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Concordance and Projection in Citizen Perceptions of Congressional Roll-Call Voting

Research on political cognition suggests that individuals absorb and retain more information consistent with their political predispositions than they do information at odds with those predispositions. When citizens view a member of Congress favorably, they should thus be more likely to recall that member's vote on a bill if it is in agreement with their own positions; additionally, if they do not recall, they will tend to assume that the member voted in accordance with their own preferences. When citizens view a representative negatively, the opposite patterns should obtain. Here, we find considerable evidence for both of these effects—concordance and projection. Attitude toward the representative and agreement on the issue substantially drive citizen perceptions of congressional roll-call voting.

Introduction

One of the central questions in democratic theory concerns the nature of representation. Debates over the proper relationship between constituent preferences and legislative behavior and over the relative merits of the trustee and delegate models have raged for more than two hundred years. Virtually all conceptions of representation, however, assume that citizens are at least somewhat conscious of their legislators' actions in the governing process. Constituents must be aware of how their representatives vote on at least a few, high-salience issues; otherwise, they cannot act collectively as "a rational god of vengeance and reward" (Key 1966), and congressional elections must be seen as largely devoid of policy messages. While potential channels of communication between constituents and representatives are plentiful in the modern age, previous research (e.g., Stone 1979) suggests that citizen knowledge of legislative activity cannot safely be assumed. Though ignorance is not universal, it is widespread, at least on an issue-by-issue basis. Thus, a careful examination of the relationship between citizen preferences, representative votes, and citizen perceptions of those votes is clearly warranted.

In addressing this complex interaction, two scholars have recently focused on the long-term relationship between representatives and constituents, established over time from a series of actions and issue positions. Building on Fenno's (1978) seminal work, they argue that representatives seek to build "trust" (Bianco 1994) or foster "credibility" (Sellers 1998). In the first case, representatives are concerned with building storehouses of goodwill, which will enable them to take controversial or unpopular stands in the future without undue risk of electoral defeat. In the second case, members seek to ensure that constituents believe them when they make claims about past behavior or promise future benefits. Both of these reputational models, though they provide for many ways in which members can curry favor with their constituents, concentrate primarily on credit claiming and "cheap talk" about legislative activity.[1] Underlying these accounts, then, is the assumption that actions in Washington, and particularly roll-call votes, receive attention from constituents back home. Citizens gain information about legislator behavior in a variety of ways—from the media, in communications from representatives, congressional challengers, or interest groups, or in conversations with other informed citizens, just to name a few. Yet regardless of how the information is conveyed, some citizen knowledge of member votes on specific issues is crucial for both the Bianco and Sellers reputational models and for any ability on the part of constituents to assess the quality of representation they receive in Washington.

In two recent papers, Alvarez and Gronke (1996a, 1996b) examine in detail the contours and limits of this citizen awareness. While cast more narrowly than studies of generalized reputations,[2] these pieces argue that there is considerable merit in examining citizen knowledge of salient roll-call votes. As the authors note, "Although infrequent, issues do arise which deeply divide Congress, which are hotly debated in the press, and which intrude upon the public consciousness. It is among this subset of issues that . . . we can determine the extent to which constituents know about their representatives' behavior" (Alvarez and Gronke 1996b, 105). These are the issues that members of Congress themselves believe are followed closely by the public and certainly by their allies and opponents in Washington and at home. The cases explored by Alvarez and Gronke (the Gulf War Resolution and the Clarence Thomas vote) constitute fairly extreme examples of this type of high-salience issue.

In their work, Alvarez and Gronke outline the factors that contribute to accuracy in citizen perceptions of legislative roll-call voting. In this paper, we, in a sense, look at the other side of the coin. We are interested here not so much in what makes citizens "get it right" but in the dynamics of "getting it wrong." More specifically, we suspect that there are substantial and systematic patterns of cognitive bias at work in citizen perceptions of the presumed facts of congressional roll-call voting. These biases affect both the likelihood of correctly recalling a member's vote and, if one does not recall, the relative probability of a "false negative" versus a "false positive" error. In the next section, we turn to a more complete discussion of these psychological hypotheses.

Concordance and Projection

Past studies of constituent knowledge of roll-call votes (e.g., Alvarez and Gronke 1996a, 1996b) and candidate ideology (Conover and Feldman 1989; Powell 1989) focus primarily on how citizen characteristics, such as political informedness, media exposure, and political efficacy, and legislator characteristics, such as ideological extremity and tenure in office, assist in the process of perception. There are two gaps in previous research, however. First, largely absent from Alvarez and Gronke's analysis is any consideration of how a citizen's *own* opinion on an issue might influence his or her perceptions of the representative's vote. In our view, this neglects two important effects at work in constituent recollections of member votes: *concordance* and *projection*. Second, past work that considers projection in presidential elections (e.g., Conover and Feldman 1989) finds no consistent impact of candidate evaluations on the magnitude or direction of projection effects. We find this result surprising and wish to test its robustness in the context of congressional roll-call votes.

Our hypothesis on concordance involves the interaction of a citizen's own position and his or her attitude toward the legislator. We argue that if citizens are favorably predisposed toward a representative, they will be more likely to retain information about his or her positions on the issues if those positions are consistent with their own preferences. Certainly, conventional variables such as political information, education, and the like play a role, as previous models would predict. The concordance effect, however, also follows naturally from cognitive psychological theory (McGuire 1969; Zaller 1992). If a citizen views a representative favorably, he or she will be more reluctant to internalize unfavorable information about the member (i.e., a roll-call vote at variance with the citizen's own position). As work in cognitive psychology documents, individuals are much more receptive to information that reinforces their existing predispositions than to information that creates dissonance and might require a reassessment (Petty and Cacioppo 1981). Thus, because of the tendency to retain concordant information, we expect that issue agreement will be an important predictor of citizens' ability to recall or guess their members' votes correctly. Of course, all of this applies only if the citizen has a positive view of his or her representative. If the member is viewed unfavorably, then it is *discordant* votes (in which the representative's position is at odds with the citizen's) that are most likely to be recalled. In sum, then, we contend that the interaction of citizen preferences and representative approval powerfully influences citizen perceptions of congressional roll-call voting, with constituents being much more likely to retain information about votes that reinforce preexisting impressions of the representative.

Our hypothesis on projection is related but conceptually distinct. Here, we are interested in those citizens who identify their representative's position on a bill *incorrectly*. These people may be divided into two groups: "false positives" (those who erroneously attribute support) and "false negatives" (those who erroneously attribute opposition). In their analysis of the Gulf War vote, Alvarez and Gronke (1996b) imply that people fall into one or the other of these categories essentially at random. Here, we question that assumption and contend that a citizen's own position and his or her assessment of the representative interact to influence systematically the likelihood of offering a false positive versus a false negative response. We maintain that individuals generally assume that political figures toward whom they are favorably disposed agree with them on important issues. If a citizen approves of his or her representative, any errors in identifying the representative's roll-call positions should tend to be in the direction of the citizen's own preferences (controlling, of course, for other influences). As an example, a pro-life citizen who is favorably disposed toward his or her member of Congress would be more likely to attribute erroneously to the member a vote against abortion than a vote for abortion. If the citizen has a negative view of the representative, exactly the opposite pattern should prevail.

The theory of projection presented here draws upon a substantial body of previous work (Bartels 1987; Conover and Feldman 1989; Martinez 1985; McAllister and Studlar 1991; Page and Brody 1972). Projection effects have not, however, been explored in the domain of citizen knowledge of legislative activity. In fact, our approach may be seen as a challenge to some of the fundamentals of extant projection models. In these models, roll-call votes (along with party affiliation, campaign materials, and news coverage) are described as "observable *facts*" (emphasis added) and are used as a basis to calculate spatial voting estimates (e.g., Fey 1994). If, however, these seemingly fixed elements are themselves subject to substantial projection effects, then spatial models will overestimate the relative power of issue voting.

It is important to consider an alternative theory, one that is more parsimonious than is ours. Concordance and projection could both be thought of as a means for the respondent to resolve a case of cognitive dissonance. Under Heider's (1958) balance theory, an individual's attitude, evaluation of an object (in our case, the legislator), and perception of the object's position (the legislator's vote) will resolve to consistency. Projection is clearly a way to achieve consistency. Respondents adjust their expectations of how a member voted in line with their own opinions about the member and their opinions about the bill. Concordance could be thought of in the same light. At least some of the accurate "knowers" are there not because they have retained more information about their representative but for the same reason that false positive and false negative responses were given, to resolve cognitive dissonance.

We believe that our theoretical framework, while more complex, is a better account of the patterns we observe here. Unlike a typical cognitive triad, we have *two* measures of the object's position, a perceptual measure (the respondent's identification of the member's vote) and a factual measure (the actual vote). In the analyses that follow, this distinction is key. The tests of concordance focus on the factual measure (was the respondent accurate or not), whereas the tests of projection focus on the direction of misperceptions. The availability of both "knowers" and "guessers" in the sample pool provides some leverage on differential levels of information retention and on how recall (and thus the concordance effect) differs from projection.[3]

Data and Methods

As a focus for our analysis, we examine citizen knowledge of legislative votes on an important piece of domestic legislation: the 1994 Omnibus Crime Bill. The Crime Bill was one of the most touted legislative initiatives of President Clinton's first term, providing seed money for up to 100,000 new local police officers, funding crime prevention programs, expanding the federal death penalty, and imposing tighter gun control regulations. The measure excited significant opposition, however, from several quarters. Criticism came chiefly from the National Rifle Association, which argued that the gun control initiatives were unwarranted and essentially unrelated to the bill's other components, but also from minority groups, who opposed the death penalty provisions. Additionally, fiscal conservatives attacked the crime prevention programs as pork-barrel nonsense, seizing on Midnight Basketball as an example of the bill's absurdities. After lengthy and often cantankerous debate, both in Congress and in the press, the House passed the Crime Bill on August 21, 1994, by a 235-195 vote. The bill was passed in the Senate later that week and eventually became law.

In many respects, the Crime Bill vote is an ideal vehicle with which to test our hypotheses on concordance and projection effects. Undoubtedly, the bill was a high-salience issue on the national political scene, dealing with an issue (crime) of great concern to many Americans and meriting considerable media coverage. It does not, however, have about it the aura of exceptionalism that characterizes the two cases examined by Alvarez and Gronke (1996a, 1996b). The Gulf War and Clarence Thomas votes were almost certainly the two most closely followed legislative roll calls of the decade, even though neither concerned a piece of legislation *per se*. One was an issue of war and peace, involving a massive commitment of military personnel and resources and a high expectation of significant American casualties. The other, with its scurrilous allegations and obvious overtones of race and gender, could not fail to titillate the nation. The Crime Bill vote, by contrast, provides a more typical forum in which to examine the interaction of legislative votes, citizen preferences, and citizen recall on important issues.

More important, the breakdown of the vote itself is well suited to our analysis. As discussed previously, the Crime Bill was not a pure Republican-Democrat or liberal-conservative issue.[4] The two parties, both in government and in the mass public, divide over *solutions* to crime, not over the need to do something about the problem (Flanagan and Longmire 1996; Marion 1994). The 1994 Crime Bill was unusual in that it combined both traditionally liberal and traditionally conservative approaches to combating crime. It paired more police with more prevention measures, greater gun control with a broader death penalty. As a result, both parties were split on the issue. While a majority of Democrats supported the measure and a majority of Republicans opposed it, over a quarter of each party's members in the House broke ranks and voted with the opposition.[5] Finally, as Jacobson (1997) notes, the Crime Bill vote was an important factor in many 1994 congressional

TABLE 1
Accuracy of Responses to Crime Bill Questions by NES Response Groups, 1994

Response	Numbers (Total N = 1496)
Respondent Recalls Vote	
Portion of Sample[a]	36.2% (542)
Portion Correct[b]	64.2% (348)
False Positives[c]	87.1% (169)
False Negatives[d]	12.9% (25)
Respondent Guesses Vote	
Portion of Sample	48.7% (728)
Portion Correct	57.7% (420)
False Positives	84.1% (259)
False Negatives	15.9% (49)
Total Sample	
Respondent Recalls Correctly	23.3% (348)
Respondent Recalls Incorrectly	13.0% (194)
Respondent Guesses Correctly	28.1% (420)
Respondent Guesses Incorrectly	20.6% (308)
Respondent Doesn't Know	15.1% (226)

[a]Portion of sample refers to the percentage of respondents who gave valid answers to the questions.
[b]Percentage correct refers to the percentage of respondents who correctly stated their representative's vote.
[c]False positives are those answering incorrectly who mistakenly believe that their member voted *for* the Crime Bill.
[d]False negatives are those answering incorrectly who mistakenly believe that their member voted *against* the Crime Bill.

campaigns and was employed by both parties in different contexts in national advertising, giving the issue a universal, baseline salience independent of the circumstances of any individual race. Clearly, these factors make the Crime Bill vote a useful and interesting one for analysis.

The format of the 1994 National Election Study (NES) questions on the Crime Bill allows us to construct several different models of citizen accuracy in perceptions of member votes.[6] In the study, respondents were first asked how their members of Congress voted on the Crime Bill. For those who provided an answer of either "for" or "against," the interviewer moved on to another topic. Those respondents who answered that they were not sure were then asked for their "best guess." Those who still offered no answer were not probed further. These two questions, when combined with records of the members' actual votes (taken from *Congressional Quarterly*), allow us to divide respondents into five general categories: correct recall, incorrect recall, correct guess, incorrect guess, and no answer given. Additionally, we can further subdivide the incorrect recall and guess categories into "false positives" and "false negatives," depending on the direction of the respondent's error. A breakdown of the sample according to these categories is provided in Table 1.

As is apparent from the table, respondents are divided fairly evenly among the five categories, in proportions ranging from 13% to 28%. Overall, this breakdown demonstrates three major patterns that merit some attention. First, while citizen knowledge of legislative roll-call voting is not overwhelming, it is not trivial. In this case, nearly a quarter of respondents correctly recalled their members' votes,[7] and a majority (51.4%) were able either to recall or to guess correctly. Secondly, despite being asked explicitly to guess if they did not know, more than 15% of respondents could offer no response as to their member's vote on the Crime Bill. The presence of this sizeable group (whom we may term the "robustly ignorant") is reassuring for our analysis, because it suggests that many of the most uninformed were willing to admit as much, rather than guessing randomly and thus polluting the pool of educated guessers (and attenuating systematic relationships in the data). Doubtless some of this randomness remains, but it will be substantially less than it otherwise could have been had all respondents ventured a guess. Finally, it is important to note the distribution of false positives and false negatives. Among both recallers and guessers, false positives outnumber false negatives by a wide margin. We will not dwell on this discrepancy here, but it will become important later, when we turn to an examination of projection effects.

Models of Accuracy in Recall and Guessing

In examining our hypothesis on concordance, we start with a model of accuracy in citizen recall of member votes. In this model, the dependent variable is correct recall of a member's vote on the Crime Bill. Respondents who answer correctly when first asked how their member voted on the bill (348 people) are coded 1; all other respondents (including those who claim to know but answer incorrectly, all guessers, and those who refuse even to venture a guess) are coded 0. According to our hypothesis, people should be more likely to recall correctly the member's vote if it is consistent with their preexisting attitudes about the member. Respondents who support the Crime Bill, for example, should be more likely to remember a "yea" vote from a representative whom they like or a "nay" vote from a representative whom they dislike. Thus, the key independent variable of interest in this model is the interaction of representative approval and issue agreement.[8] Because of the dichotomous nature of the dependent variable (whether the respondent correctly recalls the member's vote), we employ a probit model for our analysis.

Included in the model are a host of control variables. Following Alvarez and Gronke (1996a, 1996b), Powell (1989), and our own intuition, we assume that informed respondents who follow politics closely will be more likely to recall their members' positions correctly than will their less politically aware counterparts. Thus, we include measures of the respondent's level of political information,[9] education, and media exposure as predictors of accuracy in recall. Inclusion of such measures is consistent with work by Conover and Feldman (1989), who suggest that more politically attentive individuals do indeed make more accurate assessments of members' general issue orientations (though they examine issue placements of presidential contenders). While there is some multicollinearity among these variables, each taps a slightly different aspect of an individual's capacity for political reasoning and information processing.[10] Collectively, these variables essentially serve as a proxy for individual cognitive ability and intellectual engagement with politics.

We also employ in the model characteristics of the representative and contextual variables that might influence the likelihood of correct recall. It is possible, for example, that citizens might be more accurate about the votes of representatives from their own party than those from the opposing party because of generally increased ideological affinity and positive affect, as well as potentially greater exposure to information (in primary election mailings, fund-raising solicitations, and so forth). We therefore include in the model a measure of partisan agreement, coded 1 if the member and the respondent are of the same party and 0 if they are not (including all respondents who are pure independents). Additionally, we suspect that the positions of more ideologically extreme members might be more readily evident than those of members closer to the center. To test this proposition, we include a measure derived from folding the 1994 Americans for Democratic Action (ADA) member scores such that "extremists" (those with scores from 0 to 20 or 80 to 100) are coded 1 and "moderates" (those with scores between 21 and 79) are coded 0. Finally, because 1994 was an election year in which the Crime Bill was an important national issue (Jacobson 1997), we include a measure of whether or not there is a contested House race in the respondent's district, on the assumption that the positions of members who have been exposed to an electoral challenge should be better known than the positions of a representative who has not had to explain his or her votes in a campaign context.

The model's final component is a small set of demographic variables. Because women and blacks differ significantly from other Americans in their attitudes toward the death penalty and other criminal justice issues (Flanagan and Longmire 1996; Haghighi and Sorenson 1996), we allow for the possibility that their patterns of attentiveness to the Crime Bill vote might be different from those of whites and men. Moreover, because these groups are disproportionately the victims of crime, we suspect that they may follow the deliberations on crime issues more closely (and hence, perhaps, be more accurate in recall). We include dichotomous measures of race and gender (0 for nonblack, 1 for black; 0 for male, 1 for female) to test for any possible effects. More important, we employ a size-of-place measure arrayed along a 6-point scale from "most rural" to "most urban," in order to reflect our belief that urban residents are generally more concerned about crime than those in rural areas and thus more likely to follow the congressional debate over the Crime Bill. Thus, the independent variables in the model fall into three broad categories: individual cognitive measures, representative attributes, and demographic characteristics.

Our second model uses essentially the same independent variables, but it structures the dependent variable somewhat differently. In this model, correct recallers are still coded 1, and those who either recall or guess incorrectly, or who offer no response at all, are coded 0. The model differs from the previous one, however, in the respect that those who guess correctly are also coded 1. Thus, the dependent variable here is overall correct identification of the member's position, whether in response to the "recall" or to the "guess" question. This specification is based on our strong belief that knowing and guessing as captured in the NES are not two dichotomous phenomena, but rather points along a continuum of certainty and recollection.[11]

Naturally, the rate of accuracy is lower among the guessers than among the recallers (58% as compared to 64%), but it still looks appreciably different from random guessing. Indeed, as discussed previously, the fact that 15% of the sample refused to offer even a guess suggests that the number of truly clueless individuals among our guessers is probably limited. Instead, we suspect that we have in this pool a good many educated guessers, respondents who have some recollection of the member's vote but who are not confident enough to venture a response when first asked. Indeed, previous research (Alvarez and Gronke 1996b; Mondak 1993) suggests that many of the same variables that predict accuracy in recall also influence accuracy in guessing. Thus, the model here is almost exactly the same as the previous one, with the same expectations as to the direction of the coefficients. The only difference is that the second model includes a term measuring the effects of a respondent initially claiming to recall the member's vote, as opposed to being prompted to guess, on the assumption that knowers in the multivariate model (just as in the bivariate analysis) will be more accurate than guessers.[12]

Results of our analysis are found in Table 2. For both models, we report maximum likelihood probit coefficients, along with first differences to aid in interpretation.[13] Clearly, there is strong support from both groups for our central hypothesis on concordance. Indeed, the interaction of representative approval and issue agreement is the single strongest influence on the likelihood of correctly recalling or guessing a member's position and second only to political information in the recall-only model.[14] Respondents exhibit much greater ease in bringing to mind a roll-call vote if it reinforces their preexisting attitudes toward a representative. This finding holds whether we look at those respondents who offer an answer to the initial query or at those who must be prompted to guess. Put more concretely, a respondent whose representative's vote is consistent with his or her predispositions toward the member is 18% more likely to recall the vote correctly than a respondent whose representative casts a discordant vote. The figure in the pooled model is much larger still: respondents whose members cast concordant votes are fully 43% more likely either to recall or to guess the vote correctly than are those whose members cast discordant votes. Differences of this magnitude clearly indicate substantial concordance effects at work and suggest that such effects are a key component of accuracy in citizen perceptions of legislative roll-call votes.

The individual political awareness measures in both models generally work as expected; informed and well-educated individuals are more likely to recall or to guess their members' votes correctly than are other citizens. The effects, however, are no greater than those of concordance. The most informed respondents in the sample are 23% more likely to identify their member's vote correctly, all else being equal, than are the least informed, in both model specifications. Education has a somewhat smaller impact, and media exposure seems

TABLE 2
Probability that Respondents Will Correctly Identify Their Representative's Vote on the Crime Bill
(maximum likelihood probit estimates; standard errors in parentheses)

Independent Variable	Dependent Variable: Correct Recall		Dependent Variable: Correct Recall *or* Guess	
	Coefficient	*1st Diff.*	*Coefficient*	*1st Diff.*
Constant	51 (.24)***		−1.54 (.23)*'	
Agreement × Approval	.64 (.09)***	0.18	13 (.08)***	0.43
Political Information	.73 (.13)***	0.23	.58 (.13)***	0.23
Education	.07 (.03)***	0.12	.10 (.03)***	0.24
Media Attention	.12 (.16)	0.03	−.06 (.15)	-0.02
Partisan Agreement	.03 (.09)	0.01	.05 (.08)	0.02
Ideological Extremity	−.19 (.09)**	·0.09	.13 (.09)*	0.05
Contested Race	−.13 (.14)	·0.04	−.09 (.13)	-0.04
Urban Residence	.05 (.03)*	0.07	.10 (.03)***	0.20
Race (Black)	.19 (.14)*	0.06	−.16 (.13)	−0.06
Gender (Female)	−.20 (.09)**	-0.06	−.10 (.08)	−0.04
R Claims to Recall			.49 (.09)***	0.19
	N = 1107 χ^2 = 127.6, 10 df ($p < .001$)		N = 1107 χ^2 = 313.0, 11 df ($p < .001$)	

***$p < .01$, one-tailed test.
**$p < .05$, one-tailed test.
*$p < .10$, one-tailed test.

completely inconsequential. In sum, the awareness measures, while retaining some explanatory power, are no greater in magnitude than are the effects of concordance. While these variables are intuitively recognized as major determinants of citizen political knowledge (and thus have been the focus of most previous studies), they are not empirically the most important predictors of accuracy in recalling or guessing a member's vote on the Crime Bill.

Turning to the measures of representative attributes, we find a striking absence of any meaningful effects. Citizens are no more likely to recall or guess correctly the votes of representatives of their own party or those of members who have recently participated in a contested election. None of the coefficients on these variables in either model even approaches statistical or substantive significance. Additionally, the ideological extremism variable is signed oppositely in the two models and is of only marginal statistical and substantive significance. These results stand in sharp contrast to previous work (particularly Alvarez and Gronke 1996b), which found partisan agreement between citizen and representative to be an important determinant of accuracy in recalling and guessing member roll-call votes. We suspect that in the earlier work, this measure of partisan agreement was largely tapping an underlying interaction of issue agreement and representative approval, which we model explicitly here as concordance. This hidden interaction would explain the discrepancy between our findings and those of other models on this score.

The demographic variables in the models reveal some interesting patterns. As we expected, urban residents are more aware of their representatives' positions on the Crime Bill than are other Americans. Those who live in the largest cities are 7% more likely to be correct in the recall-only model, and 20% more likely in the pooled model, than are those who live in rural areas. Such a result is not surprising, given the heightened salience of crime issues in urban areas. The race and gender variables, however, reflect a more ambiguous pattern. Both are statistically significant in the recall model but not in the pooled model. The heightened salience of crime issues to African Americans, because of their disproportionate status as victims of violent crime, may have prompted greater attention to Crime Bill deliberations in the black community and hence higher rates of correct recall (though only by a modest 6%). The gender effect is more difficult to explain, as the coefficient is in an unexpected direction. In any event, however, both the race and gender effects are substantively small and do not detract from our central finding on the importance of concordance.

The final variable of interest is the term in the second model that measures the effects on accuracy of a respondent claiming to recall how the member voted (as opposed to guessing). The variable is highly significant, indicating that knowers are indeed substantially more accurate than are other respondents (a difference of 19%). This specification clearly outperforms a model estimated without the dummy variable ($\chi^2 = 32.85$, 1 df). This effect is greater than in the bivariate analysis and is reassuring for our theoretical premises. If knowers were not significantly more accurate in their identification of member votes than guessers were, one could argue that, in fact, nearly *everyone* in the sample was really guessing, thereby minimizing the information retention processes so central to our argument on concordance. The high level of statistical and substantive significance associated with the "claim to recall" term in the pooled model, however, strongly suggests that there *is* a substantial (though not absolute) distinction between knowers and guessers. This result, combined with our findings outlined above, clearly establishes the important role of concordance effects in the perceptual process.

Projection Effects

From the test of our hypothesis on concordance, we turn now to an examination of the other half of our theory. Here, we are concerned not with citizen accuracy in recalling or guessing member roll-call votes but with the dynamics of citizen *inaccuracy*. For our remaining analysis, therefore, we examine only those respondents in the 1994 NES who provided an erroneous response to either the recall or guess question about their member's vote on the Crime Bill. As we explained earlier, the theory of projection predicts that constituents will tend to believe that favored representatives hold views consistent with their own and that disfavored representatives hold views conflicting with their own. Thus, *ceteris paribus*, citizens who recall or guess their members' positions on the Crime Bill incorrectly should err disproportionately in the direction of these predispositions. In short, we test here the proposition that an individual's own position on an issue interacts with his or her attitude toward the representative to influence significantly the probable direction of error in perceiving member roll-call votes.

As a first step in examining projection effects with the 1994 Crime Bill, we employ a simple difference-of-means test, comparing respondents who themselves supported the bill to those who personally opposed it. Because the bill's passage was well publicized, we would expect a substantial bias toward false positives among all respondents, due to the naive hypothesis (i.e., "the bill passed, so my member probably voted for it"). Nonetheless, we should still see a significantly higher proportion of false negatives among those respondents who opposed the bill than among those who supported it. This is, in fact, the pattern that we find, as reflected in Table 3. While false positives are in the clear majority among both groups, respondents who themselves oppose the Crime Bill are almost twice as likely to give a false negative response as are respondents who support the bill. Thus, an

TABLE 3
Direction of Respondent Errors on Crime Bill Questions, by Respondent's Position on Bill

Respondent's Position	False Positive	False Negative
Favors Bill	87.1%	12.9%
Opposes Bill	77.8%	22.2%

Difference Estimate = 9.3% ($p < .05$).
N = 447

individual's own position on a piece of legislation seems to exert a significant biasing influence on his or her perception of a member's roll-call vote, at least in the bivariate context.

While this simple analysis certainly lends support to the theory of projection in citizen recollections of legislative roll-call votes, it is by no means conclusive. The bivariate model has no controls and does not introduce the dimension of representative approval, an important element in our theory. We therefore undertake a more rigorous test of our hypothesis by constructing a multivariate probit model of false positivity. In this model, the dependent variable is the direction of the respondent's error, coded 0 for false negatives and 1 for false positives. Our central independent variable of interest is termed "projection." This variable captures the interactive effect of a respondent's own position on the Crime Bill and his or her attitude toward the representative.[15]

We also include in the model variables to test potential competing hypotheses. One is the representative's party affiliation, to control for the possibility that respondents make errors because they expect their members to vote along party lines. If this is in fact the case, respondents represented by Democrats should be more likely to make false positive errors than respondents represented by Republicans. Additionally, we include a simple measure of the respondent's own position on the bill, to control for the possibility that respondents merely project in the direction of their own preferences regardless of how they feel about their member of Congress, an effect that Conover and Feldman term "false consensus." This suggestion is in contrast to our more nuanced, interactive hypothesis incorporating representative approval. Finally, we include variables for the respondent's education, level of political information, race, and gender, to capture any possible systematic effects of these demographic characteristics on the direction of erroneous recall. Because we have no *a priori*

TABLE 4
Projection Effects: Misperceptions of the Crime Bill Vote
(maximum likelihood probit estimates; standard errors in parentheses)

Independent Variable	Coefficient	First Difference
Constant	.7805 (.4342)**	
Projection	.2348 (.1024)***	.1188
Respondent's Position	.1757 (.1953)	.0102
Representative's Party	.2492 (.1598)*	.0571
Political Information	–.0108 (.5824)	–.0008
Education	–.0150 (.0514)	–.0110
Race (Black)	–.2641 (.2114)	–.0619
Gender (Female)	.0854 (.1533)	.0092

N = 431
Model χ^2 = 15.24, 7 df ($p < .05$)
Proportion of Cases Predicted Correctly = 84.92%

*** $p < .01$, one-tailed test.
** $p < .05$, one-tailed test.
* $p < .10$, one-tailed test.

expectations that urban residents would be more likely to err in one direction or the other (as opposed to simply recalling in the first place), we exclude size of place from this analysis.

The results from this model are reported in Table 4. Clearly, projection is the most important determinant of false positivity in citizen recall of legislator votes on the Crime Bill. Citizens who, according to our theory, would be inclined to project support for the bill are indeed 12% more likely, all else being equal, to err in this direction than are citizens who would not be so inclined. The skewed nature of the distribution (an overall false positive rate of over 80%) reveals that this effect is quite large; indeed, projection is shown to have a greater effect in this multivariate model than in the simple difference-of-means test reported previously. The only other variable to attain statistical significance is the representative's party. As predicted, respondents represented by Democrats are slightly more likely to err in the false positive direction than are respondents represented by Republicans, though the difference here is much smaller in magnitude than the effect of projection (less than 6%).

No other variables in the model have any discernible impact. An individual's levels of education and political information are apparently unrelated to the direction of his or her error, as are the respondent's race and gender. Moreover, the respondent's own position on the Crime Bill is also insignificant, once our interactive measure (projection) is introduced.[16] This result is important, as it indicates that our projection model incorporating both the respondent's position *and* approval of the representative is a more accurate specification than simply assuming that people project their own issue attitudes regardless of how they feel about the representative.

When considered in the light of Conover and Feldman (1989), this pattern of results, particularly the weakness of demographic and information measures as predictors, is not surprising. Conover and Feldman, in their study of citizen perceptions of the issue stands taken by presidential candidates, found that only the respondent's own issue stance, party affiliation, and an interaction term similar to our own (opinion*evaluation) acted as cues to the presidential candidates' positions. Even in the midst of a hard-fought, high-profile presidential contest, media attentiveness (TV and newspaper) and ideology failed to function as cues. The results here are similarly compelling. Whether it is citizen perceptions of the policy stances taken by competing presidential candidates or of member stances on a single roll-call vote, projection outperforms any theoretically plausible rival as a determinant of citizen perceptions. In our specific case, where accuracy can be precisely assessed, projection leads to *misperception* of congressional votes among a sizeable minority of citizens.

Conclusions

This research sheds light on an important dimension of the relationship between constituent and representative—citizen perceptions of legislative roll-call votes. Rather than viewing a member's vote simply as a knowable event of which citizens are either aware or ignorant, we explore the psychological dynamics of perception and misperception that color citizen knowledge of member voting records. Our analysis has shown substantial evidence of two important cognitive biases at work in citizen perceptions of legislative roll-call votes: concordance and projection. Constituents are more likely to remember a legislator's vote if it is consistent with their preexisting views of the representative. If constituents recall incorrectly, they are more likely to err in a direction consistent with their predispositions.

These psychological tendencies among constituents have interesting and important implications for several areas of research. To begin with, they provide yet another possible component of the incumbency advantage. It has long been established that incumbent members can build up goodwill among constituents through credit claiming, advertising, position taking, and casework (Fenno 1978; Mayhew 1974). Indeed, data from the 1994 National Election Study indicate that about 65% of respondents hold a favorable view of their member of Congress. Our research suggests that if a representative becomes generally well liked among constituents through these types of nonpolicy activities, he or she will have considerable freedom to vote according to personal conviction or Washington political currents, even if the vote is at odds with sentiment in the district. This argument is not entirely new—Bianco (1994), for example, talks about the role of trust in providing leeway for unpopular votes. Our account, however, is a bit different. We do not argue that citizens consciously decide to overlook discordant votes or weigh in their minds the pros of good casework and past reputation versus the cons of unpopular votes (though some of this probably goes on). Rather, we contend that if citizens have developed a favorable view of a representative, they are more likely to recall a vote if they agreed with the member on the issue than if they disagreed and, when pressed, will tend to assume that the representative voted in accordance with their own preferences. These processes operate largely at a subconscious level, substantially mitigating the negative impacts of unpopular votes cast by popular representatives.

These findings dovetail nicely with recent research on congressional popularity. Durr, Gilmour, and Wolbrecht (1997) note, ironically, that congressional approval declines when Congress acts in "its institutional role as representative and legislative body." The passage of major legislation, because it almost always involves substantial conflict and offends some segment of society, generally results in more negative public attitudes toward Congress as a whole but not necessarily toward individual members. Hibbing and Theiss-Morse (1995) similarly distinguish between individual representatives and Senators ("member"), Congress as a 535-person collective ("members"), and Congress as an institution. Citizens have a strong commitment to the ideal of Congress as an institution and tend to like their individual members. Public approval of the collective Congress, however, consistently lags behind these other measures.

Our research illustrates the individual-level dynamics of this process. Over time, if projection and concordance effects are at work, any preexisting gap in approval between individual members and the collective body will gradually be exacerbated. Individual members will almost inevitably be more popular in their own districts than will Congress as a whole because of a presumably closer ideological match, as well as casework, advertising, and so on. This initial disparity should grow over time, as constituents give the member more credit and less blame for policy outcomes than he or she is due (because of the concordance and projection effects). In other words, our findings suggest that a dynamic may be at work in which many citizens blame the membership of Congress for unpopular policies but not their individual members—even if the members in fact voted for the policies. Constituents will tend either to forget the unpopular vote (the concordance effect) or to ascribe to the member a more popular position erroneously (the projection effect). Individual members take all the credit, and Congress as a collective receives all the blame.

This pattern has troubling implications for conventional notions of representation and legislative accountability. V.O. Key's characterization of the electorate as "a rational god of vengeance and reward" is difficult to sustain in the face of our findings. Voter rationality must be questioned when significant and systematic cognitive biases shape public perceptions of a legislator's behavior in office. Certainly, the electorate would seem to dispense much more reward than vengeance for most legislators. Scholars have long questioned the appropriateness of interpreting congressional election outcomes as policy referenda, primarily due to well-established citizen ignorance of member roll-call votes. Our research here provides yet another reason for caution. Not only are many citizens (about 50%) ignorant of their members' votes even on major legislation, but there are also systematic biases (generally favoring incumbents) in the direction of these erroneous perceptions. While most of our discussion has focused on the positive side of concordance and projection (biases that favor a popular representative), a negative side (biases that hurt an unpopular representative) is at work as well. Thus, overall, concordance and projection work to establish significant inertia in citizen perceptions of elected representatives. They make it difficult for a representative to lose supporters because of unpopular votes or to win over opponents by voting in line with their preferences. This reality substantially attenuates the relationship between a member's voting record and his or her reelection prospects. The separation of behavior in office from electoral success, while doubtless welcomed by many representatives, is clearly troubling for any theory of representative democracy.

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NOTES

Data for the analyses in this paper are drawn primarily from the 1994 American National Election Study, supplemented by some additional materials gleaned from *Congressional Quarterly* and *Politics in America*. All model estimation is done using STATA version 5.0.

1. One should note here the similarity to Mayhew's 1974 argument, in which credit claiming and position taking are key components of incumbent reelection strategy.

2. Bianco 1994, for example, would hardly claim that citizens need to know the specifics of each roll-call position. From his perspective, the member's overall reputation of trustworthiness is more important than a popular position on any single roll-call vote. Member reputations, however, are built largely from citizen responses to a series of individual issue positions over time; each roll-call vote is one step in the long-term process of building credibility and trust. Thus, the dynamics of citizen knowledge of member roll-call votes warrant examination.

3. There is no completely satisfactory way to mediate empirically between a single, cognitive consistency model and one partitioned into concordance and projection. Much of the empirical analysis that follows, however, strongly suggests that two distinct processes, concordance and projection, are indeed going on. These results, in conjunction with our discussion of psychological theory, give us considerable confidence in the appropriateness of our theoretical specification. Ultimately, however, regardless of the relative frequency of concordance versus projection effects, our central theoretical and substantive arguments remain unchanged.

4. The Crime Bill vote provides a difficult test for our theories of concordance and projection. On an issue where the opposing positions are clearly defined and known among the general public (for example, on abortion), one might expect a citizen's own attitudes to play an even stronger role in the perceptual process. We believe that if concordance and projection effects can be demonstrated on a more complex vote like the Crime Bill, with nuanced positions and divisions within both parties, then they must exist on simpler votes as well.

5. This pattern is quite helpful for our analysis, since it cuts down on the number of respondents who will be misidentified as correct recallers on the basis of a simple partisan guess (i.e., if the member is a Democrat, he voted for the bill; if she is a Republican, she voted against it).

6. The exact wording of the Crime Bill question is as follows: "One of the main parts of President Clinton's program this year was his crime bill. Did Representative (NAME) vote for or against the crime bill?" The following categories were coded by the NES:

- Voted for crime bill
- R volunteers "think he/she voted for it; he/she must have voted for it"
- R volunteers "think he/she voted against it; he/she must have voted against it"
- Voted against crime bill
- DK (These respondents were asked the follow-up, "What would be your best guess? [Did {he/she} vote for or against?]")

The mention of President Clinton could provide a partisan cue to some respondents and influence their guesses as to their members' positions. This effect should be captured by the inclusion of member's party in the projection model. Additionally, for the concordance models presented in Table 2, we tested alternative specifications explicitly modeling a Clinton cue effect, including interactions between 1) member's party and Clinton approval, and 2) Crime Bill vote and Clinton approval. Both terms were insignificant in both models. We thus opted for the simpler specification, excluding these measures.

7. Of course, some of those who "recall" correctly probably only guessed correctly. A high-end estimate of this percentage in the total sample is 13% (the same percentage who recalled incorrectly). This would leave a baseline of only about 10% of the sample that we can be virtually certain recalled their member's vote correctly. For a more extended discussion of this estimation problem, see Alvarez and Gronke (1996b). The precise size of this "false knower" group, however, is not terribly important for our analysis, as we examine the psychological dynamics of concordance and projection among both knowers *and* guessers.

8. Consistent with our theory, this variable is coded 1 if either the member's vote is concordant with the respondent's opinion (i.e., they agree) and the respondent likes the representative, or if the vote is discordant with the respondent's opinion and the respondent dislikes the representative. In both cases, the vote and the opinion reinforce one another. In cases of mixed signals, when issue agreement and attitudes toward the representative point in opposite directions, or when respondents express neutrality or no opinion about the representative, the variable is coded 0.

9. We use an additive scale similar to that suggested by Zaller 1992.

10. The degree of multicollinearity is not so great as to cause serious problems for the estimation. Political information and education correlate at .52, and race and political information correlate at -.39. No other correlation is above .3, with most well below .1. The auxiliary R^2s were .35 (political information) and .26 (education); no others were above .07.

11. Despite this belief, we also test the model under the more rigorous specification in which only those who correctly answer the initial query are considered to have truly retained the relevant information. Thus, no matter which specification one finds theoretically more persuasive, substantial evidence of a concordance effect exists (as reported below).

12. This specification implies that the only difference between knowers and guessers is in the mean level of accuracy, not in the relationship between any independent variable and accuracy. We tested this assumption by running a fully interactive model in which each independent variable was estimated with a different slope for knowers and guessers, as well as a specification in which we eliminated each variable with an estimated effect of less than one times its standard error. In no case did a model including additional interaction terms outperform the model presented in Table 2. Compared to a model including only a dummy variable, the log likelihood ratio statistic for the fully interactive model is $\chi^2 = 7.47$, 10 df. For the model including different slopes on education, media usage, urban residence, and race, it is $\chi^2 = 6.89$, 4 df. Both fall far below conventional statistical significance levels.

13. First differences give the estimated effect of a change in the variable from its minimum to its maximum, holding all other variables constant at their means.

14. This finding proves quite robust, as it survives a number of alternative model specifications. The variable retains its explanatory power if we construct it from feeling thermometers instead of approve/disapprove measures, if we look only at respondents who approve (or disapprove) of their representatives, or if we look only at those who agree (or disagree) with their representatives on the Crime Bill. Also, we find no evidence of a "contrast" effect, in which positive projection is stronger than negative projection.

15. It is important to note that this variable is different from the one used in the concordance models. Here, we are interested in the interaction between the respondent's own position on the Crime Bill and approval of the representative; *agreement* with the representative on the issue does not enter into it. This variable is constructed such that those respondents who we would expect to project false positive (those who support the Crime Bill and like their representative, or who oppose the bill and dislike their representative) are coded 1, while those who we would expect to project false negative (people who oppose the Crime Bill and like their representative, or who support the bill and dislike their representative) are coded –1. Respondents who provide no answer for either question are coded 0.

16. This result could be attributed to multicollinearity. However, the bivariate correlation between the individual's own position on the bill and the projection measure is not high enough (.44) to cause great concern. All other correlations, other than education and political information (mentioned in note 10), are less than .25. When we estimate the model with projection removed, the individual's own position becomes more influential ($b = .37$) and is statistically significant, but this result is to be expected, since the variable is correlated with projection. Alternatively, when the model is estimated with the individual's position removed, the estimated impact of projection grows slightly, to .28. In both cases, the estimated standard errors are virtually identical. However, we find both of these alternative model constructions unacceptable. The first (with respondent's position only) we believe to be seriously misspecified, omitting the central dynamics of projection. The second (with projection only) does not allow for a test of the simpler competing hypothesis. Since all auxiliary R^2 measures in the multicollinearity test are less than .3, we choose to retain the fuller model specification.

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