

Peltzman, Sam

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How Efficient Is the Voting Market?

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Sam Peltzman

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Preliminary--do
not quote.

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Sam Peltzman*

It is by now well established that good macroeconomic conditions benefit the incumbent President and his Congressional party confreres at the polls. This result is not unanimous (see, e.g. Stigler, 1973 or Arcelus and Meltzer, 1975), and there are doubts about how universal it is.¹ However, the basic results in Kramer (1971), the progenitor of the modern literature on the effect of economic conditions on elections, seem to have stood the test of time. While details differ among studies, the broad consensus of this literature is that voters in Presidential and Congressional elections: (1) reward income growth and punish unemployment and inflation, (2) probably give greater weight to income growth than to unemployment and inflation, and (3) myopically ignore any information beyond the recent past, say prior to the election year.² So far, however, the data have outrun interpretation. While we know that economic conditions matter, we do not know why they matter or how plausibly to characterize the process by which voters translate information about economic conditions into voting decisions. This paper tries to fill that gap. Though it extends the empirical literature, its main concern is evaluative: do voters correctly use economic information in voting decisions?

This question presumes a standard against which voting behavior can be judged. One reason the question has not so far been faced explicitly is the lack of such standard. The model implicit in the empirical literature

is one of a self-interested voter in a principal-agent relationship with the party in power. This voter settles with his agent--the President--ex post for much the same reason that as, say, an owner compensates a manager based on past performance. It is too costly to evaluate directly the agent's actions or their implications for the principal's welfare. This analogy falters, and the theoretical lacuna emerges, over the issue of how much information the voter ought to bring to bear on his decision. Since ownership can be concentrated, owners have a plausible wealth stake in correctly processing information on their agent's performance. Voters have no such stake, because a vote is costly and the likelihood that one vote will alter the election is vanishingly small. This formulation, due to Downs (1957), has two implications. One is the still unresolved "paradox of not voting" (i.e. a rational actor shouldn't vote because there are no benefits). The other is the less stringent notion that voters should be ill-informed: if they are foolish to vote in the first place, they shouldn't waste (too many) more resources in informing themselves about candidates. On this view, even the most whimsical use of information is "correct."

This "rational ignorance" story has implicitly or explicitly conditioned the theoretical and empirical literature on the connection between voting and the economy. The most notable example is the "political business cycle" literature (see, e.g., Nordhaus, 1975), in which politicians exploit the voters' short memories by stimulating the economy in the election year and dealing with any inflationary consequences later. However, the empirical basis of this theory is surprisingly weak. For example, Monroe's (1979) survey finds that the voters' time horizon is

"not discussed explicitly in detail in the literature. Most of the literature assumes voters maximize short-term interests." (p. 162). The assumption is typically implemented by regressing the incumbent's share of the vote on a short list of macroeconomic variables measured over the year prior to election. Since voters cannot be presumed to know very much, not much attention is paid to prior specification of these variables. Anything plausibly related to how well off the voters feel in the recent past will do (see Fiorina, 1981). So the common procedure has been limited to showing that some more or less arbitrary transformations of income-output or inflation measures³ are correlated with the election outcome in plausible directions.

Such results do not, of course, "test" rational ignorance. They merely say that voters: 1) value an informed vote (contrary to the most extreme version of the Downsian model), 2) process some macrodata and 3) use the data in paying off the agent whom they hold responsible--the President. However, once it is understood that voters act as if some information about the economy is valuable in settling with their agent, the next question to address is: how much information do they use? This is the question I try to answer. If this were a paper about the stock market or the managerial labor market rather than the voting market, the obvious point of reference for the answer would be the "fully informed" (marginal) principal whose actions cause the market to fully reflect publicly available information. Given what is already known about the voting market, it seems premature to rule this out a priori. So, this paper tries to discover how close the settling up process in the voting market comes to full utilization of available information. And it turns out that the voting market is a surprisingly good aggregator of such information.

The notion that the voting market might use information more efficiently than the empirical literature or the rational ignorance story allows is not altogether new. For example, Stigler's (1973) critique of the relevance of short-run income growth is couched in terms of voters who can distinguish permanent from transitory effects. He argues that, because neither party can push the economy permanently off its long-run growth path, voters rationally concerned with permanent wealth would not reward or punish short run deviations from this path. Similarly, Michaels (1986) criticizes the unquestioning use of inflation measures without any attention to the link between inflation and voters' welfare. These are, however, exceptions which have so far not much altered empirical practice.

In the next section, I spell out how a self-interested voter would process the available economic information to correctly evaluate the policy of his political agent. Subsequent sections apply the resulting algorithm, in a variety of tests, to state level election returns for Presidential, Senatorial and gubernatorial elections from 1950-82, i.e. the post-Full Employment Act period when a connection between policy and the macro-economy is plausible.⁴ This use of cross-sectional detail from three types of elections enables me to get at theoretical nuances which might otherwise be hidden in the aggregate data commonly used in the empirical literature.⁵ The main result is that the voting market uses information efficiently. It makes essentially every distinction suggested by economic theory (e.g. between permanent and transitory income, expected and unexpected inflation), and it does not discard relevant information from early in a President's term. I also extend the empirical literature to measure the

political impact of government spending. I find that voters dislike increased spending no matter how it is financed.

I. How Should Voters Respond to Economic Information?

Once we cast aside our bewilderment about why voters respond at all, the answer to this question is fairly straightforward: voters should respond to the gain or loss in their welfare which can be attributed to their agent's policy. On the previous election day ($t = 0$), the voter estimates what his lifetime welfare (W) can be expected to be on the next election day ($t = 1$) if either party A or party B wins. These estimates of $t = 1$ welfare made at $t = 0$ (call them w_{01}^A and w_{01}^B) should reflect all the information the voter has as of $t = 0$. The voter then chooses, e.g., party A if $w_{01}^A > w_{01}^B$. (But see the qualification below.) Suppose A wins. In the interval between $t = 0$ and $t = 1$, the voter acquires new information which he uses to update the W and make his voting decision on $t = 1$. Some of the information has nothing to do with A's policy (the voter strikes oil or drills a dry well). Such information implies an equal revision in w^A and w^B and, therefore, no change in the voter's choice of party.⁶ For many voters--those for whom $w_{01}^A \gg w_{01}^B$ or \ll --the policy related change in W is also irrelevant, because it does not reverse the original inequality. But the marginal voter ($w_{01}^A \approx w_{01}^B$) will base his decision on $t = 1$ entirely on the new information about his welfare revealed by A's policy. In the context of ex-post settling up, and putting aside any non-policy related changes, the marginal voter's decision rule comes down to "vote for A if $w_{12}^A > w_{01}^A$; otherwise vote for B." This voter may understand that $w_{12}^A \approx w_{12}^B$, because competition among the parties would make any gains achieved during A's incumbency

prospectively permanent. For the sake of enforcing an efficient rule, this information is suppressed. But it is not ignored. Having settled with A, the marginal voter uses $W_{12}^A \approx W_{12}^B$ plus post $t = 1$ information to settle with the incumbent on $t = 2$. When this simple story is applied to the aggregate of voters, it says that:

1. Only new information affects the election outcome; old (pre $t = 0$) information is fully reflected in the last election.
2. The voting market ignores new information that cannot plausibly be related to the agent's policy.
3. The voting market rewards past policy success only if that permanently improves welfare. Thus, if the information revealed after $t = 0$ is that "A's policy has increased W by X but will decrease it by X after $t = 1$," the relevant W are unchanged and A is not rewarded.

There is a link between the information processing strategy just described and the time series properties of macroeconomic data that should be spelled out, because I use it in the empirical work. If voters fully process relevant information, it should be the post $t = 0$ innovations in the data that affect the current election, because the components of the post $t = 0$ data that could be forecast conditional on $t = 0$ information should have been reflected in the last election. Similarly, post $t = 1$ changes that can be forecast from post $t = 0$ data will affect the current election. To illustrate, recall Stigler's argument for the irrelevance of post $t = 0$ income changes. It implies a time series model in which any deviation of income from a fixed trend is only temporary. If this is the correct time series model, the voter can compute all the W_{01}, W_{12}, \dots at $t = 0$ and no post $t = 0$ information will alter these values.

Accordingly, no voter rationally changes his vote based on post $t = 0$ information. Suppose, however, that changes in income follow a random walk with drift. Then every income change (less the drift) after $t = 0$ is fully reflected in a revision of the lifetime value of wealth. The voter using income information to settle up with his agent would then rationally reward the agent for all post $t = 0$ income changes. As it happens, the random walk with drift model seems a tolerably good characterization of real income changes (see Nelson and Plosser, 1982). So there is a rational basis for the finding that income changes affect election outcomes.

Because of their central importance in the empirical literature, most of my empirical work focuses on the role of income and inflation. Since income changes are approximately a random walk, it will be difficult to go much beyond the existing literature to test the proposition that only post $t = 0$ income innovations matter. As long as the drift in this random walk is a constant, empirically separating the effect of the expected component of income from the "surprise" component is impossible. Accordingly, with respect to income my main focus will be on the voters' time horizon: if all income changes after $t = 0$ contain new information about lifetime wealth, all the changes, not just the most recent, should matter.

In the case of inflation, however, a more focused test is available. Conventional wisdom among economists is that if inflation is bad (see sec. IV), the evil resides mainly in unexpected inflation. Because nominal contracts should reflect any expected inflation, the allocation of wealth and resources should be approximately the same at any expected inflation rate.⁷ This would imply that sensible voters respond only to the inflation surprises during a President's term and ignore the expected component of

inflation--e.g. they would reward high but unexpectedly falling inflation and penalize low but rising inflation. Indeed, even if expected inflation is costly, sensible voters would ignore it. One reason that there is a substantial expected component in inflation lies in the unwillingness of either party to adopt policies which would markedly change inflation rates quickly. A voter who understands this reality of political competition has to impound some persistence of inflation into both W^A and W^B .⁸ So, there is no basis for penalizing the incumbent for any costs of persistent inflation.

Expected inflation, unlike expected income growth, has not plausibly remained fixed over time. Average inflation rates in the 1970s, for example, were much higher than in the 1950s. This time variation in expected inflation makes it possible to test the proposition that voters do not respond to the expected inflation.

To summarize, I want to see if voters act as if they correctly process available economic information. This means that they ignore information irrelevant to their long-run welfare or to the policy of their agent, and that they use all the relevant information. Each step in the empirical analysis entails operationalizing this proposition. In common with much of the principal-agent literature, I assume that voters use information on policy outputs (income and inflation) rather than inputs (money supply, budget deficits). That is, I do not presume that voters have done better than economists at estimating a policy production function which can ferret out the policy-related component of the output.

II. How do Voters Respond to Economic Information?

To make the preceding discussion concrete and capable of confronting data, begin with the relation:

$$(1) \quad IS_{i1} = K_i + f(W'_{i1}),$$

where IS_{i1} = incumbent party's share of the vote in jurisdiction i in the election on $t = 1$

K_i = the party's normal share of the vote in i . This is determined by the distribution of $(W^A - W^B)$ across the population of i . (Recall that voters for whom this difference is sufficiently large at $t = 0$ will not change their vote in light of new information.) I drop the i subscript for subsequent simplicity.

W'_1 = the change in W^A due to information revealed after A won the last election. So

$$(2) \quad W'_1 = g(X_{01}, \dots)$$

where X_{01} = an index or vector of the economic information revealed in the $t = 0, 1$ interval. (The ellipses allow for non-economic determinants of W'_1). The $f(\cdot)$ in (1) translates the W'_1 into the marginal voters' decisions, i.e. $f' > 0$. I assume that competitive adjustments of party policies and population turnover is sufficient to make K a constant. This seems a good approximation at the aggregate level for Presidential elections, where there is no obvious trend in party shares.⁹ So, in each election there is a roughly equal-sized coterie of marginal voters who will move according to their estimate of W' .

Equation (2) embodies the "efficient markets" assumption that only new information counts in assessing the incumbent's performance. However, it allows, in principle, the possibility that some of the new information is

ignored. Another way in which Downsian voters could sloppily process information would be to revise their welfare assessment sluggishly in light of the new information. To allow for this inefficiency I extend (2) to

$$(3) \quad W'_1 = (1 - q) g(X_{01}, \dots) + qW'_0$$

where q = an adjustment coefficient, $0 \leq q \leq 1$. If voters adjust completely $q = 0$; if they completely ignore new information $q = 1$. A linear version of (1) which incorporates (3) is:

$$(4) \quad IS_1 = K(1 - q) + m(1 - q)g(X_{01}, \dots) + q \cdot IS_0, \text{ where } a \text{ constant, } m, \text{ replaces } f(\cdot) \text{ in (1).}$$

Subsequent estimates of (4) test for voting market efficiency two ways: primarily by distinguishing the relevant from the irrelevant (i.e. the predictable, temporary or non-policy related) economic information, including both in $g(\cdot)$, and seeing if the latter get a zero weight and the former a non-zero weight; secondarily I test the hypothesis $q = 0$.¹⁰

To provide a point of reference, I first estimate (4) by imposing sensible behavior. That is, I estimate surprise elements of real income and inflation and include only these in X_{01} . The primary focus here is on the voters' alleged myopia--i.e. on the question of whether voters react to all of the relevant information in X_{01} or just the most recent components. I use three alternative estimates of income and inflation surprises. These get successively more refined and therefore permit an additional test of voter sophistication. Specifically, the crudest estimated surprises (subsequently denoted Δ) are constructed from:

Income: the monthly change in the log of real per capita personal income (PCI). This takes advantage of the near-random walk character of the series.

Inflation: the difference between the inflation rate (I) in month t and the average inflation rate over the preceding year. The latter is a proxy for expected inflation. It recognizes that, in a world of sluggishly changing inflation rates, expected inflation should reflect some averaging of past inflation.

The first refinement--is purely statistical (denoted AR). Expected income and inflation variables are generated by regressing current PCI and I on lagged values, and using the regression coefficients to generate month ahead forecasts. The surprises are then estimated as the difference between actual PCI and I and these auto-regressive forecasts.

The last refinements (denoted CON and TBIL) use economic theory. For income, I use Hall's (1978) elaboration of the permanent income hypothesis. In essence, if consumption is proportional to lifetime permanent income all the information consumers (voters) have about their permanent income streams up to the current period should be revealed by last period's consumption. If so, Hall argues, the residuals (CON) from a regression of current real per capita consumption on last period's value should be "white noise" and yield an estimate of this period's permanent income surprise. I ran such a regression,¹¹ and used the residuals as an estimate of the permanent income surprise for each quarter.¹²

The most refined estimate of unexpected inflation uses Fama and Gibbons's (1984) implementation of Irving Fisher's decomposition of the nominal interest rate into an expected real return and an expected inflation rate. In essence, Fama and Gibbons (and I) first generate a month ahead estimate of the expected real return from a time series model of past actual real returns and deduct these estimates from the current

one-month T-bill rate to generate an expected inflation rate for the next month. Unexpected inflation (TBIL) is just the difference between actual inflation and the expected rate implicit in the Bill rate.

To the extent that the more refined estimates are more accurate, they should better explain the behavior of voters who use all available information. Before that proposition is tested, some facts about these alternative surprise estimates should be kept in mind.

1. All of them, except the crudest estimate of unexpected inflation, have the desirable property of looking like white noise.¹³--I.e. they represent new information not predictable from past behavior of these series. (The crude estimate of unexpected inflation is moderately positively autocorrelated).

2. The more refined surprise measures tend to be more accurate. Specifically, if one adds up surprise estimates over periods of one to four years, the most refined measures have the lowest variance. (However, the autoregressive model of inflation does about as well as the T-bill market in forecasting inflation.)

3. However, the refined measures are too refined. The auto-regressions, and the regressions used to extract information from T-bill rates and consumption are based on data from roughly the same period (1950-80) as votes are sampled. The structure of such regressions is unknowable to the voters, of say, the 1950s or 1960s. So the refined measures have to be treated as noisy estimates of the best information potentially available to voters, and this measurement error compromises the power of any test of voter sophistication.

4. Only the alternative inflation surprises contain much independent information. For periods over a year, the correlation between alternative unexpected inflation estimates ranges from near zero to around .7. By contrast, the correlation among income surprise estimates is usually around .9 or higher.¹⁴ Thus, detection of any differential response to more accurate information should be easier for inflation than for income.

These facts and caveats understood, I use the estimates of inflation and income surprises to address questions about the information voters use.

A. Are voters myopic?

To answer this, and subsequent questions I estimate regressions of the general form

$$(5) \quad IS_{1i} = A + B_1 X_{INC, LAGT}^j + B_2 \cdot X_{INF, LAGT}^j + \dots ,$$

where

IS_{1i} = incumbent party's share of the total vote in state i in some election

X^j = the sum of monthly income or inflation surprises of type j ($j = \Delta$, AR, or CON/TBIL), converted to an annual rate, over the T months (or quarters) preceding election day.

The ellipses denote "other variables" which include

a. IS_{0i} , the incumbent party's share in state i in the previous election.

b. A vector of dummy variables which allow for different values of K , the normal probability of voting for the incumbent party, across states. Operationally, K is the incumbent party's long-run average share in a state.¹⁵

c. A dummy = +1 if the candidate is an incumbent. This is extraneous to my inquiry, but adjusts for well-known advantages of incumbency.

d. Where appropriate, dummies to shift the intercept and sign of X when the incumbent party is not the same as the President's. (See below.)

For any kind of election, I estimate a set of regressions in which T is varied, by 3 month increments, from 3 to as many as 48 months prior to an election. Thus, there are 16 regressions in each set. The first uses data only from the 3 months prior to election day, the second uses the past 6 months' data and the sixteenth uses the past four years' data. This generates a set of regression coefficients (the B 's) each of which is an estimate of the sum of the relevant distributed lag weights from T to election day. The change in B from any T to $T + 3$ is an estimate of the marginal weight voters attach to the X in that 3 month interval.¹⁶ Thus, if voters are myopic, the B 's will peak (or bottom) at a small T and tend to remain there as T is increased. This procedure is repeated for each of the three alternative types of X , and a comparison of partial correlations or t-ratios of the respective B 's will indicate how refined the voters' information set is.¹⁷

I estimate (5) for three time series of cross-sections of state election returns, one for Presidential elections, another for Senatorial elections and the third for gubernatorial elections. These comprise most contested elections at the state level for 1950-82.¹⁸ Initially, I assume that voters are settling up with the party of the incumbent President. So, for Presidential elections, (5) can be applied directly. For Senate and Gubernatorial elections, because the incumbent party may differ from the President's, I estimate (dropping unnecessary subscripts):

$$(5') \quad IS = A' + B'_1(PR \cdot X) + C \cdot PR + \dots,$$

where

$PR = +1$ if incumbent party is same as President, -1 if different.

Thus B'_1 shows the gain to a candidate from the President's party or the loss to an opposition candidate from increased X , and C is the required change of intercept.¹⁹

Half of the Senate elections and a majority of gubernatorial elections occur in non-Presidential-election years. These off-year elections raise two conceptual issues about lags on the X greater than two years. The first arises when there is a change in administration. For example, consider a Republican Senator running in 1982, two years after a Republican President, Reagan, replaced a Democrat, Carter. Do (ought) the voters in 1982 use information on Carter's performance before 1980 in settling with Reagan in 1982? Some models (e.g. Fair, 1978) allow the possibility that voters will reward Reagan in 1982 for Carter's mistakes (by reversing the sign of the pre-1980 components of X). This is, however, dubious. The pre-1980 information has already been used to settle directly with Carter in 1980. To use it again in 1982 would weaken the power of the 1982 signal and thereby create bizarre incentives. For example, if Reagan's 1980-82 performance is merely less bad than Carter's, 1982 Republican candidates would gain rather than be penalized. The sensible voter can eliminate this incentive to mediocrity by simply ignoring pre-1980 information. The preliminary evidence was consistent with this view: I estimated (5)' with two alternative constructions for X in off-year elections following a change in administration. The first reversed the sign of any component of X from the tenure of the previous administration. The second constrained

the weight on such components to zero. For both Senate and Gubernatorial elections and for every relevant lag and type of X , the second alternative fit the data better than the first.²⁰

A related issue arises in off year elections following a victory by the incumbent administration. Ought a Republican running in 1986, for example, continue to benefit from the pre-1984 policies that resulted in Reagan's reelection? The previous discussion suggests that 1986 voters would ignore the pre-1984 information. But the evidence is unclear: I estimated (5') both with and without the constraint of a zero weight on the old information for the relevant off-year elections. These two alternatives fit the data about equally well. Subsequent results are arbitrarily based on data imposing the constraint. Thus, for every off-year election, no more than the preceding two years' information is included in the X variables in (5').²¹

The results of immediate interest from estimates of (5) and (5') are in Table 1. These show the coefficients of various lags on the alternative estimates of income and inflation surprises for each type of election. These results are inconsistent with the notion that voters myopically weigh only the most recent experience. While the estimated total weights sometimes move erratically and vary across alternative estimates of the same surprise, the peak total weight never occurs before a two year lag. This holds for all types of elections and for every type of income or inflation surprise. This pattern is shown more clearly in Figures 1 and 2 and Table 2. These are based on three-term moving averages of the data in Table 1 (which attenuates the noise in those data) expressed as a percent of the peak weight. For income, Figure 1 shows a fairly consistent pattern

Table 1. Coefficients and T-Ratios for Income and Inflation
Surprises in Voting Regressions, 1950-82 Elections,
State Returns

SURPRISE TYPE:	INCOME								INFLATION															
	Δ				AR				CON				Δ				AR				TBIL			
	B		t		B		t		B		t		B		t		B		t		B		t	
	A. PRESIDENTIAL ELECTIONS 1952 - 80 (N = 313)																							
LAG	3	-.1	.9	.1	.9	.8	3.2		-2.8	4.7	-5.2	11.4	-2.8	6.9										
(MONTHS	6	.7	3.0	1.7	5.9	-1.7	6.6		1.3	1.3	-7.8	8.2	-20.0	14.9										
PRECEDING	9	1.4	8.7	.9	5.6	2.8	12.4		3.0	4.1	-5.4	8.1	6.6	6.8										
ELECTION)	12	2.2	11.8	1.9	8.9	3.3	11.7		2.1	3.9	-3.8	4.9	3.3	4.2										
	15	2.2	10.8	1.2	4.9	4.4	13.0		2.7	4.6	-7.3	8.0	4.2	4.8										
	18	2.5	10.6	.9	3.0	4.4	11.3		2.6	5.7	-10.2	8.7	2.9	4.2										
	21	3.0	10.6	2.1	5.3	4.2	9.6		2.6	6.0	-3.1	2.9	3.4	5.0										
	24	3.2	11.8	3.0	8.3	6.4	11.8		1.9	3.6	-4.0	3.6	4.6	4.9										
	27	2.7	9.0	2.1	6.6	8.9	12.1		1.0	1.3	-10.1	10.2	1.0	.7										
	30	4.5	11.6	2.5	6.0	11.8	16.9		-5.0	5.2	-10.6	13.2	-4.0	3.6										
	33	5.1	14.8	2.4	5.8	12.7	19.7		-9.0	8.9	-12.1	15.2	-13.0	9.8										
	36	2.8	8.5	.4	1.3	11.2	19.6		-11.8	8.3	-15.5	20.3	-16.6	9.0										
	39	3.6	11.2	1.4	4.5	9.3	12.9		-16.1	13.3	-14.2	20.5	-5.5	3.5										
	42	3.6	9.1	2.2	5.6	8.8	13.0		-18.5	10.0	-13.9	17.1	-5.5	1.9										
	45	3.5	9.3	3.2	7.1	12.5	17.2		-24.5	13.9	-12.1	15.4	-31.8	9.7										
	48	3.2	9.5	3.2	6.8	19.5	19.9		-25.4	18.6	-11.6	15.5	-45.0	15.4										

Note: Estimates of B are from regressions of the form

$$IS_i = A + B_1X_{T1} + B_2X_{T2} + \dots$$

where IS = incumbent party's share of the two-party vote in state i (percentage points)

X_{Tj} = estimated surprise in inflation or income growth over the T months prior to election (percentage points, annual rate).

Three alternative concepts are used to estimate X_T . The respective income and inflation surprises are:

Δ : change in the log of real per capita personal income or difference between inflation rate this month and average rate over preceding 12 months.

AR: difference between monthly log change in income or inflation and predicted income change or inflation rate from autoregressive model.

CON: residual from regression of quarterly log real per capita consumption minus durables on last quarter's value, or

TBIL: difference between inflation rate in month and expected rate implicit in one month T-Bill rate.

Each X_T is the sum of monthly (or quarterly) surprises over the T months preceding an election. For off-year Senate and Governor elections the maximum T = 24, so, e.g., X_{27} would be the sum of 27 monthly surprises for on-year elections and = X_{24} for off-year elections.

Sources:

IS: Congressional Quarterly

Personal Income, Consumption: Survey of Current Business

Price Index (Personal Income Deflator): Data Resources, Inc.

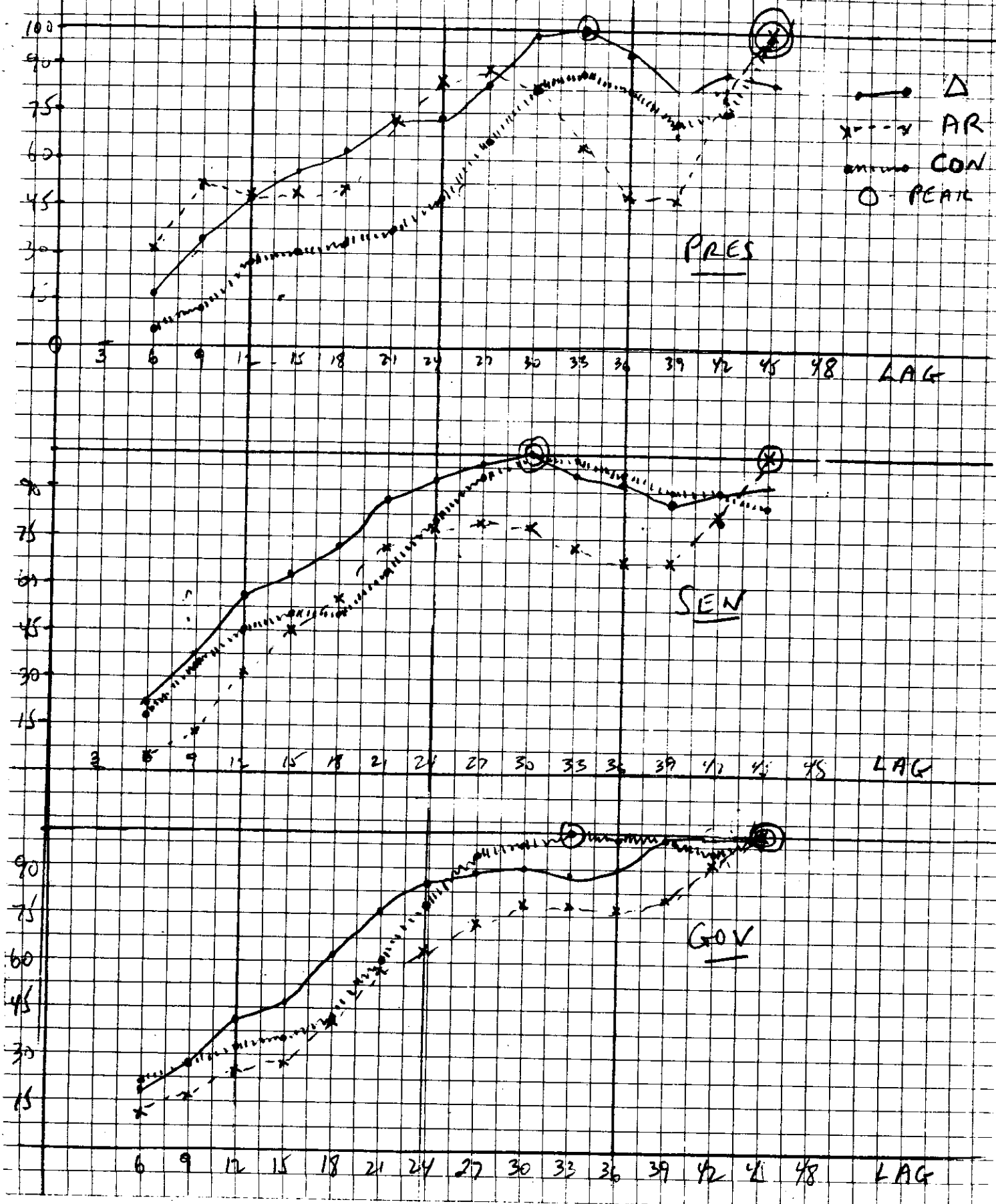
T-Bill rates: U of Chicago Center for Research in Security Prices and Federal Reserve Bulletin.




Table 2. Number of Months Before Election to Peak Total Effect,
and Distribution of Effect Over Time.
Averages of Three Surprise Measures.

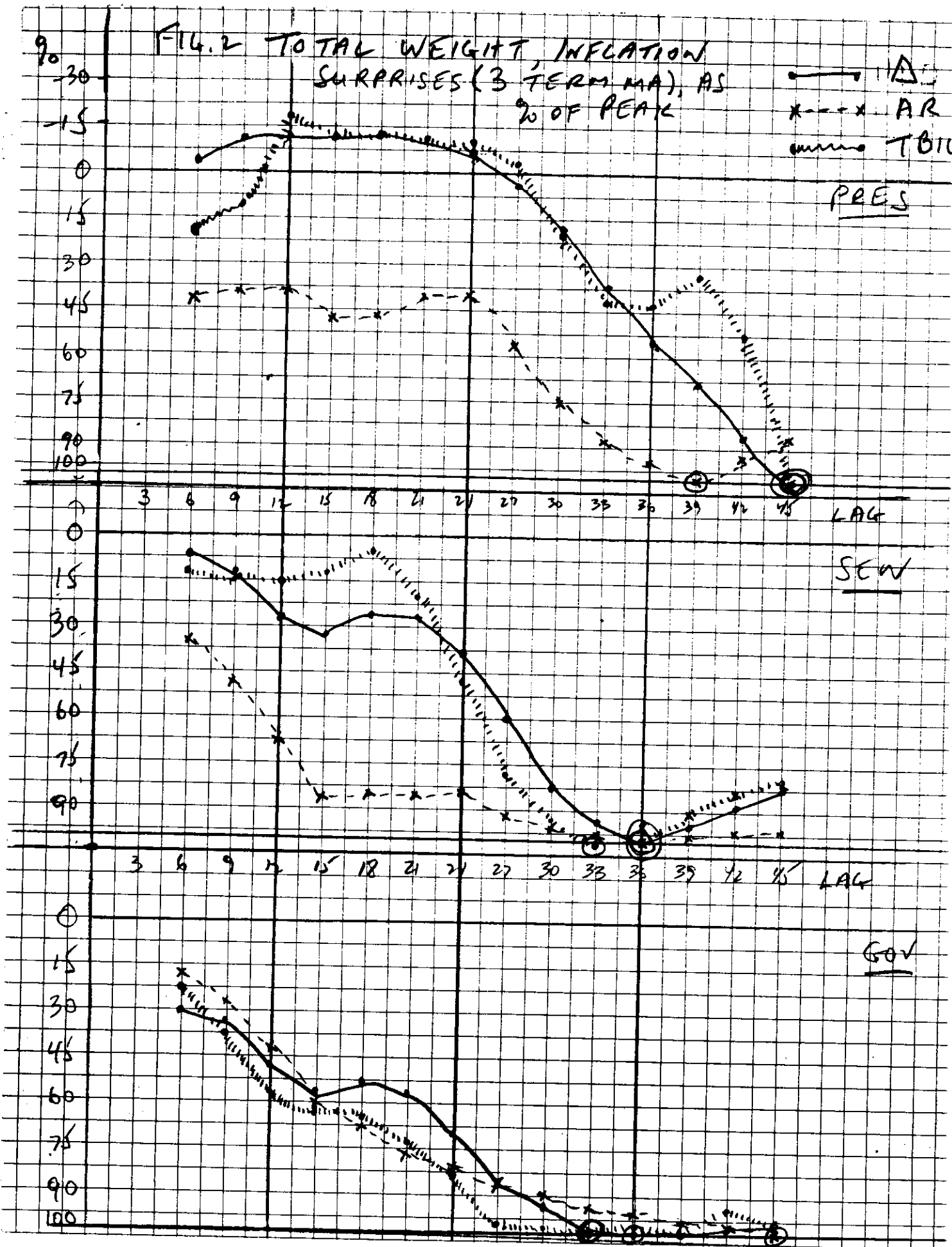
		MONTH OF PEAK (MEAN)	PERCENT OF PEAK EFFECT AT MONTH			
			12	24	36	45
A.	<u>INCOME</u>					
1.	All elections	39 mos.	39%	76	83	95
	A. Presidential	41	40	68	75	94
	B. Senate	35	44	83	84	91
	C. Gubernatorial	41	32	76	90	99
B.	<u>INFLATION</u>					
1.	All elections	39 mos.	28	47	87	93
	A. Presidential	43	0	7	64	95
	B. Senate	35	37	56	99	87
	C. Gubernatorial	38	48	78	97	98

Note: Based on three-term moving averages of data in Table 1. For each measure of income or inflation surprise, the month of peak total effect is identified and the first column shows the average of this month across the three measures. Thus, e.g., the first entry says that positive income surprises over the 39 months preceding an election help the President's party. The remaining entries show how much of the peak effect is realized at various times (averaged across surprise measures). So the first row says that 39% of the peak effect is realized in the year prior to election, 76% in the two years prior, etc. (The last entry, 95, is less than 100 because the marginal weight is not always zero after a peak.)

FIG. 1. TOTAL WEIGHT, INCOME SURPRISES (3 TERM MA)
AS % OF PEAK



 ΔE
 AR
 $TBIC$



of a more or less steady increase in the total weight over the first two years, a smaller increase in the third year and a peak which seems centered at about 3 years. For inflation, the data tell a highly varied story about the two years preceding election day. Two of the three measures suggest that inflation over this period is actually a good for incumbent Presidents. For Senators and governors from his party, inflation over the last two years is bad, but estimates of the damage vary considerably. These disagreements narrow sharply when the horizon is pushed back another year or two. It becomes clear that voters penalize the President and his party for inflation surprises and that they look back at the whole inflation record of the administration (or at least the last three years) in assessing the penalty. Table 2 gives some summary measures of the length of the voters' memory and how the weight is distributed over the life of an administration. On average, favorable income and inflation surprises over the last 39 months of an administration help the President's party. For income, the marginal weights over each of the last two years are roughly equal and together account for 80 percent of the total. For inflation, it is the last year and the second year of an administration that seem most important. Thus, while the year preceding election is important, it is consistently less than half the full story.

Two other features of Table 1 deserve mention:

1. There is not much doubt about the statistical significance of the results just summarized, given the t-ratios of the relevant coefficients. These are especially large for Presidential elections, possibly too large. The OLS procedure assumes that each observation is independent, and this is surely incorrect. For years like 1956, 1964,

1972 when the incumbent party won by a landslide OLS residuals for most states would be positive. It is unclear a priori which way such dependence across states biases the standard errors.²² So I reestimated a few of the Presidential regressions via a GLS procedure. In every case, the estimated coefficients were virtually the same as in Table 1, and the t-ratios were actually higher.²³ The proximate reason for the large t-ratios in the Presidential regressions is the much larger impact of the economic variables here than in the other elections: the relevant coefficients are on the order of five times as large in the case of income and ten times as large for inflation. If the President is the voters' primary agent for economic policy, this lop-sided distribution of punishments and rewards is sensible.²⁴

2. There is no decisive "winner" among the three types of surprise estimates. The permanent income story implies that the CON representation of income surprises should have the highest t-ratios. By and large, it does for all three kinds of elections, but only for Presidential elections is the difference noteworthy. The inflation measure-of-choice seems to be the AR representation, again by a generally narrow margin.

B. Do Voters Distinguish the Unexpected from the Expected?

If voters efficiently processed macroeconomic data, they would ignore expected inflation and transitory income because these are irrelevant to their permanent welfare. My data permit a straightforward test: Let the X in (5) or (5') to which voters actually respond be

$$(6) \quad X = X^* + m \cdot x, \quad \text{where}$$

X^* = component to which sensible voters should respond (unexpected

inflation, permanent income)

x = the supposedly irrelevant component (expected inflation, transitory income).

m = weight voters place on x in arriving at X . So, if $m = 0$ only the "right" variable would matter. If $m = 1$, there is no distinction between expected and unexpected or permanent and transitory; only current inflation and income growth would matter. So far, I have constrained $m = 0$. But if (6) is substituted into (5), we have

$$\begin{aligned} (7) \quad IS &= A + B(X^* + mx) + \dots \\ &= A + BX^* + (Bm)x + \dots, \end{aligned}$$

and m can be recovered by adding x to the regressions and dividing its coefficient by that of X^* .

Table 3 shows estimates of m for the AR and TBIL estimates of expected inflation²⁵ and with transitory income estimated as the difference between per capita income growth and the residual from the regression of consumption on its lagged value. The results are shown only for the relevant lags (two or more years).²⁶

Voters will get good grades from economists for distinguishing expected from unexpected inflation. For lags which Tables 1 and 2 suggest are more relevant--around three years and longer²⁷--the weight given to expected inflation is typically close to zero and always closer to zero than one. The picture is fuzzier for transitory income. For Presidential elections, the weight on this variable tends to be extravagant, but both sign and magnitude are highly sensitive to minor variations in the lag structure. However, both of the other elections vindicate the primacy of permanent income. For these, the weight on transitory income is always

Table 3. Estimate of Weight Voters Give to Expected Inflation and Transitory Income, Various Periods Prior to Election

Type of Election and Type of Estimate		A. <u>EXPECTED INFLATION</u>									
Mos:		24	27	30	33	36	39	42	45	48	
1.	<u>President</u>										
A. AR	m	1.42	.57	.31	-.04	-.03	-.10	-.12	.08	.43	
	t-ratio	11.8	8.4	4.2	1.7	1.7	5.7	5.2	1.0	3.8	
B. TBIL	m	-.61*	-.42*	2.69	.07	-.01	.04	.03	.02	.01	
	t	10.7	11.1	6.2	1.7	2.2	10.4	15.1	5.0	1.6	
2.	<u>Senate</u>										
A. AR	m	.14	.08	.04	.01	.06	.06	.07	.09	.10	
	t	1.2	.8	.4	.1	.6	.6	.7	.8	.9	
B. TBIL	m	.29	.23	.17	.17	.19	.28	.35	.35	.39	
	t	2.2	1.9	2.0	1.4	1.7	2.4	2.2	2.0	2.1	
3.	<u>Governor</u>										
A. AR	m	-.06	-.09	-.10	-.10	-.08	-.09	-.08	-.06	-.04	
	t	.5	.7	.9	.9	.7	.8	.7	.6	.4	
B. TBIL	m	.01	-.01	-.02	-.04	-.04	.02	.02	.0	-.02	
	t	.2	.1	.2	.5	.4	.3	.2	.0	.2	
		B. <u>TRANSITORY INCOME</u>									
1.	<u>President</u>										
	m	-3.16	2.35	-1.41*	.13	2.09	7.53	2.64	1.14	-.46	
	t	6.3	8.0	1.2	.4	5.5	8.3	7.1	3.3	8.6	
2.	<u>Senate</u>										
	m	-.10	-.39	.01	-.44	-.51	-.35	-.15	-.13	-.10	
	t	.3	1.3	.0	1.5	1.4	.8	.3	.2	.2	
3.	<u>Governor</u>										
	m	.01	-.15	-.19	-.32	-.37	-.26	-.20	-.27	-.32	
	t	.0	.5	.6	1.3	1.4	.8	.6	.7	1.0	

Note: The entries are estimates of m from regressions like text eqn. (7) where the X^* and x variables are calculated over the indicated period. The value of m shown is the quotient of the regression coefficients of x and X^* . The t-ratio is for the coefficient of x , so it is a test of the hypothesis that $B_m = 0$ (or that $m = 0$ conditional on $B \neq 0$). Two alternative sets of expected and unexpected inflation variables are used. See note to Table 1 for definitions. Transitory income is the growth in per capita income less the estimate of permanent income growth implicit in the residual of the regression of log consumption on its lagged value. (The weights for permanent income and the TBIL estimate of expected inflation are from regressions which include both variables.)

* = coefficient (not shown) of unexpected inflation or permanent income has "wrong" sign.

indistinguishable from zero. The broad conclusion from Table 3 is that voters are by and large making the relevant distinctions in processing macro-economic information.

C. Whose Income Counts--the Voter's or the Nation's?

To a crude approximation, the President's policy affects mainly aggregate income rather than its distribution. If voters understood this, they would respond only to aggregate income surprises and ignore personal deviations from the aggregate. Voters, of course, care about these personal deviations, but they are mainly irrelevant in evaluating the policy of their agent. Since the political agenda includes redistributive policies, these are assertions about relative magnitudes which lead to the following test: Since my unit of analysis is the state, let x in (6) and (7) be the difference between the growth rate of per capita income in the state and the nation, and let the latter be X^* . Then, if the regression estimate of m , the weight on x , is 1, the voters in a state respond only to their own income. If $m = 0$, they respond only to aggregate income and ignore personal deviations. The "weak form" of the relevant hypothesis would be that $m \ll 1$. Table 4 summarizes the results of this test. Most of the estimated weights are indistinguishable from zero and numerically closer to zero than one. This holds across all types of elections. Thus, the data are mainly consistent even with the "strong form" hypothesis that voters ignore completely their own deviation from national income growth when settling with the President's party.²⁸ This does not imply that these personal deviations are entirely irrelevant politically. For example, voters who experience a large favorable income deviation may switch

Table 4. Estimates of Weight on Voter's Income
Relative to National Income, Various
Periods Prior to Election

MONTHS PRIOR TO ELECTION	ELECTION					
	PRESIDENT		SENATE		GOVERNOR	
	Weight (m)	t	Weight (m)	t	Weight (m)	t
12 to 24:						
Avg.	.12	1.7	.17	0.7	-.11	.9
(Min,Max)	(.07,.15)	(1.3,2.0)	(-.01,.28)	(0,1.3)	(-.29,.21)	(.1,1.5)
27 to 36:						
Avg.	.14	2.1	.20	0.7	-.24	1.2
(Min,Max)	(.10,.18)	(1.4,2.8)	(.13,.30)	(.3,.9)	(-.30,-.21)	(1.0,1.4)
39 to 48:						
Avg.	.12	1.7	.44	1.2	-.18	.7
(Min,Max)	(.11,.14)	(1.5,2.0)	(.39,.50)	(1.1,1.3)	(-.20,-.15)	(.5,.9)

Note: Based on regressions like text eqn. (7) where, x = difference between growth rate of real per capita income in state and the nation. This regression is run for lags on income and inflation of 12, 15, ..., 48 months prior to election. The table shows the average values of the weight on x (Avg.) and the range (Min,Max) over all regressions in the indicated interval. Corresponding $|t|$ values are for the coefficient of x . (See also note to Table 3.)

State per capita income is available only annually, so intra year growth rates are interpolated: e.g. the 15 month growth rate is this year's growth rate + 1/4 last year's (annualized). For purposes of computing x , intra year national growth rates are estimated similarly. Both state and national nominal per capita income are deflated by the Personal Income Deflator. Since more refined income concepts are unavailable for states, only the Δ regression specification (see Table 1, note) is used. State per capita income is from Survey of Current Business.

allegiance from the pro to the anti-redistribution party.²⁹ My results do imply that any switch would occur no matter which party is incumbent.

D. Who is (are) the Voters' Agent(s)?

So far I have assumed that the agent whom the voters hold accountable is the President, and that the only reason his Senatorial and Gubernatorial party confreres are affected by the macro-economy is to reinforce the signal to the President. The good reason for such behavior is that voters understand that the President is the only elected official whose policies can materially affect the macro-economy. Congress as a whole may have effects, but the one Senator who faces his electorate cannot. If voters failed to make this distinction, every incumbent, not merely the President, would share the credit or blame for the voters' good or ill fortune. Voters in Senate and gubernatorial elections, however, do make the distinction: I added income and inflation surprises (uninteracted with the Presidential party dummy) to the regressions in Table 1. If incumbents share in the benefits of good times regardless of party the coefficients of these variables would be significantly positive and negative respectively. In no case was either coefficient more than two standard errors from zero, and the mean $|t|$ was less than 1.0.³⁰

A more focused test is available for governors. Formally, governors and Presidents share similar executive powers. But, as chief executive in a small open economy without a central bank, the governor cannot conduct very powerful macro-policy. Voters who understood this would not reward incumbent governors as much as Presidents for favorable income surprises.³¹ Since we have just seen that Presidents get all the credit for national income growth, the appropriate test is to add the (uninteracted) difference

between the state and national income growth rate to the first (Δ) set of gubernatorial election regressions summarized in Table 1. If the coefficient of this variable is smaller than that of the corresponding (interacted) national growth rate, voters would be rewarding the incumbent governor's party less for local income surprises than they reward the President's party for national income surprises. The evidence is consistent with this: the coefficient of the local income deviation is within two standard deviations of zero for every lag. For the longer lags (24 to 48 months), the average [range] of the coefficient is 23.3 [-2.7, 39.7] percent of the corresponding national income growth coefficient. The average [range] of the $|t|$ ratios for these coefficients is 1.0 [.1, 1.5]. Thus, while the results mildly suggest that incumbent governors reap some reward from superior performance of the local economy, voters seem to understand the limits on their ability to affect it.³²

E. How quickly do voters assimilate information?

In the absence of any costs of adjusting to new information, the election market should, like the stock market, have no memory. In this context, this means that an incumbent party's share today should be independent of its share in the last election: the share should move to whatever is its new equilibrium based on the new information revealed since the last election. As elaborated earlier, I tested this by including the lagged incumbent share in the basic regressions. The results are summarized in Table 5, and they are mixed. For Presidential elections, the lag share coefficient ranges broadly, but around a mean close to zero. For gubernatorial elections, the coefficient is consistently indistinguishable from zero. However, for Senatorial elections the lag share coefficient is

Table 5. Coefficient of Lagged
Incumbent Party Share

Lag on Income/ Inflation Variables (Mos. Before Elec.)	ELECTION					
	PRESIDENT		SENATE		GOVERNOR	
	Coeff. of Lagged IS	t	Coeff. of Lagged IS	t	Coeff. of Lagged IS	t
12 to 24: Avg. (Min,Max)	-.11 (-.28,.21)	2.1 (.2,3.9)	.26 (.24,.27)	4.4 (4.2,4.5)	.06 (.03,.08)	.7 (.3,.9)
27 to 36 Avg. (Min,Max)	-.03 (-.16,.14)	1.4 (.1,2.9)	.26 (.25,.27)	4.4 (4.3,4.5)	.05 (.03,.06)	.6 (.4,.7)
39 to 48: Avg. (Min,Max)	.12 (-.19,.40)	2.9 (.8,6.4)	.26 (.24,.27)	4.3 (4.1,4.5)	.05 (.03,.06)	.5 (.3,.7)

Note: Based on regressions in Table 1. Each of those regressions includes incumbent party share in last election as an independent variable. (For Senate, last election refers to seat.) The coefficients and t-ratios are summarized here. Since three sets of economic variables are used and lags are increments of 3 months, the summary statistics are based on 12 or 15 regressions for each lag interval.

consistently and significantly positive. Thus the data are mainly, but not exclusively, consistent with the notion of complete adjustment to new information. This does not mean that in the absence of new information voters act as if they toss a coin to pick a candidate. My procedure allows for party preferences.³³ Nor do my results imply that party preferences change only when there is new macro-economic information. They do imply that, except for Senate elections, any such change due to other information occurs episodically rather than gradually. In this sense, voters are separating the surprise from the predictable (e.g. trend) elements of that information as well as from the macro-data.

III. The Electoral Effects of Government Spending

So far I have followed the literature in focusing on the macro-economic effects of monetary and fiscal policy. However, the latter can also have direct effects on voter welfare. If politicians were simply perfect agents for the voters, the marginal value of public and private spending would be pushed to equality. Small changes in the federal budget would then have neutral-to-favorable welfare and political effects.³⁴ However a variety of political economy models suggest that government activity can be pushed beyond the point desired by the typical voter, because interest group and bureaucratic welfare also matter to politicians. In fact, one proponent of this view claims that empirically the marginal vote value of federal spending is negative for U.S. Presidents (Niskanen, 1975). Here I reexamine that claim in expanded form and extend the analysis in two directions--I examine deficits as well as spending, and, since governors have roughly as much influence on state budgets as Presidents have on the federal budget, I examine state as well as federal budget effects.

A. Federal Budgets

Table 6 summarizes the results of adding federal spending to the income/inflation variables summarized in Table 1. Specifically, because I found that the growth rate in quarterly real per capita federal spending is approximately white noise, this variable represents the relevant surprise in the regressions. Table 6 shows the coefficient of this growth rate (annualized and interacted with the Presidential party dummy) measured over the same interval as the other economic variables in the regression. There are three regressions (one for each version of the macro variables) and therefore three estimates of the federal spending coefficient at each lag. Table 6 reports the averages of these. They are uniformly negative. In addition, the individual coefficients are almost always significant, so I spare the reader that detail.³⁵ Thus voters seem to dislike budgetary expansion and penalize the President's party for it. This is most clear in the Senate and governor regressions. There the negative total weight on spending surprises grows steadily to a peak at around 30 months and then levels off, very much the same pattern earlier observed for the other macro variables. Interestingly, these federal spending coefficients are on the order of 20 or 30 percent of the companion income coefficients in Table 1. Since federal spending has averaged around 1/4 of personal income over the same period, the two sets of coefficients suggest that voters care only about disposable income--i.e. that the marginal value of federal spending is essentially nil.

The Presidential election results have an odd twist. Though the total weight is always negative, it is largest in the year prior to election and then shrinks. Taken literally this means that spending near an election is

Table 6. Average Coefficients of Growth of Per Capita
Federal Expenditures

ELECTION: LAG (MOS.)	PRESIDENT	SENATE	GOVERNOR
3	-.6	-.1	-.1 a
6	-1.2	-.1	-.0 a
9	-1.6	-.1	-.1
12	-.8	-.2	-.1
15	-.5	-.2	-.2
18	-.7	-.2	-.3
21	-.7	-.3	-.3
24	-.4	-.2	-.2
27	-.7	-.3	-.3
30	-.5	-.4	-.4
33	-.3	-.3	-.4
36	-.5*	-.3	-.4
39	-.4*	-.4	-.4
42	-.5*	-.4	-.4
45	+.1*	-.4	-.4
48	-.2	-.3	-.4

Note: The annualized change in the log of real federal expenditures over the indicated period prior to election was added to each of the three regressions with the same lag summarized in Table 1. The entries are averages of the three coefficients of this variable at each lag.

* = at least one of the three coefficients > 0

a = average $|t| < 2.0$ (> 2.0 for all others)

Federal expenditures are quarterly, from Survey of Current Business and deflated by the Personal Income Deflator.

penalized, but earlier expenditures are rewarded--just the opposite of the naive political spending cycle story.³⁶

A strain of theory since Ricardo has argued that taxpayers should ignore surpluses and deficits, because these shift the timing of tax payments without altering their present value. Does Ricardian equivalence hold politically? With the exception of another oddity in Presidential elections, it does. Table 7 summarizes my test of Ricardian equivalence. It shows t-ratios for the coefficient of the change in the real per capita federal surplus when that variable is added to the regression with the federal expenditure and macro-variables.³⁷ For both Senate and gubernatorial elections, the t-ratio of the surplus variable hovers close to zero for most all lags and definitions of the macro-variables. That is, candidates from the President's party are neither hurt nor helped by deficits or surpluses. For the President himself, however, the data imply that he should run a surplus close to election and a deficit early in his administration. This is, again, just the opposite of what a naive political cycle story would imply. This implication follows first from the positive t-ratios on the surplus variable in the 24 months preceding election. For longer lags these t-ratios (and, not shown, the associated total weights) tend to decline and begin bracketing zero. This decline implies that the marginal weight on surpluses more than two years back tends to be negative. Some caution is warranted in interpreting this and the similarly peculiar result on expenditures³⁸ as more than a quirk of the data. Both Tables 6 and 7 show that in the Presidential regressions--and only in these--signs and t-ratios of coefficients of long lags on the budget variables are highly sensitive to alternative specifications of the

Table 7. T-Ratios of Coefficient of Change in Per Capita
Federal Surplus. Averages Over Various Periods
Prior to Election

ELECTION	PRESIDENT	SENATE	GOVERNOR
Mos. Before Election:			
<u>3-12</u>			
Avg.	10.4	.5	.4
(Min,Max)	(4.3,15.5)	(-2.2,1.8)	(-2.7,.9)
<u>15-24</u>			
Avg.	11.0	.9	.1
(Min,Max)	(8.5,16.4)	(.3,1.8)	(-.7,.8)
<u>27-36</u>			
Avg.	1.1	-.1	-.5
(Min,Max)	(-3.7,7.7)	(-.8,1.0)	(-1.1,.3)
<u>39-48</u>			
Avg.	2.7	.5	-.5
(Min,Max)	(-7.0,16.4)	(-.5,1.3)	(-.9,.3)

Note: The change in real per capita (Revenues-expenditures) is added to regressions with the federal expenditures and macro-variables described in notes to Tables 6 and 1 respectively. This regression is estimated for each of the lags (3, 6, 9, ..., 48 months) and for each of the three alternative macro-variables. The entries show the average and range of the resulting t-ratio on the surplus variable for all regressions in the indicated lag interval.

Source: See note to Table 6.

macro-variables.³⁹ The internally consistent results--those from the non-Presidential elections--speak with one voice: voters do not like increased federal spending, they remember it, and they understand Ricardian equivalence.

B. State Budgets and Gubernatorial Elections

An obvious question raised by the preceding results is whether the voters' aversion to government spending extends to the state level. The answer is that it does: Table 8 shows the coefficients of the change in per capita state government expenditures in the gubernatorial regressions.⁴⁰ These are uniformly negative and they are significant at the longer lags. Once again, there is no evidence of myopia: the peak total weight occurs at a lag of over three years, and over half this total is accounted for by spending growth before the election year. This is the same pattern as for all the macro variables. Also, the voters in gubernatorial elections seem to value state spending surprises about as much as federal spending surprises, which is to say not at all. Compare the magnitude of the peak total weight in Table 8 (around -.2) with the corresponding gubernatorial peak total weights on federal expenditures in Table 6 (around -.4) and on income (Δ) in Table 1 (a bit under +2).⁴¹ State spending has been running around 10 percent of personal income and 40 percent of federal spending, which is roughly the same as the relative magnitudes of the relevant coefficients. The implication is that, at the margin, voters do not distinguish between a dollar of state and federal spending and treat each as essentially worthless.⁴² These results raise an obvious question for future research: if the incumbent executive's party is penalized politically for budgetary expansion, why has such expansion been the dominant political strategy?

Table 8. Average Coefficients. Growth of Real
Per Capita State Expenditures.
Gubernatorial Elections

LAG(MOS.)	COEFF.	t
12	-.11	1.7
15	-.15	1.9
18	-.15	1.8
21	-.15	1.6
24	-.14	1.5
27	-.18	1.9
30	-.21	2.1
33	-.22	2.2
36	-.21	2.1
39	-.23	2.2
42	-.24	2.2
45	-.23	2.1
48	-.22	1.8

Note: The annualized change in log of real state general expenditures per capita over the indicated period was added to gubernatorial election election regressions with the income, inflation and federal expenditures variables described in Tables 1 and 6. Thus there are three regressions, one for each income-inflation concept, at each lag. The entries above are averages of the three coefficients and t-ratios at each lag.

State expenditures are available annually for fiscal years ending June 30, so year-to-year changes are centered on the end of the previous calendar year. To obtain a rough match to the election data all changes are computed from expenditures in the fiscal year following the election year, so, e.g., lag 12 for a 1980 election would be the change from FY80 to FY81. Intra-year lags are estimated via the procedure described in the note to Table 4.

Source for state expenditure data: Statistical Abstract of the U.S.
Deflator is the Personal Income Deflator.

Finally, it is worth noting that adding the budget variables to the regressions does not materially change any of the previous results. Specifically, in the Senate and governor election regressions, where the budget variables have clear effects, the pattern and magnitudes of the effects of income and inflation are broadly similar to those shown in Table 1. The only notable difference is that the peak total effect of inflation tends to be closer to the election when the budget variables are added. A succinct summary is provided by the estimated number of the months' lag to this peak effect, calculated in the same way as in Table 2 but from regressions which add the relevant budget variables. These are (value from Table 2 in parentheses): For income, 37 (35) months for Senate and 40 (41) months for gubernatorial elections; for inflation 21 (35) months for Senate and 24 (38) months for gubernatorial elections. A summary comparison of the magnitudes of the coefficients is the ratio of the estimated peak total effects from regressions with and without the budget variables. These are 1.07 and 1.10 for income and inflation respectively in Senate elections, and 1.07 and 1.12 for gubernatorial elections.⁴³ So, adding the budget variables hardly changes the size of the estimated effects of income or inflation.

IV. Some Interpretive Issues

A. Are voters too averse to inflation?

Table 1 showed that voters respond at least as much to a one point move in unexpected inflation as to a similar move in income. In Presidential elections, the response to inflation is two to five times that to income. This appears strange, since it is not even clear a priori that unexpected inflation is bad. And, if unexpected inflation is bad, the widely varying

magnitudes of its political effect across the various kinds of elections could not simultaneously be correct.

The first-order impact of a positive inflation surprise is to enrich net monetary debtors and hurt net monetary creditors. In the aggregate voters appear to be net creditors: for the household sector over the sample period, monetary assets less monetary liabilities have been on the order of a year's personal income.⁴⁴ So, at least the sign of the political impact of unexpected inflation makes sense. To get at plausible magnitudes, consider a household with annual income of \$10,000 and equal net monetary assets. If it is like the aggregate of households, of its \$10,000 net monetary assets, \$2,000 will be non-interest bearing, \$4,000 will be short-term interest bearing and \$4,000 will be long-term interest bearing.⁴⁵ The detail is important, because of the diverse effects of an inflation surprise. To fix ideas, I use the AR representation for unexpected inflation, and I assume that three years before an election the inflation rate accelerated permanently and unexpectedly by 1 percent. The cash assets thus will suffer a continuing extra purchasing power loss of 1 percent per year. The only loss for short term interest-bearing assets will be due to the lag of short rates behind actual inflation as expected inflation catches up. But the long term assets lose capital value when interest rates rise to reflect the higher inflation rate. A rough calculation of the sum of the three effects is a total wealth reduction of \$600 - \$700 on the \$10,000 monetary assets.⁴⁶ By comparison, a one percent income surprise in any month has a present value of around \$1000 to this representative household.⁴⁷ The two hypothetical surprises would have comparable effects on the variables in my regression,⁴⁸ so this exercise

suggests a political weight on the income variable somewhat greater (10/6 or 10/7) than the inflation weight. The regressions in Table 1 do not satisfy this, though those for the non-Presidential elections come within hailing distance. The larger message of this exercise is that there is a plausible link between the voters' wealth position and their strong aversion to unexpected inflation. (See C. below for a further exploration of this link.)

B. The Impact of Non-Presidential Elections on Presidential Elections

The pervasive result that Senators and governors from the President's party share the impact of macro-policy raises the following interpretive question: Are voters using non-Presidential elections simply to convey a cheap message to the President, or to substantially affect the outcome of the Presidential election? The latter would be implied if vote shares in Presidential elections were sensitive to the party controlling the governor's mansion or Senate seats. I tested this by adding the following dummies to the Presidential election regressions: (1) a variable = +1 if the gubernatorial incumbent in the Presidential election year is from the same party as the President, = -1 otherwise, (2) a variable = +1 if both incumbent Senators are from the same party as the President, -1 if both are from the opposite party and 0 if the two Senators are from different parties. The results were mixed. The gubernatorial dummy had consistently insignificant coefficients for any relevant lag on the macro variables (say, $T \geq 24$ months). However, the Senatorial dummy had consistently positive coefficients with t-ratios averaging around 2 for these lags and lying within a range from close to zero to around 4. So the safest conclusion seems to be that having incumbent Senators, but not incumbent

governors, campaigning for a President is useful to the party's reelection chances. The mean coefficient is about +1, which implies that the President's party has a 2 percentage point advantage if both incumbent Senators are from his party.

C. Is the Political Effect of the Macro-economy Asymmetric?

Agency theory makes no general prediction about the structure of ex-post payoffs. So far, I have estimated only linear structures: good years are rewarded as much as bad years are penalized. However, Bloom and Price (1975) found that House candidates from the President's party lost votes in recessions but did not gain in expansions. My attempt to replicate this finding failed uniformly for the three types of elections in my sample, and it failed for both income and inflation surprises. Essentially, a linear payoff structure fits "good" and "bad" periods about equally well.⁴⁹

A more motivated search for asymmetries may lie in the interaction between real income and inflation. If there is a conventional economists' wisdom in these matters, it is that there is a policy tradeoff between income growth and inflation. If voters believe this, the political payoff structure would not be separable in income and inflation. Specifically, imposing such a structure on the data, as I have done, would generate positive residuals in periods where low income growth is accompanied by high inflation and in high growth low inflation periods.⁵⁰ These would, of course, be offset by negative residuals in the other periods. To test for this asymmetry, I correlated the residuals from the regressions in Table 1 with a dummy = +1 if the macro-economy prior to election day was characterized by both positive inflation and negative income surprises or vice versa and = -1 otherwise. The Phillips curve story implies a positive

correlation, but the results were mixed. For Presidential elections, the correlation is usually significantly positive for lags of two or more years and for each of the three income/inflation measures. The average [range] of the 27 (9 lags (24, 27, ... 48 months) x 3 regressions per lag) correlation coefficients is $+.21 [-.27, +.53]$. However, for both the gubernatorial and Senatorial elections, this correlation coefficient is always trivial ($< .1$). In broad outline, these results are similar to Chappell's (1983) finding of evidence weakly favoring the Phillips curve story in aggregate Gallup poll data.

One reason for the weak results--and another reason for the political importance of inflation--may be that the linear payoff structure appropriately reflects policy lags in the income/inflation tradeoff. To see the possibility, consider the results of regressing per capita income growth in the two years after a Congressional or Presidential election on unexpected inflation (AR method) in the two years before the election. For 1952-86 data, the coefficient (t) is $-1.1 (1.9)$.⁵¹

One way to read this result is that inflation breeds restrictive policies which ultimately reduce income growth. A voter fully using available information would then penalize recent inflation in part because of the lower post-election income that it portends. In fact, the size of the apparent long-run tradeoff--a point less post-election income growth for each point of unexpected inflation--is roughly consistent with the relative political weights on income and inflation in Table 1.

D. How Important are the Economic Variables in Determining Election Outcomes?

So far, I have shown that vote shares respond systematically to the macro-economy, but I have ignored the magnitude of that response. To

assess the practical importance of the economic variables, I compared the actual vote share of the incumbent party to the shares predicted by two alternative models: a NOECON model in which the economic variables are all set equal to the sample mean and an ECON model which removes this constraint. The difference in predictions from these models is simply the estimated marginal impact on vote shares from movements in the economic variables over the relevant pre-election period. For this purpose, I averaged the estimates for all regressions with lags of 24 or more months on the three sets of economic variables. The particular specification of the regression reflects the previous results that appear robust. Income/Inflation variables are always included. Federal expenditures are included in the Senate and governor, but not Presidential election regressions. State expenditures are included in the governor regressions. The non-economic variables always include state, party and incumbency dummies, lagged vote shares and for Presidential elections only, the number of Senators from the President's party. None of the subsequent results are sensitive to these choices, however.

For each election in my sample, I compute the squared prediction error from both the ECON and NOECON models, and I use the square root of the average of these squared errors as a measure of accuracy. The basic question is: how much is this root-mean squared error (RMSE) reduced by adding the ECON variables? The answer is that only for Presidential elections do the economic variables matter much. Table 9 shows that, for the non-Presidential elections the RMSEs from the two models are within a percentage point. It also shows the proximate reason for this: given their coefficients, the economic variables do not vary enough to produce

Table 9. Vote Share Prediction Errors,
with and without Economic Variables,
Non-Presidential Elections

	SENATE ELECTIONS (N = 477)	GOVERNOR ELECTIONS (N = 258)
1. Root Mean Square Prediction error, all elections, from		
a. NO ECON model	6.8%	6.2%
b. ECON model	6.5	5.6
2. Percent of all elections in which		
(a) Both models agree on the outcome, and	<u>91.2%</u>	<u>85.7%</u>
--(1) Both are right	71.7	67.1
--(2) Both are wrong	19.5	18.6
(b) they disagree, and	<u>8.8%</u>	<u>14.3%</u>
--(1) ECON is right	4.8	9.3
--(2) NO ECON is right	4.0	5.0
3. Share of 17 election years, 1950-82, in which ECON RMSE < NO ECON RMSE	.647	.765*
(a) Weighted by number of elections	.652	.818*

Note: 1. See text for description of root mean square prediction error (RMSE) and ECON and NOECON model.

2. Based on predicted vote shares for incumbent party. A "correct" prediction means that the predicted and actual shares of the two party vote are on the same side of 50.0.

3. The RMSE for each model is calculated for each of 17 sets of elections in the even numbered years 1950, 52, ..., 82. The data are the number of years + 17 in which the ECON model has a smaller RMSE. For 3(a), the number of elections in years satisfying this criterion is divided by total number of elections. (The occasional odd-year election is included in the next year's set.)

* = mean significantly different from .5 at 5%.

Table 10. Presidential Elections, 1952-84.
Measures of Accuracy With and Without
Economic Variables by Election

A. Incumbent Party Predicted and Actual Average Share of:

ELECTION	VOTES Predicted			STATES Predicted		
	NOECON (1)	ECON (2)	ACTUAL (3)	NO ECON (4)	ECON (5)	ACTUAL (6)
1952	43.6%	43.1%	41.1%	8.3%	5.6%	5.6%
56	58.5	60.4	59.9	94.2	94.2	97.2
60	50.0	49.3	51.9	43.2	43.2	56.8
64	52.0	58.1	61.6	66.7	94.9	97.4
68	43.9	47.9	47.2	10.3	33.3	28.2
72	58.1	61.8	62.8	95.1	97.6	97.6
76	58.1	53.7	50.8	95.2	83.3	61.9
80	52.0	43.0	41.9	62.8	7.0	11.9
84*	59.3	66.0	61.1	95.5	100.0	97.7

B. Relative Accuracy Measures

	RMSE			ABSOLUTE ERRORS, AVG. SHARE OF					
	NOECON (1)	ECON (2)	GAIN TO ECON (3)=(1)-(2)	VOTES			STATES		
	(1)	(2)	(3)=(1)-(2)	NOECON (4)	ECON (5)	GAIN (6)=(4)-(5)	NOECON (7)	ECON (8)	GAIN (9)=(7)-(8)
1952	4.7%	4.5%	.2%	2.5%	2.0%	.5%	2.7%	.0%	2.7%
56	5.1	4.9	.2	1.4	.5	.9	3.0	3.0	.0
60	3.4	3.8	-.4	1.9	2.6	-.7	13.6	13.6	.0
64	10.2	5.0	5.2	9.6	3.5	6.1	30.7	2.5	28.2
68	4.5	3.2	1.3	3.3	.7	2.6	17.9	5.1	12.8
72	6.9	5.2	1.7	4.7	1.0	3.7	2.1	.0	2.1
76	8.0	4.5	3.5	7.3	2.9	4.4	33.3	21.4	11.9
80	10.9	4.8	6.1	10.1	1.1	9.0	50.9	4.9	46.0
84*	4.1	6.1	-2.0	1.8	4.9	-3.1	2.2	2.3	-.1

markedly different predictions from the NOECON model. So, in terms of predicted outcomes, the two models agree in about 9 of every 10 elections. And, by 3 or 4 to 1, these predictions are right, because of persistence in long run party shares and the advantages of incumbency. There are only a comparative handful of elections which are close enough for the economic variables to be decisive. However, the last panel of the table shows that, even if they are small, the effect of the economic variables is more or less omnipresent rather than concentrated in a few years. And they seem to make a little more difference in the Gubernatorial than the Senatorial elections.

The markedly different pattern for Presidential elections is summarized in Table 10. The first two columns of Panel B show RMSEs by election for the eight elections (1952-80) used in the regressions. (1984 is used for a projection.) Four of these (1952, 56, 60, 68) look very much like the non-Presidential elections in that the ECON and NOECON models do about as well. For these elections, both have RMSEs ranging roughly from 3 to 5 percentage points, with the ECON model tending to be only slightly better. For the other elections (1964, 72, 76, 80) the RMSE for NOECON ranges from about 7 to 11 percentage points, while the ECON RMSE remains around 5 percent. So economic conditions yield a considerable gain in accuracy (2 to 6 points in RMSE) for these elections.

The first three columns of Panel A show the proximate reason for these dichotomous results. They summarize the predicted and actual mean (unweighted, across states) vote shares for the incumbent party. For every election the ECON model comes within 3 or 4 percentage points of the actual share, but, for the four "unusual" elections, the NOECON model is off by 5

Panel A.

Note: Cols. (1), (2) and (3) are unweighted averages across all states in the sample of the incumbent party's vote share. Cols. (1) and (2) are averages of predicted values excluding and including economic variables. (See text.) The number of states in the sample ranges from 36 in 1952 to 44 in 1984. Cols. (4), (5) and (6) first translate each state's vote share into a predicted or actual win for the incumbent party. (Vote share > 50.0).

Panel B.

Cols. (1) and (2) are root mean squared prediction errors of the vote share (see text) across all states in the sample for the specified election.

Cols. (4) - (6) are based on the vote share data in panel A.

Col. (4) = | Col. (1) - Col. (3) | from A

Col. (5) = | Col. (2) - Col. (3) | from A

A positive value in Col. (6) means that the average vote share predicted by the ECON model is closer to the actual average than the NO ECON average vote share.

Cols. (7) - (9) perform the same exercise on the state share data in Cols. (4)-(6) of Panel A (e.g., Col. (7) = |Col. (4) - Col. (6) |from A).

*All 1984 predictions are out-of-sample forecasts, based on coefficients from regressions estimated through 1980 and using post 1980 data to predict the 1984 outcome.

to 10 percentage points. Each of these cases has seemingly plausible non-economic reasons for being unusual: "extremist" challengers (Goldwater in 1964, McGovern in 1972), an unelected incumbent (Ford in 1976), a conservative "revolution" (Reagan in 1980). But my results imply that there is nothing unusual about any of these cases once economic conditions are accounted for.

To summarize crudely, call any election where the winner's mean vote share exceeds 55 percent a "landslide" and call the others "close." The ECON model perfectly distinguishes landslides from close elections. The NOECON model misses four of the eight--the 1964 and 1980 landslides and the close 1968 and 1976 contests. The table also shows results for the share of states (the last three columns of both panels). These are similar to the vote share results, though the ECON model does not do as well here in predicting the closeness of the '76 election.

The last line of each panel shows results for an out-of-sample forecast of the 1984 election. Here the ECON model correctly forecasts the Reagan landslide. But so does the NOECON model, and it comes closer to the actual result. The ECON model, driven by the liquidation of double digit inflation during Reagan's first term, predicts an unprecedented landslide which was not to be.

V. Summary

When a principal cannot directly evaluate the quality of an agent's input, the agent's compensation often depends on some measure of ex-post performance. Thus, for example, compensation of chief executives of large corporations includes stock options and performance bonuses. It is by now well known that the political market works in roughly the same way. The

vote payment to the chief executive in charge of macro-economic policy and his party depends partly on some measures of ex-post performance of the macro-economy. I have tried to determine whether the voters are using "correct" performance measures. Accordingly, I tested voter behavior in Presidential, Senatorial and Gubernatorial elections against some standards of "correctness" suggested by economic theory. The voters pass essentially every test, and the voting market emerges as a strikingly efficient aggregator of economic information.

For example, efficient markets are supposed to reflect all available information. In the present context, this would mean that, with due allowance for policy lags, voters would use information about the economy's performance over the bulk of the President's term. By contrast, much of the political business-cycle literature assumes that voters ignore all but the most recent information. But my results show that voters suffer no such amnesia. For both income and inflation, voters use information over at least the last three years of a President's term. The kind of income/inflation information used by the voters also seems correct, insofar as this means that voters should:

1. be concerned with the permanent, rather than temporary, effects of policy.
2. ignore information which is irrelevant to their welfare.
3. ignore information about their welfare which is not policy related.

With respect to inflation, economists commonly apply the first two of these criteria to suggest that only unexpected inflation can permanently affect welfare. And my results show that voters make the correct distinction. They ignore expected inflation and penalize only unexpected inflation.

Further, I show that both sign and magnitude of this penalty make sense, given the size and composition of the voters' net monetary assets and the effect of possible lags in the policy response to inflation.

It is not so easy to make a politically relevant distinction between expected and unexpected income. The behavior of income time series suggests that all income changes are unexpected and permanent. However, I was able to make two conditional tests of my criteria: First, if the permanent income hypothesis is correct, consumption data imply an aggregate estimate of permanent income, and this permits a decomposition of income changes into permanent and transitory components. And I showed that voters respond to the permanent component and essentially ignore the transitory component. Second, if the main effect of policy is on aggregate income rather than its distribution across voters, local deviations from the change in aggregate income should be ignored or weighted less heavily than the aggregate change. I found that voters clearly make this local-aggregate distinction in the predicted direction.

Finally, though theory suggests no particular link between government spending and welfare, I found that voters treat greater spending as a bad. At both the federal and state level, the political market treats an extra dollar of government spending as the equivalent of a dollar less of voter wealth. Given this preference, I also find that voters satisfy the rationality test first proposed by Ricardo: the way in which spending is financed is irrelevant.

So the broad picture which emerges here is of self-interested voters who correctly process relevant information. Indeed, one would be hard put to find non-political markets which process information better than the

voting market. For example, the reader familiar with the empirical efficient-capital-markets literature will recognize the obvious analogues to my methods and the qualitative similarity of results. Thus these results deepen the mystery with which this paper began. If all voters are rationally ignorant, whence comes efficiency in the voting market? My results also raise questions about the suppliers in this market. If the voters like negative spending and inflation surprises, why have they been so disappointed for so long?

Appendix 1.The 1988 Election

At the time this is written, I have data through the end of 1987 which permit a prediction of the outcome of the 1988 election. Table A shows the results. It is based on a one-step-ahead forecast from regressions which include 1984 data, and it assumes no surprises in the economic data for 1988. The forecast is for a decisive Democratic victory. The essential reason is that slightly sub-par economic growth reinforces an otherwise slight Republican disadvantage.⁵²

To compound my risk of embarrassment, I also provide state-by-state detail on the predictions. This shows the now familiar regional tilt of the Republican vote toward the South and West. It also shows the considerable disadvantage the Republicans must overcome to win. If they took all the close states and half the electoral vote in those states "leaning Democrat," my model still predicts that they will lose.

Table A. Predicted Outcome of the 1988 Presidential Election, Based on Data Through 1987

Party	Popular Vote (%)	Electoral Vote
DEM	53.8	439
GOP	46.2	99

Distribution of States and Electoral Votes by
Predicted GOP Vote Share and Region

Predicted Outcome (GOP Share)	EAST		MIDWEST		SOUTH		WEST		TOTAL ELECTORAL VOTE
Solid DEM (<45.0)	NY	36	IL	24	GA	12	HI	4	
	PA	25	MI	20	TN	11			
	MA	13	MN	10	WV	16			
	MD	10							
	RI	4							
	DE	3							
	DC	3							181
Leaning DEM (45.0-48.9)	NJ	16	OH	23	NC	13	CA	47	
	CT	8	WI	11	LA	10	WA	10	
	ME	4	MO	11	KY	9	OR	7	
			IA	8	AL	9	NM	5	
					MS	7	MT	4	
					AR	6			208
Toss-Up (49.0-49.9)					TX	29			50
					FL	21			
(50.0-50.9)	VT	3	SD	3	VA	12	CO	8	
					SC	8	NV	4	38
Leaning GOP (51.0-54.9)	NH	4	IN	12	OK	8	AZ	7	
			KS	7			WY	3	
			ND	3			AK	3	47
Solid GOP (≥ 55.0)			NE	5			UT	5	
							ID	4	14

Note: See text for assumptions underlying predictions. As in Table 10, all regressions with lags of 24 or more months on economic data are used to generate predictions which are then averaged. (CON/TBIL regressions not used due to lack of recent TBIL data.) Popular vote share based on 1984 state distribution of total vote.

For 6 states not in regressions (SC, TN, AL, MS, AR, LA) the long-run party vote share is estimated by deducting the estimated contribution of economic variables to 1984 Republican share from the actual share. For DC, predicted 1988 Republican share is arbitrarily set = 10 percent.

FOOTNOTES

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1. See, e.g., the international studies in Whitely (1980), which divide roughly 50-50 on the issue of whether electoral outcomes are significantly affected by economic conditions.

2. See the survey in Monroe (1979) for some details on these results.

3. For example, Kramer (1971) uses the change in unemployment, while Goodman and Kramer (1975) prefer the level of unemployment.

4. An obvious extension of my results would be to test the implicit hypothesis that the connection between voting and the macro-economy was different in, say, the pre-Federal Reserve, gold standard era.

5. For example, there have been only ten post-Full Employment Act Presidential elections, and several of these were arguably affected by non-economic events (e.g., the "unusual" Goldwater and McGovern candidacies, Nixon's resignation). These time-specific elements can severely compromise the accuracy of any estimated relationship between economic variables and the elections based on only ten aggregated observations. By, in effect, replicating the relationship over up to 50 states, the importance of the time-specific elements can be reduced. To illustrate, suppose that for each state ($i = 1, 2, \dots, N$ states) the time series ($t = 1, 2, \dots, T$ years) of some vote share (Y) is related to a macro-economic variable (X) by

$$Y_{it} = b \cdot X_t + U_t + \varepsilon_{it} ,$$

where

U_t = an unobserved time-specific element (i.e., there can be
"Republican years" or "Democratic years"), and

ε_{it} = random error

The variance (V) of the estimate of b from a regression of Y_{it} on
 X_t is

$$V_b = \frac{V_u + V_\varepsilon}{V_x} \cdot \frac{1}{TN}$$

If we average the Y_{it} across the i and estimate b' from the
resulting single time series,

$$V_{b'} = \frac{N \cdot V_u + V_\varepsilon}{TN \cdot V_x} , \text{ and}$$

$$\frac{V_{b'}}{V_b} = \frac{N V_u + V_\varepsilon}{V_u + V} > 1 .$$

I.e. the single aggregate time series yields a less precise estimate as
long as $V_u > 0$.

It may be inappropriate to assume that the N time series are independent when they share common time-specific elements. The effect of taking account of the resulting dependence across time series on the size of V_b is treated in Section II.

6. This is an approximation appropriate to the problem at hand. It ignores the possibility that B's platform might have changed, or that the voter's circumstances have changed so much that unequal revisions in w^A and w^B become appropriate. For example, suppose A favors progressive taxes and the voter struck oil. The now much wealthier voter may rationally estimate a larger increment to w^B than to w^A .

7. There is a "menu cost" to positive expected inflation--prices have to be changed periodically--and a deadweight loss in the allocation of assets between non-interest bearing money and interest bearing assets. See Feldstein (1979) for a dissent, based on the latter cost, to the view that expected inflation is harmless.

8. This is apparently what the bond market does. Over the 1950-86 period the average absolute change in yields on Aaa bonds (which contain forecasts of inflation) was 60 basis points in non-Presidential election years and 60 basis points in Presidential election years. In other words, the election outcome does not seem to radically alter expected inflation rates.

9. However at the state level, a constant K may be less plausible. For example, the weakening of the Democrats in the South and the Republicans in the Northeast might imply some trend in K for those areas. For simplicity, I ignore this possibility.

10. Note however that trends in K can show up as $q > 0$.

11. In logs, rather than dollars as Hall did, to preserve constant variance of the residuals. Since consumption has drifted upward, the variance of the residual from the dollar regression tends to increase over time. I followed Hall in deducting purchases of durables from total consumption. The motive for this is the large savings element in durable purchases.

12. Consumption data are unavailable more frequently.

13. That is, their auto correlation functions pass a test for "white noise" series due to Box and Pierce (1970).

14. This reflects the near random walk in income. Given this, the autoregressive estimate of expected income is almost a constant, and, at this level of aggregation, the average consumer cannot know much more about changes in his future permanent income than the change in his current income.

15. Specifically, let

$IP = +1$ if the incumbent is Republican

-1 if Democrat,

and

$S_i = +1$ for elections in State i , 0 otherwise. I Include IP and all but one $IP \cdot S_i$ in every regression. The coefficient of $IP \cdot S_i$ is, then half the long run difference between Republican and Democrat shares in that state (as a deviation from the excluded state).

16. To see this, assume that the dependent variable, y , is generated by

$$(a) \quad y = \sum_{i=1}^t b_i X_i + \varepsilon, \quad \text{where}$$

X = economic variable (at an annual rate)

i = periods preceding the election

ε = random error

Without loss of generality, assume:

$\sum b_i = 1$, Variance of all X_i are equal ($=V$), and $\bar{X}_i = 0$ for all i

I will estimate

$$(b) \quad y = BZ + u, \quad \text{where}$$

$$Z = \frac{1}{T} \sum_{i=1}^T X_i, \quad \text{i.e. a } T\text{-period average of the } X_i.$$

So the estimated B is

$$(c) \quad \hat{B} = \frac{E(Zy)}{E(Z^2)}.$$

The denominator of (c) is

$$(d) \quad E(Z^2) = \frac{1}{T^2} (T \cdot V) = \frac{V}{T},$$

and the numerator is

$$(e) \quad E(Zy) = \frac{1}{T} \sum_{i=1}^T X_i (\sum_{i=1}^t b_i X_i + \varepsilon)$$

If T happens to be chosen correctly ($= t$), then (e) is

$$(e)_1 \quad E(Zy) = \frac{1}{T} (\sum_{i=1}^t b_i) \cdot V, \quad \text{and} \quad \hat{B} = \sum_{i=1}^t b_i = 1.$$

If $T > t$, i.e. extraneous values of X_i are included in Z

$$(e)_2 = (e)_1 + \text{covariance terms involving the } X_i \text{ for } i > t$$

and X_i for $i < t$. If these covariance terms are zero (as they would be

if the X_i are white noise) $(e)_2 = (e)_1$. If $T < t$, i.e. the lag is too

short, we have

$$(e)_3 \quad E(Zy) = \frac{1}{T} (\sum_{i=1}^T b_i) \cdot V, \quad \text{and} \quad \hat{B} = \sum_{i=1}^T b_i < 1.$$

That is, B is the sum of weights up to $T < t$, and each unit increment of T increases \hat{B} by the b_i for $i = T + 1$.

17. There is some ambiguity in this test. In the notation of the previous footnote the variance of B is proportional to $V(u)/V$, and the variance of $\sum b_i$ is proportional to $V(\epsilon)/V$. These are related by

$$(a) \quad \frac{V(u)}{V} = \frac{V(\epsilon)}{V} + (H - \frac{1}{t}), \quad \text{where}$$

H is a Herfindahl Index of the b_i with a maximum value of 1 (first period weight approaches 1, all others trivial) and a minimum of $1/t$ (all b_i equal). Thus, choosing a type of X that minimizes $V(u)/V$ does not necessarily imply that $V(\epsilon)/V$ would also be minimized, because a better fitting X concept could have more inequality in the b_i and, hence, a larger $V(u)$.

My estimating procedure constrains the weight on each X_i to be equal, and this is less appropriate the larger is H . The resulting sacrifice in the explanatory power of my regressions (v. a general distributed lag specification) "buys" extra degrees of freedom. These are valuable, even with the large number of observations I work with (about 250 to 450), because subsequent tests, which add several variables to the basic regression in equation (5), would severely tax the degrees of freedom in a full distributed lag specification. And my procedure produces unbiased estimates of the sum of weights, which is the focus of the analysis.

18. I deleted elections with an unopposed candidate. Also, states where the winning party received over 70 percent of the vote at the beginning of the sample period are deleted until the percentage is below 70 for 2 straight elections. This criterion essentially excludes a few southern

states in which the Republican party offered only token opposition for part of the period.

The gubernatorial sample is limited to elections to four year terms. Two year terms were more common early in the sample period.

19. Since the regression goes through (\bar{IS}, \bar{X}) , the intercept has to differ by party as long as $\bar{X} \neq 0$. For example suppose X is income growth ($\bar{X} > 0$), and the President is a Republican. A Democrat would do better than a Republican if $X = 0$, so $C < 0$.

20. That is the partial correlation of IS with X for $LAG\ T > 24$ was consistently greater under the second alternative for X .

21. So, X_{27}, \dots, X_{48} include information over two years old only for on-year elections.

22. The problem is less acute for Senate and gubernatorial elections, since the incumbent party differs across states in any year.

23. The procedure allows for first order auto correlation in each state's time series of residuals as well as correlation of residuals across states. Because the procedure is computationally expensive, I selected four lags-- 12, 24, 36 and 48 months--and reestimated one of the regressions at each lag.

24. These differences may partly reflect higher participation rates in Presidential election years, with those on the margin of participation more sensitive to economic conditions than regular participants. If so, the coefficients for Senate and gubernatorial elections should also be larger in on-year elections. I tested this by interacting the economic variables with an on-year dummy for those elections. In general, the on-year

coefficients tended to be significantly larger only for Senate elections and more so for inflation than income. They did not, however, approach the magnitudes of the Presidential election coefficients.

25. These, recall, generate unexpected inflation measures which look like white noise. The results using the slightly "contaminated" Δ estimate of expected inflation are similar to those using the TBIL estimates.

26. Since Table 1 shows clearly that voters use at least two years information on the X^* , shorter lags inevitably confound the effects of X^* and x . Specifically, this year's expected inflation rate is, partially, the sum of unexpected inflation last year and the year before, etc. So it is a proxy for relevant information about unexpected inflation. Similarly, last year's permanent income growth is, partially, a prediction of this year's actual income growth. Thus, if only this year's consumption and income growth are included in the regression, the latter is a proxy for relevant information about permanent income.

27. This is the period required for substantially complete response to inflation surprises. See the previous footnote for an explanation of the importance of this point.

28. Kinder and Kiewiet (1979, 1981) obtain a similar result from answers to survey questions. Panels of voters in elections from 1956-72 were asked whether they believed (1) they were better off and (2) the nation was better off. When the respondents' vote was regressed on dummies for the answers, only the dummy for (2) was consistently significant. The authors interpreted this as the dominance of altruism ("sociotropism") over self-

interest. However, Kramer (1983), using essentially the same argument made here, shows that the result is also consistent with sophisticated self-interest.

29. See Peltzman (1985) for evidence of such switching.

30. This is based on regressions for every lag from 24 to 48 months and all three types of surprise estimates. Essentially similar results hold if uninteracted state income (i.e. the average voter's income) growth is substituted for national income growth.

31. The hypothesis is stated in its weak form, to allow for some macro-effect of state taxes, which governors can influence. The direct effects of taxes on incumbents is analyzed below.

32. There is a clear implication for international comparisons: the strength of the relationship between the macro-economy and the political fortunes of the governing party should be inversely related to the openness of the economy.

33. For example, in one state 70 percent of the voters might, *ceteris paribus*, prefer the incumbent party while in another state only 50 percent might prefer the incumbent party. I adjust for such differences with State-Party dummies. My results imply that when some voters shift from these preferences, the shift tends to be permanent.

34. They would signal changes in the equilibria, and the political reward would depend on how closely the actual budget adjusted to the new equilibria.

35. For 132 of the 144 regressions summarized in the table, the $|t|$ for this coefficient > 2 . The average values of $|t|$ are 8.2, 4.1 and 2.8 in the Presidential, Senatorial and gubernatorial regressions respectively.

36. These results do not allow for any sharing of budgetary responsibility between the President and Congress. This restriction would be innocuous if the President's party always had a majority in Congress. But for almost all of the sample period, Democrats have had a Congressional majority. Accordingly for Presidential and Senatorial elections I tested the restriction by adding an interaction of the expenditure variable with the party of the incumbent. This allows for the possibility that a Senate Democrat, as representative of the "Congressional incumbent" party, would share the blame for budgetary expansion and that a Republican President, as opponent of the "Congressional party," would be blamed less than a Democrat. This test revealed no consistent pattern: the relevant coefficients were frequently significant in the Presidential regressions, but the signs were highly sensitive to the specified lag. In the Senate regressions, the coefficients were mainly indistinguishable from zero. Thus voters appear to act as if the President has the dominant influence on budgetary surprises.

37. This surplus variable departs mildly from white noise. The first order autocorrelation in the series is +.16 and the autocorrelation function cycles between $\pm .2$. This suggests that a more complicated autoregressive process than a random walk is generating the data. In view of the generally negative results in Table 7, I did not pursue this refinement.

38. Which is essentially unaltered by adding the surplus variable to the regression.

39. This sort of sensitivity to minor changes in the data seems endemic to the Presidential data. See Table 1 (inflation weights for short lags), Table 3 (weights on transitory income), and Table 5 (the spread of weights on the lagged vote share). In all these cases, there is no similar sensitivity in the Senate and Governor data.

40. A check of the autocorrelation functions of this variable indicated that it behaves like a random walk in over 90 percent of the states in the sample. Also, I could not reject the hypothesis of a common drift in the random walk across states--the short run variability dominates any long-run differences across states--so I made no adjustment for different state means.

41. These magnitudes are unaffected by the addition of state spending to the regressions.

42. In addition, voters do not distinguish federally financed from locally financed state spending. Specifically, since $\text{per capita state spending} = \text{locally financed spending} \times (1 + \text{federal aid/locally financed spending})$, I entered the growth rates of both components separately in all the regressions. The consistent result was that each had indistinguishably different negative coefficients.

43. These are averages across the income and inflation concepts and are based on three-term moving averages of the coefficients.

44. Based on flow of funds data. I classify demand and time deposits, currency, bonds, life insurance reserves, pension fund assets and "other" financial assets as monetary (nominal) assets, and all financial

liabilities (90 percent of which are mortgages and consumer credit) as monetary liabilities.

45. Of course, both the total and its distribution can be different for the average voter than the aggregate of the household sector. However, the distribution across households is unavailable. For these purposes currency and demand deposits are considered non-interest bearing assets, net short term interest bearing assets are time deposits less consumer credit and "other" liabilities, net long term interest bearing assets are bonds, claims on life insurance and pension reserves and "other" assets less mortgage debt. (Here, I ignore future tax liabilities for servicing government bonds).

46. The cash assets lose $(.01)(2000) = \$20$ per year or \$200 present value discounting at 10 percent. The short term assets lose a total of \$33 over 36 months if changes in short rates equal the change in expected inflation from the AR model in each month.

The loss on long term assets depends, in general, on the duration of their payment streams and the level of rates. If, to make things simple, all \$4000 of long term assets are in 10 year zero-coupon bonds, the capital loss of a 1 percentage point rise in long rates (from any level) would be 10 percent or \$400. Then the total loss would be $200 + 33 + 400 = \$633$.

47. This is the present value at 10 percent of a \$100 permanent addition to the \$10,000 annual income stream.

48. The inflation variable for this exercise would be an annualized sum of monthly unexpected inflation which begins at 1/12 percent per month and tapers to zero over three years. The appropriately weighted average of

1/12 and zero, from the AR regression is about 1/40 percent per month or .3 percent per year. The one shot 1 percent income shock is 1/3 percent per year over the same period.

49. If it did not, my regressions would tend to overpredict vote shares during both very good and very bad periods and underpredict in the intermediate cases. To test for this, I regressed the residuals from each regression in Table 1 for lags > 24 months on quadratics in the macro variables. Asymmetry like that in Bloom and Price implies a negative coefficient on the squared terms. These coefficients were uniformly insignificant in the Senate and Governor regressions. While they were frequently significant in the Presidential regressions, the signs alternated depending on the lag and the form of the macrovariables.

50. The reason is that, if voters believe in a Phillips Curve, the marginal value of growth is smaller in high inflation than low inflation environments--recession would be more acceptable politically if it is needed to fight inflation. The regression imposing separability would produce an estimate of the marginal political value of growth which is an average of the low value during inflation and the higher value during deflations. Accordingly this regression will underpredict the incumbents' success when there is a recession during an inflationary period (i.e. when the marginal value of income growth is low) and when there is high growth during a deflation (i.e. when the marginal value of growth is above average).

51. Fama (1982) finds a similar tendency for higher inflation to lead reductions in real output.

52. This is due to the absence of an incumbent candidate plus a tendency for long-run Republican vote shares to be low in the larger states.

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