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Aggregate Economic Variables and Votes for Congress: A Rejoinder

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Aggregate Economic Variables and Votes for Congress:

A Rejoinder

by Francisco Arcelus and Allan H. Meltzer [*]

Our interest in the effect of aggregate economic variables on election results began in 1970 following a conversation with an administation official that we have reported elsewhere. [1]   We doubted both the implicit theory of voting behavior and the ability of the administration to achieve rates of inflation and unemployment even close to the ranges mentioned. We take this opportunity to note that the unemployment rate was higher and the inflation rate substantially higher than the adviser's estimate, but President Nixon was re-elected.

At the time, the principal econometric evidence of the effects of aggregate economic variables was a study by Kramer. Kramer found evidence of an effect of real income, but despite (or perhaps because of) the flaws in his procedure, he found no evidence of an effect of inflation or unemployment. [2]   Further, then and now, most of the reported evidence pertains to congressional not presidential elections and to votes for congressmen, not seats in the Congress.

We concluded our study by failing to reject a null hypothesis -- that there was no evidence of an effect of real income or unemployment on votes for congressional candidates. We were less certain about the effect of infla-tion. Our evidence suggests some effect, and we have continued work on the problem by analyzing presidential voting and by much more detailed analysis of Congressional votes and seats.

That more work remains to be done is evident from our current work and from the lengthy replies that our paper stimulated. Each pair of authors wrote a comment longer than our original article. They neither agree with us nor with each other on the proposition that evidence supports. Each raises some points that the other ignores.

The similarity ends there. Bloom and Price offer a scholarly criticism based mainly on their original and interesting work. Their comments are based on their assessment of evidence. We discuss their work first. Goodman and Kramer, on the other hand, offer a seemingly endless number of criticisms supported by little more than prior belief, innuendo and conjecture. Answering each of the charges would take more space and time than the criticisms are worth. We are content to support our claim by discussing a few of their charges and by presenting evidence that most of their claims are empty.

Bloom and Price

Bloom and Price devote most of their comment to testing an alternative hypothesis of the effect of economic variables on congressional elections. They find evidence to support their hypothesis. If we had developed their evidence, we would have rejected the null hypothesis, as they do.

The hypothesis that Bloom and Price accept is different from Kramer's and, we will argue, much closer to our contention than to his. Bloom and Price show that a _decline_ in real, per capita income hurts the party of the incumbent president in congressional elections. They do not show that small changes in the growth rate of real per capita income hurt the

incumbent's party. Voters are not shown to be sensitive to small fluctua-
tions in the growth rate of real income. In fact, they are relatively
insensitive; a one percent fall in real per capita income costs the
incumbent's party from 0.6% to 0.8% of its vote, according to their
estimates.

From 1948 to 1974, the maximum decline in real per capita income in
an election year was 1.6% in 1954. The largest shift of votes implied by
the hypothesis is 1%, so the maximum effect on the difference between the
parties is about 2%. The effects of inflation, unemployment and small
changes in the growth rate of output are not shown.

Per capita real output has grown at an average rate of 3%. Nothing
is shown about the range from zero to three per cent. It is entirely
consistent with the results presented by Bloom and Price that small
changes in employment and output have small effects, or no effect at all,
on voting. Recessions shift votes, and major recessions shift many votes.
Marginal adjustments of economic conditions before an election have not
yet been shown to be important On the contrary, Bloom and Price show
little or no evidence that stimulating the economy helps the incumbent's
party. The short-term effect of short-term changes in economic variables
is not established by the results Bloom and Price present.[2a/]

The asymmetry of the results raises questions. Why do voters respond
to negative changes of 3% in the average growth rate but not to positive
changes or to reductions in the growth rate to 1%? One reason may be
that the new voters include new entrants to the labor force and workers

with low seniority. These individuals bear a disproportionate share of the private cost of unemployment and recession. If they become weak or strong partisans of the party out of power, and remain loyal, we would have an explanation of the asymmetry and the effect found by Bloom and Price. An effect of this kind would not be inconsistent with our hypothesis.

All in all, we find the reformulation and the evidence presented by Bloom and Price intriguing. We hope that either they or others will investigate the asymmetry in the response to changes in real income.

Goodman and Kramer

There is, for us, a considerable difference between the proposition consistent with available evidence and the conclusion reached by Goodman and Kramer. They conclude that "on the basic question of whether such effects exist, it seems to us the evidence is clear: they do." [3/]

What are these "effects"? Do voters reward and punish? Or, do they punish only, as Bloom and Price find? Do voters respond only to recession, measured by the negative growth of real income, or to inflation and recession, as Kramer concluded? Or do they respond more to inflation, than to income as we found? Do regular voters respond or is the main effect on new voters?

Goodman and Kramer do little to advance the discussion beyond the a priori position from which they start. They offer almost no evidence to support the strong, and in our view, overstated conclusions they reach.

A typical example of overstatement is the discussion of the evidence they present in Table 2. The table shows estimates of the effects of real

income, inflation and two measures of unemployment in four separate
regressions. Only one coefficient -- the effect of inflation -- is
significantly different from zero by the usual two-tailed test at the
.05 level.

These results, unlike the results of Bloom and Price, do not cause
us to reconsider our main conclusion. Inflation appears to affect the
outcome of congressional elections; the various measures of unemployment
have not been shown to have any significant effect; the current growth
rate of real income has not been shown to have a reliable effect, and
the work of Bloom and Price suggests that there is an asymmetry. Large
negative deviations are important; other deviations are much less
important or unimportant.

The discussion of unemployment in Goodman and Kramer is an
example of their a priori approach. [4/] One result shows that changes in
unemployment benefit Republicans. This result is rejected as "anomalous."
The level of unemployment benefits Democrats, and the result is accepted
as plausible. In fact, the sign of the level of unemployment is negative
for the Democrats, and the results show that the Democrats gain only
because the Republicans are hurt more. The differences are not significant.

If this were the only example of a cavalier treatment of evidence, we
would dismiss the example as an oversight. Similar examples reoccur in
the discussion of evidence and estimation, as we show in the following
sections.

Participation

Both pairs of critics accept our hypothesis that voters can abstain
instead of shifting party preference. Bloom and Price use the percentage

of the two party vote in their work and ignore the issue. Goodman and
Kramer challenge our interpretation. They assert that "participation
dropped rapidly from 1896 to 1912" (p.1) A reasonable interpretation
of their Figure 1 is that participation declined from 1896 to 1902 or
1904, so that the "historical trend" of which they speak is based on two
or three observations.

Goodman and Kramer claim that our equation is misspecified. (p.2)
We are, frankly, puzzled at this overstatement. Their Figure 1 seems to
us to show (1) a permanent shift in the participation rate in 1920
and (2) a second permanent shift about 1932. The first is negative
but larger (in absolute value) than the shift in 1932. The coefficients
for these shifts, in our participation (VP) equation, are entirely
consistent with the evidence.

Although the word "misspecification" is used repeatedly, there is
no explicit statement of the misspecification. The only evidence
Goodman and Kramer offer is from our regression equation, and this
evidence is misinterpreted. They claim, incorrectly, that the residuals
from our VP equation are not random. The most that can be said, correctly,
is that we cannot reject the hypothesis that the residuals are not
randomly distributed. If the residuals are not randomly distributed, it
does not follow that the model is misspecified in the sense that the
estimates are inconsistent.

In short, there is no basis for the statement (p.2) "the Arcelus-Meltzer
estimates of the long-term partisan shares are incorrect." A plausible
interpretation is that the serial correlation shows our inability to fully
explain short-term fluctuations in the voting percentage by introducing aggregate
economic variables into the VP equation. More remains to be done.

The Shift Voters

Goodman and Kramer introduce a long, overly formal discussion of a simple question. Where are the shift voters? To indicate the importance of the question, they cite a previous study by V. O. Key. That study, however, discusses presidential, not congressional, elections. Our recent work suggests that shifting is much more important in presidential elections

To bolster their position, they quote selectively and inappropriately. We have underlined the words included in our proposition and omitted from their quotation. With the omitted words included, the quotation is (our p. 17, their p. 4): "the principal fluctuations in the percentage of votes received in congressional elections arise from changes in the participation rate and not from shifts between parties."

No lengthy, formal analysis is required to support our proposition. All that is required is computation of the change in voting percentage in presidential and non-presidential years. The mean difference is nearly twelve percentage points, according to the estimate in our paper. This difference is a 25 per cent change in average voting participation in congressional elections between presidential and non-presidential election years. The relevance of the comparison for the proposition becomes clear once the omitted words are restored. 5/

Basic Statistical Inference

Goodman and Kramer raise what they call a "fundamental point of basic statistical inference." (p. 8) Their point is that the "fact that a certain estimate is not significantly different from zero by no means shows that the variable has no effect....The data...may be equally consistent with the possibility of very large effects."

This is nonsense, pure and simple. Regardless of the size of the coefficient, relatively low t-statistics or large standard errors imply failure to reject the hypothesis that the variable in question has no effect.

Measurement of Economic Variables.

A number of points can be discussed briefly. Some are raised by both critics.

1. We used compensation per man hour. This ignores the unemployed. This comment is puzzling. We included measures of unemployment separately. Our procedure holds a measure of real income constant when estimating the effect of unemployment.

2. Real compensation is an inappropriate measure of real income. Moreover, it is "suspect" (p.11 of Goodman and Kramer) because real compensation per man-hour rises in recession. This comment and similar comments by Bloom and Price miss the point. One of the questions that we want to answer is whether employed and unemployed workers respond in the same or in different ways to recessions. To separate the two groups we estimate the response to earnings, holding unemployment constant, and the response to unemployment, holding earnings constant. Only from estimates of this kind can we hope to learn whether the voters' response to unemployment or recession extends beyond the particular voters affected by loss of employment. The comment that we should not have deflated by man-hours is correct. We miss the effect of reductions in the work week.

3. We take no account of the agricultural sector. This is false. We note (footnote 15) that we tried a number of other measures of economic and other issues including agricultural prices.

4. There are many additional criticisms that reveal very little more than Goodman and Kramer's prior beliefs. Several relate to the use of unemployment and the procedures for computing percentages. To find whether the criticisms are substantive, we recomputed the results using: (1) Goodman and Kramer's data series, and ours; (2) using levels of unemployment, changes in unemployment, and percentage changes in unemployment; and (3) using percentages computed on the base $t-1$ and on the base t. A small sample of our results for aggregate economic variables is shown in Table 1. Others will be sent on request. Had Goodman and Kramer used some of the time lavished on their reply to compute these results, they would have found, as we did, that their prior beliefs, conjectures about possibilities and most of their criticisms are empty. [6]

Our general conclusion is that most of the Goodman and Kramer points lack substantive content. Either they are inconsequential or they concern potential not actual bias. If we printed all of the estimates using the various data sets, we doubt whether any reader would change any conclusion as a result of reading the many pages of output. [7]

Conclusion

The effects of short-term changes in economic conditions on votes
for Congress seems to us to remain unsettled. The work to date has
produced mainly null results, our own included.

Discussion of this kind occasionally leads scientists to reformulate
the disputed proposition. We find the efforts by Bloom and Price and their
evidence of interest for this reason. The proposition for which they
find support is substantially different from earlier statements of the effect of
short-term changes in aggregate economic variables on congressional votes.

Our own work has followed a different course.   The basic unit
of interest is the distribution of seats, not votes.   Investigation of
the distribution of seats requires disaggregation to the district level.
Preliminary results suggest that incumbency alone accounts for nearly
80 per cent of the variation in the partisan distribution of seats.
That leaves very little room for aggregate economic variables, but it
does not rule out a small effect. Until such effects are found, the
null hypothesis cannot be rejected.

TABLE 1

The Effect of Alternative Measures of Unemployment on the Democrats' Share of the Vote

Coefficients (t-statistics in parentheses)

Variable	Using our data			Using Goodman and Kramer data		
$\overset{o}{P}$	-.42 (3.37)	-.46 (3.97)	-.45 (3.42)	-.46 (3.93)	-.51 (4.56)	-.52 (4.04)
U	.02 (.21)			.02 (.20)		
ΔU		-.05 (.16)			-.01 (.02)	
$\overset{o}{U}$			.00 (.15)			.00 (.07)
$\overset{o}{C/P}$	.16 (1.00)	.08 (.47)	.08 (.48)	.15 (1.03)	.08 (.51)	.10 (.58)

All Percentages are Computed as $\dfrac{t-(t-1)}{t-1}$

U   =   level of unemployment

ΔU =   change in unemployment

Other variables as defined in our paper

FOOTNOTES

1/   See footnote 2 of our paper, "The Effect of Aggregate Economic
Variables on Congressional Elections" elsewhere in this issue
of the _Review_.

2/   See G. H. Kramer, "Short-Term Fluctuations in U. S. Voting
Behavior, 1896-1964," this _Review_, 65, (March 1971) pp.
Our discussion of the flaw in Kramer's treatment of minor
party votes is in footnote 16 of our paper. Once an error in the
data was corrected, the effect of inflation was found to be
significant.

2a/   Bloom and Price accept our argument that voters can abstain, but
their work neglects the influence of economic conditions on participa-
tion.

3/   Saul Goodman and Gerald H. Kramer, "Commentary on Arcelus and
Meltzer," _APSR_ December 1975, p. 20.

4/   Ibid., p. 19.

5/   The rest of the paragraph from which the quotation is drawn leaves
little doubt about the meaning of the proposition. The paragraph
states that changes in participation have a partisan (shift?) effect.

<u>6/</u>   We made available to Goodman and Kramer a printout of all of our

results including computations of the covariance matrix and other

intermediate results to facilitate comparisons. We are therefore

surprised and puzzled at comments about our errors. If there are

errors in our data or computations, Goodman and Kramer should

report them instead of offering suggestive hints.

<u>7/</u>   One point on which comment is required. Goodman and Kramer note

(p.10) that there are some substantial discrepancies between

their estimates and the results shown in Table 1 of our paper.

We have used both sets of data and, aside from differences

attributable to computer routines, we find no substantial

differences in results.