Issue Accountability and the Mass Public[*]

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Abstract

Under what conditions, if any, does the mass electorate hold congressional members accountable for their records on specific issues? We examine this question on the issue of crime, for which salience has varied substantially and opinion has favored Republicans, and the environment, for which salience has not varied much and voters have favored Democrats. Because different parametric specifications produce divergent findings, we utilize matching analysis in addition to ordinary least squares. The tests suggest that issue accountability exists even controlling for a member's overall record. However, such accountability depends crucially on issue salience and a member's partisan affiliation.

[*] Larry Bartels, Patrick Egan, Sandy Gordon, Greg Huber, Kosuke Imai, Luke Keele, Jon Krosnick and Jee-Kwang Park are gratefully acknowledged for helpful comments and conversations. The authors also thank Gary Jacobson for providing data on challenger quality, and Tim Groseclose and Alan Wiseman for providing the code for the Groseclose, Levitt, and Snyder method of adjusting interest group ratings.

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Can U.S. congressional members ignore mass opinion on particular issues so long as their overall voting record is not too extreme, or does some sort of issue-specific accountability exist? And if it does exist, to what extent does it depend on factors such as a member's party affiliation and the level of public concern? Surprisingly, these questions have received little attention in the literature on U.S. legislative politics.

Various studies indicate that members behave as if they face issue-specific accountability, which we label "issue accountability" (e.g., Kingdon 1977; Arnold 1990). Yet, survey evidence suggests that members' beliefs may be mistaken. For instance, Wolpert and Gimpel (1997) report that forty-six percent of the public could not correctly recall how their reelection-seeking Senator voted on the Clarence Thomas nomination, either because the respondent guessed incorrectly (27.5%) or would not hazard a guess (18.5%). More generally, survey research does not uncover high levels of citizen knowledge (e.g., Jacobson 1987). These findings indicate that while on occasion an isolated roll call may affect members' electoral fortunes, more routine voting—even on highly salient issues—may not.

This paper develops general expectations about the conditions under which legislators should face issue accountability. Among other things, we argue that partisan affiliation should affect this likelihood. If voters perceive a member's party to be out of step with public opinion, or relatively incompetent on the issue, then she will be more likely to suffer electoral ramifications from voting against the district. We also argue that issue accountability will be positively correlated with the level of salience or public concern.

To test these arguments, we analyze two complementary issues, crime and environmental policy. We focus on crime because the mass salience of this policy has varied substantially over the span of several decades and at the peak was extraordinarily high. Moreover, concern about

the issue has crossed demographic and partisan categories.[1] In keeping with this broad concern, the issue has played prominently in political campaigns. Candidates have scrambled to portray themselves as "tough on crime" in order to appeal to a public that has overwhelmingly favored such toughness (e.g., Ansolabehere and Iyengar 1997[1995]; Wilson 1997).

Environmental policy provides a complementary issue in several ways, which we detail as the paper proceeds. Briefly, public concern about the environment has been lower than that for crime and has not varied substantially. Additionally, while opinion has generally favored the Republican Party's more punitive approach to crime, voters have favored the Democrats on environmental policy.[2] At the same time, however, the public has been more divided on environmental than crime policy.

To investigate accountability on these issues, we assembled new data on House members' roll call records between 1987 and 2004. Because no interest group maintained legislative scorecards on the issue of crime for these years, we created our own "crime scorecard." For the environment, we use the League of Conservation Voters (LCV) ratings. We focus on the House because research suggests that in the two years before reelection accountability is highest (e.g., Figlio 2004), candidate visibility is greatest (Franklin 1993) and correspondingly, Senators moderate their voting (e.g., Wright and Berkman 1986). We would expect the results to extend to Senators, at least for the last two years of their term, but leave that topic for future research given space constraints and the complexities involved in examining differential effects across a term.

The analysis employs ordinary least squares (OLS) and matching, a nonparametric technique that is relatively new to political science but gaining usage (e.g., Keele, et al. 2004; Ho, et al. 2007). The technique generates a "matched" dataset in which observations are paired to

be similar across all variables except for the "treatment," or key variable, and uses these pairings to estimate the effect of the treatment. By doing so, the approach can dispense with standard parametric assumptions about functional form. This is important because from a theoretical standpoint, it is not obvious what particular curvilinear relationship a member's overall ideological record or other electoral determinants should have on electoral outcomes, and the particular assumptions about functional form turn out to affect the results with OLS.

The article proceeds as follows. Section one provides a brief literature overview. Section two develops expectations regarding the conditions under which issue accountability should be most likely. Sections three and four describe the data and methods, respectively. Section five discusses the results for crime policy, and section six those for environmental policy. The seventh section concludes by discussing broader implications of the findings for issue accountability and legislative behavior.

Literature Review

The literatures on issue accountability and issue ownership have fairly substantial histories. However, by and large these literatures have not focused on U.S. legislative elections. Research on issue ownership—whereby a candidate or party can increase their electoral prospects by calling attention to issues on which the (candidate's) party has an advantageous reputation— has generally focused on parliamentary or presidential races (e.g., Budge and Farlie 1983; Petrocik 1996; Belanger and Meguid 2008). Moreover, as Belanger and Meguid (2008, 478) point out, this body of work has primarily examined the behavior of elite actors while "issue ownership *voting*…is seldom examined…in the literature in general (emphasis original)."[3]

Compared with issue ownership, the topic of issue accountability has received greater attention in the literature on U.S. legislative politics, but even here, questions remain regarding

when and even whether such accountability exists. Several studies indicate that the ideological orientation of a legislator's record affects her electoral performance, but these studies do not analyze whether accountability occurs beyond this general record (e.g., Canes-Wrone, Brady, and Cogan 2002; Griffin and Flavin 2007).[4] By contrast, a few studies examine high-profile roll calls. For instance, Bovitz and Carson (2006) discover that a *Congressional Quarterly* key vote is significantly more likely to affect an incumbent's electoral performance when the *New York Times* writes about the vote on the front page. Likewise, Jacobson (1996) finds that Democrats lost electoral votes for supporting the North American Free Trade Agreement and Deficit Reduction Act of 1993. Overall, this research indicates that issue accountability may be limited to a very small number of high-profile votes.

More significant evidence comes from work on "issue publics," which generally refers to a group of citizens whom reliably pay attention to a particular policy (e.g., Krosnick 1990, 59; see also Hutchings 2001, 2003). For instance, labor union members and their families steadily care about labor-related matters (Hutchings 2003). Central to this line of work is the idea that a tight-knit, distinct group forms the issue public. As Hutchings (2003, 141) concludes:

> "Finally, an unfortunate implication of this study is that democratic responsiveness merely requires the presence of relatively small, discrete groups that care intensely about particular issues. While there is no shortage of such groups in American society, there are also many important issues that do not easily lend themselves to groups of this type. For example, issues affecting consumers in general…do not appeal primarily to a recognizable segment of the electorate. Are politicians less responsive on these issues? It is difficult to provide a definitive answer to this question, although it would seem that such legislation does suffer from the lack of a natural constituency."

In short, the conventional wisdom suggests that members may not be held accountable for their voting on issues that lack a cohesive, recognizable group. Moreover, even when a recognizable group exists, we know little about whether accountability depends on a member's partisan affiliation or the general public's level of concern.

Some might argue an issue public exists any time a policy is highly salient to a discernible portion of the public. Such a definition seems redundant to the concept of issue salience. We accordingly do not adopt this definition. However, if one were to do so, then the findings of this paper diverge from conventional wisdom by suggesting that issue accountability is less pervasive than the literature on "issues publics" would suggest.

CONDITIONS FOR ISSUE ACCOUNTABILITY TO THE MASS PUBLIC

We argue that two key conditions should affect whether the public holds a legislator accountable on a given issue: its salience and the legislator's party affiliation. Issue salience is central to numerous theories of accountability and representation. Indeed, most spatial theories of elections simply assume that voters know and care about a given set of policy domains. Other theories focus on how variation in salience across policy areas affects the incentives of legislators and voters. For instance, Arnold (2004) argues that local press coverage on issues should increase the likelihood that voters will base electoral decisions on these issues. Kingdon (1977) argues that district opinion about a policy will have a greater influence on congressional members if the policy is visible and important to voters.

The second major factor, a legislator's party affiliation, may affect accountability for at least two sets of reasons. According to Egan's (2009) recently developed theory of issue ownership, a party can be less responsive if voters view it to be particularly competent on the issue. While Egan's (2009) argument naturally relates to the literature on issue ownership in

campaigns (e.g., Petrocik 1996), the theory gives more prominence to candidates' positions than much of this literature.[5] For instance, in Petrocik an underlying assumption is that voters do not have clear policy preferences while in Egan voters do have such preferences.

Other work predicts a party-specific effect for reasons related to parties' policy positions. Feldman and Conover (1983) show that a candidate's party affects voters' perceptions of her positions; when the party shifts to the left on an issue, voters perceive the candidate as further to the left, regardless of the candidate's actual position. Bartels (1986) suggests that this effect should be larger for challengers than incumbents; specifically, he finds voters are more uncertain about challengers' positions. In combination, these findings imply a "party positioning" perspective whereby a candidate's party affiliation should affect issue accountability. Take, for instance, environmental policy. If a Republican incumbent votes for greater environmental regulation, voters may see the two major party candidates as equally likely to pursue such policies. Yet, if a Democratic incumbent consistently supports environmental regulation, voters will tend to assume she is more likely than the Republican challenger to support such regulation.

Crime, Public Opinion, and Electoral Accountability

Previous work highlights that since the 1980s the public has overwhelmingly supported a punitive approach to criminal justice (e.g., Wilson 1997). We gathered surveys on the issues of the roll calls (which are subsequently described) and found support for this conventional wisdom. For over 90 percent of the votes, a majority favored the more punitive position.[6]

Some scholarship suggests that officials have strong electoral incentives to appear tough on crime even when public opinion itself is not particularly punitive (e.g., Huber and Gordon 2004). These incentives derive from citizens' greater ability to detect under- versus over-punishment. For instance, they are much more likely to hear about crimes committed by

individuals recently released from prison than about those forced to serve unduly long sentences. Consistent with these claims, research indicates that judges behave as if electoral pressures encourage punitive decisions. As an election nears, (elected) judges issue lengthier sentences (Huber and Gordon 2004). This research implies that congressional members, regardless of party, may face electoral incentives to appear "tough on crime." Notably, such a prediction differs from work that suggests congressional candidates benefit from ideological moderation (e.g., Erikson and Wright 1997; Canes-Wrone, Brady, and Cogan 2002); if candidates do benefit from moderation, then only Democrats should gain from supporting punitive measures. Our analysis, by analyzing the effects separately by party, allows for this possibility as well as the alternative that all members benefit from voting tough on crime.

Analyzing the parties separately also allows for the effects predicted by the party positioning perspective, given that Republicans have been associated with a more punitive approach (e.g., Western 1996). Moreover, perspectives that involve perceptions of party competence, such as Egan (2009), would also predict party-specific effects whenever voters have trusted one party more than the other to handle the issue. Some research suggests voters have generally trusted Republicans more than Democrats (e.g., Ansolabehere and Iyengar 1994, 336) while other work indicates that Clinton managed to neutralize the Republican's advantage (e.g., Sides 2006). Notably, even if Sides (2006) is correct, the party positioning perspective suggests greater issue accountability for Democrats.

The Environment, Public Opinion and Electoral Accountability

Compared with crime policy, opinion about environment policy has been more divided. Whereas huge majorities have consistently supported a punitive approach to criminal justice, often only sixty percent of the public has favored increasing environmental regulation while

forty percent has opposed doing so (e.g., Agnone 2007). Correspondingly, for some of the specific environmental issues that make up the League of Conservation Voters (LCV) legislative scorecards, a majority of the populace did not support greater regulation. For example, relative to the LCV, the public has been more favorable to expanding nuclear energy.[7] Environmental policy therefore provides a helpful counterpoint to crime, by offering an issue on which public opinion is not persistently lopsided in one direction.

Despite this difference, however, the theoretical predictions concerning partisan affiliation and issue salience remain similar to those for crime. Because the balance of opinion has favored greater environmental regulation, and the Democratic Party has been associated with this position (Shipan and Lowry 2001), the party positioning perspective would predict greater issue accountability for Republican members. Likewise, Egan's (2009) theory of issue ownership would predict greater issue accountability for Republicans because during the years of this study voters believed the Democratic Party would better handle the issue (Sides 2006). Of course, however, these coincident predictions about party-specific effects are conditional upon environmental policy being salient enough to produce issue accountability.

DATA

The largest data-related challenge involved constructing a measure of members' roll call records on crime. While for many issues, interest groups create ratings or scorecards of members, they do not exist for crime for an extended period. We decided to begin our measure in 1987 because the infamous Willie Horton ads of the 1988 presidential campaign ushered in a sense that candidates' records on crime could influence future elections. As Ansolabehere and Iyengar (1997[1995], 64) observe, "… 'Willie Horton' and its cousin 'The Revolving Door' are

widely credited with reviving George Bush's 1988 presidential bid…Whatever else can be said about these ads, they were certainly effective."[8]

To create the ratings, we used the Law and Judiciary section of the *Congressional Quarterly Almanac*, which reviews major legislation and roll call votes for each session of Congress. The ratings encompass crime-related votes on amendments, passage, and conference reports. The data focus on non-procedural votes because research suggests that electoral accountability is higher for these sorts of roll calls (Crespin, Finocchiaro, and Wilk 2006). We defined the issue of crime broadly, but sought to limit conflation with other issues. Specifically, the scores derive from votes pertaining to criminal justice, prison construction and management, sentencing, and policing. Because fear of crime does not translate to support for (or against) gun control (Roberts and Stalans 1997, 286-7), we excluded votes about gun control unless they specifically involved one of these crime-related categories. Likewise, votes on drug policy were included only if they directly related to criminal justice. For instance, a roll call regarding federal drug sentencing qualified while educational programs designed to discourage students from using drugs did not. We collected these votes through 2004.

The coding procedures produced a fair number of roll calls from the 100th through 106th Congresses (1987 through 1998). In total we have 61 votes across these years, so that the ratings for each of these Congresses are based on approximately ten votes. In the 107th through 109th Congresses, from 1999 through 2004, crime was less central to the legislative agenda and accordingly appears less frequently in the *Congressional Quarterly Almanac*; on average we acquired only four votes per Congress. In response to these and other sorts of differences in the legislative agenda, Groseclose, Levitt, and Snyder (1999) propose a technique for adjusting

voting scores so that the data can be pooled across congresses. We used this technique in analyses that involved pooling the scores across congresses.

We defined a position as tough if it promoted a more punitive criminal justice system and/or favored the rights of victims over those of the accused. Tough positions include reducing judges' discretion in sentencing, expanding the number of crimes subject to the death penalty, supporting the growth of prisons, and increasing the supervision of sex offenders. Soft positions, by contrast, include increasing defendants' rights, excluding evidence obtained under questionable circumstances, and eliminating the federal death penalty. To minimize the impact of subjectivity, authors coded the votes independently. Only three votes were the source of disagreement, and they involved legislation that contained multiple provisions; in these cases, we sought guidance from *Congressional Quarterly* and surrounding media reports as to whether the media and political leaders portrayed the legislation as soft or tough at the time.[9] The first table of the Web Appendix lists the *Congressional Quarterly* roll call number of each vote and whether it is coded as tough.

With these votes, a legislator's record was mapped to a *Tough on Crime* score using the familiar method of the Americans for Democratic Action (ADA). A vote for a tough provision or against a soft one is considered a tough position. All other votes (and absences) are recorded as soft. The total number of tough positions from a particular Congress is then divided by the number of crime votes from that Congress in order to yield an index that varies between zero and one, with zero representing a perfectly "soft" record and one a perfectly "tough" one. In order to compare members within each Congress on a similar set of votes, we excluded legislators who did not serve a full term.

For the environment, a readily available measure of members' votes was available. The League of Conservation Voter (LCV) scores have been used by previous studies to measure members' roll call behavior on environmental policy (e.g., Shipan and Lowry 2001), and we therefore adopt this approach. As with the *Tough on Crime* scores, we employ the Groseclose, Levitt, and Snyder (1999) technique for analyses that pool the scores across congresses. These scores and the *Tough on Crime* scores constitute the key independent variables.

The dependent variable and controls are defined as follows:

Vote share. As in many previous studies of electoral outcomes, the dependent variable is the ratio of votes for the incumbent to the total number of votes cast for either of the two major party candidates. We classify independents as members of the party in which they caucus. Uncontested members are excluded given that their two-party vote share is not comparable to that of contested members. However, we have examined models that include uncontested members and received substantively similar results.

De-crimed ADA and De-LCVed ADA. Arguably the most important control is the incumbent's general ideological voting record. For instance, without such a control, any effect of the *Tough on Crime* score could simply reflect that voting on crime is correlated with voting on other issues. The Americans for Democratic Action (ADA) voting scores are commonly employed to reflect a member's liberalness and we follow this lead (e.g., Canes-Wrone, Brady and Cogan 2002). However, because in many years the ADA score includes one or more roll calls that are part of the *Tough on Crime* or LCV scores, we created "de-crimed" and "de-LCVed" ADA scores for the relevant analyses. For a given election, we average the scores across the two sessions of a Congress to compute each member's *De-crimed ADA* or *De-LCVed ADA* score.

Presidential Vote Share. To control for the ideological leanings of each congressional district, we follow previous work (e.g., Jacobson 1996; Erikson and Wright 1997) and use the percentage of votes for the presidential candidate of the incumbent's party relative to the number of votes received by presidential candidates from the two major parties.

% African American. Griffin and Flavin (2007) find that African Americans are more likely than other voters to base electoral decisions on candidates' partisan affiliations, even controlling for the candidates' ideological dispositions. For this reason, we include a separate control for the percentage of African American citizens in the district; specifically, the variable equals the fraction of the population that the most recent decennial census reports to be black.[10] In addition, because recent work indicates that the effects of punitive policies disproportionately affect African Americans (e.g., Western 2006), we examined whether the impact of a member's votes on crime were lower if the district had a sufficient percentage of African Americans. No such effect was found. Full results are given in the Web Appendix.

Challenger Quality. Following Jacobson and Kernell (1983), we expect the quality of a challenger to affect an incumbent's vote share. As is common, we measure challenger quality with an indicator regarding whether the challenger has previously held elected office. Basinger and Ensley (2007) suggest this factor is endogenous, and we have conducted instrumental variables analysis under that assumption. This analysis, for which results and details are given in the Web Appendix, produces substantive findings that replicate those of the OLS tests.

ln(Challenger Spending) – ln(Incumbent Spending). As in previous studies, we control for the impact of campaign expenditures. Specifically, we include a difference-in-logs measure of the campaign spending totals reported by the challenger and incumbent candidates. Some work

separates challenger and incumbent spending into two separate variables, and research suggests that in such a specification spending should be considered endogenous (e.g., Gerber 1998). Research that utilizes one variable to reflect the difference between the candidates' spending does not suggest this difference should be considered endogenous (e.g., Erikson and Wright 1997; Rocca 2003). We adopt the latter approach given that studying issue accountability does not require estimating the individual effects of incumbent versus challenger spending.

Freshman. As is standard, we include an indicator for whether an incumbent is facing his first re-election challenge.

Rate of Violent Crime and *Change in Rate of Violent Crime.* In the tests regarding crime policy, we account for the possibility that elections may be affected by the incidence of crime, not simply members' legislative behavior on the issue. To measure the crime rate, we use the annual Federal Bureau of Investigation's *Uniform Crime Report*, from which we recorded the rate of violent crimes per 100,000 people, by state, for the year of the election.[11] Because voters might be concerned about *increases* in crime and not just the level, we additionally control for the change in the rate of violent crime between the most recent and next most recent years.

In addition to these controls, we collected data on issue salience. *Public Concern* is measured with open-ended surveys that ask about the nation's most important problem or top issues. In particular, we utilize Gallup's recurring Most Important Problem survey and a regular Harris survey that began in 1996 and asks for the two most important issues.[12] Prior work recommends measuring salience with open-ended questions (e.g., Aldrich, Sullivan, and Bordiga 1989; Geer 1991). Correspondingly, Zaller's (1992, 586) response axiom suggests that a survey item that mentions a particular issue may temporarily increase the importance of that issue to

respondents. Open-ended questions regarding public concerns, by not naming particular issues, help detect attitudes that are generally accessible and therefore more likely to influence voting behavior. For each survey item, we measure public concern with the average percentage of respondents who identified the issue, whether crime or the environment, as an important problem in all polls conducted throughout the preceding Congress. These data are presented alongside the key estimates of the OLS analysis.

METHODS

Ordinary least squares (OLS) regression is a standard method used to analyze electoral accountability (among other topics) and we begin by utilizing this traditional approach. In particular, for each party and election year, we regressed *Vote share* on the variable capturing issue accountability (*Tough on Crime* or LCV score), the ADA measure (*De-crimed ADA* or *De-LCVed ADA*), the measure of district ideology (*Presidential Vote Share*), plus all other controls. This specification, while orthodox, contains a number of strong assumptions. For instance, by including the ADA measure as a linear term, we are implicitly assuming that a member with a moderate ideological record is punished the same for a movement to their party's ideological extreme as is a member who already has a more extreme record. Such an assumption and functional form seem reasonable, yet hardly the only reasonable possibility. For instance, perhaps the electoral ramifications for having an extreme ideological record diminish after a certain level of extremity is reached. It turns out that if such an assumption is made and a natural log transformation of the ADA score is used, the results on issue accountability change. For instance, under this assumption issue accountability occurs for Democrats in 1990 (β=0.041; SE=0.018). Likewise, changes to the functional form of candidate spending and presidential vote share can affect the results; if we add a quadratic term for presidential vote share and the

difference in spending, then the effect of issue accountability for crime is no longer significant for Democrats in 1996 (β=0.034; SE=0.023).

To address this model dependence, we conduct a matching analysis. Because matching is nonparametric, the technique eliminates the potentially intractable problem of identifying the correct functional form. The idea behind matching is to simulate the conditions of a randomized experiment using data observed from environments in which conditions cannot be carefully controlled. In a well-designed experiment, the effect of a treatment is measured by comparing two groups, the treated and the control. Ideally, these two groups are composed of identical observational units that differ only by whether they are treated or not. Insofar as these conditions are achieved, the experimenter can have confidence that any differences between the groups are consequences of the treatment rather than differences in the covariates. Such experiments sidestep the methodological problems associated with parametric techniques like OLS; the assumptions about functional form are limited to the supposition that an effect is additive.

Matching analysis approximates this set-up using observational data. Conceptually, the simplest matching technique is exact matching, in which observational units are matched to each other only if the values of their associated covariates are identical and the values of the treatment variable differ. If exact matching were feasible, the resulting groups of treated and control observations would be perfectly *balanced*, i.e., the differences between the two groups beyond the treatment itself would be trivial. Of course, examples of such pairs in observational data are exceptionally rare. When there are multiple covariates and when these variables are continuous- rather than discrete-valued, as in the present context, exact matching is not feasible.

There are many alternatives to exact matching. We utilize tools from the family of nearest-neighbor matching methods, which order observations according to a function of their

covariates and then create groups of treated and control observations with similar covariate distributions. The particular type we use is genetic matching, which Sekhon (forthcoming) developed and which tests many possible nearest-neighbor matching methods before selecting the one that produces the best available balance according to certain criteria. However, there is no guarantee that the resulting groups are actually well-balanced, as they are by definition in exact matching. Therefore, we present two measures of balance. First, we conduct a paired *t*-test to compare the sample means of each covariate for the treated and control groups and a bootstrapped Kolmogorov-Smirnov (KS) test of equality between the distributions of each covariate for the treated and control groups. We then record the "fitness value," or minimum *p*-value, from all of these tests over all covariates (e.g., Diamond and Sekhon 2008). Higher fitness values are associated with better balance. Second, we present a metric based on propensity scores, which measure the probability that a particular observation is selected into the treatment group conditional upon observed covariates. In particular, the metric compares the propensity scores of the treated and control groups using a standardized difference in means (Ho et al. 2007). For this metric, a low value is associated with better balance.[13]

All forms of matching demand that the data satisfy two standard assumptions. The first is "stable unit treatment value," which requires that, conditional on the values of the covariates, the distribution of outcomes for one observation is independent of the treatment-status of any other individual observation. In the present context, this means that the electoral performance of one incumbent does not depend on the voting record of another given incumbent (Ho et al. 2007). Though this assumption is inherently debatable, we believe that the control variables detailed above adequately capture the systematic determinants of an incumbent's voting record. The second assumption, variously labeled "selection on observables" or "strong ignorability of

treatment assignment," requires that assignment to the treated group depends only on covariates that we include and observe, and that, given those covariates, such assignment is a possibility for every observation and a certainty for none. In our context, this means that we have not excluded important predictors of an incumbent's voting record on the relevant issue, and that any values of covariates represented in the treated group must also be represented in the control group. Sensitivity to the second assumption is testable, and we discuss results of two types of tests.

RESULTS FOR CRIME POLICY

Table 1 reports the OLS estimates regarding *Tough on Crime*.

[Table 1 about here]

For space reasons, the table does not report the estimates associated with the control variables, which generally perform as expected. These results are available in Appendix Table A.

The most striking component of the first column of results, which pertain to the Democratic members, is that they vary considerably by election year. In particular, the coefficients for 1994, 1996, and 1998 all have the expected sign and are significant at conventional levels ($p<0.05$, two-tailed). By contrast, the coefficients for the other years do not approach conventional levels of significance and in two cases do not even have the predicted sign.[14] Comparing these results to the survey data, reported in Columns 7 and 8, we find that they are related. For both the Gallup and Harris surveys, the level of salience is higher for the 1994-1998 elections than for other years. In fact, the difference between the survey responses for these elections to those for the election with the next-highest level of public concern, 2000, is significant at conventional levels ($p=0.05$, two-tailed).

Notably, not just the significance of the coefficients but also the magnitudes are greatest in the election years with the highest levels of public concern. The magnitudes for 1994, 1996,

and 1998 are higher than in any other election. Nor are these magnitudes negligible. For instance, consider a Democrat who switched from a soft to tough position on one-third of the roll calls that constitute the *Tough on Crime* score. In the 103rd Congress (1993-4) this sort of movement would have caused a Democrat to receive a greater vote share of 2.3 percentage points, an effect comparable to that of challenger quality.

In sum, the effect of Democrats' voting records on crime is reasonably substantial for the years in which crime was a major public concern. By contrast, the estimates for the Republicans, reported in Column 4, are almost always far below conventional levels of statistical significance. Moreover, the only coefficient that is marginally significant ($p = 0.13$, two-tailed), for 1988, is negative. According to Table 1, individual Republicans were not punished in the general election for voting soft on crime.

Overall, the results suggest that the mass public holds legislators accountable for their roll call records on specific issues and that this effect depends upon several conditions. In years in which public concern about crime was high, a Democrat's failure to support punitive policies cost him or her electoral support even beyond the impact of overall ideological orientation. By contrast, when public concern about crime was considerably lower, then Democrats did not harm themselves in the general election by voting against punitive measures. Also according to the tests, Republican incumbents were not harmed by voting against punitive measures even in years of extraordinary public concern about the issue.

This partisan difference may be related to the fact that Republicans almost always supported punitive measures. As Table 1 shows in Columns 2 and 5, which report the average *Tough on Crime* score for each party by year, the lowest mean score for Republicans is 0.79 while the mean score for Democrats is routinely below 0.5. The standard deviations further

suggest that the Republicans displayed a unity on this issue unmatched by the Democrats. In every Congress the standard deviation for the Republicans is below 0.2. For Democrats, by comparison, it is above this level in each case except for the last two Congresses.

These findings are consistent with the previously described "party positioning" school of thought in which voters' perceptions of a candidate's positions are based in part on the candidate's partisan affiliation. Separately, one could argue that the partisan difference in accountability supports Egan's (2009) competence-based theory of issue ownership, although Sides's (2006) claim that neither party owned the issue between 1992 and 1998 points to the positioning perspective. Distinguishing between these reasons for a partisan effect was not the purpose of this project, and we do not try to do so here. What we can say is that the OLS results are supportive of a partisan effect; monotonic changes in Republicans' *Tough on Crime* scores do not affect these incumbents' electoral performance, while, by contrast, such changes do affect Democrats' vote shares when crime is a highly salient issue.

Table 1 accordingly suggests that a standard functional form lends credence to our initial hypotheses. However, as highlighted earlier, these results do not always hold with other reasonable functional forms. We therefore further examine the data with matching analysis. To apply this powerful technique, we had to adjust the original approach in two ways. First, for any given election year, the sample of legislators is too small to admit successfully balanced treated and control groups. We therefore pooled across years based on the level of public concern about crime. Specifically, we combined the elections associated with the highest levels of public concern-- 1994, 1996, and 1998--and separately combined the other years.

The second adjustment concerned the choice of a treatment that would dichotomously distinguish soft from tough voting. Our first instinct was to use a treatment with respect to a 50

percent tough-on-crime benchmark, under the presumption that anyone who supported the punitive position less than half the time could easily be characterized as soft on crime. Unfortunately, too few Republicans achieved low *Tough on Crime* scores to enable a meaningful matching analysis; less than five percent had scores under 0.50. While for comprehensiveness we still attempted to run a matching analysis for Republicans with this treatment, the lack of underlying variation prohibited achieving a minimum level of balance by any conventional standard. Moreover, while balance could be readily achieved for Democrats in years in which crime was salient, for the non-salient years the balance was not strong according to at least one standard test. We therefore present results with a treatment defined with respect to the 75 percent benchmark, with a *Tough on Crime* score below 0.75 warranting inclusion in the treatment group for Republicans and a score above 0.75 warranting inclusion in the treatment group for Democrats. These results with the 50 percent treatment are substantively identical, and the Web Appendix details these findings for analyses that at least approached conventional levels of balance (i.e., the Democrats in both salient and non-salient years).

Table 2 presents the main findings.[15]

[Table 2 about here]

The first column of results reports estimates of the average treatment effect for the treated (ATT), which is the mean change in *Vote share* that tough voting Democratic (soft voting Republican) incumbents received relative to other Democratic (Republican) legislators. Each estimate of the ATT is paired with the associated Abadie-Imbens standard error (Abadie and Imbens 2006). Columns 2 and 3 present the previously discussed metrics for balance. The second column reports the fitness value, for which a higher value is associated with better balance; fitness values of at least 0.15 satisfy the levels suggested by Diamond and Sekhon (2008). For

example, the fitness value for Democrats in years of high public concern is 0.687. Before genetic matching was conducted, the analogues of each fitness values we present is less than 0.00001, indicating that in each case the genetic matching algorithm achieves a much closer approximation to the conditions of a randomized experiment than existed before.

This claim is further supported by Column 3, which reports a measure of *imbalance*. In particular, Column 3 describes the standardized difference in mean propensity scores, which is found by calculating the average propensity score for treated observations minus the average for control observations, and dividing this difference by the standard deviation for treated observations. Ho et al. (2007, 221) point out that a common rule of thumb is that this standardized difference should not exceed 0.25, or one quarter of a standard deviation. In our analysis, all of these scores are below 0.05. Additional evidence of balance can be found in Appendix Table B1, which presents balance statistics for each covariate.

Table 2 not only suggests that the matching achieved high levels of balance, but also further supports the claim that Democratic members were held accountable to tough-on-crime majority opinion. In the high salience years, the comparatively tough-voting members of the treated group saw an average increase in vote share of six percentage points. This effect is strongly significant ($p<0.01$, two-tailed). By contrast, the effect for the years of lower public concern is not significant and even negative. The results for Republicans are also not significant at conventional levels, for either years of high public concern or for years of low salience.

We also conducted two analyses to investigate the role of selection on observables assumption. The assumption is violated if, for any two members of Congress who are identical on the matched covariates, one is more likely than the other to vote tough on crime for unobserved reasons. First we conducted a placebo test. For each party and subset of years we

used matching to estimate the causal effect of *Tough on Crime* on *Lagged Vote Share*. Current legislative behavior should have no effect on past election results and therefore a positive effect would indicate the results regarding future vote share could simply be an artifact of the selection on observables assumption. Results of the placebo test are reported in Column 5 of Table 2, and suggest that our major findings are not such an artifact. In all of the analyses, legislative voting on crime has no effect on previous vote share. Rosenbaum (2002) suggests an alternative method for analyzing the selection on observables assumption, and we conducted this test using Keele's (2008) rbounds package in R. This analysis further indicated that the results are not driven by the selection on observables assumption. The Web Appendix presents further details of these analyses regarding selection on observables.

RESULTS FOR ENVIRONMENTAL POLICY

Table 3 describes issue salience and key OLS results for the environment.

[Table 3 about here]

A striking difference with Table 1 involves the level of public concern. While between 1993 and 1998 at least twenty percent of respondents mentioned crime to be one of the two most important issues the government should address, no more than three percent of respondents ever claim the environment as such an issue. Likewise, while for these years at least fifteen percent of respondents cited crime to be the most important problem, the environment never comes close to reaching that level of public salience. Given that we found no issue accountability on crime when public concern was at the levels reported for the environment in Table 3, we should not expect to find issue accountability on the environment for the 1988 through 2004 elections.

As such, the OLS results present only a few surprises. Members of the party typically associated with the more popular positions, in this case the Democrats, face no significant issue

accountability. As Column 1 shows, in no year is the coefficient on the LCV score significant at conventional levels. Moreover, there is reasonable variation in the Democratic members' voting on the issue. Whereas the standard deviation in Republican members' voting on crime was less than 0.15 for six of the elections, the analogous standard deviation for Democrats' LCV scores is always above 0.15 and often above 0.2, as detailed in Column 2. Accordingly, the lack of an effect for Democrats cannot be readily explained by a lack of variation in their voting behavior.

The effects for Republicans are also largely insignificant, with the exceptions of 1996 and 2002. In these years Republicans receive more electoral support the higher their LCV scores. The lack of variation and generally low level of the public concern data preclude making claims about how these effects relate to issue salience. Moreover, it remains to be seen whether the findings will hold with less stringent assumptions about functional form.

Table 4 presents the results from the matching analysis.

[Table 4 about here]

In this case, balance could be achieved with a threshold of 50% for both parties given the variation in members' voting behavior, and we therefore used this intuitive benchmark. However, given the data on public concern, it was not immediately obvious how to group the years into ones where the environment was salient versus years in which it was not. Accordingly, we grouped the years by whether the OLS results suggested an effect of issue accountability for at least one of the parties.

Despite stacking the deck in a way that should bias in favor of finding an effect for Republicans in 1992 and 1996, Table 4 suggests that there is no significant effect for either party in any of the years. In other words, issue accountability does not exist for environmental policy between 1987 and 2004. We have investigated whether the specific treatment alters this finding,

and this analysis continues to suggest a lack of issue accountability. For instance, the effect for Republicans remains insignificant if we use a treatment based on whether the member voted with the LCV at least twenty-five percent of the time ($p \approx 0.21$). We also conducted placebo tests and the Rosenbaum (2002) sensitivity analysis as we did with the analysis of crime. Column 5 reports the results of the placebo test; in no case did it return a significant result. The Web Appendix presents full findings for these sensitivity analyses.

Overall, the results regarding the environment comport with what we found for crime. When few people cited crime as one of the most important problems, no issue accountability existed. Few cite the environment as one of the most important problems throughout 1987 to 2004, and therefore we would not expect issue accountability for environmental legislation during this period. Together with the results regarding crime, Table 4 suggests that the level of salience necessary for issue-specific accountability is high.

DISCUSSION AND CONCLUSION

This paper has demonstrated that issue accountability exists, but depends on the level of public concern and a member's party affiliation. When only a small percentage of the public deems an issue to be one of the most important problems, we find no evidence of issue-specific accountability. However, with greater levels of public concern, members from the party that is less popular on the issue suffer from voting out of step with public opinion, even controlling for their voting records on other policies. For the issue that attained such salience in our data, crime, we found that Democrats who voted for less punitive policies were punished by voters when public concern about crime was high. This result held not only with ordinary least squares, but also with matching analysis. The use of matching analysis was particularly important because we found that various parametric assumptions produced different results in ordinary least squares.

The other issue that we examined, the environment, had substantially lower levels of public concern. Accordingly, we expected the matching analysis to fail to find any accountability on this issue, and the results confirmed these expectations. Future work may wish to examine other policies, particularly ones with lower salience than crime had from 1994 through 1998 yet higher salience than the environment had between 1987 and 2004. For instance, taxes or unemployment might fall into this category.

More broadly, what do the findings imply about legislators' incentives? We have focused on general elections, but of course candidates also face primaries (e.g., Brady, Hahn, and Pope 2007). One interpretation of the results is that a member may cater to his primary electorate without significant ramifications in the general election so long as his overall record is not too extreme. However, concluding that issue accountability in general elections is negligible would presume that legislators have a good deal of control over the salience of policies, and research indicates quite the opposite: outside events and the media have much greater influence. Therefore, while we readily acknowledge that issue accountability is not universal, we would urge against concluding that members can generally get away with bucking public opinion so long as their overall ideological record is not too extreme. The results of this paper suggest that conditional on a sufficient level of public concern and a legislator's party affiliation, the electorate will hold him or her accountable for roll calls on specific issues.

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Table 1. OLS: Impact of Voting Tough on Crime on Incumbent Vote Share, controlling for a member's ideological record and other factors

	Democrats			Republicans			*Public Concern*	
	Estimate (SE)	**μ = Mean σ=SD**	***n***	**Estimate (SE)**	**μ = Mean σ=SD**	***n***	**MIP (Gallup)**	**Two Issues (Harris)**
Year	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**	**[8]**
1988	-0.005 (0.027)	*μ =0.55 σ=0.25*	187	-0.064 (0.042)	*μ =0.88 σ=0.11*	143	3%	---
1990	0.017 (0.025)	*μ =0.41 σ=0.34*	195	0.051 (0.042)	*μ =0.92 σ=0.19*	122	3%	---
1992	0.038 (0.027)	*μ =0.31 σ=0.27*	192	0.021 (0.032)	*μ =0.87 σ=0.18*	123	4%	---
1994	**0.069 (0.035)**	*μ =0.38 σ=0.19*	206	-0.065 (0.045)	*μ =0.79 σ=0.13*	118	32%	22%
1996	**0.049 (0.024)**	*μ =0.31 σ=0.27*	158	0.044 (0.051)	*μ =0.97 σ=0.06*	199	15%	30%
1998	**0.066 (0.018)**	*μ =0.34 σ=0.24*	148	-0.016 (0.032)	*μ =0.90 σ=0.14*	151	16%	22%
2000	0.009 (0.019)	*μ =0.43 σ=0.29*	171	0.012 (0.015)	*μ =0.82 σ=0.20*	165	12%	16%
2002	-0.018 (0.025)	*μ =0.90 σ=0.19*	155	0.045 (0.042)	*μ =0.97 σ=0.09*	153	4%	7%
2004	0.038 (0.027)	*μ =0.69 σ=0.18*	160	0.0003 (0.025)	*μ =0.82 σ=0.13*	173	2%	4%

Notes*:* Dependent variable equals the incumbent's two-party vote share. All regressions control for *De-crimed ADA*, *Presidential Vote Share*, *% African American*, ln(*Challenger Spending*) – ln(*Incumbent Spending*), *Challenger Quality*, *Freshman*, *Rate of Violent Crime*, and *Change in Rate of Violent Crime*. Appendix Table A describes results on the control variables. Results in bold are significant at $p \leq 0.05$, two-tailed. MIP signifies the Most Important Problem survey, and Two Issues the Harris survey that asks respondents to identify the two most important issues the government should address.

Table 2. Matching Analysis: Impact of Voting Tough on Crime on Incumbent Vote Share

	Estimate (Std. Err.)	Fitness Value	Std. Diff. in Propensity Score Means	Total *n* Treated *n*	Placebo Estimate (Std. Err.)
Democrats					
	[1]	**[2]**	**[3]**	**[4]**	**[5]**
1994-1998	**0.063 (0.018)**	0.687	-0.0004	508 29	-0.026 (0.017)
1988-1992 & 2000-2004	-0.007 (0.005)	0.291	-0.001	991 264	-0.006 (0.010)
Republicans					
1994-1998	0.014 (0.010)	0.200	0.044	476 67	-0.013 (0.014)
1988-1992 & 2000-2004	-0.006 (0.006)	0.211	0.002	889 170	-0.012 (0.013)

Notes*:* All analyses include as co-variates *De-crimed ADA*, which is adjusted to be used across congresses by the Groseclose, Levitt, and Snyder (1999) method; *Presidential Vote Share*; *% African American*; ln(*Challenger Spending*) – ln(*Incumbent Spending*); *Challenger Quality*; *Freshman*; *Rate of Violent Crime*; and *Change in Rate of Violent Crime*. Results in bold are significant at $p \leq 0.05$, two-tailed. To balance Democrats in years 1988-1992 & 2002-2004 it was also necessary to include an indicator for whether a representative was from a southern state.

Table 3. OLS: Impact of LCV Score on Incumbent Vote Share, controlling for a member's ideological record and other factors

	Democrats		Republicans		*Public Concern*	
	Estimate (SE)	**μ = Mean σ=SD**	**Estimate (SE)**	**μ = Mean σ=SD**	**MIP**	**Two Issues**
Year	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
1988	0.025 (0.034)	*μ =0.69 σ=0.20*	-0.028 (0.035)	*μ =0.37 σ=0.21*	1%	---
1990	0.017 (0.037)	*μ =0.73 σ=0.17*	0.020 (0.032)	*μ =0.38 σ=0.27*	4%	---
1992	0.013 (0.034)	*μ =0.61 σ=0.23*	-0.016 (0.036)	*μ =0.20 σ=0.24*	2%	---
1994	0.038 (0.036)	*μ =0.71 σ=0.20*	-0.077 (0.040)	*μ =0.26 σ=0.21*	1%	2%
1996	-0.002 (0.023)	*μ =0.77 σ=0.24*	**0.090 (0.021)**	*μ =0.19 σ=0.22*	1%	2%
1998	0.026 (0.027)	*μ =0.75 σ=0.23*	-0.027 (0.031)	*μ =0.26 σ=0.24*	2%	2%
2000	-0.047 (0.035)	*μ =0.79 σ=0.20*	0.038 (0.028)	*μ =0.19 σ=0.21*	2%	2%
2002	-0.063 (0.037)	*μ =0.80 σ=0.22*	**0.081 (0.029)**	*μ =0.18 σ=0.20*	2%	3%
2004	-0.026 (0.039)	*μ =0.86 σ=0.18*	0.006 (0.028)	*μ =0.14 σ=0.19*	1%	2%

Notes*:* Dependent variable equals the incumbent's two-party vote share. All regressions control for *De-LCVed ADA*, *Presidential Vote Share*, *% African American*, ln(*Challenger Spending*) – ln(*Incumbent Spending*), *Challenger Quality*, and *Freshman*. Results on the control variables available in Web Appendix. Results in bold are significant at p≤0.05, two-tailed. MIP signifies Gallup's Most Important Problem survey, and Two Issues the Harris survey that asks respondents to identify the two most important issues the government should address.

Table 4. Matching Analysis: Impact of LCV Score on Incumbent Vote Share

	Estimate (Std. Err.)	**Fitness Value**	**Std. Diff. in Propensity Score Means**	**Total *n* Treated *n***	**Placebo Estimate (Std. Err.)**
	[1]	**[2]**	**[3]**	**[4]**	**[5]**
Democrats					
1996 & 2002	0.008 (0.012)	0.188	0.110	312 45	-0.018 (0.017)
1988-1994, 1998, 2000, & 2004	0.009 (0.010)	0.154	0.009	1256 195	-0.022 (0.014)
Republicans					
1996 & 2002	0.013 (0.023)	0.197	0.075	351 41	0.014 (0.028)
1988-1994, 1998, 2000, & 2004	-0.018 (0.010)	0.157	0.022	993 180	0.018 (0.020)

Notes*:* Dependent variable equals the incumbent's two-party vote share. All regressions control for *De-LCVed ADA*, *Presidential Vote Share*, *% African American*, ln(*Challenger Spending*) – ln(*Incumbent Spending*), *Challenger Quality*, and *Freshman*. Results in bold are significant at $p \leq 0.05$, two-tailed.

Appendix Table A. OLS Control Variable Results for Impact of Voting Tough on Crime on Incumbent Vote Share

	1988	1990	1992	1994	1996	1998	2000	2002	2004
Democrats									
De-crimed ADA	-0.099	-0.043	-0.050	-0.058	-0.126	-0.118	-0.095	-0.053	-0.066
	(0.038)	(0.041)	(0.042)	(0.035)	(0.037)	(0.031)	(0.036)	(0.031)	(0.041)
Presidential Vote Share	0.440	0.343	0.516	0.626	0.679	0.597	0.529	0.442	0.571
	(0.063)	(0.063)	(0.069)	(0.061)	(0.054)	(0.053)	(0.043)	(0.047)	(0.051)
% African American	0.024	0.127	0.085	0.032	0.026	0.020	0.001	0.049	0.021
	(0.024)	(0.036)	(0.035)	(0.026)	(0.023)	(0.023)	(0.023)	(0.029)	(0.029)
ln(Challenger Spending) – ln(Incumbent Spending)	-0.030	-0.019	-0.031	-0.030	-0.026	-0.029	-0.028	-0.024	-0.021
	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Challenger Quality	0.016	-0.030	-0.016	-0.027	-0.027	-0.026	-0.011	-0.015	0.006
	(0.014)	(0.014)	(0.012)	(0.011)	(0.010)	(0.010)	(0.010)	(0.013)	(0.012)
Freshman	-0.021	0.004	0.036	-0.016	-0.010	-0.011	-0.025	0.012	0.000
	(0.016)	(0.019)	(0.014)	(0.009)	(0.013)	(0.009)	(0.011)	(0.016)	(0.014)
Rate of Violent Crime	-0.026	0.014	-0.056	-0.057	-0.062	-0.038	-0.040	-0.038	-0.004
	(0.020)	(0.019)	(0.017)	(0.018)	(0.019)	(0.023)	(0.023)	(0.025)	(0.027)
Change in Rate of Violent Crime	0.219	-0.052	0.212	-0.258	-0.136	-0.279	0.004	0.038	0.088
	(0.159)	(0.166)	(0.142)	(0.113)	(0.091)	(0.121)	(0.113)	(0.071)	(0.167)
Constant	0.490	0.438	0.333	0.236	0.310	0.332	0.403	0.414	0.321
	(0.046)	(0.043)	(0.039)	(0.041)	(0.041)	(0.035)	(0.036)	(0.041)	(0.048)
N	187	195	192	206	158	148	171	154	158
R^2	0.615	0.535	0.628	0.756	0.856	0.852	0.826	0.725	0.766
Republicans									
De-crimed ADA	0.126	0.156	0.189	-0.025	0.111	0.124	0.097	0.036	0.137
	(0.031)	(0.046)	(0.037)	(0.046)	(0.034)	(0.037)	(0.025)	(0.039)	(0.036)
Presidential Vote Share	-0.481	-0.148	-0.459	-0.258	-0.493	-0.434	-0.389	-0.257	-0.510
	(0.081)	(0.103)	(0.071)	(0.074)	(0.047)	(0.068)	(0.049)	(0.057)	(0.054)
% African American	-0.054	-0.041	0.050	-0.012	-0.024	-0.092	-0.027	-0.023	-0.030
	(0.067)	(0.081)	(0.102)	(0.075)	(0.040)	(0.055)	(0.037)	(0.056)	(0.044)
ln(Challenger Spending) – ln(Incumbent Spending)	-0.036	-0.031	-0.022	-0.027	-0.030	-0.021	-0.024	-0.025	-0.014
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Challenger Quality	-0.003	-0.023	-0.034	-0.013	-0.020	-0.023	-0.008	-0.005	-0.009
	(0.012)	(0.016)	(0.012)	(0.012)	(0.007)	(0.010)	(0.008)	(0.011)	(0.009)
Freshman	-0.015	-0.007	0.011	-0.008	-0.039	-0.006	-0.006	-0.014	-0.014
	(0.013)	(0.019)	(0.014)	(0.010)	(0.007)	(0.012)	(0.010)	(0.012)	(0.009)
Rate of Violent Crime	-0.026	-0.020	-0.030	0.001	-0.002	-0.000	0.005	0.011	0.032
	(0.019)	(0.022)	(0.015)	(0.020)	(0.014)	(0.024)	(0.019)	(0.020)	(0.021)
Change in Rate of Violent Crime	-0.066	0.197	-0.011	-0.074	0.056	0.163	0.169	-0.085	0.144
	(0.160)	(0.218)	(0.136)	(0.123)	(0.070)	(0.117)	(0.096)	(0.054)	(0.138)
Constant	0.975	0.696	0.956	0.784	0.887	0.944	0.742	0.659	0.934
	(0.064)	(0.069)	(0.058)	(0.063)	(0.060)	(0.060)	(0.040)	(0.067)	(0.050)
N	143	122	123	118	199	151	165	153	173
R^2	0.638	0.455	0.605	0.560	0.795	0.569	0.729	0.611	0.567

Notes: Each entry reports the coefficient and the estimate of its standard error. Note that *De-Crimed ADA* should have an opposite effect according to party because a higher score reflects a more liberal member.

Appendix Table B1. Standardized Differences in Means for Matching Analysis of Impact of Voting Tough on Crime on Incumbent Vote Share

Democrats	*1994-1998*			*1988-1992 & 2000-2004*		
Covariates	Before	After	Improvement	Before	After	Improvement
Turbo De-crimed ADA	-1.211	0.021	98.3%	-0.481	-0.058	87.9%
Presidential Vote Share	-1.319	0.029	97.8%	-0.320	-0.001	99.7%
% African American	-0.548	0.101	81.6%	-0.102	0.007	93.1%
ln(Challenger Spending) – ln(Incumbent Spending)	0.288	-0.081	71.9%	-0.135	-0.048	64.4%
Challenger Quality	0.212	0	100.0%	-0.002	0.032	-1500.0%
Freshman	-0.526	0	100.0%	0.074	0.065	12.2%
Rate of Violent Crime	-0.262	-0.040	84.7%	-0.637	0.063	90.1%
Change in Rate of Violent Crime	0.113	0.026	77.0%	-0.480	0.027	94.4%
South	n/a	n/a	n/a	0.297	0	100.0%
Propensity Score	0.878	-0.000	99.9%	0.920	-0.001	99.9%

Republicans	*1994-1998*			*1988-1992 & 2000-2004*		
Covariates	Before	After	Improvement	Before	After	Improvement
Turbo De-crimed ADA	0.811	0.020	97.5%	0.445	0.052	88.3%
Presidential Vote Share	-0.277	-0.048	82.7%	-0.465	-0.051	89.0%
% African American	-0.033	0.099	-200.0%	-0.013	0.119	-815.4%
ln(Challenger Spending) – ln(Incumbent Spending)	-0.113	0.166	-46.9%	0.195	-0.066	66.2%
Challenger Quality	-0.045	0.151	-235.6%	-0.036	0.035	2.8%
Freshman	-0.005	0.166	-3220.0%	-0.049	0.065	-32.7%
Rate of Violent Crime	0.199	0.062	68.8%	0.028	0.076	-171.4%
Change in Rate of Violent Crime	0.280	0.055	80.4%	0.029	0.067	-131.0%
Propensity Score	0.989	0.044	95.6%	0.508	-0.002	99.6%

Appendix Table B2. Standardized Differences in Means for Matching Analysis of Impact of LCV Score on Incumbent Vote Share

Democrats	*1996 & 2002*			*1988-1994, 1998 & 2004*		
Covariates	Before	After	Improvement	Before	After	Improvement
Turbo De-LCVed ADA	-1.68	-0.08	95.3%	-1.653	-0.009	99.5%
Presidential Vote Share	-1.969	-0.167	91.5%	-1.288	0.059	95.4%
% African American	-0.437	0.002	99.6%	-0.169	-0.012	93.1%
ln(Challenger Spending) – ln(Incumbent Spending)	0.640	0.146	77.2%	0.141	0.045	67.9%
Challenger Quality	0.137	0.218	-59.3%	0.084	0.116	-39.4%
Freshman	-0.077	0.215	-178.1%	-0.174	0.052	70.2%
Propensity Score	1.931	0.110	94.3%	1.647	0.009	99.4%

Republicans	*1996 & 2002*			*1988-1994, 1998 & 2004*		
Covariates	Before	After	Improvement	Before	After	Improvement
Turbo De-LCVed ADA	-1.737	-0.067	96.1%	-1.497	-0.019	98.7%
Presidential Vote Share	-1.528	-0.104	93.2%	-0.652	0.089	86.3%
% African American	0.190	0.153	19.4%	0.069	0.043	38.0%
ln(Challenger Spending) – ln(Incumbent Spending)	0.119	0.055	54.3%	0.055	-0.001	97.5%
Challenger Quality	0.070	0.058	16.4%	0.021	0.125	-488.9%
Freshman	-0.204	0.192	5.8%	-0.178	0.040	77.3%
Propensity Score	1.833	0.075	95.9%	1.509	0.022	98.5%

ENDNOTES

[1] See, for instance, responses to the CBS news poll asking "What do you think is the most important problem facing this country today?" The percentage of respondents rating crime or drugs as the most important problem was 21% overall, 24% for Democrats, 24% for Republicans, 20% for Whites and 33% for Blacks. See "CBS: Only 21% Think Clinton Less Honest Than Other Polls." *Hotline*, June 16, 1997. The poll was conducted June 10-11, 1997. All surveys in the paper are available via the Roper Center for Public Opinion online database.

[2] It is worth highlighting that we do not presume voters are well-informed about crime policy or that well-informed voters hold normatively appealing views about the issue.

[3] Egan (2009), which is discussed subsequently and is not yet published, is an exception in that it does focus on voting in congressional elections.

[4] Related research suggests that the ideological orientation of congressional candidates' positions, as measured by the candidates' responses to surveys, affect electoral performance. See, for instance, Erikson and Wright (1997) and Burden (2004).

[5] Although cf. Popkin (1991, 61-3) for a discussion of how voters in presidential elections care about both issue proximity and competence.

[6] Among other things, a vast majority of respondents consistently supported expansion of the death penalty, limiting parole and appeals, trying juveniles as adults, increasing the length of sentences, building more prisons, and increasing the penalties imposed on drug users and dealers.

[7] The LCV has consistently opposed nuclear energy, and opinion data suggest that in recent years a majority of the public supported building more nuclear plants. See, for instance, the June 2001 CBS-New York Times poll that asks "Would you approve or disapprove of building more

nuclear power plants to generate electricity?" Fifty-one percent of respondents approved of building more nuclear plants.

[8] Scholarship has examined the racial subtext of the Willie Horton advertisements (e.g., Mendelberg 2001) and a good deal of research suggests public opinion about crime generally relates to beliefs about the race of criminals as well as racial attitudes (e.g., Gilliam and Iyengar 2000). We take these findings as given; our focus is not on the sources of punitive public opinion but rather the political consequences.

[9] These disagreements involved roll calls 144 and 416 in 1994 and roll call 497 in 2004. The first two concern different versions of the Omnibus Crime Bill of 1994, which contained a number of punitive provisions but also Clinton's "midnight basketball" program. Because media coverage emphasized the midnight basketball program we coded these votes as soft. The 2004 vote involved passage of a bill that would create new victims' rights. The victims' rights portion of the bill played prominently in press coverage, and we therefore coded it as tough.

[10] Due to reapportionment in the mid-1990s, there are several districts in Texas and Georgia for which we use the results of the 2000 census rather than those of the 1990 census.

[11] Crime rates by congressional district are not available; according to the GIS librarian that we consulted, privacy-related protections prevent disaggregating county-level crime data. Moreover, research suggests perceptions of the incidence of crime are affected by television news (e.g., Chiricos, Eschholz, and Gertz 1997), and media markets traverse congressional district boundaries.

[12] Gallup asks "What do you think is the most important problem facing this country today?" and Harris asks "What do you think are the two most important issues for the government to address?"

[13] Additional balance metrics, including the empirical standardized differences in means for each of the covariates, are contained in Appendix Tables B1 and B2.

[14] We have tested for heteroskedasticity using the Cook-Weisberg test and found it does not exist for most years (the exceptions being 2002 for both parties, 2004 for both parties and 1992 for Republicans). Using White's robust standard errors in these years does not at all change the substantive findings.

[15] Two technical matters bear noting. First, in some cases, it was necessary to apply a caliper and drop outlying observations in order to achieve balance. In Tables 2 and 4, Column 4 reports the number of dropped observations. Second, to achieve balance for Democrats in years in which crime was low salience, it was necessary to balance on an indicator for whether the member represented a Southern state.