How Do Changes in Military Spending Affect the Economy? Evidence from State-Level Data

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A principal task for macroeconomics is to explain business cycles—why economies experience somewhat regular patterns of expansion and contraction, with a high degree of correlation across industries and regions. This is also a difficult task; for example, there is still no broad consensus on what caused the U.S. recession of 1990–91, much less on what policymakers could have done to avoid it. Among the hardest things to understand about business cycles are the sharp and unpredictable turning points when the economy reverses course and begins an expansion or contraction.

One reason that business cycle turning points are hard for economists to understand and predict is a direct result of the tools that they employ: their theoretical and statistical models. A large and growing body of evidence suggests that a key component in most models is specified too restrictively to adequately capture the behavior of the economy. This key component is the *propagation mechanism*, which translates variables that a model takes as given (or "exogenous") into variables that the model explains. The overly restrictive part of the specification is the requirement that the propagation mechanism be linear, which limits the form of the translation to adding a constant and a factor of proportionality.[1]

Such a modeling framework puts most of the burden of explaining business cycle fluctuations and turning points on movements in the exogenous variables.[2] Some exogenous variables are both important enough to have macroeconomic impacts and subject to large, sudden movements—oil prices, for example, rose dramatically in 1973 and again in 1979 and are widely believed to have precipitated the recessions that followed—but such direct associations are the exception rather than the rule.[3] In general the plausibly exogenous variables that we observe move more slowly and regularly.

Military spending is one such variable. While many commentators believe that it has played a significant role in business cycles, most recently in the Northeast and California, economic models typically assign it quite a modest role. For instance, the Congressional Budget Office (1992) and the U.S. Department of Defense (1992) estimated that the reductions in defense expenditures in recent years account for no more than a few tenths of a percentage point of the unemployment rate.[4] Estimates of the impacts of military spending have also tended to be very imprecise, so that the hypothesis that it has *no* effect often cannot be rejected statistically.[5] One possible route to better understanding the contributions of changes in military spending to business cycles is to move outside the linear model framework.

Recent years have seen renewed interest in nonlinearity, as research programs in several subareas of macroeconomics have advanced models incorporating nonlinear propagation mechanisms. In these models, the effects of exogenous variables may differ depending on the size and direction of their movements or on the current state and recent history of the economy. This article examines the relationship between military spending and economic activity in a framework that allows for several of these nonlinear features. The goals are both to see if it yields a better understanding of the contribution of military spending to business cycles, and to help decide on the relative merits of these new theories for interpreting macroeconomic fluctuations.

Such an endeavor with aggregate data is likely to be frustrated; at that level, military spending has not varied much and thus will not be very useful for identifying different types of responses. Essentially two cycles in military spending have occurred since the early 1960s, a buildup and drawdown associated with Vietnam, and a second buildup under Presidents Carter and Reagan that was reversed beginning in 1987. At the state level, however, a much greater diversity of experience can be found, in addition to a far larger set of observations. Both the average exposure to military spending and its fluctuations over time differ considerably across states.

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Rather than attempting to identify the detailed interrelationships between the variables in each state's economy, this study estimates unconstrained forecasting equations that simply capture the statistical relationship between economic activity and military spending. The equations allow for state-specific and year-specific effects, and also include oil prices and exchange rates as control variables that affect the states' economies separately from military expenditures. The effects of the recent military drawdown are estimated, and the evidence is interpreted in light of several of the new macro theories mentioned above and outlined below.

The results suggest that military spending is a significant determinant of economic activity at the state level, with a modest impact on most states and a sizable impact on those with a large exposure to the military sector. The transmission of military spending changes to personal income (and employment) appears to be nonlinear and asymmetric, with large cutbacks having proportionally larger responses than either large contract awards or small changes.

The article is organized as follows. The next section discusses three of the new macro theories, focusing on their propagation mechanisms and the evidence that has been presented for them. The follow-

[1] The general way to represent a linear translation of an input x into an output y is $y = mx + b$, where b and m are the constant and the factor of proportionality, and may take any fixed values. Widely used examples of linear models include vector autoregressions, most "real business cycle" models, and many large-scale policy models like those reviewed in Hickman (1987). The main advantage of linear models is their ease of use.

[2] Linear models can generate cycles, but the cycles are not sharp or unpredictable.

[3] The OPEC oil shock of 1979 was followed by a brief recession in 1980 and a longer and very deep recession that lasted from July of 1981 until November of 1982. Monetary policy probably played a role both in the sharp recovery in the second half of 1980 and in the length and severity of the second recession. Hamilton (1983) has argued that oil price rises have preceded *most* postwar recessions, although the magnitudes in the pre-OPEC cases were not sufficient to account, in a linear framework, for a large fraction of the downturns.

[4] Kodrzycki (1995) estimated that military employment cutbacks directly accounted for a 1.1 percent reduction in total nonagricultural employment over the 1989–94 period, an average of about 0.2 percent per year. Thus the models exhibit an employment "multiplier" of about 1, that is, no net employment reductions in employment spill over to the civilian economy. Multiplier results are discussed further below, in sections I and II.

[5] For instance, Shea (1993) found that aggregate military spending was not a significant determinant of any 2-digit manufacturing industry's output; both Rotemberg and Woodford (1992) and Guthrie (1993) reported wide ranges of possible responses, with the former including "no effect."

ing section presents the data and the model used to estimate the military spending–economic activity relationship, and the estimation results. A final section discusses the results in light of these new macro theories.

I. Some New Macro Theories

The earliest of the new theories that we consider is the sectoral shifts hypothesis, proposed by Lilien (1982). It begins with the fact that most economic shocks do not have a uniform impact, but rather alter the relative rewards from investing and working in different sectors. If these shocks are large or persistent enough, then individuals and firms respond to the changed reward structure. They quit or are laid off from jobs, invest in new education, and buy or sell plant and equipment. These reallocations of resources across sectors are often slow and costly. Lilien claimed that such reallocations account for a large share of unemployment in recessions; that assertion's strong and controversial policy implications have led most empirical work to concentrate on measuring its validity. (Since the theory interprets most measured unemployment as productive investment in creating new worker–job matches, government policy should attempt, if anything, to facilitate the transfer of resources, rather than expanding output as would be the case if unemployment arose from insufficient aggregate demand.)

Much less has been made of the implications that the theory has for the economy's response to shocks (changes in exogenous variables). One implication is that aggregation may obscure important information. For instance, a reallocation of federal spending from military hardware to medical services may involve no change in the amount of overall government demand, yet have large effects on areas concentrated in those industries. Macroeconomics has seen increasing use of disaggregated data in recent years for a variety of reasons; such data may be necessary rather than just helpful in identifying transmission mechanisms.

A second implication is that both adverse and favorable shocks have reallocative components which reduce output and income. This is clearest in the case of adverse shocks. For example, if a region is hit with a reduction in defense contracts, it will suffer a direct loss of income (the size of the contracts) and some spillover losses to adjacent industries as well. It will also experience some "sectoral shifting"—diversion of labor and capital from their now less attractive positions to search for jobs and investment opportunities in industries and regions that have relatively better prospects. The theory also claims that favorable shocks lead to some diversion of resources from current employment to search for new opportunities, which shows up as a muting of employment and investment gains. While it is true that job quits rise in expansions, it seems unlikely that the increased frictional unemployment from favorable shocks would lead to a significant muting of their expansionary effects. However, linear models imply that responses to equal but opposite shocks are mirrors of each other, and they will be misspecified if the sectoral shifts hypothesis is empirically important.

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The second theory considered here was developed originally for the analysis of inventory investment. The models from this literature, referred to as "*S,s*" models, have also been used in studies of money demand and durable goods consumption. The theory builds from the idea that "fixed costs" are often associated with an action, in addition to costs that vary with its magnitude. For example, training represents a substantial per-worker fixed cost that must be paid when increasing the size of the work force in many occupations and industries. In general it is optimal to act only when the benefits of an action outweigh its costs. Thus it would not make sense to hire and train new workers for small increases in labor demand; that demand should be met with increased hours from existing workers. The values S and s refer to the boundaries of the "zone of inaction," shown in Figure 1a. Continuing the hiring example, the benefits from having additional workers—reduced overtime pay, better-rested workers, or simply the ability to meet demand—will offset the costs at some point, where the firm should hire and pay training costs.

Figure 1a

Employment Adjustment Policy with Fixed Costs

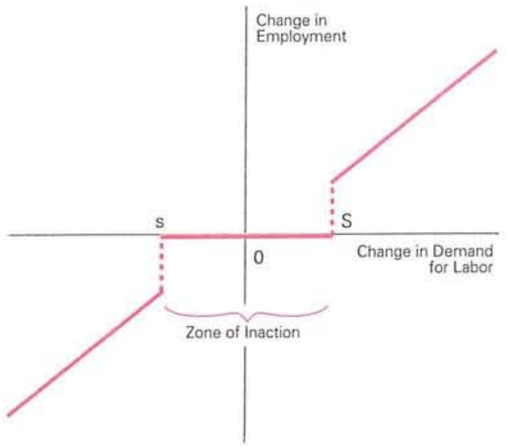
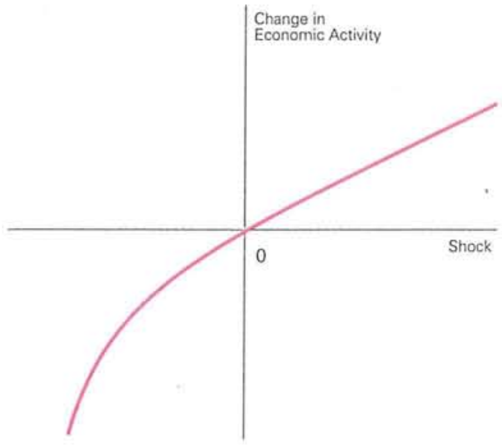


Figure 1b

Financial Accelerator Propagation Mechanism



At the individual level, *S,s* behavior generates an important nonlinearity: Shocks that leave the target variable within the boundaries have no observable effect, while larger shocks do. This nonlinearity will also be present in the aggregate either if shocks to different firms have enough of a common component or, as discussed in Cooper and John (1988), if the linkages across firms' behavior are sufficiently strong. Also, *S,s* models do not make the asymmetry prediction that the sectoral shifts hypothesis does. If military spending shocks to a region affect many different firms, then this analysis allowing for differential responses across different shock sizes has the potential to detect *S,s* behavior.

In order to distinguish between *S,s* models and other models where larger shocks also have larger than proportional effects, this study will compare the impacts of military spending shocks to two different dependent variables: employment and personal income. Employment, as just described, is thought to involve substantial fixed costs associated with both hiring and firing, while income should adjust more smoothly. Therefore, no response in employment to small shocks, but responses to larger ones (a nonlinear response of employment to military spending shocks) and a more linear response of personal income, would be evidence in support of *S,s* employment effects.

The third theory examined here, referred to as the financial accelerator, emphasizes the importance of limitations on credit.[6] In an ideal financial market, anyone could obtain financing for a profitable investment project. However, owing to a variety of market "imperfections," attributes of borrowers and lenders matter as well. One important imperfection is that borrowers have information that lenders do not about the quality of investment projects and the likelihood of repayment. Therefore, lenders require protection in forms like maintenance of collateral and of a specified ratio of cash flow to debt service.

While these forms of protection are the market's response to the difficulties of debt contracts, they create a mechanism that propagates shocks to the value of collateral or debt service ability. For example, a cutback in defense procurement contracts to a region

[6] The various components of this theory have been developed largely in the context of the "credit channel of monetary policy" literature, but they describe propagation mechanisms that are not particular to monetary policy shocks.

may cause property values (and thus collateral values) to fall, which reduces the ability of *all* of the region's residents to borrow. Thus the mechanism propagates the shock from defense to other sectors, and may depress the overall level of business activity.

Another set of financial market imperfections operates on the lending side. Open market borrowing (selling corporate bonds) entails large fixed costs of underwriting and securities law compliance. These costs preclude most small firms from borrowing on the open market and render them dependent on banks and other intermediaries for financing.[7] Credit available to bank-dependent firms may be limited in a variety of ways: Banks face geographical restrictions on lending, must maintain ratios of capital to assets, and may have available funds constrained by monetary policy.[8] This again creates a propagation mechanism for shocks that limit the ability of banks in a region to make loans; for example, cutbacks in defense contracts may cause loan defaults, which diminish bank capital and reduce lending in the area.

These financial accelerator propagation mechanisms are likely to have nonlinear and asymmetric features.[9] Since many of the limitations on firms' ability to borrow and banks' ability to lend are expressed and enforced as ratios, such as loan to value, cash flow to debt service, or capital to assets, these limitations bind in some ranges and not in others. A small shock may reduce a firm's ability to borrow, while a larger shock may eliminate, rather than just further reduce, that ability. No such threshold exists for favorable shocks. Figure 1b illustrates the relationship, assuming that the firm begins from a "neutral" position.[10] Increases in demand lead to proportional increases in output, while decreases in demand beyond a point cause the firm to face financing difficulties. Output then falls more than the drop in demand warrants, because inventory shortages lead to stock-outs, suppliers withhold shipments, and so on. Again, as in the *S,s* case, these nonlinearities are at the individual but not the aggregate level; sufficient aggregate components in shocks or linkages across firms and sectors will mean that they are also exhibited at the aggregate level.

A cutback in defense procurement contracts to a region may cause property values to fall, which reduces the ability of all of the region's residents to borrow—an example of the financial accelerator mechanism.

Considerable evidence has been found at the microeconomic level for each of these theories.[11] Several sources also provide evidence that these types of models, with their nonlinear and asymmetric features, can deliver on the promise of a better understanding of business cycles. Rissman (1993) found that incorporating the effects of sectoral shifts improves the stability of the Phillips curve (relating inflation and unemployment). Caballero, Engel, and Haltiwanger (1995) and Abel and Eberly (1995) have found that allowing for *S,s* behavior in models of employment and investment, respectively, yields substantial improvement relative to linear models. Bernanke and Gertler (1995) have shown that the credit channels help remedy some gaps in the traditional monetary policy transmission mechanism.[12]

Another supporting body of evidence comes from

[7] One can think of this as an *S,s* mechanism where small firms are always in the "do not issue bonds" zone of inaction. A shock that made it worthwhile for a small firm to issue bonds would also make it a large firm.

[8] Deregulation of interstate banking and of many activities of thrift institutions in recent years has reduced geographical restrictions. On the other hand, capital-asset restrictions have been both increased and implemented more strictly. In addition, some less-regulated bank competitors like finance companies have increased their market share in recent years.

[9] A study such as this one will not be able to distinguish whether the mechanism is operating primarily through shocks to firms' ability to borrow or through banks' ability to lend. Answering this question requires a more detailed examination of data at the industry and firm level, like that of Peek and Rosengren (1995). It is interesting to note that they find evidence that both the supply of and demand for bank loans were unusually weak in New England in the 1990–91 recession, when the region was suffering from large military spending cutbacks.

[10] The starting position of the firm or economy also matters, as it does in the *S,s* case: A small shock to a high-debt firm is like a large shock to a low-debt one.

[11] See, for instance, Starr-McCluer (1993), Eberly (1994), Gertler and Gilchrist (1994), Sharpe (1994), and Peek and Rosengren (1995) and the references therein.

[12] The traditional view has held that a monetary easing decreases short-term interest rates, which lead to lower long-term interest rates because they are substitutes from the lenders' point of view, which lead to increased investment. The main weaknesses in the story are that long-term rates often do not respond to short rate changes, and that many types of investment are not very responsive to interest rates. The first credit channel instead (or in addition) suggests that lower interest rates—both short-term and long-term—reduce debt service and thus improve the creditworthiness of firms.

some recent atheoretical econometric work. Several authors, including Hamilton (1989), Rappoport and Reichlin (1989), and Potter (1995), have found that models with thresholds, switching between different regimes, and other nonlinear propagation mechanisms appear to describe and forecast many macroeconomic time series reasonably well. The atheoretical models do not provide an economic rationale for their propagation mechanisms, but they do suggest that taking nonlinear behavior into consideration will aid in understanding the business cycle.

II. Empirical Analysis

This section turns to the empirical work, which attempts to measure the propagation mechanism translating military spending shocks to state personal income and employment.

The Model

Military spending effects are assessed using a reduced-form panel regression model. The dependent variable is growth in real, per capita personal income, and the military spending variable is the change in real, per capita procurement contracts awarded. Observations are for the 50 states plus the District of Columbia, for the years 1963 to 1994. Procurement spending is used because it is convincingly exogenous to state economic activity, whereas other types of military spending (especially personnel on bases) may not be;[13] it also accounts for approximately 30 percent of the total defense budget and nearly half of the current drawdown, so it is an important component of military spending. Per capita personal income is one of the best measures of state economic activity, and it is readily available back to 1963 (the beginning of the procurement data set); the results are also compared to those that Hooker and Knetter (1995) obtained with employment growth as the dependent variable in a similar framework.

The reduced-form system of equations estimated here follows the approach of Marston (1985) and Davis, Loungani, and Mahidhara (1995), who analyzed the behavior of state unemployment rates. State-specific constants are included to capture features that are relatively constant over time but vary across states, like industry mix, weather and geography, and laws and institutions. Similar time-specific constants capture the impact of factors common to all states that vary over time, such as demographic trends and aggregate demand and supply disturbances.

In addition to these fixed effects, other possible sources of variation in state personal income are considered.[14] Most shocks to regional activity are difficult to measure, but two that can be measured are changes in oil prices and exchange rates. Hamilton (1983) found that oil price shocks generally, and not just those caused by OPEC in the 1970s, are associated with aggregate downturns, and Keane (1993) found that oil prices served well as a control, accounting for most of the variance around trend in a panel of real wages.

The experience of the mid-1980s in the United States suggests that real exchange rate fluctuations may also have important differential impacts on regional economic activity due to state variation in exposure to international competition. By specializing in financial services, for instance, New York is more insulated from fluctuations in the dollar than is Michigan, which is dependent on conditions in the automobile market and thus subject to much greater pressure from international producers. Singleton (1993) provides some evidence on this relationship.

The equation to be estimated here thus relates the growth rate of real, per capita personal income, *PY*, to the annual change in real per capita procurement contracts, ΔMIL; the percentage change in real oil prices, *OIL*; and the percentage change in the trade-weighted exchange rate, *EXCH*.[15] With *MIL* entered in changes, the equation implies that a permanent change in procurement spending will not affect the long-run growth rate of personal income in a state; tests indicate that the data do not reject this constraint.

[13] If the allocation of military spending across states is jointly determined with states' income—as is the case when states are spared base closures because of their poor current economic conditions—then ordinary least squares regression techniques produce biased estimates. To obtain unbiased and consistent estimates, it is necessary to instrument for military spending with variables exogenous to state economic conditions. Hooker and Knetter (1995) discuss the statistical evidence and a priori arguments that procurement spending is exogenous to state economic conditions.

[14] Omitting from the equation exogenous factors that both affect personal income growth and are correlated with military spending causes the estimates to be biased. Of course one of the main reasons why military spending has been used as an instrumental variable in macroeconometric research (for example, Hall (1988) and Ramey (1989)) is that it is driven largely by noneconomic factors, making it unlikely that it would have systematic correlation with other exogenous factors. This means that the effect of omitted variables on the military spending coefficients will primarily be higher variance.

[15] Real oil prices are defined as the producer price index for crude oil divided by the GDP deflator, and the exchange rate is in foreign currency per dollar.

Personal income and procurement contracts each vary across states, indexed by i, and years, indexed by t. Oil prices and exchange rates are common to states and thus vary only by year, and year-specific and state-specific effects are denoted Θ_t and Λ_i, respectively. Oil prices and exchange rates are entered with a one-year lag to capture the delay in their effects on the economy.

Thus, the basic equation to be estimated is

$$PY_{it} = \Theta_t + \Lambda_i + \sum_{j=0}^{J} \beta_j \Delta MIL_{i,t-j} + \phi_i OIL_{t-1} + \delta_i EXCH_{t-1} + \varepsilon_{it}. \quad (1)$$

J is the number of lags of ΔMIL; at least two are included based on estimates of the duration of contracts (a contract awarded in one year has a typical spend-out pattern of 60 percent in the first year, 30 percent in the second, and 10 percent in the third, although this varies considerably across types of contracts). The coefficient on MIL is constrained to be equal across states (some constraint is needed for variation in spending across states to identify the responses); the constraint is relaxed, allowing different responses to different-sized MIL changes, below. Oil price and exchange rate coefficients vary across states, with only a single lag included, again to conserve degrees of freedom. The error term, ε_{it}, captures the influence of unmodeled factors on state personal income and is assumed to be uncorrelated across time and across states.[16]

Estimates of the β coefficients may be biased towards zero for at least two reasons. First, the MIL data contain some measurement error; not all of the work on a procurement contract is performed in the state to which the contract was allocated. Simulations indicate that for plausible values of the parameters, the bias may be as much as 30 percent. The second source of bias is migration; people systematically leave states with relatively poor economic conditions for states with better prospects. This will tend to reduce both MIL and PY values when they are large and increase them when they are small, since population is in the denominator of both variables, resulting in a reduced correlation between them.

The Data

Personal income data come from state tables on personal income, population, and per capita personal income from the U.S. Department of Commerce, Bureau of Economic Analysis. The military spending variable is prime contract awards over $25,000 by the Department of Defense for procurement of supplies, R&D, services, construction, and civil projects.[17] Not all contract work is performed in the state where the contract is awarded; construction and service contracts are attributed to the state where the largest dollar amount of work was produced, while contracts for transportation and communications services are allocated to the state where the contractor's home office is located. A few negative contract values occur, when more contracts are canceled than extended to a state in a particular year; this is the primary reason for entering MIL in differences rather than percentage changes. Both the state contract data and the personal income data are then deflated by the U.S. GDP deflator and state population in each year so that they are in real, per capita terms before taking differences or percentage changes.[18]

Figure 2 displays the average level of real procurement spending per capita for the 50 states and the District of Columbia over the 1963–94 period. The chart reveals large differences across the states. Figure 3 shows that the time series behavior of procurement spending also differs substantially across states. In the 1980s, for example, contract awards fell much sooner in Connecticut and California than in Massachusetts. Differences in amplitude are also large; for instance, California and Kentucky have very smooth paths of spending while those in Missouri and Connecticut are subject to substantial, sudden increases and decreases. This heterogeneity of experience across states and across years aids us in determining both the magnitude of the contributions of procurement spending to state economic activity and its nature. The fact that contracts are awarded in one year, but spent over several, smooths the economic impacts of large

[16] One state or year effect must be omitted in estimating equation (1). Lagged dependent variables were allowed for, in case personal income growth adjusts gradually to changes in the variables on the right-hand side of (1), but they were insignificant. With lagged dependent variables, the model should be estimated in differences via two-stage least squares because panel estimation with fixed effects and lagged dependent variables yields inconsistent estimates of the true parameters. However, as (1) is written it may be estimated via ordinary least squares.

[17] For part of the sample, the cutoff value for the data base was $10,000 per contract. Since prime contracts between $10,000 and $25,000 accounted for a small fraction of total contract awards, this definitional change in the series has been ignored. In any case, it probably affected all states similarly, so it would have little impact on the results.

[18] Gross State Product deflators may be constructed from data available through 1992; the U.S. data include figures through 1994. Using GSP deflators produced very similar results.

Figure 2

Average Level of Procurement Spending per Capita, by State, 1963 to 1994

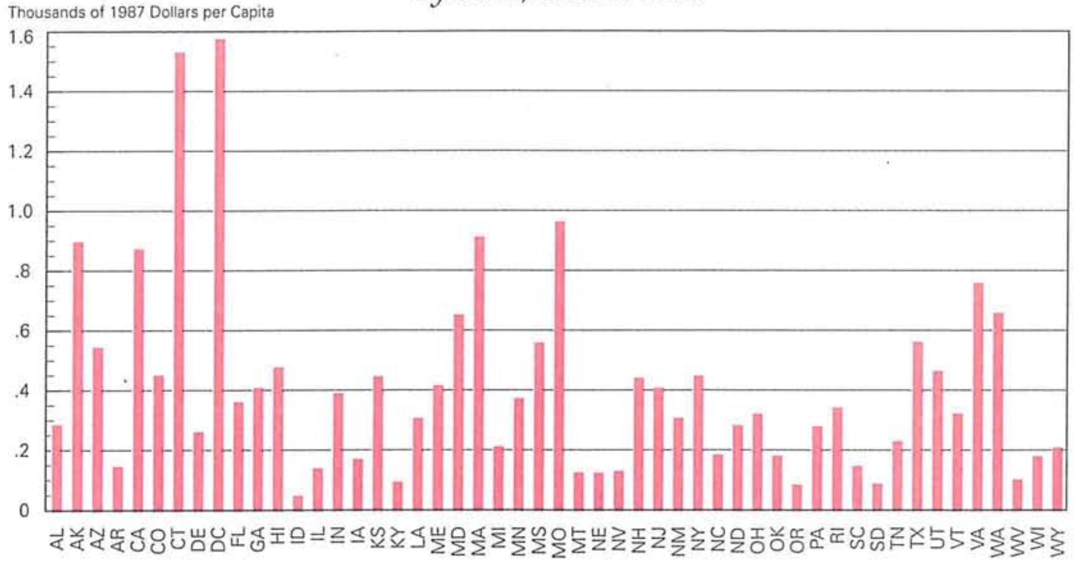
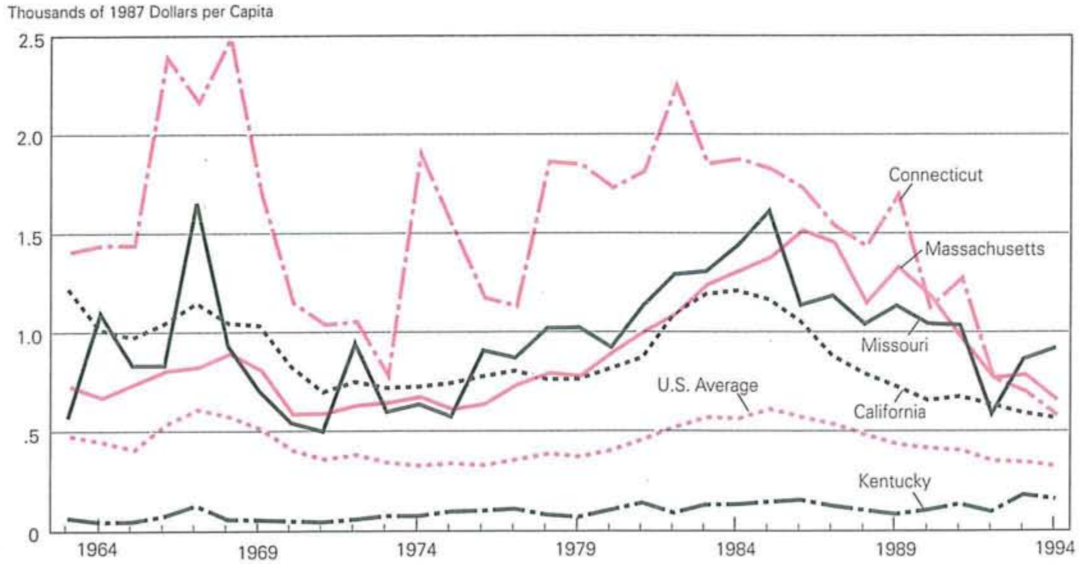


Figure 3

Procurement Spending in Several States



changes, and the lags allowed for in equation (1) should pick this up. One final point is that the personal income data for three states with small populations, Alaska and the Dakotas, display anomalous behavior. This may be due to their size (relatively small changes then generate large percentage changes). Given that together they represent less than 1 percent of the population or of GNP, their inclusion or omission should not be important, but dropping them reduces the standard error of the regression by nearly one-third. The analysis is therefore carried out using the remaining 48 states (including Washington, D.C.).

Estimation and Results

The estimated coefficients on the military spending variable from equation (1) are presented in Table 1. The time- and state-specific coefficients are difficult to interpret: Since a state or a time period must be omitted to estimate the equation (not all of them are econometrically identified), the estimates are scaled relative to the omitted state or year. The same holds true for the exchange rate and oil price variables since they do not vary across states. Those coefficients accordingly are not reported, to save space.

The estimated military spending coefficients are positive and strongly significant (their sum is 3.90 with a t-statistic of 5.20). The 95 percent confidence interval for this sum spans 2.43 to 5.37, showing that the disaggregated data have indeed provided a more precisely estimated effect than typically found in the literature. The point estimate of 3.90 implies that if a state's procurement expenditures were to rise by one thousand 1987 dollars per capita—roughly bringing it from the lowest to the highest average level of *MIL*—the estimated impact would be a temporary increase of almost 3.9 percent in real, per capita personal income (ignoring the biases). The average change in *MIL* in the data set is 0.08, implying more common effects in the 0.3 percent range for personal income.

Equation (1) relates changes in procurement to growth rates of income, which implies that the "multiplier" for procurement spending—how much income $1 spent in a state generates—is not constant across the range of personal income data. The multiplier may be computed at any particular value, however; for example, a per capita reduction in procurement of $100 (in 1987 dollars) is estimated to reduce the growth rate of personal income by 0.39 percent, which for a state with the average level of per capita income ($12,500) translates to only $49, less than the direct spending itself. Such a result is theoretically possible—the standard argument is that government spending "crowds out" some private sector spending via increases in interest rates and monopolization of finite productive resources—and several authors, including Hall (1986) and Barro (1981), have estimated multipliers for government spending that are less than one. However, it seems likely that the mismeasurement of the contract locations and migration, discussed above, are contributing a significant downward bias to the multiplier estimate. (Hall's estimate for the multiplier was 0.62; combining Kodrzycki's (1995) estimates of military employment reductions with the CBO unemployment projections gives a multiplier of approximately 1.)[19]

Table 1
Key Parameter Estimates for Basic Equation

A. Estimates for Personal Income Equation

$$(1)\quad PY_{it} = \Theta_t + \Lambda_i + \sum_{j=0}^{J} \beta_j \Delta MIL_{i,t-j} + \phi_i OIL_{t-1} + \delta_i EXCH_{t-1} + \varepsilon_{it}$$

lag: 0	$\beta_0 = 1.27$ (3.40)	
lag: 1	$\beta_1 = 1.87$ (5.45)	
lag: 2	$\beta_2 = .77$ (2.52)	
	$\Sigma\beta = 3.90$ (5.20)	

B. Estimates for Employment Growth Equation

lag: 0	$\beta_0 = .64$ (2.58)	
lag: 1	$\beta_1 = .44$ (1.37)	$\rho_1 = .35$ (2.86)
lag: 2	$\beta_2 = .85$ (3.10)	$\rho_2 = -.30$ (−4.18)
	$\Sigma\beta = 1.93$ (2.74)	

Note: For the personal income equation, SSR = 3407.76; SEE = 1.54; N = 1440 (sample 1965–94). Lag lengths chosen according to Schwartz-Bayes criterion. Heteroskedastic-consistent t-statistics in parentheses. First year-dummy excluded for identification; year and state dummies, and OIL price and EXCHange rate coefficients not reported. ρ's in employment growth equation are coefficients on lagged dependent variable; estimation is in differences with instruments for the first lag of the dependent variable. See Hooker and Knetter (1995) for details.

[19] See footnote 4 above.

The estimates obtained by Hooker and Knetter (1995) using employment data in the corresponding specification also imply a small but precisely estimated expansionary effect. Their results are reproduced in part B of Table 1; the ΔMIL coefficients are 0.64, 0.44, and 0.85 on the contemporaneous change and two lags, respectively. The sum of the coefficients is 1.93 with a t-statistic of 2.74, so again the 95 percent confidence interval does not include zero. These numbers imply that a \$100 per capita increase in a state's contracts increases its employment growth rate by about 0.2 percent, roughly the same magnitude as the effect on personal income.

Next is an estimation that allows for nonlinear and asymmetric effects. The distribution of changes in MIL is fairly tightly concentrated around zero, with over 80 percent of the observations representing changes of less than \$100 per capita in 1987 dollars. However, that still leaves several hundred larger changes with which to identify different effects from different-sized shocks. A cutoff level of \$100 per capita (in 1987 dollars) for ΔMIL was chosen to distinguish large from small changes. Then, using dummy variables, reductions in MIL of more than the cutoff level, increases in MIL of more than the cutoff level, and changes smaller than the cutoff level each may take separate coefficients. The equation is then

$$PY_{it} = \Theta_t + \Lambda_i + \sum_{j=0}^{2} \alpha_j(\text{DLGN}*\Delta MIL_{i,t-j}) + \sum_{j=0}^{2} \beta_j(\text{DSM}*\Delta MIL_{i,t-j}) + \sum_{j=0}^{2} \gamma_j(\text{DLGP}*\Delta MIL_{i,t-j}) + \phi_i OIL_{it-1} + \phi_i EXCH_{it-1} + \varepsilon_{it} \quad (2)$$

Table 2

Key Parameter Estimates for Unconstrained Model

A. Estimates for Personal Income Equation

$$(2) \quad PY_{it} = \Theta_t + \Lambda_i + \sum_{j=0}^{2} \alpha_j(\text{DLGN}*\Delta MIL_{i,t-j}) + \sum_{j=0}^{2} \beta_j(\text{DSM}*\Delta MIL_{i,t-j}) + \sum_{j=0}^{2} \gamma_j(\text{DLGP}*\Delta MIL_{i,t-j}) + \phi_i OIL_{t-1} + \phi_i EXCH_{it-1} + \varepsilon_{it}$$

	large ⇓'s	small Δ's	large ⇑'s
lag: 0	$\alpha_0 = 1.11$ (2.01)	$\beta_0 = .19$ (.15)	$\gamma_0 = 1.05$ (1.61)
lag: 1	$\alpha_1 = 2.55$ (3.88)	$\beta_1 = 1.59$ (1.41)	$\gamma_1 = 1.00$ (1.77)
lag: 2	$\alpha_2 = 1.50$ (3.17)	$\beta_2 = 1.30$ (1.02)	$\gamma_2 = .23$ (.35)
	$\Sigma\alpha = 5.15$ (5.45)	$\Sigma\beta = 3.07$ (1.43)	$\Sigma\gamma = 2.28$ (1.95)

B. Estimates for Employment Growth Equation

	large ⇓'s	small Δ's	large ⇑'s	
lag: 0	$\alpha_0 = .83$ (2.00)	$\beta_0 = .42$ (.46)	$\gamma_0 = .52$ (1.39)	
lag: 1	$\alpha_1 = 1.26$ (2.63)	$\beta_1 = -.66$ (−.59)	$\gamma_1 = .07$ (.14)	$\rho_1 = .26$ (2.14)
lag: 2	$\alpha_2 = 1.51$ (3.86)	$\beta_2 = .30$ (.31)	$\gamma_2 = .47$ (1.04)	$\rho_2 = -.25$ (−7.02)
	$\Sigma\alpha = 3.60$ (3.56)	$\Sigma\beta = .06$ (.02)	$\Sigma\gamma = 1.05$ (.95)	

Note: For the personal income equation, SSR = 3420.46; SEE = 1.54; N = 1440 (sample 1965–94). Lag lengths chosen according to Schwartz-Bayes criterion. DLGN, DSM, and DLGP are dummy variables which equal one if the variable ΔMIL is less than −\$100 per capita, between −\$100 per capita and \$100 per capita, and greater than \$100 per capita, respectively. Heteroskedastic-consistent t-statistics in parentheses. Year dummies, OIL price, and EXCHange rate variables included but coefficients not reported. ρ's in employment growth equation are coefficients on lagged dependent variable; estimation is in differences with instruments for the first lag of the dependent variable. See Hooker and Knetter (1995) for details.

where DLGN is a dummy variable which takes the value 1 if the $\Delta MIL < -100$ and zero otherwise, DSM likewise is 1 if $-100 < \Delta MIL < 100$, and DLGP is 1 if $\Delta MIL > 100$.

The results of this estimation are presented in part A of Table 2. The first column shows that large reductions in procurement expenditures have well-determined effects that are much larger than those estimated in Table 1, while large increases (third column) have effects that are similar in size to those in Table 1, but are less significant. An *F*-test for equality of the coefficients on the large decreases and large increases can be rejected at about the 10 percent level of significance. Small changes (of either sign) also have effects similar to those in the restricted specification, but are not significantly different from zero. The sum of the coefficients on large decreases is 5.15, and again zero lies well outside the 95 percent confidence interval; the bounds are 3.29 and 7.01.

The estimates using employment growth display a similar pattern. Hooker and Knetter's results, reproduced in part B of Table 2, show that the effects of large reductions (using the same $100 per capita threshold) are significant, while the effects of both small changes and large increases in procurement spending are small and not significantly different from zero. The sums of coefficients (and *t*-statistics) corresponding to the estimates in Table 2 are 3.60 (3.56), 0.06 (0.02), and 1.05 (0.95) on large decreases, small changes, and large increases, respectively.

One way to translate these estimates into magnitudes implied for actual data is to compute the effects of recent years' reductions in procurement spending. This is illustrated in Figures 4 and 5 showing the change in the growth rate of real per capita personal income attributable to the drawdown, for the United States as a whole, and for seven states that suffered particularly large cutbacks. Using actual state procurement data, each state's response is calculated and then a population-weighted average is taken. The shaded bars use the estimates from Table 1, where each response is constrained to be equal, and the white bars allow the responses to vary depending on the size of the shock, according to Table 2.

As Figure 4 shows, the estimates of the overall effect of the drawdown are increased somewhat when the Table 2 estimates are used. The overall contribution of the drawdown is still estimated to be moderate, however: The peak years of the drawdown reduce the growth of real per capita personal income by two-tenths of a percentage point, from its state-year average of 2.1 percent to 1.9 percent. Figure 5 shows that allowing for nonlinearity yields much larger effects in seven states that underwent large cutbacks (Arizona, Connecticut, the District of Columbia, Maryland, Massachusetts, Missouri, and Virginia). For these states, the nonlinear estimates are roughly double the linear estimates, implying reductions in personal income growth of half a percentage point or more each year from 1989 through 1993.

III. Discussion

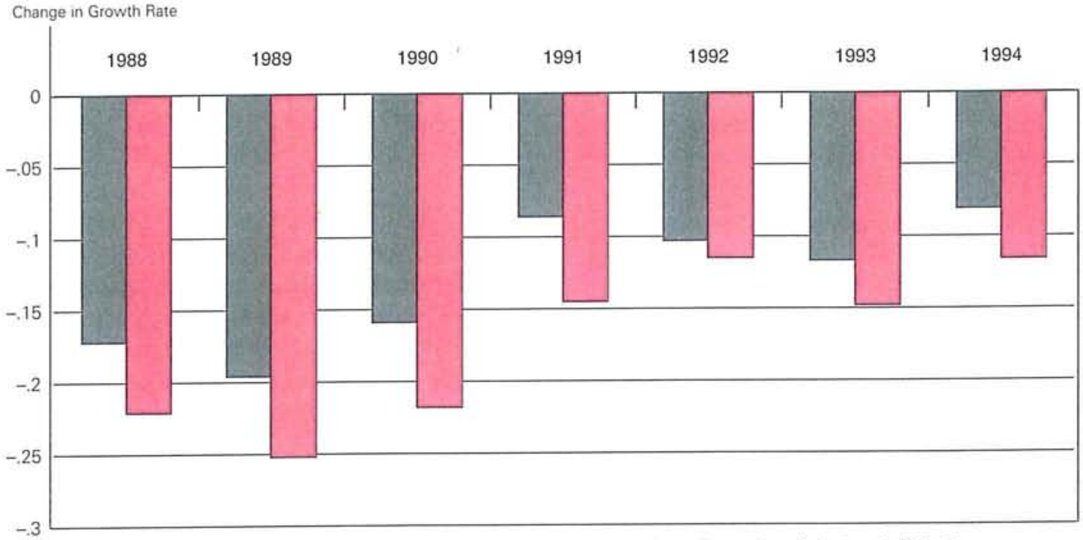
The results in Table 2 provide evidence of both nonlinearity and asymmetry in the response of state personal income growth to military spending shocks. Large cutbacks appear to have moderately large impacts, concentrated on the exposed states, while small changes of either sign and large increases have smaller, and less precisely estimated, impacts. A similar pattern of coefficients was found using employment growth as the dependent variable, and in earlier work using state unemployment rates as well.

Which of the three theories discussed earlier in this article are consistent with the results obtained? The sectoral shifts hypothesis predicts that large shocks should have more than proportionally larger impacts, and that adverse shocks should have larger effects than equal-sized favorable shocks. It is difficult to believe, however, that the sectoral reallocation resulting from new procurement contracts is sufficient to offset *over half* of the income and employment gains: The degree of asymmetry found exceeds that predicted. Since the biases in the estimation should reduce the impact of both types of shocks, they do not seem to provide an explanation. Sectoral reallocation of resources may be muting some of the expansionary effects of new contracts, but it is likely that other factors are at work as well.

The results are also somewhat consistent with the financial accelerator theories, as they predict proportionally larger responses to adverse procurement shocks than to favorable shocks, although again the weakness of the large increases is puzzling. The results are not very supportive of *S,s* theories. These theories also predict the observed nonlinearity (proportionally larger impacts of large shocks) but do not predict the asymmetry. Further, since fixed costs are thought to be important in hiring and firing, it was expected that *S,s* implications would be observed in employment to a greater extent than in income. The results indicate that estimates for the two variables are similar.

Figure 4

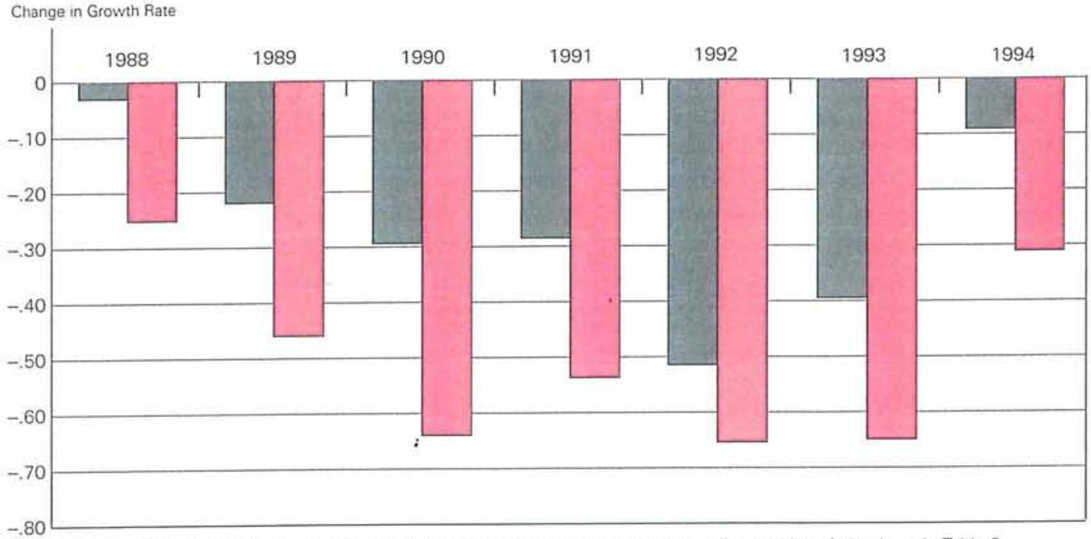
Impact of the Current Drawdown on Growth of Real per Capita Personal Income in the United States



Gray: responses constrained to be equal. Red: responses vary depending on size of shock, as in Table 2.

Figure 5

Impact of the Current Drawdown on Personal Income in Defense-Intensive States



Gray: responses constrained to be equal. Red: responses vary depending on size of shock, as in Table 2.
States included: Arizona, Connecticut, District of Columbia, Maryland, Massachusetts, Missouri, Virginia.

Several directions for future research are suggested by these results. One is to examine whether microeconomic data support the hypothesis that the financial system played an important role in propagating procurement shocks to the overall economy. A second is to examine the extent to which the effects of a shock depend on the current state of the affected economy, as the financial accelerator implies. Finally, the estimates implying that large reductions in procurement have much stronger impacts on the economy than like-sized increases are difficult to explain, and stand as a challenge for future research.[20]

[20] It is interesting to note that several authors, including Mork (1989), Dotsey and Reid (1992), and Hamilton (1996) have found evidence that oil price drops have no impact, while increases are contractionary, and that Cover (1992) has found that expansionary monetary shocks have no impact while contractionary ones do.

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