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Theory: The political business-cycle theory is used to test for state-level accelerations in contract awards and deobligations induced by presidential elections. Hypotheses: Government contracting agencies accelerate contract awards, and reduce the level of deobligations, immediately before presidential elections. The cycles should not involve any total increase in spending. Methods: Pooled-time-series analysis of monthly Department of Defense and civilian agency contract awards and deobligations in each state between October 1985 and December 1992. Results: Both contract awards and deobligations behave as expected. Civilian agency contract awards rise immediately before presidential elections and primaries, and both Department of Defense and civilian agency deobligation activity virtually cease as the elections approach. Although the total level of activity is unchanged in election years, the cycles are clear. This supports the notion that the cycles are important politically even though they have little macroeconomic impact.

Critics of the literature on political-business cycles assert that, despite evidence that such cycles exist (Nordhaus 1975, 1989; Hibbs 1987; Tufte 1978), presidents and policy makers are unable to orchestrate economic expansions to coincide with elections.⁴ The dependent variables in most studies of political-business cycles (growth rates, inflation, unemployment) “are only indirectly controllable by government” (Weatherford

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¹The contract-award data used in this analysis are available from two sources. The Federal Procurement Data Center (FPDC, in Vienna VA) provides at moderate cost raw data on individual contract awards for all federal agencies. A commercial software firm, Eagle Eye Software, also in Vienna, licenses a CD-ROM database version of the FPDC data; my analysis uses this version.

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1988, 105), and Kettl (1991, 239) concludes that voter expectations of the president vis-à-vis the economy far exceed his limited capabilities. Even when models detect economic accelerations that coincide with elections, they cannot explain how policy makers make and implement the decisions necessary to create those accelerations. "The most notable characteristic of [political-business-cycle] work," concludes Weatherford (1988, 113, emphasis in original), "is that the ability to model outcomes has advanced far beyond our ability to explain process."

Advances in our understanding of political-business cycles, and success in deciding whether they even exist, require research on the economic policy tools used by policy makers, and on the implementation process that translates decisions into economic outcomes. Explored here are so-called electoral-contracting cycles, or the intentional clustering of government contract awards around elections to create short-term economic benefits that create voter goodwill on election day. Contract awards are a likely candidate for electoral manipulation, as they are easy to time and have swift economic effects. The Department of Defense timed about $3 billion in prime contracts to coincide with national elections (Mayer 1991, 195), with no corresponding increase in nonelection years.

The previous analysis of contract accelerations had a number of flaws, however. First, the positive findings were confined to the pre-1976 period, when the fiscal year ran from July 1 to June 30. After 1976, when the federal government switched to October 1–September 30 for a fiscal year, the electoral-effects disappeared, absorbed into the huge increase in contract awards that occurs in the last month of the every fiscal year (Mayer 1991, 202). Second, the analysis used only national totals for contract awards, so there was no way to test for targeted accelerations that might have occurred in key states, or for other measures of contracting activity that might vary in response to electoral pressures (contract cancellations, for example). Finally, only Department of Defense contracts were examined. There was no analysis of civilian contract activity (from agencies like NASA, the Department of Energy, or the General Services Administration) which can be substantial.

Using new data from the Federal Procurement Data Center, which permits accurate measurement of total federal government contracting activity broken down by month and state, I correct these flaws. In an analysis of these data, I test for contract accelerations during general elections and presidential primaries in 1988 and 1992. Electoral cycles in contract awards are real, but have only minuscule economic effects. Although no evidence indicates that Department of Defense contracts
correlate with elections, civilian-contract cycles appear in strategic states. A much stronger finding is that deobligations nearly cease, as contract agencies slow their activities as the election approaches. The macroeconomic effects of these cycles are undetectable, since they involve changing award dates of existing contracts by a few months rather than an increase in total activity. One key implication is that the focus on economic effects of electoral cycles may be misguided, as researchers overlook the importance of their political or credit-claiming potential. Even if electorally timed contract awards have no measurable economic impact, incumbents will time announcements for maximum political impact.

Contract cycles form a critical test of the political-business cycle theory, because they rely on an instrument directly under the control of actors who possess both the ability and motivation to use it. Though I find evidence of electoral cycles, the lack of any significant economic effect implies limits on what incumbents can do, and what their agents will do to use policy tools for explicitly electoral purposes. In turn, this implies that electoral cycles are even less likely for other policy instruments not under the direct control of incumbents or their agents. Future research should focus less on uncovering covariance between macroeconomic indicators and elections, and more on the actual decision and implementation processes necessary to produce such cycles. In the absence of such process explanations, and given the unevenness of evidence of electoral cycles, positive findings may result more from statistical artifact than real political manipulation.

Political-Business Cycles in Practice and Theory

The study of political-business cycles is in its second generation. Following Nordhaus (1975) and Tufte (1978), scholars have continued to refine ideas about how political and economic factors interrelate, particularly with regard to elections and policymaking. The basic motivation of incumbents—the desire to tinker with macroeconomic policy to insure an election win—remains constant, but scholars have used more discerning tools to uncover the nature of the tinkering itself (Nordhaus 1989). Suzuki (1992) found that voter expectations about economic outcomes vary coincidentally with elections; voters expect lower unemployment and inflation, and also show more consumer confidence, during election years than during non-election years. Alesina and Tabellini (1989), and Alesina et al (1993) have extended work on political-business cycles into more general models of the U.S. economy and how elections affect macroeconomic outcomes.

Timing strategies appear commonplace in a number of policy areas.
Studies of defense spending in Israel show a robust pattern of election-year acceleration (Mintz 1988). Russett and Barzilai (1992) found evidence that the Arab military activity against Israel was timed to avoid coincidence with Israeli elections, because at these times a forceful retaliatory strike was most likely (since incumbent governments needed to portray an image of toughness during campaigns). Nincic (1990) argued that U.S.-Soviet relations have followed a predictable pattern based on the electoral cycle, with relations worsening in the fourth year of the presidential term. Though there is little evidence that the Federal Reserve responds to political pressure to lower interest rates in election years (Beck 1987; see Grier 1989, and Williams 1990, for contrasting views), Nordhaus (1989, 47) found that since 1914 the Fed has never changed the discount rate in the month before a presidential election.

Yet we must view this literature in light of Weatherford’s critique that past research is better at modeling outcomes than at explaining processes. Most studies of political-business cycles assume that policies are created by a unitary rational decision maker and are implemented perfectly, simplifying assumptions which violate the reality of economic policymaking in the United States. The assumption that one institution or actor in government (excluding the Federal Reserve) can control economic policy to induce electoral cycles is wrong, both theoretically and empirically. Indeed, presidents do not implement economic policy as a unitary actor, nor do they make economic policy this way. Far too many actors participate in the process who may have divergent interests from the president to support this view. In one simple and common case—divided government—the majority party in Congress and the President have opposite goals: the president would benefit from an economic expansion during the election year, but Congress has an incentive to block any such effort, in order to create a more favorable environment for the challenger. Even unified government will not remove this obstacle: Clinton’s failure to push an economic stimulus package through Congress demonstrates that even when one party controls both branches, the president may not always get what he wants (Healy 1993).

Even if policy makers had the envisioned authority to make electorally motivated economic-policy decisions, their menu of potential instruments would be limited. The president’s ability to control macroeconomic outcomes, never great to begin with, has shrunk over the past few decades as fewer resources are available to generate expansionist economic policies. The rise of “uncontrollable” spending and persistent budget deficits reduces the pool of funds available to policy makers who may want to stimulate the economy, and an increasingly interdependent global economy means that the United States will be affected by interna-
tional forces over which American political leaders have no control (Kettl 1991). Weaver (1988) notes that Congress has turned to automatic mechanisms to make cost-of-living adjustments to many major programs (such as Social Security), taking away one of Tuft's (1978) more egregious examples of political-business cycle activity in the Nixon Administration. What passes for an economic growth package in recent years may not have much impact: President Clinton’s ill-fated 1993 stimulus package budgeted only about $16 billion, an amount skeptics claimed would not matter in a $6 trillion economy.

Finally, even if presidents and others had access to the appropriate instruments, they would have great difficulty timing the effects, as most macroeconomic policies have neither swift nor predictable impact. Consider fiscal policy as an example. Not only must presidents carefully anticipate the election-time state of the economy several years in advance, they must also decide what policies to manipulate before they know what types of adjustments the economy will need: expansionist tinkering when the economy is booming may lead to serious inflationary spirals. Effects are difficult to time because most macroeconomic tools under the president’s control do not have much effect until well after they are implemented, a stage which may trail the original decision by years.

The only consensus in the literature is that political-business cycles may occur occasionally, but are dwarfed in their effects by more important economic variables. Keech and Pak (1989, 909), for example, concluded from their investigation of electoral cycles in veterans benefits that "electoral cycles may exist in certain programs in certain times, but . . . such cycles are not an important general political phenomenon." In the same vein, Alt and Chryystal (1983, 83) summed up their view of work on political business cycles in an often cited passage:

No one could read the political business cycle literature without being struck by the lack of supporting evidence. There must be cases where politicians have undertaken electorally motivated interventions. It is difficult to imagine politicians not exploiting some extra information or other resources. But while this clearly happens, and happens particularly clearly in some cases, such cycles may be trivial in comparison to other economic fluctuations.

Tufts (1978, 32) noted that a few days before the 1972 presidential election, 25 million Social Security recipients received their November check, which incorporated a 20% increase, along with a flyer stating that the law granting the increase was "signed into law by President Richard Nixon."
Studies that find electoral cycles in economic policy should be able to explain how the incumbents control the relevant policy tools, who controls the levers, and how decisions are implemented.

Electoral Cycles in Government Contract Awards

Analyzing government contract awards as the possible instrument for electorally timed economic cycles has several advantages over investigating broader macroeconomic instruments. First, one can abandon the unitary rational actor assumption and specify how the acceleration process works, identifying the relevant actors and focusing on the actual sequence of events that leads to the observed result. Second, the instrument—prime contract awards—is both easy to time (Fisher 1975) and has almost immediate economic effects (Greenberg 1967). Third, the amount of money available for tinkering is large—well over $1.4 trillion in the period studied here (1985–1992).

Finally, anecdotal evidence abounds of politically motivated announcements of government contracts. Consider the following examples from the 1992 presidential election. In September 1992, Bush announced approval of a $9 billion sale of F-15s to Saudi Arabia (Rosenthal 1992)—which led to a proposed balancing sale of AH-64 Apache and UH-60 Blackhawk helicopters to Israel (Tolchin 1992)—and promised to lift a decade-old ban on advanced arms sales to Taiwan, which would lead to a $5.8 billion order for F-16 fighters (Sutherland 1992). In October 1992 Vice President Quayle announced in Philadelphia that Bush had decided to support the V-22 Osprey aircraft, a tilt-rotor plane that Bush had been trying to cancel since 1989 (Schmitt 1992b). These announcements were made in front of people employed by the relevant contractors: the V-22 is built by Boeing and Bell Helicopters in Philadelphia, the F-15 by McDonnell-Douglas in St. Louis, the F-16 by General Dynamics in Ft. Worth (since sold to Lockheed). The affected states (Missouri, Pennsylvania, and Texas) were considered competitive “must wins” for Bush, and all had been hurt by defense cutbacks. The F-15 and F-16 sales alone would have saved 10,000 jobs in St. Louis and Fort Worth, and several thousand more at subcontractors in California, Connecticut, and Florida (Schmitt 1992a; Sims 1992). The announcements were timed to maximize the electoral impact: one Department of Defense official admitted that “the timing of [both the F-15 and F-16] decisions owed much to the U.S. campaign season” (Diehl and Von Drehle 1992).

The Timing Model: Assumptions, Actors and Process

The hypothesis of the electoral-contracting cycle is that the timing of contract awards changes—either accelerated or slowed—so that the
awards coincide with presidential elections. The assumptions behind this model are straightforward and weak, and support the argument that contracts are a prime candidate for electoral manipulation.

The key assumption is that timing strategies do not require any increase in total defense spending or contract awards. They simply maximize political and economic advantages of existing contracts by timing awards to coincide with elections. This assumption frees the model from the familiar constraint of expansion-inflation tradeoffs. A decision to speed up a contract award by a few months does not require any subsequent balancing to counteract the consequences of the original decision (unlike the need to check inflation after a period of economic growth). Because policy makers do not have to make tradeoffs between the goals of economic stability and getting reelected, they may be more likely to implement timing strategies. In addition, tradeoffs impose a sharp limit because cycles that produce benefits during presidential election years may result in a contraction during the mid-term election year (precisely what occurred during the 1982 congressional elections). Presidents with any goals beyond simply winning the next election may be reluctant to sacrifice their congressional party in the process of working toward their own victory.

The hypothesized lack of an increase in total election-year activity suggests that the political impact of contract accelerations outweighs their macroeconomic impact. If the overall level of contract awards does not change, then any contract accelerations would produce at most only small changes in the national economy. At a micro level, however, the accelerations can still produce substantial political advantages to incumbents who want good news to deliver around election time (Anagnoston 1982, 559). This contrast between the macro (nationwide) and micro (individual) levels is critical, and overlooked by most political-business cycle research. Stein and Bickers (1994, 394) found that the macro- and micro-level benefits of particularized goods do not always move in parallel and may even be inconsistent: even though the change in the number of new grant programs in a congressional district does not affect the incumbent's electoral success, "at the individual level . . . the increase in proportion of new grant awards [is] significantly related to an individual voter's support for House incumbents."

What sorts of behavior does contract acceleration require? Most contracting decisions, both in the awarding and administration of contracts, are made within subunits of the executive branch agencies. Bureaucrats, not White House officials or legislators, actually carry out contracting activities, and direct White House involvement is rare.
Thus, the premise that policy makers generate and implement election-year cycles requires modification. Bureaucratic officials will adjust the timing of their activities to create political advantages for incumbent political actors. The ability to speed up or slow down contracting activity has been well established, and is most noticeable in the annual rush to spend money in the final month of the fiscal year (Mayer 1991, 188–91). Contracting agencies clearly have the ability to manipulate the timing of contract awards, but do they have the motive?

Bureaucrats implementing timing strategies should see a potential benefit to their organization in doing so. Contracting officials might feel external pressure to expedite procedures during an election campaign so that the awards are made at an opportune time, but that pressure has to come from somewhere. They might also conclude that their agency could benefit by creating a contract acceleration which would enhance the reelection prospects of a president who favors high defense spending. The 1988 and 1992 elections offered a clear contrast between the candidates, with the Republican incumbent more supportive of high defense budgets than the Democratic challenger. We might assume, then, that both civilian and military contracting personnel would prefer that the incumbent win. There is thus a motive to support, even if only quietly, the incumbent by speeding up contract awards to create favorable impressions among voters. Agency personnel might anticipate the needs of presidents, and create accelerations as a routine matter even in the absence of pressure or informal requests. It might be simply a smart strategy designed to foster good relations with political authorities (on this process with respect to federal grants, see Gist and Hill 1984; Anagnoson 1982; Arnold 1979; Hamman 1993).

Contract acceleration strategies, in sum, constitute a critical test for the political-business cycle. The instrument is easily implemented, not affected by long-term tradeoffs, and under the exclusive control of the executive branch. The people who are ultimately responsible for carrying out the task have an incentive to match their ability. If electoral cycles are a regular feature of the political process, they should show up here.

Data and Model Estimation

The data set used to test this model of presidential political-economic cycles is a monthly time series of federal government contract (both DoD and civilian) awards for the 50 states, from October 1985 to December 1992, stacked to form a pooled time series set. The data set was constructed from a complete record of all government contract
actions over $25 thousand and many below that amount, constituting well over 3 million separate actions. The total value of contract awards in the database was $1.445 trillion (in 1993 dollars), of which $1.06 trillion reflected DoD contract awards and $384.7 billion civilian agency awards.

The analysis uses two dependent variables that reflect different ways of measuring the impact of contract awards: the monthly dollar amount of contract awards in each state, and the monthly total of deobligations—instances where the government cancelled or reduced the value of a contract. The total awards variable tests for an overall acceleration in contract activity, which would represent the highest level of politicization. The deobligation variable reflects the government’s ability to take money back as well as to award it (between October 1985 and December 1992, DoD deobligated $52.6 billion, civilian agencies $15.4 billion). Throughout the contract administration process, the government can alter contract requirements, lower purchase quantities, exercise various options which lower the total contract value, or cancel contracts altogether. If contract agencies can change the timing of awards, they might try to avoid deobligating money around elections.

3 The Federal Procurement Data System (48 CFR 4.6) requires all federal agencies to maintain computer records of all unclassified contract actions over $25,000; defense contracts under that threshold to a firm in a “designated industry group under the small business competitiveness demonstration program” (48 CFR 204.670-2[a][2]); and civilian-agency contracts under $25,000 when the agency makes follow-up awards under the same contract and the original contract exceeded $25,000. The individually reported actions constituted only 2.5% of all contract actions by federal agencies during fiscal year 1992 (501,136 out of 20,150,974), but 89% of all contract dollars ($177.9 billion out of $199.8 billion). The exclusion of the unreported awards, despite their volume, should not pose a problem for the analysis since they are typically far too small to be of much importance: the average value of the excluded awards during fiscal year 1992 was $125.37 (U.S. General Services Administration 1993, 3).

4 The mean level of deobligations, while much smaller than the level of contract awards, is still large enough to be appropriate for an analysis of the dollar values. The mean value of monthly DoD deobligations is $40 million for top quintile states, $11.6 million for second quintile states, and $32.8 million for battleground states. Mean monthly values for civilian-agency deobligations are $10.6 million for top quintile states, $4.7 million for second quintile states, and $8.9 million for battleground states.

5 In their study of how the flow of federal grants affects the electoral fortunes of legislators, Stein and Bickers argue that the number of grants may be more politically significant than their dollar value, since “each award provides an opportunity for a legislator to demonstrate his or her efficacy . . . [and] a variety of fiscally modest grant awards provides many different constituencies the opportunity to identify a project that is closely tied to their interests” (1994, 380). Moreover, they find that constituents are more aware of the existence of a grant than its dollar value (1994, 394). To check for this effect, I ran a second set of regressions using as dependent variables the number of contract awards over $1 million and the number of deobligations exceeding $100,000.
Both variables are based on prime contract-award data, whose use as an indicator of the economic impact of defense spending has been criticized on a number of grounds. The most significant problem is that 50% or more of large prime contracts are said to be subcontracted to other firms. The distribution of prime contracts, then, may not accurately reflect the distribution of the final economic effects. However, the subcontracting problems are mitigated by several factors. First, evidence suggests that the overall distribution of subcontractors roughly parallels that of prime contractors (Malecki 1984, 1988). Second, the political advantages of a prime contract are not reduced by the fact that much of the work will not be performed by the prime contractor; this is especially important since it is possible that subcontractors pay attention to prime contract awards, and the local political officials respond accordingly.

I hypothesize that contract awards in a given month are a function of a base level of contract awards, with variation from month to month in seasonal and predictable patterns. This system will feel external shocks, especially those imposed by the presidential election cycle. In general, the model tests for evidence that the seasonal variation of contract awards during election years is different than it is for nonelection years, with bunching of awards around primary and general elections.

Coefficients for the equations based on the number of contract awards and deobligations were estimated using an event-count model for pooled time series, based on the negative binomial distribution, derived by Hausman, Hall, and Griliches (1984). This model, after reparameterization, is equivalent to King’s (1989), which can be estimated using his COUNT program. The negative binomial distribution assumes that the events are positively correlated with each other. Individual contract actions and deobligations should be correlated, because the dollar value of large programs is usually distributed (or taken back) over time as a series of contract actions; therefore, the existence of at least some of the actions in a state at any point in time depend on previous actions in that state.

The results of the event-count analysis were almost identical to the results based on the dollar value of contract awards and deobligations. Because of this similarity, I do not report the event-count-model estimates separately. The results are available from the author.

*If the model specified here is theoretically valid, the process is likely affected by other electoral shocks, especially the congressional election cycle. In a related project, I am investigating the importance of congressional elections, but justify their exclusion here on methodological grounds: if the acceleration process is driven more by congressional elections than presidential contests, the analysis here should show mostly negative results. First, since I count the midterm elections of 1986 and 1990 as nonelection years, accelerations in those years would reduce the significance of any comparable cycles in 1988 and 1992. Second, a congressionally induced cycle is more likely to occur over a longer period and to vary with the peculiar circumstances of individual races (Stein and Bickers 1994).
Since contract awards have almost immediate economic effects (Greenberg 1967), any heaping should occur shortly before the election. I test for accelerations in September, October, and November of election years, the month of and the month before a state presidential primary. I include as controls tests for similar accelerations in nonelection years. A complete listing and description of the variables is included in the appendix.

The coefficient estimates for the dollar values of contracts and deobligations were derived using OLS from a Least Squares Dummy Variable model incorporating dummy variables for each state. Pooled cross-sectional time-series data present a spectrum of problems, especially non-constant residual variance and autocorrelation (Sayres 1989; Stimson 1985). Both the Goldfield-Quart and Park-Gleiser tests (Pindyck and Rubenfeld 1982, chapter 6) indicated that the residuals had unequal variance across both lagged values of contracts and across the 50 states. Weighting the variables produced no improvement. To correct the problem, I split the states into quintiles based on total DoD and civilian-contract awards in each state, and estimated separate equations for the top two quintiles. This eliminated both sources of unequal residual variance. I also estimated a separate regression for states considered especially critical in 1988 and 1992 (I denoted these Battleground States; see Cook 1988, 1992). The states in each group are identified in the appendix. I converted all contract award values to constant 1993 dollars, using the total obligatory authority deflator for non-personnel accounts produced by DoD (Department of Defense 1992, 37) and transformed the contract data into natural logarithms to take into account diminishing marginal returns.

Since the model contains lagged values of the dependent variable, I tested for the presence of autocorrelation of up to twelfth order (which would reflect correlation of the residual in one month with the residual of the same month in the previous year) using Lagrange Multiplier (LM) techniques (Maddala 1992, 249–252). The LM test statistic, which has a $\chi^2$ distribution with 12 d.f. (the degrees of freedom are equal to the number of lag periods), is reported for each equation as the pooled-average LM and is calculated as the average of the LM values for each state in the regression. In every case, the probability that autocorrelation is present is low.

Results and Discussion

The results of estimating the model over the pooled data set for the dollar value of contract awards for both DoD and civilian agencies are shown in Table 1. The strongest indication of electoral cycles would be
evidence that awards (or deobligations) in September and October of election years are significantly higher (or lower) than the corresponding months in nonelection years. For contract awards, this can occur in two ways: either the election-year coefficients can be positive and significant and the nonelection-year coefficients zero, or the election-year coefficients can be zero and the nonelection-year coefficients negative and significant (the reverse would be true for deobligations). Either pattern would show that contract awards are higher, or deobligations lower, during election periods compared to the controls.  

By this standard, the model produces strong evidence of electoral cycles in contract awards. As expected, awards jump in September because of the fiscal-year end effect, but the increase tends to be higher in nonelection years. Surprisingly, there is no year-end effect in election years for civilian agencies, a pattern due to the relative “lumpiness” of large civilian awards.  

The coefficient estimates indicate no acceleration in DoD contract awards during October of election years, although in second-quintile-state contract awards drop significantly during October of nonelection years. By the slimmest of statistical margins, there is a small increase—approximately 21%—in DoD contract awards in battleground states in the month those states hold their primaries.

Civilian agencies show a stronger pattern of election-year acceleration. Civilian contract awards rise sharply in October of election years in every group of states, by 42% in top quintile states, 79% in second quintile states, and 86% in battleground states. The largest increase in the crucial battleground states is consistent with political tinkering. However, since civilian contract awards also rise during September of nonelection years in top quintile and battleground states, but not during election years, the overall September-October accelerations are roughly the same in both on years and off years.

Civilian-agency contract awards are affected by the primary season,

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7I estimated separate coefficients for December of election and nonelection years to see if electoral acceleration resulted from a shift of awards from after the election to before. If agencies produced election-year accelerations by moving up the award date from December to, say, October, then the December coefficient for election years should be negative, or at least smaller than the December coefficient for nonelection years. There is some evidence that this sort of acceleration is responsible for electoral cycles, especially for civilian agencies, but in most cases the coefficients are small or of the wrong sign.

8Between fiscal 1988 and 1992, for example, all 232 civilian contracts over $100 million were awarded to either the Department of Energy or NASA. Of these contracts 94% were for operation of nuclear weapons labs and facilities or the space program, and none of 50 largest contracts was awarded in September.

9The percentage change, given a coefficient \( \beta \), is determined by calculating \( \exp(\beta) - 1 \).
Table 1. Regression Results
Dependent Variable: Ln(Prime Contract Awards), 1993 $
Electoral Variables Only

<table>
<thead>
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<th>Independent Variable</th>
<th>Department of Defense</th>
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<th>Civilian Agencies</th>
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<td></td>
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<td>Second Quintile</td>
<td>Battleground</td>
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<td>(.86)</td>
<td>(.89)</td>
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<tr>
<td>December, nonelection year</td>
<td>.033</td>
<td>.166*</td>
<td>.021</td>
<td>.005</td>
<td>.053</td>
<td>.326***</td>
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<tr>
<td></td>
<td>(.43)</td>
<td>(1.73)</td>
<td>(.29)</td>
<td>(.05)</td>
<td>(.48)</td>
<td>(3.87)</td>
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<tr>
<td>Month of presidential primary</td>
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<td>.053</td>
<td>.195*</td>
<td>-.187</td>
<td>.072</td>
<td>-.069</td>
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<td></td>
<td>(.16)</td>
<td>(.38)</td>
<td>(1.91)</td>
<td>(1.04)</td>
<td>(1.41)</td>
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<td>Month of primary nonelection year</td>
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<td>-.043</td>
<td>-.050</td>
<td>-.130</td>
<td>-.273**</td>
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<td></td>
<td>(.22)</td>
<td>(.22)</td>
<td>(.43)</td>
<td>(.28)</td>
<td>(.76)</td>
<td>(2.23)</td>
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<td>Month before primary</td>
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<td>.118</td>
<td>-.162</td>
<td>.213</td>
<td>.053</td>
<td>.013</td>
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<td></td>
<td>(.64)</td>
<td>(.82)</td>
<td>(1.54)</td>
<td>(1.18)</td>
<td>(.30)</td>
<td>(.10)</td>
</tr>
<tr>
<td>Month before primary nonelection year</td>
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<td>-.089</td>
<td>.049</td>
<td>-.407**</td>
<td>-.200</td>
<td>-.169</td>
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<tr>
<td></td>
<td>(.05)</td>
<td>(.74)</td>
<td>(.56)</td>
<td>(2.67)</td>
<td>(1.37)</td>
<td>(1.59)</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>.62</td>
<td>.33</td>
<td>.83</td>
<td>.45</td>
<td>.33</td>
<td>.75</td>
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<td>Pooled average LM</td>
<td>11.9</td>
<td>11.7</td>
<td>11.6</td>
<td>11.3</td>
<td>13.6</td>
<td>14.8</td>
</tr>
<tr>
<td>Standard error</td>
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<td>.571</td>
<td>.410</td>
<td>.72</td>
<td>.70</td>
<td>.50</td>
</tr>
<tr>
<td>N</td>
<td>870</td>
<td>870</td>
<td>783</td>
<td>870</td>
<td>870</td>
<td>783</td>
</tr>
</tbody>
</table>

T-statistics are in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$.

Lagged dependent variables, month and state dummies, and time variable not shown. State categories defined in Appendix A.
as indicated by the significant drops that occur during these months in nonelection years. Although there is no clear acceleration in either the month of a presidential primary or the previous month, for top-quintile and battleground-state award levels in these months are higher than they are during the same months in nonelection years.

Award patterns during October and November suggest a source for the electoral contracting cycles by civilian agencies. Civilian contract awards are significantly higher during off-year Novembers, especially for top- and second-quintile states. One possible explanation is that during election years awards that would have normally occurred in November are accelerated so that the contracts are released before the election. That the total October-November change in contract awards is roughly the same in both election and nonelection years supports this.

Much stronger indications of election-year effects emerge from the analysis of deobligations (Table 2). For both DoD and civilian agencies, deobligation activity comes close to a halt during October of election years. The dollar value of DoD deobligations drops by 49% in top-quintile states, 37% in second-quintile states, and 34% in battleground states. The decline in civilian-agency activity is more dramatic: down 61% in top-quintile states, 79% in second-quintile states, and 57% in battleground states. The sharp drop in deobligations supports the notion that taking away existing benefits is harder, and more politically significant, than providing new benefits. As with contract awards, though, these election-year declines are offset by the increase in deobligation activity that occurs in September, because the election-year increases in that month are typically much larger than the nonelection year increase.

I performed a series of F-tests on constrained and unconstrained models to verify that the election-year effects differed significantly from the nonelection year effects. In one series, I tested for significant differences in October awards. In this case the unconstrained models are simply those reported in Tables 1 and 2; in the constrained models, I forced the election-year and nonelection-year coefficients for October to be equal. Because the September and October effects often move in opposite directions, in a second series I tested for the significance of the total election-year acceleration (in both September and October). Here, the constrained model includes only a single variable that measures the change in awards in September and October, and the unconstrained model includes separate coefficients for election and nonelection years. These tests confirmed (p < .05) that October awards were significantly higher in election years for civilian contracts in second-quintile and battleground states; DoD deobligations in top-quintile and battleground states; and civilian deobligations in second-quintile states.

With the exception of civilian contracts to battleground states, how-
ever, the F-tests indicated that the total election-year contract accelerations (or deobligation decelerations) were no different than the nonelection-year controls. Despite the clarity of the cycles that emerged immediately before the election, then, the overall election-year effect was insignificant: contract award levels were typically no higher, and deobligations no lower, in election years. This finding confirms the key assumption of the underlying model that the acceleration process does not involve an overall increase in contract awards, but rather a redistribution of awards that would have been awarded in due course anyway.

Conclusion

The pattern uncovered in this analysis—strong October accelerations but no overall election year change in contract levels—points strongly to the conclusion that contracting accelerations (and deobligation slow-downs) result from minor changes in the timing of existing awards rather than an increase in total awards. The October accelerations found here are consistent with this interpretation, because they are clearly related to the upcoming election even if their economic impact was offset by countervailing patterns in the previous (or following) month. A consequence is that these accelerations can hardly be a factor in any macroeconomic expansion. What is economically trivial, however, can still be politically crucial, as carefully timed contact awards can be useful to presidents beyond their economic impact. Presidents could use timed awards as part of an effort to show that they care about the economic plight of constituents, or raise the salience of defense issues in the electorate (especially since nearly all large civilian contracts are related to the defense program through either weapons labs or the space program; see note 8). Coordinating a campaign appearance with a large contract award gives the incumbent an opportunity to highlight support for defense spending and distinguish himself from his opponent. In this respect, contract-award cycles form part of a broader campaign strategy, not as an economic instrument but rather as a way of marshalling the government apparatus to highlight issue positions that benefit the incumbent (for an example of press coverage of this phenomenon during the 1992 election, see Healy 1992; for 1988, Towell 1988; Morrison 1988). Bush's 1992 campaign announcements were surely designed not only to create economic stimulus and political goodwill in key areas, but also to raise questions about Clinton's commitment to national defense. Those contract announcements, in fact, add additional weight to the conclusion that the political value of an award does not depend on its economic impact: the announcements actually had no economic effect at all since the policy changes they reflected would not take place until after the election.
### Table 2. Regression Results  
**Dependent Variable: Ln(Deobligations), 1993 $**  
**Electoral Variables Only**

<table>
<thead>
<tr>
<th>Independent Variable</th>
<th>Department of Defense</th>
<th>Civilian Agencies</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>Top Quintile</td>
<td>Second Quintile</td>
</tr>
<tr>
<td>September of election year</td>
<td>.899***</td>
<td>1.087***</td>
</tr>
<tr>
<td></td>
<td>(4.74)</td>
<td>(4.20)</td>
</tr>
<tr>
<td>September, nonelection year</td>
<td>.817***</td>
<td>.566***</td>
</tr>
<tr>
<td></td>
<td>(5.10)</td>
<td>(2.63)</td>
</tr>
<tr>
<td>October of election year</td>
<td>-.670***</td>
<td>-.466*</td>
</tr>
<tr>
<td></td>
<td>(3.55)</td>
<td>(1.81)</td>
</tr>
<tr>
<td>October, nonelection year</td>
<td>-.181</td>
<td>-.024</td>
</tr>
<tr>
<td></td>
<td>(1.29)</td>
<td>(.12)</td>
</tr>
<tr>
<td>November of election year</td>
<td>-.441***</td>
<td>-.342</td>
</tr>
<tr>
<td></td>
<td>(2.71)</td>
<td>(1.33)</td>
</tr>
<tr>
<td>November, nonelection year</td>
<td>-.129</td>
<td>-.442***</td>
</tr>
<tr>
<td></td>
<td>(.92)</td>
<td>(2.32)</td>
</tr>
<tr>
<td></td>
<td>December of election year</td>
<td>December, nonelection year</td>
</tr>
<tr>
<td>--------------------------------</td>
<td>----------------------------</td>
<td>----------------------------</td>
</tr>
<tr>
<td></td>
<td>-.188</td>
<td>-.377</td>
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<td></td>
<td>(.10)</td>
<td>(.53)</td>
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<tr>
<td></td>
<td>-.001</td>
<td>-.089</td>
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<td></td>
<td>(.68)</td>
<td>(1.09)</td>
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<td>-.286</td>
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<tr>
<td></td>
<td>(.24)</td>
<td>(.43)</td>
</tr>
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</table>

$T$-statistics are in parentheses.

*p < .10; **p < .05; ***p < .01.

Lagged dependent variables, month and state dummies, and time variable not shown. State categories defined in Appendix A.
One question that cannot be answered with certainty with this data is how much political benefit the contracting cycles actually create. Do they truly help the incumbent, or is their impact overwhelmed by broader economic and political forces? Based solely on the election outcomes, the cycles apparently make little difference: incumbent Bush won eight of the nine battleground states in 1988 (losing only New York), and then lost seven in 1992 (winning only Florida and Texas) despite the furious pace of politically motivated contract-award announcements. Even if the cycles do not help much, however, they are even less likely to hurt, and a desperate incumbent will likely reach for any tactic that has potential.

Despite the small scale of the electoral cycles discovered here, the results confirm the expectation that, at least at the margins, bureaucrats and incumbents manipulate the timing of contract activity to provide some electoral advantages to incumbent candidates. Contract awards, particularly those let by civilian agencies, accelerate in key states at opportune times. The slowdown in deobligations is nearly uniform, and demonstrates that contracting officials lose their taste for taking money back from contractors as the election approaches. The agencies responsible for awarding contracts clearly have an eye on the political calendar, even though there are limits to either their willingness or ability to politicize the timing of contract awards.

The findings here also pose somthing of a dilemma for research on political-business cycles: in an area where policy makers should have had an ability to generate substantial effects, the cycles were real but inconsistent, and the accelerations had only trivial overall economic impact. It is difficult to see, therefore, how policy makers could use other tools, over which they have much less unfettered control, to generate electoral cycles in macroeconomic indicators. Future investigations of political-business cycles, therefore, should focus less on the connection between broad macroeconomic phenomena and elections, and more on the kinds of institutional activity that may have little economic impact but substantial political importance. Incumbents will always try to use their position and the levers of government to obtain advantages over the opposition, and they will always take credit for any positive economic signs that emerge during election years. Their ability to do so, however, is often less a function of actually being responsible than of being in the right place at the right time.

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APPENDIX A

The complete regression model, illustrated for DoD prime contract awards, is as follows:

\[
\text{Contracts}_{\text{state }, i, \text{month }, t} = \beta_0 + \beta_1 \text{Contracts}_{\text{state }, i, t-1} \\
+ \beta_2 \text{Contracts}_{\text{state }, i, t-3} + \beta_3 \text{Contracts}_{\text{state }, i, t-12} \\
+ \beta_4 \text{Septelect} + \beta_5 \text{Octelect} + \beta_6 \text{Nwoelect} \\
+ \beta_7 \text{Nseptelect} + \beta_8 \text{Noctelect} + \beta_9 \text{Nnwoelect} \\
+ \beta_{10} \text{Decelect} + \beta_{11} \text{Ndecelect} + \beta_{12} \text{Primary}_{i,t} \\
+ \beta_{13} \text{Primary}_{i,t-1} + \beta_{14} \text{Offprim}_{i,t} + \beta_{15} \text{OffPrim}_{i,t-1} \\
+ \beta_{16} \text{Feb} + \ldots + \beta_{22} \text{Aug} + \beta_{23} \text{time} \\
+ \beta_{24} \ldots \beta_{34} \text{State}_i + \epsilon
\]

where

\text{Contracts}_{\text{state }, i, \text{month }, t} \quad = \text{the logarithm of the value of prime contracts awarded to firms in state } i \text{ during month } t, \text{ in constant 1993 dollars.}

\text{Contracts}_{\text{state }, i, \text{month }, t-1} \quad = \text{the lagged value of Contracts}_{\text{state }, i, \text{month }, t}, \text{ with lags of 1, 3 and 12 months.}

\text{Septelect, Octelect, Nwoelect, and Decelect} \quad = 1 \text{ during designated month of election year, 0 otherwise.}

\text{Nseptelect, Noctelect, Nnwoelect, and Ndecelect} \quad = 1 \text{ during designated month of nonelection years, 0 otherwise.}

\text{Primary}_{i,t} \quad = 1 \text{ during month of primary in state } i \text{ in 1988 and 1992, 0 otherwise. If primary occurred before the tenth of the month, the previous month is set to 1.}

\text{Primary}_{i,t-1} \quad = 1 \text{ during month prior to 1988 or 1992 primaries, 0 otherwise.}

\text{OffPrim}_{i,t} \quad = 1 \text{ during primary month in year before and/or year after primary was held, 0 otherwise.}

\text{OffPrim}_{i,t-1} \quad = 1 \text{ during month preceding the month of the primary in the year before and/or year after primary was held, 0 otherwise.}

\text{Feb} \ldots \text{Aug} \quad = \text{series of dummy variables, coded 1 in designated month, 0 otherwise.}
APPENDIX A (continued)

Time = trend variable set to 1 in October of 85, incrementing by one each month through December 1992.

State\_i = dummy variable set to 1 for each state.

The same structure was used for deobligations, with the appropriate dependent and lag variables substituted.\textsuperscript{10} Despite the presence of the lagged dependent variables, the state dummy variables were necessary. I performed $F$-tests comparing the unconstrained models with constrained models to test the hypothesis that the coefficients for the state dummy variables were all equal to 0. In every case the results indicated that this hypothesis was incorrect. The inclusion of the time variable raises questions about the stationarity of the series. To test for non-stationarity, I performed the Dickey-Fuller test (Greene 1993, 564) on the residuals of the regression of Contracts\_i, on Contracts\_i,_{-1} for the entire sample, and calculated a pooled-average test statistic. This test indicated that the series was stationary, even though the variable TIME was significant for most equations.

The states were divided into categories based on either (a) the total dollar value of DoD or civilian-agency contracts between October 1985 and December 1992; or (b) their designation as politically important and competitive. The categories are as follows:

Top Quintile DoD States: California, Connecticut, Florida, Maryland, Missouri, New York, Ohio, Texas, Virginia

Top Quintile Civilian States: California, Florida, Maryland, New Mexico, New York, South Carolina, Tennessee, Texas, Virginia, Washington

Second Quintile DoD States: Arizona, Colorado, Georgia, Indiana, Michigan, Minnesota, Mississippi, New Jersey, Pennsylvania, Washington

Second Quintile Civilian States: Alabama, Colorado, Illinois, Kentucky, Louisiana, Massachusetts, Missouri, Nevada, Ohio, Pennsylvania

Battleground States: California, Florida, Illinois, Michigan, New Jersey, New York, Ohio, Pennsylvania, Texas

\textsuperscript{10}I modified one dependent variable, civilian-agency deobligations, because some data points had zero activity and therefore undefined logarithms. I added $1,000$ to each data point, because the risk of bias through this corrective was outweighed by the problems involved in losing the information in the missing data (but still substantively meaningful) points. This should not introduce much bias in any event, because the number of cases involved is so small (16 data points in top quintile states, 12 in second quintile states, and four in battleground states).
REFERENCES


