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Gary W. Cox; Michael C. Munger

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# CLOSENESS, EXPENDITURES, AND TURNOUT IN THE 1982 U.S. HOUSE ELECTIONS

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GARY W. COX  
*University of California  
San Diego*

MICHAEL C. MUNGER  
*University of Texas  
Austin*

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**S**tudents of elections have repeatedly found that the closeness of an election is modestly correlated with turnout. This may be due to a direct response of instrumentally motivated voters, but recent theoretical work casts doubt on the adequacy of this explanation. Another possibility is that elite actors respond to closeness with greater effort at mobilization. We explore the latter possibility by using FEC and state data on campaign expenditures in House, Senate, and gubernatorial races. Our results indicate that closeness has an effect at both the mass and elite levels. We also provide quantitative estimates of the effect of Senate and gubernatorial expenditure on House turnout.

**S**tudents of elections have repeatedly found that the closeness of an election is modestly correlated with turnout, whether one speaks of gubernatorial elections (Barzel and Silberberg 1973; Gray 1976; Patterson and Caldeira 1983), state legislative elections (Caldeira and Patterson 1982), congressional elections (Crain, Leavens, and Abbot 1987; Dawson and Zinser 1976; Silberman and Durden 1975), or presidential elections (Ashenfelter and Kelley 1975; Chambers and Davis 1978; Crain and Deaton 1977; Foster 1984; Kau and Rubin 1976). This pattern of evidence is consistent with two different hypotheses. First, more citizens may vote in close elections because they believe their participation has a greater chance of affecting the outcome. Alternatively, *elites* (candidates and their chief financial supporters, for example) may be more likely to mount

“get out the vote” campaigns in close elections, for precisely the same reason. At present we have little empirical basis for deciding who responds to closeness: voters only, elites only, or both.

There are, however, substantial *theoretical* reasons to expect that the direct response of ordinary voters to closeness is mild at best, so that any correlation between closeness and turnout is predominantly a mediated one: closeness stimulates elite effort, elite effort stimulates turnout.<sup>1</sup> With Federal Election Commission data on campaign expenditures, we can investigate a specific version of the elite-level hypothesis for the 1982 U.S. House elections—that closer elections stimulate campaign expenditure, which in turn stimulates turnout.<sup>2</sup>

Previous literature finds a positive correlation between total campaign expenditures and turnout (Caldeira and Patterson

1982; Dawson and Zinser 1976; Patterson and Caldeira 1983; Settle and Abrams 1976). If campaign expenditure excels closeness as a proxy for elite activity and if such activity explains the correlation between closeness and turnout, controlling for expenditure should eliminate the apparent impact of closeness on turnout. The evidence is against this last prediction, however: closeness and expenditure both matter in existing studies. Indeed, according to Dawson and Zinser (1976), expenditure—not closeness—is *insignificant* when both are used as regressors.

This indicates that (1) expenditure is not a good proxy for elite effort, (2) voters respond directly to the closeness of elections, or (3) the existing multivariate regression equations have been misspecified.<sup>3</sup> We investigate the last of these possibilities first, and then move to a consideration of the other two.

We first discuss the theoretical basis for believing that ordinary voters or elites will respond to the closeness of elections. Then we specify a model of turnout based on existing work and demonstrate that coefficient estimates can be improved by employing a better specification of electoral competition and by controlling for urbanization. We will add to existing specifications a more complete treatment of the potential impact of Senate and gubernatorial races on House turnout. And finally, we present some conclusions and directions for further research.

### Closeness and Turnout

Much research into the relationship between closeness and turnout has been inspired by the seminal work of Downs (1957) and of Riker and Ordeshook (1968). Their premise is that citizens will participate in elections if and only if the expected benefits of voting exceed the costs, a formulation that appears to predict that closeness and turnout will be

positively correlated. (The closer the election, the more likely that a single vote will prove decisive, the larger the instrumental value of voting, and the more likely that a typical voter will vote.) The problem with this line of reasoning is intimately related to the well-known "paradox of not voting" (Ferejohn and Fiorina 1974). Because the probability that a single vote will be decisive is negligible even in close elections, the instrumental value of voting is also negligible even in close elections. But then there is no prediction of a direct correlation between closeness and turnout. More precisely, the correlation is expected to be positive but trivial. As Schwartz asserts, "saying that closeness increases the probability of being pivotal . . . is like saying that tall men are more likely than short men to bump their heads on the moon" (1987, 118).

It has been suggested by various scholars that voters may overestimate the probability of their votes being decisive.<sup>4</sup> If this is so and if especially large numbers overestimate their impact in close elections, a direct correlation between closeness and turnout would again be predicted. But if we do not care to hypothesize that significant numbers of voters misperceive their chances of decisiveness, we must accept the negligibility of instrumental motivations in mass elections.<sup>5</sup>

This leaves us, however, without any direct explanation of the correlation between closeness and turnout. The only remaining possibility is some form of indirect explanation: perhaps closeness causes *elite* actors to increase their activity in the electoral process, thus stimulating turnout. The particular version of this explanation considered here is that (1) closer elections stimulate campaign expenditures and (2) expenditures increase turnout. We now consider the two parts of this hypothesis in greater detail.

A close race might cause greater expenditure for any of three related reasons. First, instrumentally motivated contribu-

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tors might give more in close elections (because their contributions have a greater chance of affecting the outcome), thereby allowing candidates to spend more. Second, even if no contributors are instrumental and all seek "access" or a specific favor (see Denzau and Munger 1986; Ferejohn and Noll 1985), one might still expect an increase in contributions in closer races. The candidates in such races will put forth a greater fund-raising effort and will be willing to promise more in the way of access or favors for a contribution of given size. Third, because candidates value a given amount of money more highly in closer races, they will be more willing to borrow, to use their own financial resources, and to incur debts that they may not be able to pay. All of these financial practices allow greater campaign expenditure in closer contests.

In practice, one finds that aggregate contributions do tend to be greater in closer elections. The correlation between percentage margin of victory in the 1982 House elections and total campaign receipts reported to the Federal Election Commission is  $-.46$ . We have been unable systematically to test whether borrowing and unpaid debts increase with closeness, but a cursory examination of the data is consistent with this hypothesis. Given that closeness does appear to stimulate campaign expenditure, the next question is whether expenditure affects turnout.

### Specifying an Empirical Model of Turnout

#### Existing Work

Existing work on turnout has used a wide variety of specifications. Rather than review them all, we focus on the specifications used by Caldeira and Patterson (1982), Dawson and Zinser (1976), Patterson and Caldeira (1983), and Settle

and Abrams (1976), all of whom examine turnout using both expenditure and closeness as independent variables. The general model is

$$\begin{aligned} \text{turnout} = & \beta_0 + \beta_1(\text{spending}) \\ & + \beta_2(\text{closeness}) \\ & + \beta_3(\text{demographics}) \\ & + \beta_4(\text{conditioning variables}) + \mu_i \end{aligned}$$

that is, turnout in the  $i$ th race is a function of candidate spending, anticipated closeness, district demographic characteristics, and other conditions that are specific to the  $i$ th election. Within this general framework, there remains wide latitude for alternative variables and functional forms. As regards spending, for example, Settle and Abrams (1976) use total per capita expenditures. Dawson and Zinser (1976) use total spending (not per capita) and also include the percentage *difference* between candidate expenditures. Caldeira and Patterson (1982) use total expenditures and the square of total expenditures to determine whether spending has a diminishing marginal impact. Patterson and Caldeira (1983) use the same two variables but in per capita form. Similar differences occur in the measurement and specification of demographic and conditioning variables.

There is less diversity in the measurement of closeness. Caldeira and Patterson 1982 and Patterson and Caldeira 1983 both use the percentage margin of victory, or the winner's percentage minus the loser's. Settle and Abrams (1976) use the winner's percentage of the vote (which, given that nearly all the elections they studied were two-candidate affairs, is econometrically equivalent to using the percentage margin). Dawson and Zinser (1976) employ voter registration figures instead of vote totals but otherwise proceed as do Caldeira and Patterson (1982).

Of the various possible specifications we have chosen that of Patterson and Cal-

**Table 1. Turnout Regressions**

Variables	Model 1	Model 2	Model 3
<b>Demographic</b>			
Education	.0415 (2.09)	.044 (2.27)	.085 (4.77)
Income	$.97 \times 10^{-6}$ (.59)	$.81 \times 10^{-6}$ (.51)	$.25 \times 10^{-5}$ (1.70)
Age	-.0043 (-3.22)	-.0042 (-3.25)	-.0049 (-4.23)
Region	.091 (8.37)	.094 (8.92)	.089 (9.40)
Percentage urban	—	—	-.101 (-5.22)
Density	—	—	$-4.3 \times 10^{-5}$ (-4.95)
<b>Expenditure</b>			
Hexp	.011 (.83)	.040 (3.14)	.039 (3.50)
Hexp <sup>2</sup>	-.0012 (-.49)	-.0053 (-2.17)	-.005 (-2.32)
<b>Other</b>			
Margin	-.102 (-3.14)	—	—
Closeness	—	$-.17 \times 10^{-5}$ (-3.20)	$-.16 \times 10^{-5}$ (-3.48)
Closeness <sup>2</sup>	—	$.28 \times 10^{-10}$ (4.42)	$.22 \times 10^{-10}$ (5.12)
Senate	.007 (.72)	.0028 (.30)	.013 (1.57)
Closing	-.0017 (-3.46)	-.0013 (-2.64)	-.0011 (-2.50)
Intercept	.23 (.92)	.15 (.63)	-.26 (-1.20)
$\bar{R}^2$	.39	.42	.55
F	20.37	20.85	28.59
N	270	270	270

Note: Dependent variable is turnout; t-statistics are in parentheses. The relevant t-statistics and significance levels are 1.65, .10; 1.96, .05; 2.29, .025.

deira 1983 (PC) as a baseline of comparison for our own work. This specification is more complete than that in Caldeira and Patterson 1982 or Settle and Abrams 1976 and is easier to replicate than that in Dawson and Zinser 1976. The PC model is as follows (precise definitions of the variables can be found in the Appendix):

$$\begin{aligned} \text{turnout} = & \alpha_0 + \alpha_1(\text{income}) \\ & + \alpha_2(\text{age}) + \alpha_3(\text{education}) \\ & + \alpha_4(\text{Hexp}) + \alpha_5(\text{Hexp}^2) \\ & + \alpha_6(\text{margin}) + \alpha_7(\text{Senate}) \\ & + \alpha_8(\text{region}) + \alpha_9(\text{closing}) + \gamma \end{aligned}$$

We have estimated this model using data from the 1982 U.S. House elections (see Model 1 of Table 1). Only those races were used for which data on spending

were available and in which there was both an incumbent (no open seats) and a challenger (no unopposed incumbents)—270 in all.<sup>6</sup> The results are on the whole similar to those obtained by Patterson and Caldeira (1983, Tbl. 2). We find, as Patterson and Caldeira and many others have, that the greater the proportion of total population older than 65, the higher the turnout and that turnout is significantly higher outside the Old South. Median education and median income both have the expected positive sign, though the coefficient on income is insignificant. Margin has the anticipated negative sign: turnout increases in close races. A simultaneous Senate race in the same state has a positive but insignificant impact. Finally, the number of days separating the close of registration from the election (“closing”)

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has the impact predicted by Rosenstone and Wolfinger (1978): the easier it is to register, the greater the turnout.

The expenditure variables perform less well. Although expenditure appears to increase turnout at a decreasing rate ( $\text{Hexp} > 0$ ,  $\text{Hexp}^2 < 0$ ), neither coefficient approaches significance. Taken at face value, Model 1 of Table 1 indicates that purely atomistic individual choices based on the probability (proxied by margin) of having an impact on the election *do* affect turnout but that actions by elites (as measured by the expenditures variables) *do not*. We argue that this conclusion is the artifact of a misspecified estimating equation. We focus in particular on the use of the margin variable and on the exclusion of variables measuring the urbanization of district populations.

### The Role of Electoral Margin in Determining Turnout

The most common measure of the closeness of elections is what we have labeled "margin," or the winner's percentage of the vote minus the loser's. Cox (1988) has urged two objections to this variable, one conceptual and one econometric. The conceptual objection is simply that a given percentage margin of victory may not indicate the true capacity of individuals to affect the outcome. Consider two different elections in which margin equals 33%. In the first, assume that there are 100 thousand voters, so that 33 thousand votes separate the winner from the loser. No single campaign worker or voter is likely to think that a little extra effort in the fourth precinct—much less a single vote—will swing this election. If on the other hand there are but three voters, a 33% margin implies a two-to-one tally, and a single vote is decisive.

The econometric objection to the use of percentage margin is more serious than the conceptual. Because total votes cast appear by construction in the *numerator*

of turnout and in the *denominator* of margin, it is clearly possible that the observed negative correlation is spurious.<sup>7</sup> Cox (1988) argues that a superior specification is to use closeness and closeness<sup>2</sup> in place of margin (where closeness is the raw vote margin, or the number of votes for the winner minus the number of votes for the loser). The test of the "closeness-counts" hypothesis then rests on the sign—which should be negative—of the estimated coefficient on closeness.<sup>8</sup>

Our estimates of the PC model using the closeness variables instead of margin are given in Model 2 of Table 1. Three points should be emphasized. First, the coefficients of both closeness and closeness<sup>2</sup> are of the predicted sign and significance at the .001 level. Second, the overall fit of the regression, as measured by the adjusted r-squared, is improved. Finally, the performance of the expenditure variables,  $\text{Hexp}$  and  $\text{Hexp}^2$ , is markedly improved. Indeed, the estimated coefficient on  $\text{Hexp}$  is increased nearly fourfold, from .011 to .040, while the standard error is practically unchanged.<sup>9</sup> Given the similarity of our model to the PC model, the use of margin likely entails underestimation of the impact of spending in the PC model of gubernatorial races as well.

### Omitted Variables: Controlling for Urbanization

The problems of bias associated with omitted variables are well known (see, e.g., Judge et al. 1980, 410–13). One variable currently omitted from our regression is the level of urbanization of each congressional district. This omission may be important, because urbanized districts tend to have both higher costs of campaigning (partly because everything is more expensive in the city) and lower turnout (a phenomenon that Squire, Wolfinger, and Glass [1987] attribute to the higher mobility—hence higher regis-

tration costs—of urban dwellers). These two separate influences of urbanization—higher campaign costs and higher voting costs—may have introduced a downward bias in the estimated coefficient of expenditure in Models 1 and 2. We employ two variables to measure urbanization in investigating this possibility. The first—percentage urban—equals the proportion of the district population living in places with 25 hundred or more persons. Because this variable by itself does not distinguish between “small town urban” and “big city urban,” we also include a variable measuring the population density of each district.<sup>10</sup>

The results obtained by adding these two urbanization variables to Model 2 are displayed in Model 3 of Table 1. As can be seen, the coefficient on the expenditure variable—Hexp—is not increased as we had conjectured it might be; rather, it remains stable, and increases somewhat in significance.<sup>11</sup> Nonetheless, it is clear that including the urbanization variables improves the specification of the model considerably. Both density and percentage urban have estimated coefficients that are of the expected sign and significantly different from zero. The coefficients of education and income (both of which covary with the previously omitted urbanization variables) double in size, while their standard errors remain stable or decrease. Nearly every other estimated coefficient is similarly improved in precision. Finally, the adjusted r-squared increases noticeably, from .42 to .55.

The main point to emphasize about both Models 2 and 3 is that expenditures (Hexp) do appear to stimulate turnout in House races. We turn now to another possibility—that expenditures in other, simultaneous races also affect House turnout.

### Getting to the Polls

Studies of turnout typically focus on a

single office without fully accounting for the impact of campaigns for other offices. At most, a dummy variable is inserted in regression equations to control for the influence of a presidential or senatorial contest.

There is reason, however, to expect that specific characteristics of presidential, gubernatorial, and senatorial campaigns will affect turnout in lower offices. This can be seen by considering the case of House elections. Let  $t$  denote the number of votes cast for U.S. representative in a given district, and let  $e$  denote the number of adults over eighteen in the same district. Then  $t/e$  is the turnout rate as usually calculated. This figure, however, can usefully be expressed as the product of two slightly different turnout rates, as follows:

$$\frac{t}{e} = \frac{b}{e} \cdot \frac{t}{b}$$

In this equation  $b$  is the number of persons who enter the voting booth. Thus,  $b/e$  is the proportion of age-eligible citizens who go to the polls. The second term,  $t/b$ , is the proportion of those in the booth who participate in the House election.

Metaphorically,  $t/b$  (or  $1 - (t/b)$ ) measures only the last and lowest hurdle facing voters. The first hurdle is registering. The second hurdle is getting to the polls and entering the booth. The last is actually marking the ballot to signify a vote in the race for U.S. representative.

From the perspective of explaining levels of turnout this last hurdle is also the least important. Typically over 90% of those entering the booth cast a vote for a House candidate; it is the previous two hurdles that account for the vast majority of those age-eligibles who do not.<sup>12</sup> This point is important because the proportion of age-eligibles entering the booth ( $b/e$ ) is potentially affected by a wide variety of factors. The closeness and level of expen-

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diture in the House election may affect the decision to enter the booth. But so may the closeness and level of expenditure in other major contests, especially those for governor, senator, and president. Thus it is sensible to consider including closeness and expenditure variables for *all* major races, even when the researcher's interest focuses on House turnout. The extent to which such other races affect House turnout is a matter for empirical investigation.

In order to begin such an investigation, we have respecified Model 3 as follows:

$$\begin{aligned} \text{turnout} = & \alpha_0 + \alpha_1(\text{demographic} \\ & \text{variables}) + \alpha_2(\text{House expenditures}) \\ & + \alpha_3(\text{House closeness}) + \alpha_4(\text{closing}) \\ & + \text{Senate}[\beta_0 + \beta_1(\text{Senate} \\ & \text{expenditures}) + \beta_2(\text{Senate} \\ & \text{closeness})] + \text{governor}[\delta_0 \\ & + \delta_1(\text{gov expenditures}) \\ & + \delta_2(\text{gov closeness})] + \epsilon. \end{aligned}$$

The dummy variables Senate and governor take values of unity only when a Senate or gubernatorial race is held simultaneously with the House race; otherwise, both are zero. Thus, in states having neither a Senate nor a governor's race (so that Senate = governor = 0), the model is exactly as before; but in states with a Senate or gubernatorial race or both the separate impacts of these races are measured. The constant terms, Senate \*  $\beta_0$  and governor \*  $\delta_0$ , allow us to test the proposition that the mere occurrence of these races, with associated media attention, influences turnout—independently of the closeness of the races or the level of campaign expenditures.

Four estimations are presented in Table 2, each adding additional variables to the specification in Model 3. Model 4 simply adds the governor dummy, which has virtually no effect on the size and significance of the previously estimated coefficients. The governor dummy itself is posi-

tive and marginally significant, as is the Senate dummy, lending some support to the idea that concurrent gubernatorial or Senate races do boost House turnout.

The possibility remains, of course, that gubernatorial or Senate contests boost House turnout significantly only when they entail significant expenditures. We test this idea by first adding total per capita expenditures in the Senate campaign (Sexp) and then total per capita expenditures in the gubernatorial campaign (gexp). Unfortunately, both these variables take the same value for every congressional district in a given state, because campaign expenditure data for the offices of senator and governor are not broken down by district. This limitation in the data should bias our test against finding any significant cross-election expenditure effects.

Nonetheless, both Senate expenditures and gubernatorial expenditures do show a positive and significant impact on House turnout. When Sexp is added to the equation (Model 5), there is a slight adjustment of some of the demographic variables (most notably, the estimated coefficient on income increases and becomes significant at conventional levels); but the major effect on previously included variables is that the Senate dummy is reduced to insignificance (both substantively and statistically). The estimated coefficient on Sexp is positive and significant, and the adjusted r-squared of the equation improves, from .55 to .58. Adding gexp has very similar consequences (Model 6). The governor dummy becomes insignificant. The estimated coefficient on gexp is positive and significant. The adjusted r-squared improves again (this time to .61). Little else is changed.

Model 6 demonstrates convincingly that the use of dummy variables to control for cross-election effects on turnout is inadequate. An F-test comparing Model 6 (dummies and expenditure variables) to Model 4 (dummies only) rejects the null



**Table 2. Turnout Models with House, Senate,  
and Gubernatorial Spending Figures**

Variables	Model 4	Model 5	Model 6	Model 7
<b>Demographic</b>				
Education	.082 (4.63)	.074 (4.20)	.075 (4.46)	.076 (4.51)
Income	$.25 \times 10^{-5}$ (1.71)	$.35 \times 10^{-5}$ (2.41)	$.34 \times 10^{-5}$ (2.45)	$.33 \times 10^{-5}$ (2.39)
Age	-.0049 (-4.21)	-.0052 (-4.55)	-.0065 (-5.70)	-.0064 (-5.74)
Region	.091 (9.54)	.086 (9.22)	.081 (9.03)	.082 (8.90)
Percentage urban	-.104 (-5.09)	-.11 (-5.43)	-.101 (-5.24)	-.10 (-5.46)
Density	$-.43 \times 10^{-5}$ (-5.27)	$-.39 \times 10^{-5}$ (-4.85)	$-.43 \times 10^{-5}$ (-5.45)	$-.42 \times 10^{-5}$ (-5.35)
<b>Expenditure</b>				
Hexp	.038 (3.38)	.036 (3.28)	.031 (2.94)	.032 (3.01)
Sexp	—	.015 (3.77)	.016 (4.03)	.013 (3.14)
Gexp	—	—	.011 (4.82)	.011 (4.62)
Hexp <sup>2</sup>	-.0048 (-2.23)	-.0046 (-2.20)	-.0040 (-1.99)	-.0014 (-2.08)
<b>Other</b>				
Closeness	$-.17 \times 10^{-5}$ (-3.60)	$-.14 \times 10^{-5}$ (-3.02)	$-.15 \times 10^{-5}$ (-3.48)	$-.14 \times 10^{-5}$ (-3.26)
Closeness <sup>2</sup>	$.22 \times 10^{-12}$ (5.18)	$.19 \times 10^{-12}$ (4.43)	$.19 \times 10^{-12}$ (4.63)	$.19 \times 10^{-10}$ (4.49)
Senate	.014 (1.65)	-.002 (-.256)	-.003 (-.31)	-.017 (-1.35)
Governor	.011 (1.52)	.014 (1.93)	-.0059 (-.73)	.0097 (.97)
Closing	-.001 (-2.34)	-.001 (-2.17)	-.001 (-2.51)	-.0014 (-3.17)
Senate margin	—	—	—	-.00085 (-3.21)
Governor margin	—	—	—	.033 (.857)
Intercept	-.24 (-1.105)	-.13 (-.60)	-.019 (-.095)	-.049 (-.19)
$\bar{R}^2$	.55	.58	.61	.61
F	26.70	27.09	29.03	26.23
N	270	270	270	270

Note: Dependent variable is turnout; t-statistics are in parentheses.

hypothesis that the coefficients of gexp and Sexp are both zero. Including these variables clearly improves the specification of the model.<sup>13</sup>

Model 6 also demonstrates that both expenditure levels and closeness have independent impacts on turnout. An increase of \$1 in spending per capita by House candidates increases turnout by 3.1%. A thousand-vote increase in raw vote margin decreases turnout by .15%. It is difficult to compare the magnitude of these coefficients, since the units of measurement differ. In terms of beta weights (a comparison that of course has its own

problems; see Hanushek and Jackson 1977, 78), closeness has a larger impact. A one-sample standard deviation increase in closeness leads to a .45-sample standard deviation increase in turnout; the corresponding figure for Hexp is .33.

It is interesting to note that at least in our sample both Senate and gubernatorial expenditures are less effective in stimulating House turnout than are House expenditures: the estimated coefficient of Hexp is .031, roughly twice that of Sexp (.016) and three times that of gexp (.011). This difference may in part be artificial—due to our inability to disaggregate Senate and

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gubernatorial spending by district. It may also be due to the fact that House spending should increase both  $b/e$  (the proportion of the electorate going to the polls) and  $t/b$  (the proportion of those getting to the polls who participate in the House election), whereas Senate and gubernatorial spending should increase only the former. Although the similar magnitude and significance of the  $Sexp$  and  $gexp$  coefficients suggests that the marginal impact of Senate and gubernatorial campaign spending is similar, an F-test rejects the null hypothesis that the two coefficients are equal at the .01 level.<sup>14</sup>

Finally, we consider the possibility that the closeness of other races affects House turnout, separate from expenditures on these simultaneous campaigns. In Model 7, margin variables for both Senate and gubernatorial races are included. The use of the percentage difference (margin) rather than the numerical difference (closeness) is dictated by two considerations.<sup>15</sup> First, since the potential electorate in the simultaneous races is the entire state population rather than the district population, the econometric objections to the percentage variable do not apply. Second, in electorates of varying sizes, the percentage margin is more appropriate as a measure of closeness because it controls for the size of the electorate. This consideration was unimportant for House districts, since all are more or less the same size, but it may affect results estimating the impact of competitiveness in Senate or gubernatorial races.

As it turns out, our results indicate that more competitive Senate races *do* increase House turnout in the same state but that more competitive gubernatorial races *do not*, once expenditures in both races are controlled. The coefficient on Senate margin indicates that the average effect of going from a 51%–49% Senate race to a 56%–44% one was to decrease House turnout in the same state by .8%. The coefficient on governor margin was of the

wrong sign, and insignificant.

One possible explanation for the contrast between Senate and gubernatorial effects is as follows. Both Senate and gubernatorial *expenditure* have an impact on House turnout because some expenditure goes toward get-out-the-vote efforts. These efforts get people to the polls more or less independently of their awareness of issues. And once in the booth, a substantial fraction of these people also vote in the House election. Smaller Senate margins increase House turnout in the same state because instrumentally motivated voters turn out in greater numbers. Once in the booth, these voters—who are more likely to care about some federal issue—also vote in the House election. Smaller gubernatorial margins have no effect because they stimulate participation by those interested in state issues, who then roll off (do not vote in the House election). Another possibility is that Senate and gubernatorial closeness (once expenditures are controlled) have little impact on people's going to the polls ( $b/e$ ), but do affect roll-off, with gubernatorial closeness affecting only roll-off in state offices and Senate closeness affecting roll-off in federal offices. This hypothesis is falsifiable and could be tested, given information on the number of ballots cast in each election.<sup>16</sup>

### Conclusion

To what extent are the turnout decisions of ordinary citizens instrumentally motivated? Are instrumental calculations regarding turnout confined to those with bigger stakes and more ample resources? Most current research, both in rational choice models and elsewhere, argues against any large instrumental component in the decisions of ordinary citizens. Nonetheless, a fairly substantial body of empirical literature, using aggregate data, finds a persistent—albeit modest—corre-

lation between turnout and the closeness of elections, suggesting that instrumental calculations are taking place at some level.

We began this research believing that the closeness-turnout correlation was real despite some shortcomings in previous research but also that the correlation probably did not arise primarily in the reactions of the mass electorate. Rather, it seemed more plausible that closeness stimulated elite activity, thereby stimulating turnout, much as V. O. Key argued long ago in *Southern Politics* (1949). Our particular approach to exploring and testing this hypothesis is to use campaign expenditures as a measure of elite activities.

Using campaign finance data from the Federal Election Commission and the states, we have examined the impact of expenditures on turnout in the 1982 U.S. House elections. The expenditure data include not just spending by candidates in each House election but also spending by candidates in concurrent Senate and gubernatorial elections. Senate or gubernatorial expenditures may get people to the polls. Once there, they are likely also to participate in the House race.

Our results indicate that the correlation between closeness and turnout is due in part to an elite-level response rather than to a mass electoral response. Closeness clearly stimulates House expenditures and House expenditures do boost turnout. However, even controlling for House expenditures, raw vote margins still have an independent impact.<sup>17</sup> Even if the omitted elite activities (in-kind contributions, volunteer mobilization, etc.) were important determinants of turnout, such activities are likely to covary more with expenditures than with closeness. Thus, the significant coefficient on closeness implies at least some *direct* response on the part of ordinary citizens to the closeness of elections. We have incrementally adjusted our prior beliefs accordingly. Whether this influence results from cal-

culations by "subelectorates" (Schwartz 1987) or some other path remains to be seen.

The work reported here also demonstrates that expenditure levels in other races (specifically, Senate and gubernatorial races) affect House turnout. Our results are not surprising, but represent the first quantitative estimates of cross-election effects. In the 1982 congressional elections, an extra dollar in per capita spending by Senate and gubernatorial candidates increased *House* turnout by 1.6% and 1.1%, respectively. It remains to determine whether these figures remain stable from year to year and to estimate the impact of presidential expenditure.

Another topic for future research is the stage at which closeness counts. One interpretation of our results suggests that closeness may not be very important in getting people to the polls. It appears that one or more high expenditure races (of several concurrent contests) bring voters out, and closeness exerts its predominant effect only on participation inside the booth (i.e., roll-off).

### Data Appendix: Descriptions and Sources

The data used in the analysis came from the 1982 federal elections plus simultaneous gubernatorial races. Technically, 435 House races were available, along with 33 Senate and 32 governors' contests. All the Senate and gubernatorial data were employed in the analysis, but only 270 House races were appropriate for our purposes; the remaining 165 districts were deleted from the data set.

Four criteria were used to determine whether to retain an observation:

1. Open seats (i.e., seats with no incumbent) were deleted. We expect competition for such seats to be of a fundamentally different nature and beyond the scope of the present work.

## Closeness, Expenditures, and Turnout

2. Unopposed incumbents were deleted. "Unopposed" incumbents are those who had no challengers or whose challenger spent no money and received less than 5% of the vote. For such seats, no "election" really took place, and almost all votes cast were the result of turnout for other races.
3. Elections for which campaign contribution records were incomplete or unavailable were deleted. If no spending reports were made to the FEC by either the incumbent or the challenger, the observation was not used because of its potentially misleading impact on the expenditure coefficients.
4. Elections in which both candidates spent less than one thousand dollars were treated as being effectively unopposed and were also deleted. Because the FEC does not record expendi-

tures totaling less than one thousand dollars, we really do not know the expenditure levels in these races in any event (except that they all lie somewhere between zero and two thousand dollars).

The breakdown of observations was as follows:

435 House races  
 -57 open seats  
 -27 unopposed  
 - 1 incomplete spending data  
 -80 \$1000 or less spending  
 270 total observations

A description of the actual variables used and their sources is presented in Table A-1.

**Table A-1. Variable Descriptions and Sources**

Variable Name	Description	Data		
		Mean	Minimum	Maximum
Turnout	number of ballots cast in 1982 House race divided by total district voting age population <sup>a</sup>	.426	.148	.675
Margin	votes for winner minus votes for loser, divided by total votes <sup>b</sup>	.256	.006	.695
Senate margin	votes for Senate winner minus votes for loser, divided by total <sup>b</sup>	.119	.003	.446
Governor margin	votes for governor winner minus votes for loser, divided by total <sup>b</sup>	.007	.005	.512
Closeness	votes for winner minus votes for loser <sup>b</sup>	39,390	1,197	122,700
Hexp	total expenditures in House race (incumbent plus challenger) divided by district voting age population <sup>c</sup>	\$1.2360	.0958	6.3900
Sexp	total expenditures in Senate race divided by state voting age population <sup>c,d</sup>	\$.732	.001	6.938
Gexp	total expenditures in gubernatorial race divided by state voting age population <sup>e</sup>	\$1.12	.51	22.55

TABLE A-1 (continued).

Variable Name	Description	Data		
		Mean	Minimum	Maximum
Age	percentage of voting age population younger than 65 <sup>f</sup>	84.5	69.3	95.7
Region	dummy variable equal to zero if district is in one of the 12 "confederate states," one otherwise	.80	.00	1.00
Income	median family income <sup>f</sup>	\$20,531	11,578	33,404
Education	median years of school completed by voting age population <sup>f</sup>	12.5	11.5	13.8
Governor	dummy equal to one if there was a simultaneous gubernatorial election in the state, zero otherwise <sup>b</sup>	.63	.00	1.00
Senate	dummy equal to one if there was a simultaneous Senate election in the state, zero otherwise <sup>b</sup>	.72	.00	1.00
Closing	the number of days between the close of registration and the election, according to state election laws <sup>d</sup>	24.89	.00	50.00
Percentage urban	percentage of the total population that is "urban" or lives in a place of population $\geq$ 2,500 (U.S. census definition) <sup>f,h</sup>	.7200	.1889	1.0000
Density	population density, or voting age population divided by the area of the district in square miles <sup>f</sup>	1,381.00	.47	63,485.00

<sup>a</sup> Ballot data from *Congressional Quarterly Weekly* (various issues). District voting age population from U.S. Bureau of the Census, *Congressional District Data Book* (Washington: GPO, 1982).

<sup>b</sup> *Congressional Quarterly Weekly*.

<sup>c</sup> Federal Election Commission, *FEC Reports on Financial Activity 1981-1982* (Washington: FEC, 1983).

<sup>d</sup> State voting age population from J. L. Gardner and L. E. Purcell, eds., *The Book of the States 1982-1983* (Lexington, KY: Council of State Governments, 1982).

<sup>e</sup> Thad L. Beyle, "The Cost of Becoming Governor," *State Government* 56(1983):74-84.

<sup>f</sup> U.S. Bureau of the Census, *Congressional District Data Book* (Washington: GPO, 1982).

<sup>g</sup> J. L. Gardner and L. E. Purcell, eds., *The Book of the States 1982-1983* (Lexington, KY: Council of State Governments, 1982).

<sup>h</sup> U.S. Bureau of the Census, *1980 Census of Population and Housing*, pts. 1-52 of *Congressional Districts of the 1980s* (Washington: GPO, 1980).

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## Notes

The authors thank Robert Arseneau, Gregory Caldeira, Evelyn Fink, Morris Fiorina, Kevin Grier, Gary Jacobson, Mathew McCubbins, and Kenneth Shepsle for their helpful comments. Timothy McGinnis provided us with valuable assistance in obtaining the data on spending in gubernatorial races. James Ingram and Jocelyn Sargent provided valuable research assistance.

1. Eligible citizens personally contacted by party workers are more likely to vote both in local elections (Bochel and Denver 1971; Eldersveld 1956; Rossi and Cutright 1961) and in national elections (Kramer 1970-71). Kramer estimates that among people contacted who would not otherwise have voted, between one- and two-thirds were prompted to vote.

2. Not all campaign expenditure increases turnout, of course. Many types of expenditure may be neutral or nearly so with respect to turnout (e.g., efforts of persuasion targeted at areas or groups whose turnout is already high). Some may even depress turnout (see Cox and Kousser 1981). Because the FEC reports campaign expenditure by category, one can only ask whether, on balance, expenditures increase or decrease turnout.

3. We take a fourth possibility—that elites do not influence turnout—as having been rejected by the existing literature. We also note but do not explore the possibility that Hinich (1981) advances—that voting is itself an act of contribution at the atomistic level.

4. See, e.g., Riker and Ordeshook 1968 and Moe 1980, both of which point out that candidates have an incentive to mislead their followers on this score. Further, psychological research indicates that people do often make errors when dealing with small probabilities (Fischhoff and Goitein 1984; Kahneman and Tversky 1973, 1979; Lichtenstein and Slovic 1971).

5. Another approach to restrengthening the theoretical importance of instrumental motivations is Ledyard's (1984). Palfrey and Rosenthal (1985) show that this approach fails.

6. When races without challengers were included, significance of the demographic variables improved, implying that poor, uneducated districts are less likely to have effective challengers, as one might expect. See the Appendix for further discussion of the criteria used to select data.

7. Turnout = total votes cast/citizens old enough to vote; margin = (votes for victor - votes for loser)/total votes cast.

8. The inclusion of closeness<sup>2</sup> controls for the fact that beyond a certain point raw vote margin can increase only if total turnout also increases. It is expected that closeness<sup>2</sup> will have a positive coefficient.

9. Beyond the three differences noted in the text,

Models 1 and 2 are quite similar. The demographic coefficients are virtually unchanged in size and significance, and the conditioning variables (Senate and closing) only slightly reduced.

10. The correlation between density and percent urban is .327—positive but not so high as to pose any problem with multicollinearity. For a useful discussion of the media factors that influence public opinion see Page, Shapiro, and Dempsey 1987.

11. It is possible, however, to derive the predicted effect of urbanization on expenditures: if the sample is confined to districts that are both (1) more than median percentage urban and (2) more than median population density, the coefficient on Hexp is significantly reduced in size (from .039 to .025).

12. In fact, over the past five presidential elections, the ratio of total House vote to total presidential vote exceeds 90%, with individual district ratios near 100% not at all uncommon (data from various issues of *Congressional Quarterly Weekly*).

13. Inclusion of the square of Senate and of gubernatorial expenditure did not reveal any diminishing returns.

14. The value of  $F(1,254)$  is 23.24. If one performs the same F-test in Model 7, however, the difference between the coefficients on Sexp (.013) and gexp (.011) is not significant.

15. We are grateful to Gary Jacobson for making this suggestion.

16. Crain, Leavens, and Abbot (1987) provide some support for the "closeness matters in the booth" perspective, but they use only ratios of House-to-Senate turnout and exclude expenditures as regressors.

17. One possibility is that were we able to measure other forms of elite activity (volunteer man-hours of activists? unrecorded in-kind contributions?) accurately and enter them separately in the regression, then, finally, the coefficient on closeness would be insignificant.

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Gary W. Cox is Associate Professor of Political Science, University of California at San Diego, La Jolla, CA 92093.

Michael C. Munger is Assistant Professor of Government, University of Texas at Austin, Austin, TX 78712-1087.



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### **Residential Mobility and Voter Turnout**

Peeverill Squire; Raymond E. Wolfinger; David P. Glass

*The American Political Science Review*, Vol. 81, No. 1. (Mar., 1987), pp. 45-66.

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