1}^{6} \beta_6 \dot{\text{STEMP}}_{t-i} + e_{ct}
\end{aligned}
$$

$$
\begin{aligned}
(3)\quad \dot{\text{STEMP}}_t = a_3 &+ \sum_{i=1}^{6} \beta_7 \dot{\text{NATEMP}}_{t-i} \\
&+ \sum_{i=1}^{6} \beta_8 \dot{\text{CALEMP}}_{t-i} + \sum_{i=1}^{6} \beta_9 \dot{\text{STEMP}}_{t-i} + e_{st}
\end{aligned}
$$

The estimated coefficients and standard errors of the individual coefficients are numerous and difficult to interpret. Following standard procedure I instead report summary statistics from the Granger tests, forecast error variance decomposition, and impulse response analysis.

First, I consider whether California has a Granger causal effect on its neighbor states. Granger causation is tested through an F test of the joint significance of the lagged STEMP variables in the CALEMP equation. An F statistic greater than the critical value of 2.10 results in rejection of the null hypothesis of non-Granger causation. Results of these tests are shown in the first column of Table 2.

When the other Twelfth District states are aggregated together into STEMP, California does not appear to have a leading predictive relation. The F statistic for non-Granger causation is 1.09, which is below the critical value of 2.10. When only contiguous states are included in STEMP, however, the F statistic is 3.55, suggesting that developments in California do have predictive power. Likewise, when individual states are examined, shocks to California appear to have predictive power for Arizona, Nevada, Oregon, Utah, and Washington, but not for Alaska, Hawaii, and Idaho.

Second, I consider the reverse relationship, that is, whether growth in neighboring states has a Granger causal effect on California. The results (Table 2, second column) show that, except for Arizona, the null hypothesis of non-Granger causation is not rejected for all states when tested either individually or together. Since this reverse effect is not significantly different from zero, the results show that California is causally prior to Nevada, Oregon, Washington, and Utah, and to the contiguous states when aggregated. In other words, changes in California employment growth have a predictive power for employment growth in these neighboring states. California and Arizona appear to be jointly determined, with employment growth in each state having predictive power for the other.

While the tests identify a statistical leading effect of California on its neighbors, measuring the magnitude (or economic importance) of these dynamics requires identifying assumptions regarding the causal ordering of the contemporaneous errors. As discussed in the previous

[7]The choice of lag length is somewhat arbitrary. A lag of over one year was desired to accommodate seasonal fluctuations. Alternative lag lengths yield qualitatively similar short-run effects, though different long-run dynamics. As I am interested in short-run spillovers, a relatively short lag length is chosen. Long-run dynamics, of course, may be biasing our short-run estimates.

Table 2

Results of Granger Causality Tests

State	California "Granger-Causes" State	State "Granger-Causes" California
Other 12th District States	No 1.09	No 0.51
Contiguous States (OR, NV, AZ)	Yes 3.55	No 1.24
Alaska	No 1.87	No 0.50
Arizona	Yes 5.02	Yes 2.69
Hawaii	No 0.44	No 0.91
Idaho	No 1.63	No 0.82
Nevada	Yes 3.10	No 1.32
Oregon	Yes 3.75	No 1.44
Utah	Yes 2.81	No 2.00
Washington	Yes 3.47	No 1.08

Note: *F* test statistic of null hypothesis of non-Granger causality. The critical value for rejecting the null hypothesis is 2.10.

Table 3

Contemporaneous Correlation and Variance Decomposition

State	Contemp. Correlation (%)	Variance Decomposition (%) California	Nation	California (Reverse Order)
All Other 12th District States	0.45	17.1	21.0	5.4
Contiguous States	0.65	32.3	30.9	11.5
Arizona	0.39	28.3	16.1	17.8
Nevada	0.46	27.5	10.5	11.0
Oregon	0.60	25.8	24.6	17.5
Washington	0.48	24.9	27.6	16.2
Utah	0.33	21.0	25.9	18.9
Idaho	0.40	17.7	18.4	16.9
Alaska	0.20	9.1	7.0	7.8
Hawaii	0.14	3.0	25.2	2.9

Note: Percent of forecast error variance attributable to California after 24 quarters

section, the causal ordering I assume is that contemporaneous shocks flow from the nation to California and its neighbors, and from California to the neighbor states.

The first column of Table 3 reports the contemporaneous correlation of errors between California and its neighbors from the estimated covariance matrices. The correlation between California and all other District states is 0.45. For contiguous states the correlation is 0.65. For individual states, the correlation ranges from 0.60 in Oregon to 0.14 in Hawaii. In general these correlations are large, and point out the importance of the identifying assumption. The contemporaneous shocks are assumed to be due to the impact of California on its neighbors. If the reverse is true, or if some unobserved common factor is affecting both states, the VAR results will be inconsistent.

Subject to this identifying assumption, the forecast error the model makes for a neighbor state can be decomposed into the error due to the state's own lags, the error due to the nation, and the error due to California. I use this variance decomposition as a measure of how states are linked to California. Column 2 in Table 3 reports the proportion of the forecast error at 24 quarters attributable to California. For all other Twelfth District states, 17.1 percent of the forecast error variance is attributable to California. In contrast, the linkage to the nation is 21.0 percent. For contiguous states, however, the proportion of the forecast error attributable to California rises to 32.3 percent (30.9 percent for the nation).

Among individual states, Arizona exhibits the largest degree of linkage: 28.3 percent of the error the model would make in forecasting Arizona is attributable to errors (innovations) in the California equation. Arizona is followed closely by Nevada (27.5 percent), then Oregon, Washington, Utah, (all in the 21 to 26 percent range), then by Idaho, Alaska, and Hawaii, which exhibit relatively little linkage to California.

The sensitivity of these results to the predeterminedness assumption is tested by reversing the ordering of the equations, that is, assuming that the neighbor states are predetermined with respect to California. These results are shown in the final column of Table 3. When the states are aggregated, reversing the ordering reduces the measured linkage by over half. For all Twelfth District states it falls from 17.1 to 5.4, and for contiguous states it falls from 32.3 to 11.5. The results for the aggregate measures of neighboring states thus are very sensitive to the ordering assumption. For individual states, however, changing the ordering assumption has less of an effect. Arizona's linkage falls from 28.3 to 17.8, Oregon from 25.8 to 17.5, and Washington from 24.9 to 16.2. Utah and Idaho change relatively little. Nevada, however, drops more than half (from 27.5 to 11.0). The sensitivity of the results points out the importance of the contemporaneous correlations in measuring spillovers.

An alternative measure of the effects of California on its neighbors is obtained through impulse response analysis. The effects of a one-standard-deviation shock to California on neighboring states over 24 quarters is graphically shown in Charts 1 through 3.

For all Twelfth District states (shown in Chart 1) a one-standard-deviation shock to quarterly employment growth in California of 0.0043 (in log difference form, or approximately 0.43 percent) results in a 0.29 percent higher growth rate in the rest of the District in the first quarter. The response goes away by quarter 5. (It slightly overshoots, then dampens to zero by quarter 18.) For contiguous states (shown in Chart 2) the response to a shock to California is

Chart 1
Response of All Twelfth District States and California to One-Standard-Deviation Shock to California

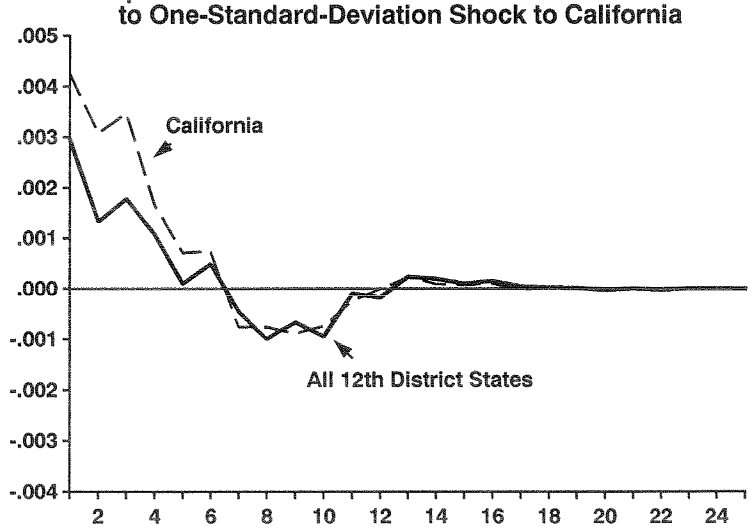
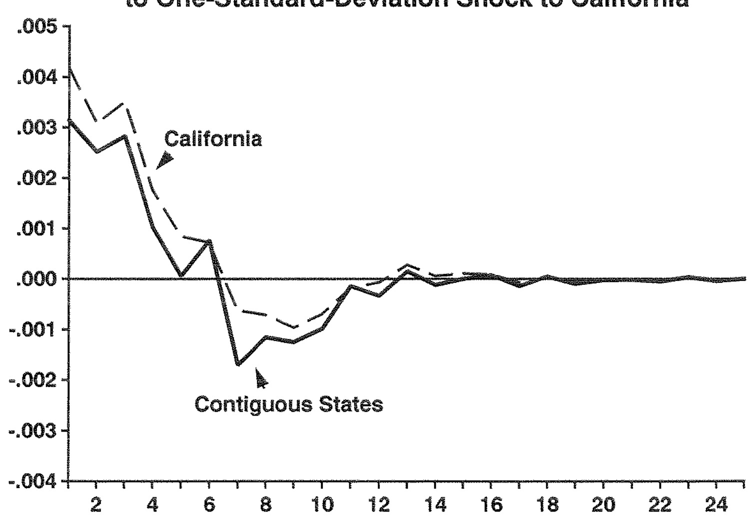


Chart 2
Response of Contiguous States and California to One-Standard-Deviation Shock to California



larger. The response rises to 0.31 percent in the first quarter, and remains above the response for the all-Twelfth-District aggregate until quarter 4. This suggests that the magnitude of spillovers from California is larger for contiguous states.

Responses for individual states are shown in Chart 3. These results also suggest that spillovers are larger in states that are geographically closer to California. The largest peak responses are seen in Oregon (0.38 percent in quarter 1) and in Arizona and Nevada (both at 0.35 percent in quarter 3). In contrast, smaller responses are seen in Washington and Utah. (Idaho, Alaska, and Hawaii exhibit small responses but are not shown for clarity of exposition.) Nevada shows the largest sustained spillover (remaining positive through quarter 6), while Oregon's is of short duration, reaching zero by quarter 4. As with the aggregate measures, the responses in the individual states slightly overshoot, then dampen to zero by quarter 18.

Are these spillovers statistically significant? Charts 4 and 5 report the impulse responses for the all-District and contiguous states, respectively, with 95 percent confidence bounds calculated through a Monte Carlo simulation. The confidence bound for the all-District response is greater than zero in quarter 1, touches zero in quarter 2, is just above zero in quarter 3, then contains zero from quarter 4 on, suggesting that the measured spillover is not significantly different from zero beyond three quarters. The results for contiguous states, however, suggest that the impulse is estimated more precisely. The confidence bound is well above zero through three quarters, then as with the all-District response, contains zero from quarter 4 on. Results

Chart 3
Response of Neighbor States to One-Standard-Deviation Shock to California

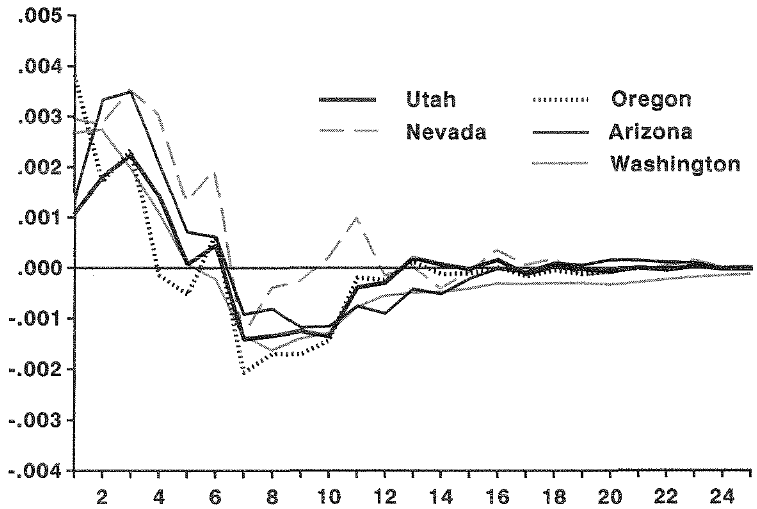
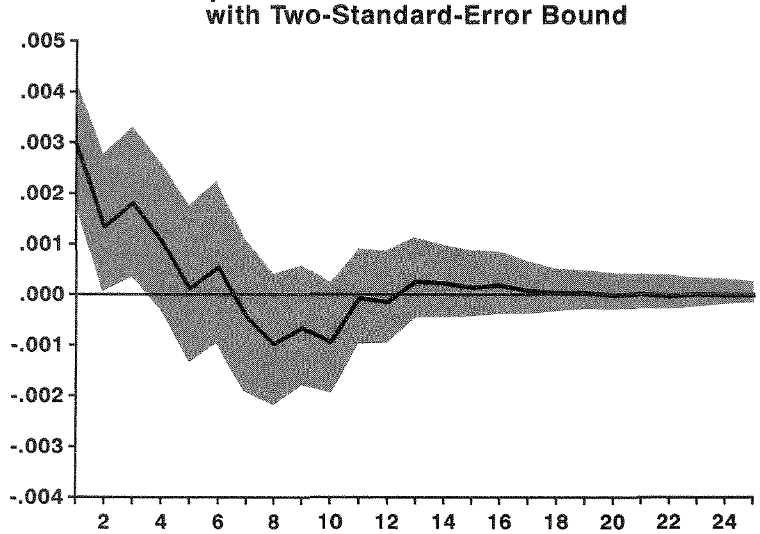


Chart 4
Response of All Twelfth District States with Two-Standard-Error Bound



for individual states reveal statistically significant spillovers in Nevada (through quarter 6), Arizona (through quarter 4), and Oregon, Washington, and Utah (through quarter 3).

The robustness of the results is tested by splitting the sample into two periods (1947.Q1-1970.Q1 and 1970.Q2-1991.Q4). This tests for structural change, at the cost, however, of reducing the degrees of freedom. In general, splitting the sample period lowers the value of the F statistics for Granger causality, with the leading relationship becoming insignificant in certain states. While the overall qualitative pattern of the results does not change, for most states the measured linkage to California appears larger in the first period than in the second, while the measured linkage to the rest of the nation rises. This suggests that western states are becoming more integrated into the national economy over time, while the relative linkage to California is falling. The impulse responses in both sample periods, however, both reveal significant spillovers for three quarters following a shock to California.

For the aggregate of states contiguous to California, for example, the F statistic for Granger causality is 2.90 for the first period, but only 0.6 in the second period, and the measured linkage to California declines from 38.2 to 25.9. The linkage to the nation, however, rises from 34.1 to 40.9, suggesting some substitution in linkage from California to the nation. The pattern of the impulse responses (shown in Chart 6) to a shock from California, however, is little changed between the two sample periods and remains significantly greater than zero for three quarters. While these results are suggestive of structural change, testing for this will involve a modeling approach that allows for time-

Chart 5
Response of Contiguous States with Two-Standard-Error Bound

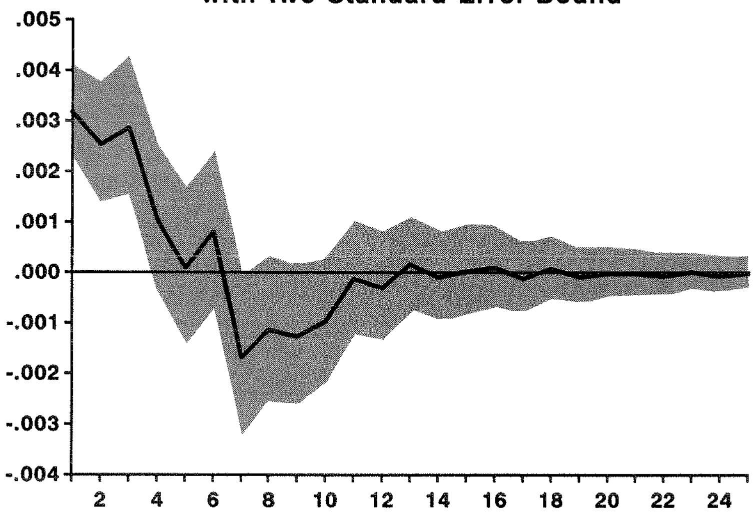
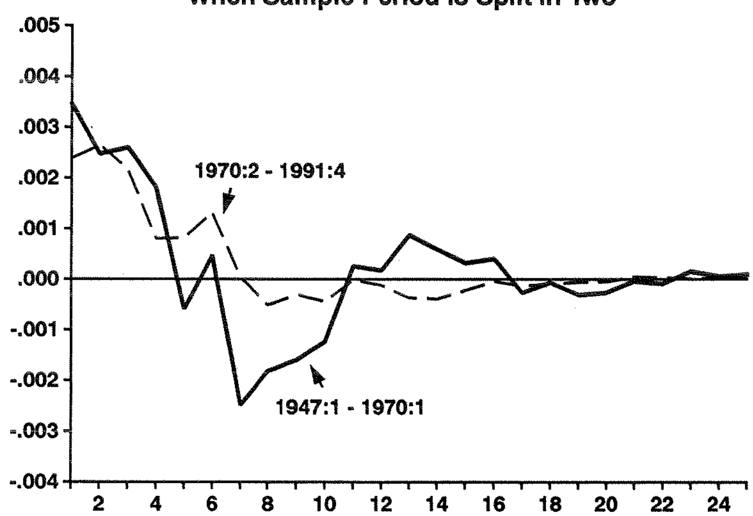


Chart 6
Response of Contiguous States When Sample Period is Split in Two



varying coefficients and represents an area for future research.

To summarize these initial results, California has statistically significant leading relationships for several neighboring states, including Arizona, Nevada, Oregon, Washington, and Utah. With the exception of Arizona, a reverse effect on California is not seen. Furthermore, under the identifying assumption that observed contemporaneous shocks flow from California to its neighbors, California appears to have significant economic spillovers to its neighbor states. The largest spillovers appear in states geographically near California. The results are sensitive, however, to the assumption of predeterminedness and the choice of sample period. There is some indication that the linkage of California to neighboring economies may be decreasing over time relative to their linkage to the national economy.

III. Sectoral Linkages

To explore linkages between sectors in California and sectors in its neighbors, I expand the three-equation VAR model estimated in Section II to a six-equation system. NATEMP and CALEMP remain unchanged from the initial period. STEMP, however, is divided up into the following sectors: manufacturing, services, "other," and finance. An equation is included for each sector. As before, each of these six components then is regressed on lagged values of itself and lagged values of the other components. I conduct this analysis only on states for which California had a significant overall Granger causal effect.

The results are reported in Table 4. California appears to have a leading effect on manufacturing in Arizona, Oregon, and Utah. California also appears to have a leading effect on the service sectors of its neighbors, with significant results seen for Arizona, Nevada, and Oregon. Of particular interest is the strong result for Nevada, showing the expected impact of California on the casino-related service sector of the state. A significant effect is also seen for the "other" sectors in Utah and Oregon. (Service employment is included in the "other" sector for Utah due to data availability.) California does not appear to have an effect on any specific sector in Washington, though the "other" sector has the strongest measured effect with an F statistic of 1.9 that is significant at the 80 percent level. Finally, the California economy does not have a Granger causal effect on the financial sectors of its neighbors.

To estimate the magnitude of these linkages, a causal ordering is again needed. I again assume that the nation is predetermined with respect to California and its neighbors, and that California is predetermined with respect to its neighbors. More problematic, however, is determining the direction of causality among the sectors. The results for the linkage to California, however, are invariant to the ordering of sectors.

The forecast error variances of the state sectors due to California shocks are shown in Table 5. Note that in a six-

Table 4

California vs. Sectors in Neighbor States

Results of Granger Causality Tests

State	Manufacturing	Services	Other	Finance
Arizona	Yes 3.4	Yes 3.5	No 1.5	No 0.6
Nevada	No 1.1	Yes 4.3	No 2.1	No 1.2
Oregon	Yes 4.3	Yes 2.3	Yes 4.1	No 1.1
Utah[a]	Yes 2.2		Yes 3.3	No 0.6
Washington	No 1.3	No 1.1	No 1.9	No 0.4

Note: F test statistic for null hypothesis of non-Granger causality. The critical value for rejecting the null hypothesis is 2.10.

[a]For Utah, no service sector data are available, so Services are included in Other.

Table 5

Percent of Forecast Error Variance in State Sector Attributable to California after 24 Quarters

	Variance Decomposition			
State	Manufacturing	Services	Other	Finance
Arizona	26.4	11.6	17.1	10.0
Nevada	9.4	8.1	9.8	7.2
Oregon	16.2	11.9	11.3	9.7
Utah[a]	9.8		14.6	6.0
Washington	10.3	5.5	10.1	5.8

[a]For Utah, no service sector data are available, so Services are included in Other.

equation system, the observed linkage to California declines because shocks in the other sectors affect the forecast variance. The results are thus not strictly comparable to the three-equation model, but are used to suggest relative strengths of linkages across states and across sectors.

In general, manufacturing displays a higher degree of linkage to California than the other sectors. Arizona manufacturing appears to be most linked to California, followed by Oregon and Washington. In services, Arizona and Oregon display the greatest linkage. The "other" category displays large linkages in Arizona and Utah. In spite of the significant Granger-test of California on Nevada, the estimated linkage is of relatively small magnitude.

The observed spillovers in manufacturing are consistent with a model of linkages propagated through trade flows between firms. The spillovers in the service sector suggest that linkages also exist in sectors such as tourism and recreation. This is particularly true in the case of Nevada, where growth in California has strong effects on the casino-dominated recreation sector. The lack of spillovers in finance suggests that growth in this sector is largely determined by developments internal to the state, rather than spillovers from California.

IV. Conclusion and Future Research

Using a set of three-equation VAR models of the nation, California, and other Twelfth District states, this paper established that California has a statistically significant leading relationship with employment growth in several of its neighbor states—Arizona, Nevada, Oregon, Utah, and Washington. The sectors affected are manufacturing in Arizona, Oregon, and Utah, and services in Arizona, Nevada, and Oregon. The financial sectors of these states are not affected.

The magnitude of these linkages were then measured through VAR variance decomposition and impulse response analysis. This measurement requires identifying assumptions regarding the observed correlation of contemporaneous shocks. I assume that the causal ordering flows from the nation to California and other states, and from California to its neighbors. Under this assumption, the measured spillovers appear to be important, but dampen relatively quickly.

These results are broadly consistent with a model of regional linkages occurring through trade of goods and services. Positive shocks to California have positive short-run spillovers. The spillovers in manufacturing can be attributed to orders for goods, while spillovers in services potentially are due to demand for recreation and tourism.

An extension of this research will further explore these linkages and the reasonableness of the identifying assumptions. Alternative explanations for the joint regional shocks to California and its neighbors could include industrial mix (aerospace or tourism, for example), or shocks associated with being located on the Pacific Rim. An explicit accounting for aerospace between Washington and California, for example, could explore whether this industry is driving the observed overall linkage in manufacturing.

This paper also suggests, however, that simple VAR modeling of regional economies can be pushed only so far. The results are sensitive to structural change, and imposing a standard model on unique states results in dynamic patterns that suggest problems in specification. While VAR modeling may effectively pick up trade flows, measuring longer-run factor flows suggests a modeling approach that explicitly accounts for structural change.

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Changing Geographical Patterns of Electronic Components Activity

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Some observers have argued that high technology industries are leaving early technology centers, such as Silicon Valley, for lower-cost locations. These assertions are consistent with a world in which early innovations tend to be concentrated geographically, but proximity to the innovating region becomes less important than other costs as the product's market grows and standardized production technologies are developed.

This study finds little evidence of such patterns in electronic components activity within the U.S. In contrast, the U.S. share of the total worldwide electronics market has fallen dramatically, while nations with lower costs and less developed technological infrastructures are gaining market share.

During the 1980s, some observers argued that high technology industries were leaving early technology centers, such as Silicon Valley, for lower-cost locations in the U.S. and abroad (see, for example, Saxenian 1984). These assertions are consistent with the views of some economists, who believe that the factors affecting firms' location decisions may vary during the course of a product's life cycle (Vernon 1966). In this view, innovation in a particular industry tends to be concentrated in a region that offers access to technological expertise, even if the general level of costs in that region is relatively high. As the market for the new product grows and standardized production technologies are developed, proximity to the innovating region becomes less important, freeing firms to seek lower land and labor costs elsewhere. Thus, according to this theory, infant industries may be concentrated in high cost regions, while mature industries are more likely to be located where production costs are low.

Previous studies addressing similar issues (Malecki 1985, Park and Lewis 1991) found that geographical dispersion did not occur within the technology-oriented industries they studied. However, these results are not necessarily inconsistent with Vernon's hypothesis. First, they looked only at changes within the U.S. Vernon's paper, in contrast, discussed these changes in an international context, and the forces suggested by his theory may be more readily apparent by making international comparisons.

Second, they looked for "dispersion," defined as an even distribution of activity across all geographic areas. However, a search for low-cost production sites would not result in an even distribution of production across localities if production moved *en masse* to a region that offered lower land and labor costs. Thus, the level of geographic concentration in the industry could remain constant, even if the location of that concentration were to change. In addition, the geographic areas used for previous studies (census regions and states) may be too large to capture some of the changes that do occur.

An alternative explanation for the empirical studies' results is that the product life cycle theory may not hold for high-tech industries. One possible reason is that the pace of innovation has been so rapid that many products have life-spans of only a few years. In this dynamic environment, the investments in standardized production technology that allow production to move away from the innovation site may never be economically feasible. Moreover, the frequent changes mean that an "industry" as defined by data classifications does not describe a single homogeneous product, but instead includes a series of several distinct products, which substitute for each other over time.

This study examines the issues raised by Vernon's theory in the context of the electronic components industry. The paper is organized as follows. The first section discusses the factors that could cause firms' location decision parameters to change over the course of a product's life cycle. Section II addresses whether the changes described by the product life cycle theory have occurred internationally. Section III addresses the same question within the U.S., using more detailed U.S. data and defining the questions somewhat differently from previous U.S. studies. Section IV draws conclusions.

I. The Product Life Cycle

Vernon (1966) provided a rationale for why firms' location decisions might change during the course of a product's life cycle. Like many stage theories, the stages themselves are somewhat arbitrary, and the events of one stage do not provide a compelling explanation of why events ought to progress to the next stage. Nevertheless, its major points are both plausible and consistent with popular notions of the changes that have occurred in high technology industries. Moreover, it provides a convenient framework for a more general discussion of the changes in the industry's structure.

Initially, production might be concentrated geographically simply because each innovation must take place somewhere. However, some regions are more likely to be seedbeds for innovations than others, since a critical mass of related activity can yield external economies of scale that make each input more productive. For example, a region with a cluster of related activities is likely to have the business service and financial infrastructure in place to serve firms with similar needs. Moreover, workers with appropriate skills are likely to be more plentiful in such a location, and if these skills are relatively unusual in the general population, the location will be particularly appealing to firms. For innovations in which work force characteristics and local infrastructure are critical, proximity to these factors is likely to outweigh other considerations in firms' location decisions at the innovation stage. Therefore, during the innovation stage a cluster of activity could occur even in a location where the general level of costs is high.

After the initial innovation comes a transition stage, in which increased demand for the product makes investment in production technology feasible and the technology of production can be transferred from one location to another. At this point, firms are not tied to the site of the original innovation as closely as they were in the first stage. Nevertheless, the continued need for technological expertise as the production process is refined may lead the firm to confine its site search to a smaller region than it might otherwise explore. Thus, in this second stage, the industry may spread out somewhat from its initial concentration of activity.

The third and final stage of the product life cycle is standardization. In principle, standardization occurs when technological innovations are complete, so the research activities that previously were concentrated in the innovating region are no longer necessary. In practice, some technological inputs may be required even when production is relatively standardized, but in any case, site location decisions should be based primarily on the costs of inputs to a standardized production process. Most interregional cost differences would be expected to result from differences in land and labor costs.

If patterns in the electronic components industry were consistent with the product life cycle theory, and if the initial activity were concentrated in high-cost areas, activity should have left the regions where early innovations took place, and current activity should be most prevalent in areas that offer low levels of production costs for the industry. Wages and other direct costs should have become more important locational determinants over time, while attributes associated with technological expertise should have become less important. These trends should have been especially prevalent in the line production activities, which would require relatively little technological expertise if they were standardized. Moreover, if the product life cycle theory accurately describes changes in the electronic components industry, research and production activities should have become less closely linked to each other over time.

In some ways, these kinds of changes seem plausible for the high-tech industries in general and for the electronic components industry in particular. Personal computers, an important end use for many electronic components, provides an obvious example of a growing market for components during the 1977 to 1987 period.

II. International Comparisons

Table 1 presents various measures of production and costs for the electronics industries in several important producing countries, along with measures of technological sophistication for those countries.[1] The table suggests that, in 1988, the U.S. dominated the world's electronics industry by most measures. The U.S. made 38 percent of the world's electronic products. Japan and the European Community (EC) also contributed significantly to world production, with shares of 26 and 24 percent, respectively. Thus, these three entities accounted for 88 percent of total worldwide electronics production in 1988. Other producers, including India, Taiwan, Singapore, Brazil, and South Korea, together accounted for only 6.4 percent of the world's electronics production.

The U.S. outranks other producing countries by most measures of technological sophistication. The U.S. holds commanding leads in terms of the number of telephones per capita and the number of scientists and engineers. The U.S. also ranks second (to Japan) in the number of scientists and engineers relative to total population.

Gross domestic product (GDP) per capita provides a rough measure of the differences across countries in the cost of doing business. Relatively high GDP per capita reflects the high labor costs in those countries and also suggests that the level of investment in both human and physical capital is sufficient to generate relatively high returns to land and other factors. By this measure, the U.S. ranked second, at $18,393, with only Japan ($19,448) posting a higher GDP per capita. Both the U.S. and Japan have significantly higher GDP per capita than the EC's $13,137, and none of the other producing countries listed in Table 1 has GDP per capita that reaches even half of the U.S. or Japan levels.

There are signs, however, that the U.S. domination of the industry may be waning. The growth rate in U.S. production between 1984 and 1988 was only 1 percent, by

[1] "Electronics" is defined here to include electronic materials and components, software, computers, telecommunications equipment, business equipment (copiers, fax machines, and so forth), and instruments.

Table 1

Electronics Sectors in Selected Countries

	U.S.	Japan	EC[a]	S. Korea	Taiwan	Singapore	Brazil	India	World
Value of Electronics Production ($M, 1988)	186,232	127,208	115,136	9,103	7,890	7,651	3,876	2,314	486,718
% of World Total (1988)	38.3	26.1	23.7	1.9	1.6	1.6	0.8	0.5	100
% of World Total (1984)	43.0	22.5	21.9	0.9	1.1	0.8	0.6	0.2	100
Real Annual Growth Rate (%, 1984-88)	1	8	6	24	15	23	11	23	4
Production/GDP (%, 1987)	3.9	4.6	2.6	6.0	10.0	28.6	1.0	1.7	N/A
Production ($000)/Employment	104.9	105.9	79.2	35.8	40.7	107.8	15.1	11.6	N/A
Total Electronics Employment (000, 1986)	1776	1201	1454	254	194	71	257	200	N/A
Annual Growth Rate (%, 1980-86)	1.3	9.6	1.5	9.8	2.6	−0.2	N/A	N/A	N/A
General Technology Characteristics									
Telephones/1000 Pop. (1986)	791	558	520	186	228	417	84	4	N/A
Scientists & Engineers (000, 1986)	787	575	468	47	42	2	33	100	N/A
Scientists & Engineers/Million Pop.	3,230	4,712	1,443	1,116	2,149	923	230	128	N/A
GDP per Capita ($, 1987)	18,393	19,448	13,137	2,881	3,794	7,654	2,304	326	N/A

Note: "Electronics" is defined here to include electronic materials and components, software, computers, telecommunications equipment, business equipment (copiers, facsimile machines, and so on), and instruments.
[a]EC data exclude Portugal and Greece.

far the slowest growth among the group of countries included in Table 1. Electronics industry growth rates in countries that offer much lower costs were all in double digits—stronger growth than in the countries that dominated worldwide production in 1988. In addition, according to the Semiconductor Industry Association (SIA), the share of U.S. company production in total world semiconductor production fell fairly steadily, from 65 percent in 1977, to 60 percent in 1982, 45 percent in 1987, and 38 percent in 1988 (SIA 1990), before picking up slightly to 40 percent in 1990.[2] It is likely that these numbers understate the movement of semiconductor production outside of the U.S., since U.S.-based companies have moved their own production offshore even as foreign companies have increased their production.

Thus, in some ways, the industry's patterns appear to be consistent with the product life cycle hypothesis. The U.S., a technology-oriented, high-cost country, dominated the industry in its early days, suggesting that the U.S. was the site of early innovations. Between 1984 and 1988, the electronics industry grew fastest in the countries that offered lower production costs. This change is consistent with the industry moving toward the standardization stage, with firms seeking locations that offer lower costs.

Another characteristic that is broadly consistent with the product life cycle theory is that, within the electronics industry, the product mix varies substantially across countries. Korea, for example, specializes in computer assembly, while Japan's electronics industry is dominated by semiconductor fabrication. Differences in product mix and capital intensity are reflected in wide variations in the value of production per worker across countries. In the U.S., Japan, and Singapore, the average value of production per worker is $100,000. In sharp contrast, the value per worker is $40,000 or less in India, Brazil, Taiwan, and South Korea.

Nevertheless, it is worth noting that the U.S. does retain the world lead in some segments of the electronics industry. U.S. producers dominate world production in highly profitable areas, such as processors that are vital to the calculating, graphics, and sound functions in computers. Japanese producers, in contrast, dominate the less profitable market for standard memory chips (Pollack 1992).

In a similar vein, Saxenian (1990) suggests that during the middle and late 1980s, Silicon Valley spawned a new generation of flexible, interconnected firms that specialize in particular aspects of technological development or production. These firms do not attempt to make high-volume commodity chips (as Intel and Advance Micro Devices had in an earlier generation), but instead seek out small, specialized niches in which they can take advantage of their technological expertise and flexibility.

The view that U.S. producers continue to play an important role in the high-tech industries, but not in mass-produced commodity products, also is supported by the pattern of employment growth within the U.S. Between 1977 and 1987, the number of electronic components workers in nonproduction functions (including research and development, headquarters functions, and marketing) grew 88 percent. During the same period, the number of production workers grew by a relatively modest 28 percent. These figures are consistent with a change in the U.S. industry away from mass production and toward custom products with smaller markets.

The overall picture that emerges is one of a very heterogeneous industry, in which different countries have tended to specialize in different functions, and there is still a role for U.S. producers, although that role is different from its role in the past. The product life cycle theory is not strictly applicable to a group of products as heterogeneous as this one. The continued role of U.S.-based firms in developing and implementing the new technologies, and the dominance of the U.S., the EC, and Japan in total worldwide production, suggests that the product life cycle story does not fully capture the dynamics of the electronic components industry. At the same time, though, the rapid growth in assembly activity in Taiwan and South Korea is consistent with the product life cycle theory's assertion that standardization allows production activities to move to regions that offer relatively low costs, even though their technological infrastructures are less well developed.

III. The Electronic Components Industry in the U.S.

Malecki (1985) and Park and Lewis (1991) examined whether the kinds of geographic changes predicted by the product life cycle theory are at work in high technology industries within the U.S. Malecki found that there was little dispersal across four census regions between 1973 and 1983 in the four 4-digit Standard Industrial Classification (SIC) categories he examined.[3] Park and Lewis conducted shift-share analysis using state-level data for three of Malecki's four 4-digit industries, with mixed results.

[2] Note that semiconductors are just one type of electronic component. Semiconductors are a subset of the electronic components category measured by SIC 367.

[3] Those four industries were Electronic Computing Equipment (SIC 3573), Semiconductors (SIC 3674), Medical and Surgical Instruments (SIC 3841), and Computer Programming (SIC 7372).

They concluded that their results did not support the product life cycle model.

Glasmeier (1986) disaggregated employment in 2-digit SIC categories by occupation.[4] She found that the technical and professional jobs were concentrated geographically, but that many of the locations of these concentrations had relatively few production workers in the same industry groups. This supports the product life cycle contention that production and nonproduction activities tend to become more separate as the product's life cycle progresses, although the continued existence of a large cadre of nonproduction workers suggests that production was not yet fully standardized at the end of her sample period.

[4]These industries are Chemicals (SIC 28), Nonelectrical Machinery (SIC 35), Electrical Machinery (SIC 36), Transportation Equipment (SIC 37), and Scientific Instruments (SIC 38).

This section addresses these issues using Census of Manufactures data for the 3-digit SIC industry of electronic components (367). (See Box for a description of the data.) The present study differs from previous work in that it explores the question at the metropolitan area level rather than at the level of the state (Park and Lewis, Glasmeier) or census region (Malecki).

Characteristics of Innovating Regions

Table 2 presents information on various characteristics of the ten Metropolitan Statistical Areas (MSAs) that

Box 1
Census of Manufactures Data

The manufacturing data in this study come from the Census of Manufactures, produced by the U.S. Commerce Department. The Census of Manufactures provides metropolitan area data on such variables as the number of production and nonproduction workers, work hours, and payroll costs. The Standard Industrial Classification (SIC) used for this study is electronic components (SIC 367), which includes electron tubes, printed circuit boards, semiconductors and related devices, and electronic capacitors, resistors, coils, transformers, and connectors. It does not include finished technology products such as computers or scientific instruments.

The number of Metropolitan Statistical Areas (MSAs) for which complete data are available varies among the sample years. Data are withheld for MSAs with only a small number of employers in SIC 367, and for MSAs in which a single employer dominated that MSA's industry. The data set includes 44 MSAs for 1977 and 68 MSAs for 1987.

The MSAs for which complete data are reported leave much of the U.S. employment in the industry unaccounted for. For example, the 44 MSAs for which 1977 data are available account for only 49 percent of U.S. employment in SIC 367. For 1987, the sample size rises closer to 60 percent, but clearly a large portion of the industry remains unreported. This large unreported portion of the industry could potentially affect the empirical results, if many of the industry's changes are occurring outside MSAs or in MSAs that do not report complete data for SIC 367.

One problem with using SIC 367 is that the characteristics of the products classified in this 3-digit SIC category have changed over time. Between 1977 and 1987, the share of semiconductors in the total rose from 30.5 percent to 33.8 percent. The share of electronic connectors also rose, and printed circuit boards were added as a separate category in 1987. (Prior to 1987, printed circuit boards were included in "Electronic Components, not elsewhere classified.") During the ten-year period, electronic capacitors, resistors, and tubes became significantly less important to the entire industry. While going to the 4-digit industry level would alleviate many of the problems associated with changes in the composition of SIC 367, too few MSAs report 4-digit data to allow a meaningful analysis.

Another potential problem with these data is that the manufacturing census is conducted by establishment, and an establishment is considered to be in SIC 367 only if production occurs on the site. Therefore, an establishment engaged only in research and development, sales, administration, or other "auxiliary" functions would not be included in the totals for SIC 367, even if the firm produces nothing but electronic components. This means that the data regarding production activities are likely to be more accurate than the data for nonproduction activities, although the extent of the problem with nonproduction data is impossible to determine.

Table 2

Characteristics of Regions Producing Electronic Components, 1977

MSA	Percent of U.S. Employment: SIC 367 All	SIC 367 Production	SIC 367 Nonprod'n	Total	Per Capita Personal Income ($)	Production Worker Wage ($/Hour)	Nonprod'n Worker Salary ($/Year)	High School Graduates[a] (%)	4+ Years of College[a] (%)
San Jose, CA	10.1	8.1	14.4	0.7	8,865	5.41	19,850	79.5	26.4
Chicago, IL	5.4	5.7	4.8	3.7	8,885	4.63	15,855	67.5	18.5
Los Angeles/Long Beach, CA	5.2	5.7	4.2	3.9	8,473	4.78	19,163	69.8	18.5
Phoenix, AZ	4.1	3.0	6.6	0.6	7,059	5.02	14,053	75.0	18.3
Dallas/Fort Worth, TX	3.7	3.8	3.7	1.4	7,878	5.49	15,233	70.0	20.2
Boston, MA	3.4	3.3	3.5	1.6	7,984	4.86	17,463	77.2	24.7
Anaheim/Santa Ana, CA	3.3	3.4	3.3	0.8	8,968	4.61	19,711	80.4	22.6
Nassau, NY	2.0	2.0	2.1	1.0	8,870	4.81	17,167	75.8	20.9
New York, NY	1.9	2.0	1.6	4.4	8,643	4.61	16,526	63.5	19.2
Philadelphia, PA	1.8	2.0	1.4	2.2	7,844	4.97	17,125	66.0	13.6
Ten MSAs	41.1	39.0	45.7	20.3	8,426	4.97	17,640	72.5	20.3
U.S.	100	100	100	100	7,297	4.83	18,158	68.6	17.0

[a]Education data are for 1980.

accounted for the largest shares of U.S. electronic components employment in 1977. Together, the top ten areas accounted for 41 percent of electronic components employment. By way of comparison, these ten MSAs accounted for about 20 percent of the nation's total employment across all industries.

Of these ten MSAs, eight (San Jose, Chicago, Los Angeles/Long Beach, Phoenix, Dallas/Fort Worth, Boston, Anaheim, and Nassau) are much more strongly represented in the electronic components industry than their sizes would suggest. In contrast, New York and Philadelphia are large metropolitan areas whose contributions to the electronic components industry derive primarily from their large sizes. Their contributions to electronic components employment actually are smaller than their contributions to total employment.

These ten areas accounted for a significantly larger share of the nation's nonproduction workers in the electronic components industry than their share of production workers. They contained 46 percent of the nation's nonproduction workers in the electronic components industry, but only 39 percent of national production workers. The greater concentration among nonproduction workers than among production workers is due to sharp differences in only two MSAs: San Jose and Phoenix. The other important electronic components producing areas were either proportionately represented by production and nonproduction workers, or were relatively over-represented by production workers.

One of the most striking observations from this table is that San Jose clearly dominated the industry. The San Jose area, which accounted for only 0.7 percent of the nation's total jobs in 1977, provided fully 11 percent of the nation's electronic components employment. Moreover, the San Jose MSA had more than twice as many electronic components jobs as Chicago, which ranked second. San Jose's share of the industry's nonproduction jobs was even greater, at over 14 percent. These jobs, which cover functions other than line production, include research and development, sales, and headquarters functions. Thus, nonproduction jobs are more likely to require advanced education and technological training.

In most respects, the characteristics of the San Jose area during the late 1970s were those that tend to be associated with technological innovations. Stanford University, with one of the nation's top electrical engineering programs, is located in the area. The University of California at Berkeley, with another top-rated electrical engineering program, is only about 50 miles away. Moreover, the San Jose area boasts a highly educated population. Of residents over 25 years old, 80 percent had high school diplomas and 26 percent had at least 4 years of college in 1980—much higher proportions than the nation, where the figures among the same age group were 69 percent and 17 percent, respectively.

The San Jose area had high costs by any measure. The average hourly wage for production workers in the electronic components industry was $5.41 in San Jose, compared with $4.83 nationally. The average annual salary for a nonproduction worker in San Jose was $19,850, much higher than the $18,158 national average. Per capita personal income, a more general measure of the level of incomes (and presumably costs) in an area, also was relatively high in the San Jose area, at $8,865 compared to a national average of $7,297. Housing prices are not listed in the table, but home prices also were relatively high in the San Jose area. 1980 Census data, for example, revealed that the median monthly mortgage payment for an owner-occupied dwelling was $475 in San Jose, much higher than the $365 national median. Rents in San Jose also were much higher than the national average, with a median monthly rental payment of $365, compared with the U.S. median of $243.

Venture capital, an important source of funding for high-tech start-up companies, also was more readily available in the San Jose area than in many other parts of the country. While the New York/New Jersey/Connecticut area clearly dominates the venture capital field with over a third of the 100 biggest funds in 1986, Northern California had 20 funds on the list, followed by the Boston area with 15 ("Venture Capital 100 for 1986"). In fact, 73 of the 100 largest funds were located in these three areas alone, with the remainder scattered throughout the rest of the United States.

The overall picture of the San Jose area that emerges is one of a quintessential innovating region. It clearly dominated the industry early on, particularly in the nonproduction areas that require highly educated, technical personnel. The area was near universities with first-rate electrical engineering programs, and had a highly educated population and relatively good access to venture capital. Moreover, the high level of costs in the area, both generally and in terms of labor for the electronic components industry, suggests that if the industry were moving toward standardization during the late 1970s and early 1980s, firms would have had an incentive to move their operations to lower-cost regions.

Most of the other areas listed in Table 2 had some of the characteristics of an innovating region. In particular, nine out of ten had a higher percentage of college graduates than the nation, and seven out of ten had a higher percentage of high school graduates. But the characteristics of the other MSAs are not as striking as those of San Jose. For example, in or adjacent to each of these metropolitan areas there is at least one university with an electrical engineering program. However, the only top-20 programs in areas producing electronic components are in San Jose (Stanford), Los Angeles/Long Beach (UCLA, USC, and Cal Tech), and Boston (MIT).[5] Nevertheless, most of the others have top-ranked electrical engineering departments within a few hours' drive. Anaheim is adjacent to the Los Angeles area and its universities. Princeton is within 50 miles of both Philadelphia and New York. Similarly, Purdue and the University of Illinois at Urbana-Champaign, both top-ranked departments, are within 150 miles of Chicago. Dallas/Fort Worth is almost 200 miles from the University of Texas at Austin. Among the most important metropolitan areas for producing electronic components, only Phoenix does not have a top-ranked electrical engineering department within a few hundred miles.[6]

If there were an incentive to shift activity to lower-cost regions since 1977, costs in these cities would be expected to have been relatively high. Table 2 shows that, for these ten cities as a group, labor costs in the electronic components industry were only slightly higher than the national average. Indeed, the average annual salary for nonproduction electronic component workers actually was *lower* for these cities than it was nationally. San Jose is the only metropolitan area in this group with both production wages and nonproduction salaries higher than the national average. In contrast, personal income per capita, a more general measure of the MSA's level of costs, was substantially higher in these areas than it was nationally. Taken together, these figures suggest that, even if production and nonproduction activities became less closely linked over

[5]According to the author's calculations based on information provided by the Conference Board of Associated Research Councils (1982), the top twenty electrical engineering programs were: MIT (Cambridge, MA), Stanford (Stanford, CA), Illinois (Urbana/Champaign, IL), California (Berkeley, CA), UCLA (Los Angeles, CA), USC (Los Angeles, CA), Purdue (West Lafayette, IN), Maryland (College Park, MD), Cornell (Ithaca, NY), Carnegie-Mellon (Pittsburgh, PA), Ohio State (Columbus, OH), Michigan (Ann Arbor, MI), Wisconsin (Madison, WI), Texas (Austin, TX), Rensselaer (Albany, NY), Princeton (Princeton, NJ), Cal Tech (Pasadena, CA), Florida (Gainesville, FL), UCSD (San Diego, CA), and UCSB (Santa Barbara, CA).

[6]The University of Arizona, in Tucson, is 34th of the 91 ranked electrical engineering departments; Arizona State in Tempe (a Phoenix suburb) ranks 57th.

time, as the product life cycle theory suggests, the potential cost savings from shifting electronic components activity elsewhere may be relatively modest, except in the San Jose area.

Around the late 1970s and early 1980s, the personal computer became an important fixture in offices and universities. With the huge growth in the industry, demand for components increased enormously. Given these changes in the industry, the characteristics of the regions that were important producers of electronic components in 1977, and the discussion of the changes that occur over a product's life cycle, we would expect to see a significant reduction in the importance of the San Jose area over time. In contrast, we would expect to see much less dramatic changes in the patterns among the other technology-oriented areas listed in Table 2.

Changes within the U.S.

To see whether such changes have in fact occurred, Table 3 provides similar data for the ten most important areas in the industry in 1987.[7] Contrary to expectations based on the product life cycle theory, San Jose's share of national employment in the electronic components industry *rose* from 10.1 percent in 1977 to 11.5 percent in 1987. Moreover, San Jose became more dominant in both the production and nonproduction parts of the industry. The San Jose area accounted for 14.4 percent of the nation's nonproduction workers in 1977 and 15.4 percent in 1987; the area's share of total production workers rose from 8.1 percent to 9.0 percent during the same period.

Changes among the other producing cities also were modest. One change is that the composition of the list

[7]Since educational attainment data were available for only one year, the figures in Table 4 are identical to those in Table 2.

Table 3
Characteristics of Regions Producing Electronic Components, 1987

MSA	Percent of U.S. Employment SIC 367 All	Production	Nonprod'n	Total	Per Capita Personal Income ($)	Production Worker Wage ($/Hour)	Nonprod'n Worker Salary ($/Year)	High School Graduates[a] (%)	4+ Years of College[a] (%)
San Jose, CA	11.5	9.0	15.4	0.8	21,547	11.60	38,701	79.5	26.4
Los Angeles/Long Beach, CA	5.1	5.8	4.0	3.9	17,680	9.39	32,535	69.8	18.5
Phoenix, AZ	4.1	3.4	5.1	0.9	16,064	8.36	32,090	75.0	18.3
Anaheim/Santa Ana, CA	3.7	4.0	3.3	1.1	21,405	9.72	34,458	80.4	22.6
Boston, MA	3.5	3.4	3.6	1.7	20,330	9.44	31,936	77.2	24.7
Chicago, IL	3.1	3.6	2.3	3.0	17,662	8.19	30,360	67.5	18.5
Dallas/Fort Worth, TX	2.9	2.8	3.2	1.8	16,998	9.91	39,406	70.0	20.2
Nassau, NY	2.2	2.3	2.1	1.1	22,139	8.82	34,311	75.8	20.9
San Diego, CA	1.8	1.9	1.6	0.8	16,658	8.98	36,114	78.0	20.9
Minneapolis, MN	1.4	1.5	1.1	1.3	18,205	9.20	36,087	79.9	21.9
Ten MSAs	39.3	37.7	41.6	16.3	18,490	9.71	35,585	75.3	21.3
U.S.	100	100	100	100	15,511	9.32	34,751	68.6	17.0

[a]Education data are for 1980.

varies slightly between the two years. In 1987, New York and Philadelphia moved down to ranks 12 and 11, respectively, while San Diego and Minneapolis moved into the top ten. Chicago fell from number 2 to number six, but other changes in rank within the top ten were small.

Taken together, the top ten MSAs accounted for 39 percent of national employment in the electronic components industry in 1987, down from 41 percent in 1977. The ten cities as a group also accounted for a smaller share of nonproduction employment in 1987 (42 percent) than they did in 1977 (46 percent). This change is consistent with the notion that technological expertise might diffuse or become less important as the product progresses through its life cycle. The change in share for production workers, however, was quite small, from 39 to 38 percent. This small change tends to contradict the notion that firms are moving production activities from their early centers to other, lower-cost locations within the United States.

An additional prediction of the product life cycle theory is that production and nonproduction activities become less closely linked over time, as production processes become more standardized. To see whether this pattern has emerged within the U.S., I run simple correlations between each MSA's share of U.S. production and nonproduction employment for each year, using the entire sample of MSAs.[8] If the linkage between the two has become weaker over time, the correlation coefficient would shrink over time.

In 1977, the correlation coefficient between MSAs' shares of national production employment in SIC 367 and their shares of nonproduction employment was quite high, at 0.914. In 1987, the correlation was even higher, at 0.927. These figures suggest that, within the U.S., the linkage between production and nonproduction activities remains strong. This result contradicts the expectations based on the product life cycle theory.

The Census of Manufactures data run only through 1987, and data for SIC 367 are not available for most MSAs for non-census years. However, many MSAs do report intercensal data on SIC 36, electric and electronic equipment, the 2-digit category that includes SIC 367.

For MSAs in which SIC 36 data are available, the shares of the top ten cities remained relatively stable from 1977 to 1987, following the pattern seen in SIC 367 (see Appendix). The share dropped off sharply between 1987 and 1991, from 34.6 percent to 24.6 percent. However, over half of the drop-off in SIC 36 between 1987 and 1990 occurred in Los Angeles, falling from 8.9 percent to 3.5 percent. Since the Bureau of Labor Statistics (BLS) does in fact report 3-digit data for SIC 367 for Los Angeles, we can check to see if the SIC 367 is responsible for the sharp decline in Los Angeles' share of SIC 36 activity. The BLS numbers for SIC 367 reveal a much smaller decline in Los Angeles' share of electronic components employment, from 4.5 percent to 3.9 percent. Thus, the evidence leaves open the possibility that dramatic changes may have occurred in the electronic components industry (SIC 367) since the 1987 manufacturing census, but the changes probably were not as dramatic as the changes in SIC 36 were.

IV. Conclusions

This paper started with the observation that many are concerned about shifts in electronic components activity away from historical centers such as Silicon Valley. Such a shift is consistent with the views of some economists who argue that the factors affecting firms' location decisions may vary during the course of a product's life cycle.

This study analyzed a variety of data at the international and national level. Consistent with previous work by Malecki (1985) and Park and Lewis (1991), the analysis found little evidence to support the contention that the product life cycle theory explains changes in the location of electronic components activity within the U.S. In particular, the San Jose metropolitan area, which includes the Silicon Valley, continues to play a dominant role in the electronic components industry within the United States.

In contrast, an examination of the international data revealed that the U.S. share of the total worldwide electronics market has fallen dramatically, and that nations with lower costs and less developed technological infrastructures are gaining market share. This finding is consistent with expectations based on the product life cycle theory. Nevertheless, the U.S. does continue to play an important role in the industry, suggesting that complete standardization of the industry either has not yet occurred or will never occur in the fast-changing world of high-tech production.

[8]The sample includes 44 MSAs for 1977, and 68 MSAs for 1987.

Appendix

MSA Employment as a Percentage of National Employment

	SIC	1977	1982	1987	1991
San Jose, CA	367	10.1	11.7	11.5	
	36	3.9	5.8	5.7	5.1
Chicago, IL	367	5.4	4.2	3.1	
	36	7.7	6.2	5.0	4.9
Los Angeles/Long Beach, CA	367	5.2	5.5	5.1	
	367 (BLS)	4.4	4.2	4.5	3.9
	36	7.1	8.2	8.9	3.5
Phoenix, AZ	367	4.1	4.1	4.1	
Dallas/Fort Worth, TX	367	3.7	3.5	2.9	
	36	2.8	3.3	4.2	3.7
Boston, MA	367	3.4	3.5	3.5	
	36	3.0	3.3	3.3	2.2
Anaheim/Santa Ana, CA	367	3.3	3.8	3.7	
	36	2.7	3.1	3.6	2.1
Nassau, NY	367	2.0	2.1	2.2	
New York, NY	367	1.9	1.6	1.0	
	36	2.4	2.0	1.6	1.2
Philadelphia, PA	367	1.8	1.4	1.0	
	36	2.6	2.5	2.3	1.9
10 MSA Average	367	38.3	39.1	36.7	
	36	30.3	32.2	31.4	21.4

Note: Unless noted otherwise, SIC 367 data are from the Census of Manufacturers. SIC 36 data are from the Bureau of Labor Statistics and are not available for Phoenix or Nassau.

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