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Source: *The American Political Science Review*, Vol. 88, No. 1 (Mar., 1994), pp. 63-76

Published by: American Political Science Association

Stable URL: <https://www.jstor.org/stable/2944882>

Accessed: 12-02-2020 11:10 UTC

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## SHORTCUTS VERSUS ENCYCLOPEDIAS: INFORMATION AND VOTING BEHAVIOR IN CALIFORNIA INSURANCE REFORM ELECTIONS

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**V**oters in mass elections are notorious for their apparent lack of information about relevant political matters. While some scholars argue that an electorate of well-informed voters is necessary for the production of responsive electoral outcomes, others argue that apparently ignorant voters will suffice because they can adapt their behavior to the complexity of electoral choice. To evaluate the validity of these arguments, I develop and analyze a survey of California voters who faced five complicated insurance reform ballot initiatives. I find that access to a particular class of widely available information shortcuts allowed badly informed voters to emulate the behavior of relatively well informed voters. This finding is suggestive of the conditions under which voters who lack encyclopedic information about the content of electoral debates can nevertheless use information shortcuts to vote as though they were well informed.

**D**ecades of survey research have provided us with countless examples of voter ignorance. While the existence of the badly informed voter is now a central part of political science's intellectual heritage, the implications of voter ignorance continue to be vigorously debated. Many scholars and pundits argue that widespread voter ignorance leads to voting behavior and electoral outcomes that are meaningless representations of voter interests.<sup>1</sup> Other political observers argue that voters find ways to adapt to their apparent lack of information. These scholars conclude that voting behavior and electoral outcomes provide valuable measures of voter interests.<sup>2</sup>

To understand whether or not voters can successfully overcome their lack of information, it is instructive to consider briefly how voters can obtain information about the personal consequences of electoral outcomes. I conduct this review in the particularly appropriate context of a direct legislation election. In direct legislation elections (e.g., the initiative and the referendum), voters make selections from an exogenously determined menu of specific policy alternatives called propositions. A defining characteristic of many propositions is complexity.<sup>3</sup> Since voters who encounter a complex proposition for the first time are likely to be confused about the consequences of its approval, the extent to which voters can adapt to their initial condition of ignorance will determine how well they can promote their own interests through the act of voting.

Voters who have an interest in the outcome of a direct legislation election might first consider gathering facts about a proposition from the official document that describes its content. However, these documents are usually lengthy and/or filled with technical language. As a result, voters in large electorates who consider their opportunity costs may decide that the acquisition of "encyclopedic" information is not a worthwhile activity.

As an alternative to the costly acquisition of ency-

clopedic information, voters may choose to employ information shortcuts. For example, voters can acquire information about the preferences or opinions of friends, coworkers, political parties, or other groups, which they may then use to infer how a proposition will affect them. The appeal of these information shortcuts is that they generally require relatively little effort to acquire. The drawback of these shortcuts is that they may be unreliable, since relatively well informed information providers may have incentives to mislead relatively uninformed voters.

Because many of my colleagues and I were curious about the extent to which relatively uninformed voters could use information shortcuts to cast the same votes they would have cast if better informed, I designed and administered an exit poll of California voters who, in 1988, were confronted by five distinct and complex insurance reform propositions. The survey responses allow me to identify a class of voters who, while appearing to possess relatively low levels of encyclopedic knowledge about the content of insurance reform initiatives, used an information shortcut that allowed them to emulate the behavior of well-informed voters. Specifically, I find that relatively uninformed voters who could correctly identify the insurance industry's official position on a particular proposition were much more likely to emulate the behavior of relatively well informed voters on that proposition than were similarly uninformed voters who did not know the insurance industry's position. I conclude from this finding that if relatively well informed voters are most likely to cast the votes that are consistent with their own interests, then knowledge of the insurance industry's position allowed voters who might otherwise be classified as ignorant to act as they would have if they had taken the time and effort necessary to acquire encyclopedic information. While the analysis I present does not lead me to conclude that shortcuts will always be sufficient to help uninformed voters overcome their lack of knowledge, I believe that it is suggestive of the

conditions under which voters who have not acquired encyclopedic knowledge can vote as though they had.

Next, I offer a brief history of the events leading up to the insurance reform elections. I then place relevant aspects of this history within the context of previous theoretical and empirical research on voter decision making in order to generate predictions about how different types of information should affect voting behavior. I then offer a description of the survey instrument's construction and execution and employ a number of statistical tests to identify the effects of certain types of information on respondent voting behavior. With this survey, and related theoretical and experimental research on the responsiveness of direct legislation,<sup>4</sup> I hope to advance more general debates about the nature of voter competence and the substantive meaning of electoral outcomes.

## INSURANCE REFORM AND THE INFORMATION PROBLEM

What events led up to the insurance reform elections and how do they figure in the context of previous work on voter decision making?

### A Brief History

In 1987, California drivers paid the third highest auto insurance rates in the nation. Both the state's trial lawyers—who receive half of their case load from automobile accident claims (Reich 1988a)—and the insurance industry recognized the widespread public support for regulatory reform and both were interested in influencing the legislative reform agenda. Each group publicly blamed the other for the recent rapid increase in auto insurance premiums. Insurance industry spokesmen argued that higher rates were caused by skyrocketing legal costs and publicly supported regulatory reforms that assigned most of the costs of reform to trial lawyers (e.g., laws that limit attorney fees, decrease the likelihood of large settlements, or both.) In contrast, the California Trial Lawyers Association (CTLA) publicly portrayed the insurance industry as greedy oligopolists who were conspiring against consumers. The CTLA supported regulatory reform that assigned most of the costs of reform to the insurance industry (e.g., the elimination of the insurance industry's exemption from many of California's antitrust laws).

Both groups attempted to influence insurance reform through their lobbying efforts in the state capitol. In this domain, the insurance reform debate was a battle of Titans. Both the insurers and attorneys had what were among the most influential lobbies in California (Reich 1987a). Perhaps coincidentally, all attempts at reform died in state legislative committees.<sup>5</sup>

When the legislative stalemate seemed destined to outlast calendar year 1987, a number of consumer

groups, who had been effectively shut out of the insurance reform debate in the legislature, voiced their intent to place an initiative on the November 1988 ballot. By submitting an initiative, consumer groups would not only gain control of the reform agenda, but they might also compel other groups, who would be opposed to their initiative, to spend millions of dollars to protect the relatively favorable status quo. In order to avoid a costly initiative campaign, insurance industry representatives and the CTLA made several attempts to form coalitions with the consumer groups that were preparing an electoral strategy. In addition, insurer and attorney groups attempted to form legislation-supporting coalitions with each other and each drafted its own separate initiative in the event that no agreement could be reached. By the time the deadline for qualifying ballot measures had arrived, however, no coalitions had formed.<sup>6</sup> As a consequence, the three groups (the insurance industry, the trial lawyers, and the consumer activists) placed five different insurance reform initiatives on the November 1988 ballot.

Only one of the five qualifying propositions passed on Election Day: Proposition 103 was sponsored by the consumer activist group Voter Revolt (whose primary spokesman during the campaign was Ralph Nader) and received 51.1% of the vote. Proposition 103 called for the removal of the insurance industry's antitrust exemption, public hearings as a prerequisite for rate changes, auto insurance premiums to be determined primarily by driving record (as opposed to where one lived), a premium discount for "good drivers," election of the state's insurance commissioner (rather than gubernatorial appointment), and a mandatory 20% reduction on all auto insurance premiums.

Of the four initiatives that lost, one was sponsored by trial lawyer interests, and three were sponsored by insurance industry interests. Proposition 100 was sponsored by the CTLA and received 40.9% of the vote. This proposition called for the reduction of "good driver" rates by 20% and the institution of health insurance rate regulation. It also allowed banks to sell insurance and allowed claimants to sue insurance companies for acting in "bad faith." Proposition 104, the no-fault initiative, was the insurance industry's most favored proposition (as evidenced by the fact that the insurance industry spent much more to promote this proposition than it did to promote the others). Despite this status, it was approved by only 25.4% of the voters. This proposition called for the establishment of a no-fault system of auto insurance (thus eliminating the need for many types of legal recourse in the event of an accident), the reduction of some premiums by 20% for two years, a restriction on future insurance regulation legislation, limitations on damage awards against insurance companies, limitations on attorney contingency fees, and the preservation of the insurance industry's antitrust status. Proposition 101 was sponsored primarily by one insurance company, received 13.3% of the vote, called for a temporary reduction of the bodily injury portion of

insurance premiums and limited injury claims for pain and suffering and required that all other sources of compensation be exhausted before an insurance company was required to pay. Proposition 106 was sponsored by the insurance industry and received 46.9% of the vote. This measure placed limits on attorney contingency fees in tort cases.

Several characteristics of the insurance reform elections made them particularly appropriate for testing the effect of different types of information on voting behavior. Two of these characteristics led me to believe that most voters would not take the time or effort needed to become well informed about many of the issues relevant to the insurance reform debate. The first of these characteristics is that the text of the five insurance reform ballot initiatives was lengthy (totaling over 26,000 words) and technical. Second, insurance reform voters were likely to have been occupied with other political matters, since the November 1988 general election ballot in California was unusually long.<sup>7</sup>

Other characteristics led me to believe that there would also be a vigorous campaign. The first of these characteristics is that reform of the state's insurance regulation was—and still is—a highly salient issue. Each of the five initiatives corresponded to a significant change in the insurance regulatory framework and each had the potential to affect the price or expected value of many insurance policies, as well as insurance industry profits and trial-lawyer case loads. Second, the potential effect of different electoral outcomes on their profit margins led me to expect, *a priori*, that there would be high levels of campaign expenditure by the insurance industry and trial lawyers. (Over \$82 million was actually spent on the insurance reform campaigns.) Because the insurance reform elections had these four characteristics, I expected that there would be complex electoral decisions, many confused voters, a vigorous campaign, and a unique opportunity to examine how different types of information affect voter behavior.

### Sources of Information

Insurance reform voters had access to different types of information than voters in candidate-based elections typically do. The most notable difference was the absence of two types of information that are widely believed to affect voting behavior in elections involving candidates. The first type of missing information was the *partisan cue*. Not one of the initiatives had a party label attached to it.<sup>8</sup> This absence is particularly relevant because much of the contemporary understanding of voting behavior relies on the notion of voters taking cues from party labels.

The second familiar type of missing information was *the past*. Retrospective evaluations, of the type considered by Downs (1957), Key (1966), and Fiorina (1981) are thought to help voters simplify their evaluations of electoral alternatives and depend on the existence of relevant past histories. For instance, retrospective voting hypotheses tell us that voters can

evaluate a candidate's credibility and potential effectiveness by reviewing particular features of the candidate's past. While credibility is less of an issue for the content of a ballot proposition, an electorate's ability to evaluate the consequences of a particular proposition retrospectively could help it better understand the future consequences of the proposition's acceptance. Unfortunately for voters, no such history was available for the five insurance reform propositions, since none of the proposed laws had ever previously been enacted in California.

In addition to the absence of some common types of information, the insurance reform elections included two sources of information that are not generally available in other electoral environments. First, the state provided a *summary* of each initiative. The summaries for the five insurance reform initiatives appear in Appendix A and were available in many places, including on the ballot itself. The summaries were between 25 and 100 words long and were intended to help voters distinguish between the initiatives. While each summary provided some information about an initiative, certain characteristics of the summary limited its effectiveness. For instance, a summary's brevity often resulted in the omission of important components of a complex initiative.

A second source of state-provided information was the *California Ballot Pamphlet*. The pamphlet for the November 1988 election was a thick document that contained, for each proposition, the summary just described, one signed argument by proponents, one signed counterargument by opponents, one signed argument by opponents, one signed counterargument by proponents, and the actual text of the proposed changes to the law. The fact that voters could obtain a great deal of information from the California Ballot Pamphlet is not in question. However, whether voters might reasonably be expected to spend the time and effort required to learn from the pamphlet is questionable and undermines its potential effectiveness.

A final source of information was the campaign waged by the three affected interest groups: the insurance industry, the CTLA, and consumer activists. Together, these three groups spent more than \$82 million on the campaign. Table 1 details the names, preferences, and expenditures of all the campaign organizations that were involved in the insurance reform campaign and registered with the state board of elections. Each organization is classified by its primary source of funding. After reading through the campaign receipt and expenditure documents that were filed with the state, I can confidently assert that there did not exist an organization for which this classification was not obvious. (All contributors of \$25 or more must be listed by name and occupation in a California direct legislation campaign's contribution filings.)

A cursory inspection of Table 1 reveals more about the campaign than just dollars expended. All of the large groups claim to be consumer- or citizen-oriented. These claims are indicative of the campaign

TABLE 1		
Campaign Expenditure by Organization		
CONTESTANT ORGANIZATION	RECOMMENDED VOTE	EXPENDITURE (\$)
Insurance Industry		
Citizens for No Fault	yes on 104 and 106, no on 100 and 103	\$41,402,392
Californians against Unfair Rate Increases	no on 100 and 103	14,951,162
Consumers for Lower Auto Insurance Rates	yes on 101	5,401,934
Committee for Fair Lawyers Insurance Fees	yes on 106	2,523,599
American Insurance Association	yes on 104	730,575
Committee for Fair Auto Insurance Ratings	no on 100 and 103	39,323
Total		\$65,048,985
Trial lawyers		
Good Driver Initiative	yes on 103	\$13,786,653
No on 106	no on 106	624,449
Consumer Coalition against 106	no on 106	401,704
No on Proposition 106 Committee	no on 106	172,737
Consumer/Legal Equal Justice Committee	no on 106	20,000
Total		\$15,005,543
Consumer activists		
Voter Revolt for Lower Insurance Rates	yes on 103	\$ 1,932,902
Californians for Honest Insurance	no on 101,104, and 106	24,005
Friends of Motorcycling	yes on 100, no on 101, 104, and 106	22,500
Santa Cruz Committee for Consumer Justice	yes on 100, no on 104	6,117
		\$ 1,985,524
Total	—	\$82,040,052

strategies employed by the large information providers. In the advertisements purchased by the insurance industry and trial lawyers, the identity of the sponsor was as well hidden as the law would allow. Except for the small print at the bottom of print advertisements or the rapidly disappearing disclaimer that surfaced in broadcast media, the fact that a particular message was associated with the preferences of the insurance industry or trial lawyers was not mentioned. These strategies gave all of the paid advertisements the quality that groups supporting an initiative attempted to represent themselves, and their initiative, as “proconsumer,” regardless of their source of financial support, while groups opposing an initiative represented the initiative they were campaigning against as “anticonsumer”. So, unlike either scientists and engineers (Kuklinski, Metlay, and May 1982) or major political parties in candidate-centered elections, the insurance industry and trial lawyers did not want to be used as reference groups by the voters—a circumstance that further complicated the voters’ ability to understand the likely effects of five complex propositions.

Shortcuts and Voter Inference

If insurance reform voters faced complex alternatives, were too busy or disinterested to acquire encyclopedic information, and could rely on neither the past nor partisan cues, how did they decide which propositions to vote for? To place this case study in a broader context and to motivate the empirical tests that follow, I briefly review some research that is

relevant in attempting to understand the behavior of insurance reform voters.

Particularly appropriate for this case study are the dynamics described in strategic models of communication, more commonly known as “signaling” games. In a signaling game, a relatively well informed information provider has information that is relevant to a relatively uninformed decision maker.<sup>9</sup> The information provider can attempt to affect the decision maker’s behavior by sending a “signal” about the consequences of the decision maker’s actions. The inferences that the decision maker is able to draw from the content of the signal depend on prior beliefs about both the information provider’s knowledge and the information provider’s incentives for truth telling. Since the information providers in the insurance reform example also drafted the propositions, I proceed as though it were common knowledge that the information providers understood the content of the propositions and focus this review on the relationship between an information provider’s credibility and voter inference.

When either the information provider or the content of the signal is known to be perfectly credible, several scholars have argued that voters can use the content of a signal to make more accurate inferences about the personal consequences of an electoral outcome. Calvert (1985), McKelvey and Ordeshook (1985, 1986), and Grofman and Norrander (1990) use spatial models of candidate-centered elections, while I (Lupia 1992) used a spatial model of direct legislation, to identify conditions under which the existence of perfectly credible signals are sufficient to allow in-

completely informed voters to emulate the behavior of better or completely informed voters. In these studies, uninformed voters tend to find it in their interests to cast the same vote as the information provider when they know that they and the information provider have similar preferences over outcomes. The same dynamic allows relatively uninformed voters to emulate the behavior they would exhibit if well informed by voting against the information provider's preferred alternative when they know that the information provider's interests are contrary to their own. Unfortunately, these arguments are of limited helpfulness when we attempt to understand voter decision making in circumstances where information providers are not perfectly credible and may, in fact, have an incentive to mislead voters.

Sobel (1985) and Brady and Sniderman (1985) have presented arguments suggesting that the existence of a "reputation for honesty" or "likability" is sufficient to allow voters to condition their voting behavior on the content of a signal provided by an information provider who is not perfectly credible. Sobel uses a repeated-play signaling model to show that if an information provider (who is not initially assumed to be perfectly credible) can establish a reputation for some consistent quality, like honesty, then badly informed decision makers can make more accurate inferences about the options available to them from the content of a signal whose truthfulness cannot otherwise be easily established. Brady and Sniderman use the National Election Studies to show that voters in candidate-centered elections rely on their feelings toward information-providing groups better to understand the relationship between their own preferences and those of the candidate.<sup>10</sup>

In the absence of perfectly credible signals, could insurance reform voters have used their knowledge or perceptions of a particular information provider's reputation to help them understand the personal consequences of a particular electoral outcome? The theories I have reviewed—and others like them—suggest that the answer is *yes*, but only if voter perceptions of an information provider's preferences (i.e., the information provider's reputation for supporting certain types of policy) were correct. If correct, perceived information-provider preferences are equivalent to the information providers having a known and informative reputation for supporting certain types of policy. If incorrect (which is quite possible in this case considering the insurance industry and trial-lawyer attempts to shield their identities), then perceived information-provider preferences will not be sufficient to allow badly informed voters to overcome their lack of encyclopedic knowledge.

For our purposes, it is unfortunate that evaluating the accuracy of insurance reform voter perceptions is quite difficult. Fortunately, we need not make this explicit evaluation to determine whether knowledge of a prominent information provider's preferences allowed otherwise uninformed voters to vote as

though they were well informed. We can simply compare the behavior of voters who differ only in the amount and types of information they possess. With this type of comparison in mind, I developed and executed an exit poll of insurance reform voters. In an attempt to distinguish relatively informed voters from those who were relatively uninformed, I designed the survey to provide individual-level measures of the amount of encyclopedic information that a respondent possessed. To determine what impact, if any, information shortcuts had on voting behavior, I also asked respondents to report their perceptions of opinions that could have been used as shortcuts, relevant demographic questions, and their voting behavior.

My plan was to use these measures to compare the behavior of well-informed voters, relatively uninformed voters who knew a shortcut, and relatively uninformed voters who did not know that shortcut. Finding that the voting behavior of "relatively uninformed voters who knew a shortcut" was significantly different from the voting behavior of "relatively uninformed voters who did not know a shortcut" would lead me to reject the hypothesis that knowing the shortcut did not affect voting behavior. Finding that the voting behavior of "relatively uninformed voters who knew a shortcut" was also very similar to the voting behavior of "relatively informed voters" would provide support for the assertion that voters who appeared to possess low levels of encyclopedic knowledge used certain types of shortcuts to emulate the voting behavior they would have exhibited if they were relatively well informed. In contrast, finding that the voting behavior of "relatively uninformed voters who knew a shortcut" was very different from the voting behavior of "relatively informed voters" would suggest that knowing the shortcut was insufficient for voters to overcome their lack of encyclopedic information.

## A SURVEY TO TEST FOR THE EFFECTS OF INFORMATION

The survey consists of exit interviews with 339 voters in Los Angeles County. Respondents were asked how they voted on the insurance reform propositions, socioeconomic and insurance-rate-related questions, and a series of questions designed to elicit the level of information (or confusion) that each respondent had on the issue of insurance reform. The information questions were designed to learn not only what respondents knew about the content of the insurance reform debate but also to gauge respondent beliefs about information-provider preferences. The information questions used in the analysis appear in Appendix B.

To execute the survey instrument, I recruited 30 undergraduates from the California Institute of Technology, California State University (Northridge), and Pasadena City College. The pollsters received extra

credit in political science and economics classes in exchange for attending two instructional sessions and administering the poll for four hours on Election Day. Lecturers at these institutions received a day off from lecturing in exchange for providing me with access to their students. In effect, I received low-cost pollsters in exchange for lecturing about the results of the exit poll in the students' classes. The entire survey was conducted for less than three hundred dollars.

In the instructional sessions, I stressed careful execution of the polling, as opposed to maximizing the number of respondents. I directed the pollsters to use a randomizing mechanism to select respondents in the hope that this would dampen any selection bias. Specifically, I instructed pollsters to choose every fifth exiting voter, with counting to begin only after an interview was completed. In addition, there was no reward to a pollster for the number of surveys completed.

The sample we obtained is neither large enough nor diverse enough to make broad generalizations about insurance reform voting behavior. Fortunately, making these generalizations was not our intention. Our purpose was to contact voters with varying amounts and kinds of information in an attempt to learn more about the effect of different types of information on voting behavior. Financial and physical limitations led me to restrict our polling universe to a ten-city area located in Los Angeles County and centered about the city of Pasadena. I chose polling places in areas across which there was a variance in the cost of auto insurance, the average number of accidents, and the cost of insurance claims.<sup>11</sup> I reasoned that these variables would be correlated with insurance rates, interest in the insurance reform issue, and the propensity to obtain information. Given these constraints and goals, I was able to obtain a sample that included enough variation for me to explore the relationships I have described. Appendix C shows how the 339 respondents compared to the voters of Los Angeles County and the State of California.

For the purposes of analysis, I feel justified in assuming that our respondents were also consumers of insurance, since nearly all of our respondents (98.7%) reported living in a household where a car was insured. Correspondingly, I also assume that (1) respondent preferences over electoral outcomes do not vary systematically with the propensity to possess certain types of information, (2) the ability to determine the relationship between the proposition and one's own well-being does vary with the propensity to possess certain types of information, and (3) all respondents prefer electoral outcomes that result in either a lower premium or a higher expected value of compensation in the event of an accident.<sup>12</sup>

Finally, each respondent was given a two-sided card at the beginning of the interview. One side of the card had a description of each insurance proposition exactly as it appeared on the official ballot (see Appendix A). We allowed respondents to see the summaries during the interview in order to minimize

the effects of variance in non-information-related recall ability. The other side of the card was used for asking potentially sensitive socioeconomic and insurance-rate-related questions. Instead of giving numbers for income, age, traffic violations, or insurance premiums, respondents selected from lettered categories that were listed on the card.

### Empirical Model

I shall now use a series of multivariate logit regressions to identify the determinants of respondent voting behavior. I present one logit for each of the five propositions. In each logit, the dependent variable is the binary variable *Vote*. *Vote* equals 1 for a *yes* vote on the proposition and 0 for a *no* vote. Thus, positive coefficients for the independent (explanatory) variables imply that having the characteristic presumed to generate the independent variable is associated with a greater propensity to cast a *yes* vote. The model used in the estimation is as follows:

$$\begin{aligned} \text{Vote} = & \alpha + \beta_1 \text{INSURANCE} + \beta_2 \text{LAWYER} + \beta_3 \text{NADER} \\ & + \beta_4 \text{KNOWLEDGE} + \beta_5 \text{INSURANCE} * \text{KNOWLEDGE} \\ & + \beta_6 \text{NADER} * \text{KNOWLEDGE} + \beta_7 \text{LAWYER} * \text{KNOWLEDGE} \\ & + \beta_{8, \dots, 11} [\text{other insurance-reform-related variables}]. \end{aligned}$$

The first three independent variables are measures of a respondent's possession of a certain type of information shortcut—knowledge of an information-provider's preferences. *INSURANCE* equals 1 if the respondent correctly identifies the insurance industry preference on the proposition and 0 otherwise (correct responses are *yes* on 101, 104, and 106, *no* on 100 and 103). Similarly, *LAWYER* equals 1 only if the respondent correctly identifies trial-lawyer preferences (*yes* on 100, *no* on 106); and *NADER* equals 1 only if the respondent can identify Ralph Nader's preferences (*yes* on 103). As defined, the values of *INSURANCE*, *LAWYER* and *NADER* are proposition-specific. For example, if a respondent could identify correctly the insurance industry's preference with respect to Propositions 100 and 104, then the value of *INSURANCE* would be 1 for the Proposition 100 estimation, 1 for the Proposition 104 estimation, and 0 for the other three estimations for that respondent. Also notice that each of these variables is used only in estimations that correspond to campaigns with which the relevant information provider was directly involved (see Table 1).

To determine the extent to which a respondents' knowledge of these endorsements enhanced their ability to vote as though they were relatively well informed, I must first show that "knowing the shortcut" affected voting behavior. To test for this effect, I use the survey responses to evaluate the null hypothesis, namely, "The information shortcut has no effect on voting behavior (e.g.,  $H_0 : \beta_4 = 0$ ). When the coefficient of *INSURANCE* (or *LAWYER* or *NADER*) is significantly different from zero, I can reject the null

hypothesis.<sup>13</sup> The ability to reject these hypotheses is a first indication that these information shortcuts affected insurance reform voting behavior.

After determining whether or not these shortcuts affected voting behavior, I then want to address the question that motivated this research project: To what extent did possession of a shortcut affect relatively uninformed respondents' ability to cast the same votes they would have cast if they had been well informed? The first step in addressing this question is to find a measure of a respondent's encyclopedic knowledge. The independent variable, KNOWLEDGE, is such a measure.

KNOWLEDGE is derived from responses to the four questions (listed in Appendix B) of the form, "Which of the propositions have *characteristic X*?" For each of the questions, there were five correct responses, since each proposition either had *characteristic X* or did not have it. KNOWLEDGE takes on its highest value, 20, only if respondents provided all correct responses, and its lowest value, 0, for respondents who provided no correct responses.<sup>14</sup> I assume that respondents for whom KNOWLEDGE was high were more likely to understand the personal consequences of a particular proposition's approval than were respondents for whom KNOWLEDGE was low.<sup>15</sup>

If respondents prefer lower premiums and higher-valued policies (as their status as consumers had led me to assume), then they should be likely to favor propositions that mandate lower insurance premiums (100, 103, 104) and increase competition in the provision of insurance (100, 103). For the same reasons, respondents should also be likely to oppose propositions that restrict a claimant's ability to collect from an insurance company (101, 104, 106). Since I expect that respondents with greater KNOWLEDGE would be relatively likely to understand which propositions were consistent with these interests, I predict that the sign of KNOWLEDGE will be positive for Propositions 100 and 103 and negative for Propositions 101 and 106. The sign of KNOWLEDGE for Proposition 104 depends on the respondents' willingness to trade off lower premiums for a decrease in the amount that they can expect to receive in the event of an accident. Since I possess neither a measure of, nor a compelling theory about, our respondents' willingness to make this trade, I cannot predict the sign of KNOWLEDGE for Proposition 104.

The second step in determining whether or not relatively uninformed voters could adapt to their lack of encyclopedic information is to determine the extent to which the shortcuts represented by the variables INSURANCE, LAWYER, and/or NADER were effective substitutes for encyclopedic knowledge. Since I expected each of these shortcuts to be effective, I expected that the coefficients of INSURANCE, LAWYER, and/or NADER would have the same sign as the coefficient of KNOWLEDGE. If these coefficients are also significantly different from zero, then I have supporting evidence for the assertion that these shortcuts were an effective substitute for the acquisition of encyclopedic information. Given my previous predic-

tion of the signs of KNOWLEDGE, I expect positive and significant signs on the three information-provider coefficients for Propositions 100 and 103 and negative and significant signs on these coefficients for Propositions 101 and 106.

The theories reviewed earlier suggest that knowledge of an information-provider's preferences should have a greater effect on the inferences and voting behavior of relatively uninformed respondents (who have more to learn) than it does on the behavior of relatively well informed respondents. Therefore, I include interactive explanatory variables that condition the effect of the INSURANCE, LAWYER, and NADER variables on the respondent's level of KNOWLEDGE. Because relatively uninformed respondents have more to learn, I expect the signs of the interactive term coefficients to be different from the signs of its components. In addition, I expect the magnitude of the interactive term coefficients to be smaller than the coefficients of INSURANCE, LAWYER, and NADER, since even relatively well informed persons may be able to form more accurate inferences about proposition content from their acquisition of knowledge about the information-providers' preferences.

In each of the estimations, I also include measures of potential within-group variance in preferences over insurance reform initiatives to account for the possibility that some respondents may care more about the insurance reform issue than others and, therefore, may feel even more strongly about obtaining lower insurance premiums or higher-valued policies. These variables are described in Appendix D.

## Results

Table 2 reports the logit results. Since I have predicted that many of the coefficients will have the same sign, I used a singular value decomposition to determine the existence of multicollinearity. This test (explained in Belsley, Kuh, and Welch 1980) failed to show significant levels of collinearity.<sup>16</sup>

I first discuss the effect of the variable INSURANCE. To determine whether or not knowledge of insurance industry preferences was an effective shortcut, we must first determine whether or not INSURANCE affected voting behavior. My analysis suggests that respondents who knew the insurance industry's preferences on a particular proposition exhibited very different voting behavior from that of respondents who did not have this information. Table 2 shows that in three of the five elections, knowledge of the insurance industry's preferences was a statistically significant determinant of voting behavior. In the other two cases, conventional levels of significance were narrowly missed. To better understand how meaningful the INSURANCE coefficients are, consider the fact that the existence of 20 categories in the KNOWLEDGE variable make it relatively unlikely that any effects I capture with variables like INSURANCE are actually the result of within-group variance in the KNOWLEDGE categories. This formulation allows me to make a meaningful distinction between the effect of

TABLE 2

## Effects of Information and Rate Determinants on Voting Behavior

INDEPENDENT VARIABLES	PROPOSITION				
	100	101	103	104	106
Constant	-1.50 (1.11)	-.90 (1.48)	-3.95 (1.26)	-.57 (1.16)	.29 (1.13)
KNOWLEDGE	.05 (.05)	-.09* (.05)	.22** (.08)	-.06 (.05)	-.07 (.05)
INSURANCE	2.57** (.91)	-1.89 (1.28)	3.35** (.90)	-1.15 (.82)	-2.28** (1.14)
INSURANCE * KNOWLEDGE	-.10 (.07)	.41 (1.19)	-.20** (.07)	.04 (.07)	.02 (.10)
LAWYER	-2.42** (1.04)	—	—	—	-1.25 (.86)
LAWYER * KNOWLEDGE	.15* (.08)	—	—	—	.04 (.07)
NADER	—	—	.86 (.85)	—	—
NADER * KNOWLEDGE	—	—	-.09 (.07)	—	—
Traffic violator	-.24 (.37)	.09 (.60)	.07 (.36)	.22 (.44)	.32 (.43)
Male-under-25 violator	.06 (.68)	-8.23 (42.25)	-.18 (.70)	-.02 (.72)	-.12 (.74)
CARTYPE	.22 (.22)	.01 (.34)	.21 (.22)	-.32 (.27)	-.16 (.24)
Income	.05 (.15)	.00 (.22)	.32** (.14)	.14 (.16)	.01 (.15)
N	219	220	236	221	220
Begin-log likelihood	-151.8	-152.49	-163.58	-153.19	-152.49
End-log likelihood	-135.55	-65.13	-139.25	-112.51	-119.64
% correctly predicted	67	90	72	78	75

Note: The dependent variable is binary, equals 1 if the respondent reported voting for the proposition in question, and equals 0 if the respondent reported voting against the proposition in question. The observations used in this analysis include only those respondents who voted on the proposition in question and answered all of the relevant knowledge and demographic questions. The coefficients presented are unstandardized and the numbers in parentheses are standard errors. The coefficients and standard errors were derived using the logit macro in the SST program (Dubin/Rivers Research, Pasadena, California).

\* $p \leq .10$ .

\*\* $p \leq .05$ .

encyclopedic information on voting behavior and the effect of the shortcuts. Therefore, given the strength of the test and the relatively small sample size, the levels of significance in all five cases are quite remarkable.<sup>17</sup>

Since logit coefficients do not provide much intuition about the absolute magnitude of the hypothesized relationships, I report relevant "first differences" in Table 3. A "first difference" is a straightforward translation of a logit coefficient into a percentage. The first differences I present are estimates of how much the probability of voting *yes* on the relevant proposition would change, given a particular change in a single independent variable (or set of independent variables) while holding the other independent variables constant at their mean values. For example, the first differences tell us that a respondent who was average in every way except that she provided no correct responses to the proposition

content questions had a .17 higher probability of voting for Proposition 100 than did a respondent who was similarly average except for her ability to provide 20 correct responses.

An examination of Table 3 once again shows the relatively large effect that knowing the insurance industry's position had on respondent voting behavior. The lower part of Table 3 shows that whether they knew the insurance industry's preferred electoral outcome or not was the largest single determinant of low-*KNOWLEDGE* respondents' voting behavior. Notice also that this effect decreased as *KNOWLEDGE* increased (because *INSURANCE* and *INSURANCE \* KNOWLEDGE* always had different signs, as expected). For example, knowing that the insurance industry was against Proposition 100 increased by 48 percentage points the probability that a respondent who could provide no correct answers to proposition knowledge questions voted for Proposition 100. The

TABLE 3									
First Differences: Change in the Probability of a "YES" Vote									
INDEPENDENT VARIABLE OR CONDITION	CATEGORY CHANGE		PROPOSITION						
	FROM	TO	100	101	103	104	106		
KNOWLEDGE	lowest (0)	Highest (20)	17	-7	19	-5	-22		
INSURANCE	Does not know Insurance preferences	Knows	34	-7	26	-11	-34		
LAWYER	Does not know Lawyer preferences	Knows	-17	—	—	—	-15		
NADER	Does not know Nader preferences	Knows	—	—	-3	—	—		
Household Income	\$22,500-\$40,000	over \$75,000	0	0	14	3	2		
INSURANCE, if									
KNOWLEDGE = 0	Does not know Insurance preferences	Knows	48	-18	68	-24	-46		
KNOWLEDGE = 10	Does not know Insurance preferences	Knows	36	-8	32	-14	-35		
KNOWLEDGE = 15	Does not know Insurance preferences	Knows	26	-5	8	-8	-30		
LAWYER, if									
KNOWLEDGE = 0	Does not know Lawyer preferences	Knows	-40	—	—	—	-28		
KNOWLEDGE = 10	Does not know Lawyer preferences	Knows	-21	—	—	—	-16		
KNOWLEDGE = 15	Does not know Lawyer preferences	Knows	-5	—	—	—	-10		

*Note:* A concise explanation of when and how this and related methods can make limited-dependent-variable-model coefficients more intuitive is provided in King 1989 chap. 4. The first differences of KNOWLEDGE, INSURANCE, NADER, and LAWYER and their interactive terms are computed using the appropriate conditional means. For instance, the change in the dependent variable due to knowledge of the insurance industry's preferences is determined by increasing INSURANCE from 0 to 1 and INSURANCE \* KNOWLEDGE from 0 to the mean of KNOWLEDGE.

magnitude of this effect was roughly double that of a respondent who could provide correct responses on 15 of 20 occasions.

I now use the data to evaluate the extent to which use of the INFORMATION shortcut allowed low-KNOWLEDGE respondents to emulate the behavior of high-KNOWLEDGE respondents. Tables 2 and 3 provide evidence for the prediction that relatively uninformed respondents used their knowledge of the insurance industry's preferences to emulate the behavior of relatively well informed respondents. In every logit, the sign of INSURANCE is the same as the sign of KNOWLEDGE. This implies that whether respondents used encyclopedic information or their knowledge of insurance industry preferences, they formed the same types of inferences about the relationship between a proposition and their own well-being.

To show the effect of the INSURANCE shortcut in a more concrete manner, in Table 4 I divide the sample into three mutually exclusive categories based on the respondents' values of KNOWLEDGE and INSURANCE. Presented in Table 4, column 1, is the behavior of respondents who had relatively high KNOWLEDGE and who knew the insurance industry's preferences. Table 4, column 2, contains the behavior of respondents who had relatively low KNOWLEDGE, but (like the respondents in column 1) knew the insurance industry's preference. In column 3 are respondents who also had relatively low KNOWLEDGE but (unlike the second group) did not know the insurance industry's preferences. A comparison of the behavior of these three groups makes my main point in a dramatic fashion. Voters in the first two columns look very much like each other, while voters in column 3 look very different from voters in the first two columns. The relationship between these three groups strongly supports the assertion that knowledge of the insurance industry's preferences provided an effective shortcut for voters whose (encyclopedic) KNOWLEDGE of proposition content was low.

TABLE 4			
Percent Voting Yes by Information Category			
PROPOSITION	INFORMATION CATEGORY		
	HIGH KNOWLEDGE (10-20) WITH KNOWLEDGE OF INSURANCE INDUSTRY PREFERENCES	LOW KNOWLEDGE (0-9) WITH KNOWLEDGE OF INSURANCE INDUSTRY PREFERENCES	LOW KNOWLEDGE (0-9) WITHOUT KNOWLEDGE OF INSURANCE INDUSTRY PREFERENCES
100	53	53	27
101	8	5	15
103	72.5	73	26
104	17	17	34
106	11	12.5	45

*Note:* Cell value is percent yes. Percentages are rounded to the nearest percentage point except where the true value is exactly half a percentage point.

It is also interesting to note that the coefficients of INSURANCE (and KNOWLEDGE) are negative for the three propositions that insurance industry favored (101, 104, and 106) and positive for the two propositions that the insurance industry opposed (100 and 103). In all cases, the signs of KNOWLEDGE and INSURANCE are as predicted. The similarity of the two sets of coefficients supports the notion that relatively informed respondents concluded—and relatively uninformed respondents perceived—that the insurance industry's preferences over electoral outcomes were contrary to their own.<sup>18</sup> This finding reflects well on the insurance industry's attempt to hide its identity, in that respondents who were able to identify its preferences tended to use them as a negative reference. Given the closeness of the vote on Proposition 103 (only 11 out of every 1,000 voters would have had to cast a different vote to change the outcome), we can conjecture that a slightly more successful attempt to detach the insurance industry's identity from their endorsements may have lead to a different electoral outcome.

For voting behavior on Proposition 100, the effect of the variable LAWYER is similar to the effect of INSURANCE but less dramatic. The coefficient for LAWYER is significant at the .05 level in the Proposition 100 logit. The negative, significant, and correctly predicted sign of LAWYER in the Proposition 100 logit is consistent with the assertion that respondents perceived the trial lawyers' preferences to be different from their own. However, the magnitude of this coefficient suggests that this shortcut did not affect voter inferences as much knowledge of the insurance industry's preferences did. In contrast, the negative coefficient of LAWYER in the Proposition 106 logit is of a different sign from that of either KNOWLEDGE or INSURANCE and thus contrary to my prediction. These findings suggest that knowledge of the LAWYER endorsement was not as effective a shortcut as was knowledge of the INSURANCE endorsement. Unfortunately, I do not have sufficient data to determine whether this signal was less effective because the lawyers waged a smaller campaign than the insurance industry (less costly and observable effort) or because respondents were relatively uncertain about lawyer views on the value of an insurance policy (lack of reputation).<sup>19</sup>

Finally, the relatively small coefficient on the NADER variable may surprise those who recall Nader's role in this election. The interpretation that Ralph Nader had no effect on voting behavior, however, would be going a bit too far. Unlike Voter Revolt (the sponsors of Proposition 103), Ralph Nader had a relatively well known past. I believe that Nader's past and his widely covered and frequent speeches about the insurance industry's big money campaign against 103 were Voter Revolt's greatest asset. With Nader, Voter Revolt was able to prevent the insurance industry from monopolizing the transmission of widely accessible (i.e., mass-media) information. Nader's presence certainly made it easier for some voters to locate the insurance industry's preferences over out-

comes. Without someone like Nader, it is unlikely that Voter Revolt could have either received the media attention it did or acquired credibility sufficient to affect voter opinions for those occasions when it did have access to the media.

## CONCLUSION

I used a survey instrument to identify the effect of different types of information on voting behavior in the complex insurance reform initiatives of 1988. I showed that respondents who possessed relatively low levels of factual (or encyclopedic) knowledge about the initiatives used their knowledge of insurance industry preferences to emulate the behavior of those respondents who had relatively high levels of factual knowledge. If we believe that well-informed voters make the best possible decisions, then the fact that relatively uninformed voters can emulate them suggests that the availability of certain types of information cues allows voters to use their limited resources efficiently while influencing electoral outcomes in ways that they would have if they had taken the time and effort necessary to acquire encyclopedic information. In closing, I believe that the policy implications of this analysis are quite straightforward: while scholars and pundits propose that we educate the public about politics in order to lessen the impact of uninformed votes on the responsiveness of democratic decision-making institutions, a deeper understanding of how people adapt to the uncertainty that characterizes many of their important decisions suggests that directing our efforts into the provision of credible and widely accessible "signals" may be a more effective and cost-efficient way to ensure the responsiveness of electoral outcomes to the electorate's preferences.

## APPENDIX A: SUMMARIES OF INSURANCE REFORM PROPOSITIONS

Each of the following summaries are presented here exactly as they appeared on the ballot, in the California Ballot Pamphlet, and on the card given to each respondent during the interview.

100 INSURANCE RATES, REGULATION, INITIATIVE. Reduces good driver rates. Requires automobile, other property/casualty, health insurance rate approval. Adopts anti-price-fixing, antidiscrimination laws.

101 AUTOMOBILE ACCIDENT CLAIMS AND INSURANCE RATES. INITIATIVE. Reduces automobile insurance rates, limits compensation for non-economic losses for four years.

103 INSURANCE RATES, REGULATION, COMMISSIONER, INITIATIVE. Reduces auto, other property/casualty rates. Requires elected Insurance Commissioner's approval of rates. Prohibits price-fixing, discrimination.

104 AUTOMOBILE AND OTHER INSURANCE. INITIATIVE. Establishes no-fault insurance for automobile accidents. Reduces rates for two years. Restricts future regulation.

TABLE B-1						
Information Questions						
QUESTIONS	RESPONSES					
	100	101	103	104	106	DK
About information provider preferences						
Which of the propositions do you believe were supported by the <i>insurance industry</i> ?	65	129*	39	171*	93*	61
Which of the propositions do you believe were supported by the <i>trial lawyers</i> ?	78*	35	26	27	58	119
Which of the propositions do you believe were supported by <i>Ralph Nader</i> ?	15	16	188*	15	10	71
About Proposition Content						
Which proposition(s) establish a <i>no-fault</i> system of auto insurance?	21	19	17	191*	8	54
Which proposition(s) mandate giving <i>discounts</i> in insurance premiums to <i>good drivers</i> ?	117*	35	65*	16	4	52
Which proposition(s) <i>limit attorney contingency fees</i> ?	7	13*	14	22*	193*	59
Which proposition(s) mandate insurance rates which are <i>not based on where you live</i> ?	56	19	88*	16	7	140

106 ATTORNEY FEES LIMIT FOR TORT CLAIMS. INITIATIVE. Limits amount of contingency fees which an attorney may collect in tort cases.

### APPENDIX B: INFORMATION ABOUT INFORMATION MEASURES

The starred cell entries in Table B-1 identify correct responses. Respondents were permitted to give multiple responses to each of the following questions. Notice that for some questions there were multiple correct responses.

### APPENDIX C: ELECTION AND POLL RESULTS

The cell entries in Table C-1 are percentages voting in favor of the proposition. The source of state and county level election results is the Statement of Vote provided by the California Secretary of State, Sacramento.

### APPENDIX D: OTHER INDEPENDENT VARIABLES

VIOLATOR is a binary variable that equals 1 if the respondent admitted to a traffic violation in the last three years. Since violators face higher premiums than nonviolators, the out-

come of insurance reform elections are likely to have a greater effect on violators than nonviolators. Mv25 equals 1 if the respondent was a male "violation" under 25 years of age, and 0 otherwise. As a member of this group at the time I designed the exit poll, I knew that male respondents who were under age 25 would pay higher premiums than other violators. Therefore, the outcome of insurance reform elections should have had a greater effect on these respondents than on other violators.

The variable CARTYPE is a discrete variable with values 1–5. As CARTYPE increases, so does the retail price of the particular model. Since insurance rates are correlated with the price of the respondent's car, I expect that (all else held constant) respondents having higher-priced cars will be more affected by the electoral outcome than will respondents with lower-priced cars. INCOME is a discrete variable that is valued 1–5 and increases with household income. Cutoff levels for the discrete categories were \$15,000, \$22,500, \$40,000, and \$75,000. Insurance reform should have a relatively large effect on persons with higher incomes, who tend to purchase more insurance.

As interesting as these concepts are, the variables described in this appendix do not produce significant coefficients. In fact, only two out of these variables' 20 coefficients have coefficients that are larger than their standard errors. While the inclusion (or exclusion) of these variables does not alter the results I have presented, I include them because I expect many readers to question the viability of a statistical model that tests for the effects of different types of information by including only independent variables that are measures of information.

TABLE C-1				
Comparison to County and State Vote				
PROPOSITION	SPONSOR	STATE	LA COUNTY	SURVEY (N = 339)
100 (good driver initiative)	Trial lawyers	40.9	50.4	44.3
101 (Polanco initiative)	Insurance industry	13.3	15.5	10.8
103 (Voter Revolt/Nader initiative)	Consumer activists	51.1	62.6	65.2
104 (no-fault initiative)	Insurance industry	25.4	22.3	21.5
106 (contingency fee initiative)	Insurance industry	46.9	43.6	29.4

## Notes

I wish to thank Richard McKelvey and Peter Ordeshook for guidance and support through all stages of this project. Helpful comments and suggestions were offered by Elisabeth Gerber, Mat McCubbins, Chris Achen, Henry Brady, Bruce Cain, Gary Cox, Jonathan Katz, Rod Kiewiet, Sharyn O'Halloran, Sam Popkin, Larry Rothenberg, Roger Solin, Jim Stimson, and numerous seminar participants. I am in the debt of Phyllis Pugh, Joy Hansen, Shawn Kantor, Ramy Farid, Susan Davis, and 30 enthusiastic pollsters whose participation and/or cooperation were necessary conditions for the successful execution of the poll. Financial support for this project was supplied by the Division of Humanities and Social Sciences at the California Institute of Technology.

1. For a review of these arguments as they apply to direct legislation elections, see Cronin 1989.

2. For examples of this type of argument as they apply to direct legislation, see, e.g., Kuklinski, Metlay, and May 1982 and Lupia 1992. Popkin 1991 provides a more general review of the consequences of voter information problems.

3. For example, the most comprehensive analysis of proposition complexity that I am aware of (Magleby 1984) shows that the average number of words in the propositions that appeared on the California ballot during the 1970s was 8,121. He goes on to argue that over 17 years of formal education would be required to comprehend the text of the average proposition.

4. Theoretical analyses are developed in Lupia 1992, 1993a. For experimental tests of the theory, see Lupia 1993b.

5. I have reviewed the votes of insurance-related bills in the three years preceding the election and have found two systematic relationships: insurance-industry-supported tort reform bills die in the Assembly Judiciary Committee, while lawyer-supported insurance reform bills die in either the Assembly Finance and Insurance Committee or the Ways and Means Committee. Republicans (the minority party in both houses of the California legislature) tended to support tort reform measures. Democrats on these committees tended to support bills more favorable to the trial lawyers, yet each of these measures tended to fall one or two votes short of passage in committee through the defection of two to four Democrats.

6. In October 1987, an insurance industry spokesperson in the course of a conversation about a consumer-activist-supported insurance reform ballot proposition estimated, "We could spend \$10 million and still not be assured of beating it" (from Reich 1987b). One wonders how the negotiations might have changed had the insurance industry and trial lawyers anticipated that campaign expenditures would total in excess of \$80 million and that a consumer-sponsored initiative would be the only resulting change to the status quo.

7. In addition to nonpolitical concerns, there were elections for the office of president and U.S. Senator; seats in Congress, the state senate, and the state assembly; local ballot measures; and 22 other statewide measures on the same ballot.

8. Some prominent figures of both parties took public stands on the insurance reform issue, but these endorsements provide no evidence of a clear partisan consensus. See Campbell et al. 1960 for a definitive statement of the effect of partisan cues.

9. For well-known examples of this type of research, see Crawford and Sobel 1982; Milgrom and Roberts 1986; Spence 1973.

10. Related research on "cue taking" merits attention. Kuklinski, Metlay, and May (1982) showed that voters in nuclear power referenda conditioned their strategies on the stated positions of reference groups. Feldman and Conover (1983) identified emotional bases for certain types of voter responses to new political information. Popkin (1991) argued that certain images allow voters to simplify their voting calculus. Page and Shapiro (1992) examine responses to over

one thousand survey questions to support their argument that electorates have somewhat parallel adaptive capabilities.

11. The sources used to make these evaluations included California, Department of Insurance 1988a; California, Department of Insurance, Consumer Affairs Division 1987; and California, Department of Insurance, Statistical Services Unit 1988a-c.

12. This is not to say that respondents knew, initially, which propositions would achieve this goal. If they had, the campaign would have been meaningless. In addition, two respondents in the sample admitted to working for insurance agencies. Both voted in a manner consistent with a "job security" hypothesis. Otherwise, there was nothing in the data suggesting that this voter preference hypothesis did not represent respondent preferences.

13. As it is important not only to provide support for one causal hypothesis but also to reject other plausible causal hypotheses, I reran each of the estimations in Table 2 four times, replacing INSURANCE with an equivalently defined binary variable that measured a voter's ability to answer one of the four questions about proposition content. For instance, the first of the four questions that links a proposition to an issue is "Which proposition(s) establish a *no-fault* system of auto insurance?" (see Appendix B) Responses to this question were used to produce five binary variables that equaled 1 when the respondent correctly identified the relationship between the statement and the proposition and 0 otherwise. In 19 of the 20 cases, the replacement variable performed very badly. The T-statistic for the replacement variables were usually below 1 and often below .5. In addition, the difference between the initial likelihood function and the final likelihood function was generally much smaller than the equivalent estimations using INSURANCE. The one exception to this rule was the respondent's ability to correctly identify Proposition 104 as setting up a no-fault system of insurance. As Proposition 104 was referred to as the no-fault initiative, the effect of this particular variable in the 104 logit is hardly surprising. I reran the 104 logit once more, using INSURANCE, the no-fault replacement variable, and the corresponding interaction terms to find that the coefficient of neither variable was affected by the simultaneous inclusion of the other.

I also reran each estimation adding the respondent's reported partisanship. Implemented a number of theoretically consistent ways, the effect of party never attained conventional levels of significance and was omitted to preserve degrees of freedom. This lack of effect was especially true of PARTY'S interaction with NADER, where the use of a party term seems most appropriate. In this case, the standard error was 30 times greater than the coefficient.

Since some readers may still be concerned about the absence of a party term in this analysis, an additional history of these elections may be insightful. Recall that insurance reform was a very touchy issue, one that the revealed behavior of California legislators suggests that few were willing to take public positions on. Despite repeated attempts over the five years preceding the November 1988 election, neither house of the California legislature could get an insurance reform bill out of committee and onto the floor for a vote. This inability to get out of committee is especially surprising, given the relatively strong Democratic control of both chambers. If Democrats had reached some type of consensus on how to reform the insurance industry, it would have been easy for them to use their control of the legislature to produce a bill for the governor to sign. In this case, "Democrat" might have served as an effective cue. However, the state legislature never achieved this consensus.

Likewise, voters who followed the actions of Republican governor George Deukmejian may have been confused about either his or his party's stand on insurance reform. Prior to the passage of Proposition 103, the insurance commissioner was appointed by the governor. Deukmejian's appointee, Roxani Gillespie, was previously a lobbyist for a large insurance company. This action might lead voters to believe that the Republican position was proinsurance. However, Deukmejian also encouraged voters to vote against all five proposi-

tions, including the three drafted by the insurance industry. That the governor and the legislature never once publicly locked horns on this issue, that the issue had to be decided using the initiative, and that during the campaign season the few partisans who did take stands differed in their stances is suggestive of the lack of observable partisan consensus on the insurance reform issue. Therefore, we should not be surprised that party was not an effective cue for relatively uninformed voters.

14. The distribution of respondents achieving each knowledge score (from 0 to 20) was 14, 1, 3, 11, 4, 7, 2, 25, 15, 7, 26, 16, 43, 26, 10, 35, 16, 33, 7, 2, and 0.

15. The arguments underlying this type of assumption are evaluated in greater detail by Bartels (1990) and Gerber and Lupia (1993).

16. The values of the singular value decomposition for each of the logits, ordered by proposition number, from lowest to highest, were 23.79, 20.48, 30.59, 22.14, and 23.37. Values of over 100 indicate collinearity, values from 30 to 100 indicate mild collinearity, and values under 10 indicate no collinearity. While the Proposition 103 logit is just over the threshold for mild collinearity, I reran it, collapsing several KNOWLEDGE categories and achieved very similar results to those presented, with less collinearity.

17. In case of the 101 and 104 logits, it is worth noting that (1) the dependent variable had relatively little variance and (2) this variable did achieve conventional levels of significance when I used weaker tests of the effect of INSURANCE. Of the respondents who voted on the relevant proposition and answered all of the information and demographic questions, only 22 of 220 voted *yes* on 101 and only 49 of 221 voted *yes* on 104. Thus, by attempting to make inferences about voting behavior that rely heavily on the observable differences between these *yes*-voters and the rest of the sample, we are asking a lot of the data. When we combine the limited variance of the dependent with our relatively small sample size, the fact that our coefficients even come close to conventional levels of significance (both are about one-and-a-half times the size of the standard error) is encouraging. Another part of the failure to achieve conventional levels of significance in the two extreme cases may be that I set up an empirical test in which rejection of the null hypothesis was quite difficult. The advantage of the difficult test is that I am more confident that the magnitude of the INSURANCE coefficient represents the effect of that particular shortcut. The drawback is that if processes that generate INSURANCE and KNOWLEDGE are correlated, this makes the likelihood of a large T-statistic relatively small. I reran the estimations replacing the KNOWLEDGE variable with limited measures of encyclopedic information that had two or 3 categories (*low knowledge, somewhat knowledgeable, high knowledge*), instead of 20. I found that all of the INSURANCE coefficients achieve conventional levels of significance in these reestimations.

18. Notice that for me to conclude that INSURANCE served as an effective shortcut, it matters not whether low-*KNOWLEDGE* respondents knew that they were economizing on their information costs by relying on the insurance industry preference cue or whether they just did not like insurance companies. For my conclusion to be valid, it is sufficient that use of the shortcut allowed relatively uninformed respondents to emulate the behavior of relatively well informed respondents who were otherwise like themselves (my estimate of how these respondents would have voted if better informed).

19. The insurance industry was able to stifle early trial-lawyer attempts to advance their preferences by running a series of advertisements pointing out the lawyer sponsorship of certain advertisements. This particular series of advertisements proved so effective that not only were lawyer-sponsored campaign advertisements removed from the airwaves in the final weeks of the campaign, but so were noncampaign related advertisements for trial-lawyer services, at the request of CTIA (Reich 1988b).

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