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# What Citizens Know Depends on How You Ask Them:

# Political Knowledge and Political Learning Skills

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**Abstract**

Surveys provide widely-cited measures of political knowledge. Do unusual aspects of survey interviews reduce their relevance? To address this question, we embedded a set of experiments in a representative survey of over 1200 Americans. A control group answered political knowledge questions in a typical survey context. Respondents in treatment groups received the same questions in different contexts. One group received a monetary incentive for answering questions correctly. Others were given more time to answer the questions. The treatments increase the number of correct answers by 11-24 percent.

Our findings imply that conventional knowledge measures confound respondents' recall of political information and their motivation to engage the survey question. The measures also provide unreliable assessments of respondents' abilities to access information that they have stored in places other than their immediately available memories. As a result, existing knowledge measures likely underestimate peoples' capacities for informed decision making.

A basic premise of democratic governance is that citizens use information about politics and policy to hold elected officials accountable. A related premise is that how such information is distributed affects who has political power. For these reasons, scholars devote considerable energy to the study of what citizens know about politics. The most widely-used measures of political knowledge come from responses to fact-based questions in political surveys (e.g., "How long is the term of office for a U.S. Senator?"). These data yield a focal conclusion: many citizens can't answer the questions (e.g., Converse 1964; Delli Carpini and Keeter 1996; Kinder and Sears 1985).

The surveys from which such conclusions are drawn have two noteworthy features. First, *they do not offer respondents an explicit incentive to consider a question carefully or answer it thoughtfully*. Second, when political knowledge questions appear during a survey, *they catch respondents by surprise*. While some firms give respondents advance warning (e.g., a letter in the mail) that a survey is coming, many others give no such notice. Of those who offer notice, few, if any, provide details about the questions they will ask.

In this paper, we examine the implications of these administrative features for the validity and relevance of widely-cited claims about political knowledge. Our research addresses this question through the use of new experimental designs. The experiments, which are embedded in a nationally representative survey, show that standard survey measures of political knowledge do not reflect people's capacity for informed decision-making as well as they could.

Our first experiment is designed to determine whether performance on political knowledge questions improves when we encourage survey respondents to try harder. It allows us to evaluate an important null hypothesis.

**Null Hypothesis #1:** Knowledge questions in conventional mass opinion surveys accurately assess whether or not respondents hold the relevant political facts in memory. Providing an incentive for correctly answering knowledge questions will not affect the likelihood of offering a correct answer.

In the absence of an incentive to consider survey questions carefully, the frequency of incorrect answers to political knowledge questions may reflect a lack of effort by the survey respondent rather than a true lack of knowledge. Survey respondents may perform poorly on political knowledge tests not because they are incapable of answering the questions, but because they are unmotivated to perform well. Two otherwise identical respondents may not be equally likely to answer a knowledge question correctly if one is more motivated than the other to consider the question and search her memory for an answer. Even though both respondents have acquired and stored the same political information, only one may answer the knowledge question correctly. Differential motivation during the survey interview can distort conclusions about political knowledge.

We evaluate this first null hypothesis by offering (a randomly selected) half of our respondents a small monetary reward ($1) every time they answer a politically-relevant knowledge question correctly. The control group answers the same questions under standard survey conditions – no payment for a correct answer.

Our second experiment examines the effect of giving respondents extra time to answer knowledge questions. It allows us to evaluate an important null hypothesis.

**Null Hypothesis #2:** The ability to answer factual survey questions in conventional opinion surveys correctly (political knowledge) and the ability to figure out correct answers to factual questions (political learning skills) are not sufficiently different to

require separate measurement. Even if giving respondents extra time increases the number of correct answers, the change will be uninteresting: it will be constant across respondents or it will simply amplify differences between strong and weak performers.

The conventional practice in surveys is not to inform respondents in advance that they will be asked political knowledge questions. While it may seem reasonable to draw conclusions about a person's ability from their immediate responses to unexpected survey questions, in other cases this kind of inference can backfire. To see how, consider a simple example: "Professor, what percentage of the vote did John Kerry receive in Kansas in the 2004 general election?" Such questions from an eager undergraduate can strike fear into the heart of many lecturers. Few political scientists can answer such questions when they are asked without warning. Although many scholars know where and how to find the answers, and would do so quickly if given an opportunity, the normal pace of a classroom lecture usually precludes halting the interaction to consult trusted references. In such cases, mumbling something about "a book on my shelf" or "a website that has the answer" is the best one can do from the lectern. While most people would consider it unfair for students to base broad judgments of a professor's competence on his or her immediate responses in such circumstances, common evaluations of citizens' capabilities rest on just this kind of inference.

This example illustrates a problem with survey-based assessments of political capabilities. The professor's failure to recall Kerry's vote share in Kansas does indeed indicate a lack of a particular kind of knowledge. What common sense rebels against is the proposition that the lack of such knowledge should determine students' evaluations of the professor's teaching skills.

Of course, if the professor were unable to figure out Kerry's vote share despite trying to do so, low evaluations of his or her teaching (and research) skills would be more justified. But there

are circumstances where the ability to find necessary information is at least as important a performance criterion as is the ability to recall such facts instantaneously. Knowing where to find information expands the foundation of knowledge upon which people base many decisions. For example, many people expand their capabilities by computers to organize large amounts of information in ways that permit quick retrieval that they can access when they need it.

Political surveys offer people no opportunity to draw on this broader foundation of political knowledge, even though citizens can draw on more than their own memory when they make political decisions. Our second experiment documents whether this omission alters conclusions about citizens' capacity for informed decision-making.

We evaluate the second null hypothesis by giving one (randomly selected) half of the respondents only one minute to answer each question, whereas the other half can take 24 hours to respond. This variation transforms a knowledge quiz into a knowledge hunt. Conceptually, it transforms a measure of political knowledge into a measure of the ability to learn the correct answers to political knowledge questions when given an opportunity to do so – a concept that we call political learning skills.

Our experimental evidence is sufficient to reject both null hypotheses. On average, offering a small monetary incentive led to an 11 percent increase in the number of questions answered correctly. Extra time has an even larger effect. Simply offering people a little money for responding correctly or extra time to find the answers does not transform them into political encyclopedias, but it does affect how they answer knowledge questions. A substantial share of those who appear to be "know nothings" according to existing research on political knowledge, can answer questions correctly when given a small incentive or the opportunity to do so.

Our main finding is that common attributes of survey interviews contribute significantly to claims of citizens' low political capabilities. As a result, existing political knowledge measures likely underestimate the ability of the American public to make informed decisions. We also find important differences between political knowledge and political learning skills. The people who can instantly recall politically relevant facts on a survey are not the same as those who can find correct answers when given an opportunity to do so. Since many political decisions (such as elections) allow citizens to seek information while making a choice, political learning skills will affect decision quality. When political learning skills are high, poor performance on an unannounced and rushed survey-based political knowledge quizzes will be less indicative of low political ability. Taken as a whole, our results show that analysts should be more cautious when using past political knowledge research as the basis for drawing broad conclusions about citizens' political inabilities.

The paper continues as follows. In the next section, we motivate and explain the experimental design in greater detail. Then, we describe the survey in which the experiments were included. Next, we present the results of our experiments. In the conclusion, we discuss the value of measuring political learning skills in surveys and spell out further implications for how to interpret existing political knowledge data more thoughtfully.

## Political Knowledge and Memory

To measure political knowledge in surveys, researchers typically use a set of factual questions about politics. According to previous research, motivation is a factor that determines how much political information people acquire (Delli Carpini and Keeter 1996; Luskin 1987). But what we know about survey practice and the workings of memory suggests that motivation of a different kind is also at work when survey respondents answer knowledge questions.

The pace of a survey interview is established in part by conversational norms (Schwarz 1996, ch.5) and in part by the incentives of the interviewer (Blair and Burton 1987; Krosnick and Alwin 1987). Interviewers often want to complete numerous interviews within a short period of time. Respondents often want to finish the survey quickly. Such dynamics can lead interviewers to move quickly from one question to the next and respondents to satisfice—to offer answers without thinking hard about them.

When asked to quickly recall a fact, respondents will first draw upon the kind of memory known as "declarative memory" (see, e.g., National Research Council 1994). Existing approaches presume that simply asking a political knowledge question will induce respondents to take sufficient time and exert sufficient effort to retrieve all relevant facts. Declarative memory, however, does not work this way.

In declarative memory, there is a correspondence between the amount of effort one devotes to recalling facts and the range of facts recalled (Kandel, Schwartz, and Jessell 1995, 656-664; National Research Council 1994, 28-29). With minimal effort, a relatively small set of facts from declarative memory will emerge. With greater effort, more facts can be recalled. Therefore, respondents may fail to answer a question correctly not because they lacked the motivation to acquire the relevant information, but because they are not sufficiently motivated to think about the survey question. To the extent that existing political knowledge measures are based on a limited draw from declarative memory they are likely to be biased downward.

An incentive for greater respondent effort may reduce this bias and encourage respondents to base their answers on a more extensive search of declarative memory. One kind of incentive, commonly used in experimental economics, is a monetary incentive:

> "The presence and amount of financial incentive does seem to affect average performance in many tasks, particularly…where increased effort improves performance.

> Prototypical tasks of this sort are memory or recall tasks (in which paying attention helps)…which are so mundane that monetary reward induces persistent diligence when intrinsic motivation wanes." (Camerer and Hogarth 1999, 8).

In our first experiment, we use a monetary incentive to motivate more thorough memory searches. Regardless of the precise nature or magnitude of the incentive, rejecting the null hypothesis would demonstrate that typical survey procedures do not elicit all that respondents know about the questions we ask them. In that case, we could conclude that people acquire and store more political information than previous research leads us to believe.

A second attribute of memory is also relevant. Cognitive psychologists distinguish fact-based declarative memory from rule-based "procedural memory."[1] Knowing where and how to find things, such as Kerry's vote share in Kansas, is an important form of procedural memory. Procedural memory "accumulates slowly through repetition over many trials, is expressed primarily by improved performance, and cannot ordinarily be expressed in words" (Kandel, Schwartz, and Jessell 1995, 658). To figure out which candidate they prefer or how they feel about a new policy proposal, many people draw on procedural memories of how to gather information that might help their decision.

To assess how much their procedural memory can help people in making informed decisions, we have to modify our measurement approach. Unlike declarative memory, procedural memory cannot be observed directly. But we can observe its consequences. Whereas the pace and incentives of many surveys inhibit respondents from using such procedural memories, we design an experiment where a randomly chosen half of our respondents can use their procedural

[1] Many scholars use the terms "declarative" and "procedural" to distinguish the two kinds of memory. Kandel et al. (1995, 656) refer to declarative memory as "explicit" memory and to procedural memory as "implicit memory."

memory when answering political knowledge questions. Instead of asking them for answers right away (as is standard practice in political surveys), we give them 24 hours to respond to our knowledge questions.

## Research Design

Typical surveys do not provide a context conducive to thorough searches of declarative memory and the application of procedural memory. To clarify the extent to which the survey context undermines attempts to measure citizens' potential for informed decision-making, we experimentally manipulate two elements of the survey interview, the incentive for answering questions correctly and the time respondents have to complete knowledge questions. To accomplish this manipulation efficiently, we randomly assigned respondents to one of four experimental groups within a single representative survey (whose attributes we describe below). Each respondent was equally likely to be placed in one of the four groups depicted in Figure 1.

(Figure 1 goes here)

We offered one randomly selected half of our sample a monetary reward, one dollar, for each correct answer. We chose $1 per question (which amounts to a maximum possible payoff of $14) because we assumed that the amount would be non-trivial for many respondents and because this amount allowed us to stay within our budget while generating a sufficient number of cases per cell for rigorous statistical evaluations (see Bassi, et. al. 2006 for a recent review of the consequences of incentive payments for respondent effort in political science experiments.)

In our Internet-based survey, which respondents completed using a computer or a WebTV unit, the knowledge questions appeared after an initial battery that solicited the respondent's party identification, interest in politics, and previous turnout. After this battery, all respondents saw a common introduction:

> In the next part of this study, you will be asked 14 questions about politics, public policy, and economics. Many people don't know the answers to these questions, but it is helpful for us if you answer, even if you're not sure what the correct answer is. We encourage you to take a guess on every question. At the end of this study, you will see a summary of how many questions you answered correctly.

Respondents in the pay conditions then received the following instructions:

> We will pay you for answering questions correctly. You will earn 1,000 bonus points ($1) for every correct answer you give. So, if you answer 3 of the 14 questions correctly, you will earn 3,000 bonus points ($3). If you answer 7 of the 14 questions correctly, you will earn 7,000 bonus points ($7). The more questions you answer correctly, the more you will earn.[2]

The second experimental factor is time. To measure respondents' political learning skills, we gave one randomly selected half of our sample 24 hours to answer a total of 14 knowledge questions. The other half had only one minute to answer each knowledge question. Respondents in the "one minute" condition were informed that

> You will have 1 minute to answer each question. After 1 minute, you will be automatically forwarded to the next question. If you finish answering a question before 1 minute is up, you may proceed to the next question by clicking on the 'Next Question' button.

Each of the knowledge questions was programmed to be on screen for up to one minute. If respondents answered the question within that period or if one minute had expired, the screen

[2] Respondents received credit for correct answers in the form of "bonus points." The firm that conducted our study, Knowledge Networks, sends their panelists checks for $25 when they reach 25,000 points (which they can also earn in other surveys they take.) For all practical purposes, we consider our incentives direct cash rewards. The instructions in the pay condition mentioned the bonus points as well as their dollar equivalents. Respondents in the pay conditions were reminded on every screen with a knowledge question that a correct answer would earn them a specific monetary reward.

changed to show the next question. In the "one minute" condition, respondents could not go back to a previous knowledge question after they had moved past it in the interview.

Respondents in the "24 hour" condition were informed that

> You will have 24 hours to answer these questions from the time you see the first question. Once the 24 hours are up or whenever you decide that you are done, you will be forwarded to the next section and will not be able to return to the knowledge questions. However, before you reach the next section, you may go back to previous knowledge questions by clicking the 'back' button.

Starting from the moment at which respondents saw the first knowledge question, they had 24 hours to complete the knowledge series. During this period, they could go back and forth between knowledge questions (but not to the initial questions about interest, turnout, and partisanship), change their answers, and interrupt and resume the survey as often as they liked. When respondents reached the end of the knowledge sequence, a screen informed them that they could modify their answers until their 24 hours were up or move to next part of the survey (at which point they were "locked out" of this part of the survey and could not return to the knowledge questions.)[3]

## The Knowledge Questions

The dependent variable in our study comes from answers to 14 knowledge questions. Some of these questions are open-ended, others are multiple choice. To facilitate payment for open-ended questions – all of which asked for a number between and including 0 and 100 -- in the relevant experimental conditions, we specified in advance a range of answers (e.g., "within X percentage points of the true percentage") that would earn compensation. Respondents were told

[3] We conducted a manipulation check to determine if respondents spent more time answering questions in the "Pay" and "24 hour" conditions. They did. Increased interview length also correlated with better performance. More details of the analyses are available upon request.

the number of questions they answered correctly (and the rewards they had earned) at the very end of the interview. This sequence is necessary because we asked some post-treatment questions about the election and wanted to avoid the possibility of performance feedback contaminating responses to these final questions.

We chose 12 of the 14 questions for their relevance to the 2004 presidential election (the exceptions are questions about the length of a Senate term and the number of Republicans in the Senate). All of the topics covered in these questions reflected active campaign themes in 2004. Some of these questions were about candidate policy positions. We asked about the candidates' positions on tax cuts, education, and the line-item veto. Other questions were about political circumstances that were relevant to the presidential campaign, such as the Senate vote on the Iraq authorization and the 9/11 commission's findings about links between al-Qaeda and Iraq. Another set of questions focused on economic factors referenced during the campaign. We asked about official government statistics pertaining to the number of Americans who were not covered by health insurance, the number living in poverty, and the number of unemployed. We also tested their knowledge of the estate tax and the federal debt. In short, we asked challenging questions about matters relevant to the 2004 election. A complete list of questions and their wording is in Appendix Table 1.

We followed recommendations by Mondak and Davis (2001) and Krosnick et al. (2002) to discourage "Don't Know" responses by not giving respondents explicit "Don't Know" options. Respondents could of course hit the "next question" button without marking any answer, but almost none of them did. Discouraging "Don't Know" responses reduces distortions because some people are more likely to guess than others in the absence of encouragement.

In our analysis, we use the number of correct responses as our dependent variable. This choice raises the question of how to determine the range of answers to open-ended questions that we consider correct. The ranges we use are listed in Appendix Table 1. Running the analyses with different ranges of the same general magnitude yields similar treatment effects.[4]

## The Survey

Our experiment was embedded in a representative survey of U.S. residents conducted by Knowledge Networks between October 19 and November 1, 2004. Knowledge Networks interviews national probability samples over the Internet by providing a large panel, selected through Random Digit Dialing, with WebTV units and/or free Internet connections in exchange for taking surveys. The participants for this study constitute a randomly selected subset of the KN panel and approximate a random sample of the U.S. adult population. Our survey was assigned to 1,550 panelists of whom 1,220 (79 percent) completed it. Eighty percent of the respondents who completed the survey did so within 4 days of the fielding date.

[4] A second coding decision pertains to respondents who did not see all of the knowledge questions. This situation arises in the "24 hour" conditions for respondents who reach the 24-hour time limit before completing the whole battery. In particular, some respondents in those conditions started the knowledge section, took a break, and never returned to complete the remaining questions. Excluding respondents who saw only some of the knowledge questions would bias our sample because we would be excluding the less motivated respondents who forgot to finish the questionnaire. Hence, we use the total number of correct answers as our dependent variable and code all non-answered questions as incorrect. Only 24 of the respondents who started the knowledge section did not see all 14 knowledge questions. This coding decision does not affect the substance of our findings.

Knowledge Networks' survey methodology makes our study a conservative test of our hypotheses. The company informs its panelists by email when a new survey is waiting for them. They can then take the survey at a time of their own choosing. Hence, even respondents in our control group ("one minute, no pay") are not literally caught during dinner or at other inopportune moments and asked to answer the knowledge questions on the spot. In fact, they even had the opportunity to pause the interview when they learned that they would be asked political knowledge questions. (However, they could not stop the relevant timers once they saw the first knowledge question.) Clearly, we do not capture the true inconvenience of a typical phone interview. Moreover, panelists receive compensation just for participating because Knowledge Networks pays for their WebTV unit and/or an Internet connection to their PC. To be sure, this compensation does not represent an incentive to answer thoughtfully on any particular question, but the conditions in our control group do not recreate the conditions of a typical phone interview perfectly. Therefore, respondents in the control group are likely more motivated and less inconvenienced than respondents in the telephone surveys from which many claims about political knowledge are derived. All else constant, these attributes should make our null hypotheses harder to reject. [5]

[5] We examined whether assignment to the experimental conditions affected completion rates (i.e., whether providing extra time for responses or paying respondents for correct answers would affect the likelihood that they complete the entire interview). If it does, then we must estimate this indirect effect of the experimental manipulations as well as their direct effects. Part of this complication is avoided because the assignment of the money factor occurred only when respondents reached the knowledge section of the interview. Respondents who quit the survey before that point could not have been affected by the monetary incentive as we had not yet

## The Effect of a Monetary Incentive

We begin the analysis by testing our first null hypothesis—that survey respondents perform thorough memory searches under typical survey conditions, so that a monetary incentive will not increase their performance. Figure 2 plots the distribution of our dependent variable, the 15-point knowledge index, in the control (one minute, no pay) and treatment (one minute with pay) conditions. The monetary incentive shifts the distribution to the right. Fewer respondents answer only one or two questions correctly when they are offered an incentive. In the "control" condition, 28 percent of the sample provides correct answers to fewer than three questions. That share drops to 21 percent as a result of the incentive. At the high end of the distribution, the monetary incentive increases the share of respondents who answer more than eight questions correctly from 10 to 15 percent.

(Figure 2 goes here)

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revealed that aspect of the survey. Only seventeen respondents quit after reaching that point in the interview. Ten were in the "24 hour" condition and may have forgotten to resume the interview with the 24-hour period. Assignment to the time condition was determined at the beginning of the interview but revealed to the respondents only at the beginning of the knowledge sequence. The completion rates in the two time conditions are not statistically different. Eighty percent of the respondents assigned to the "one minute" condition completed the interview, compared to 78 percent in the "24 hour" condition. Of the seventeen respondents who never made it to the knowledge questions, seven would have been assigned to the pay condition and ten to the no pay condition. Hence, selection effects are very unlikely. Therefore, we consider experimental differences between respondents who completed the interview as valid estimates of the true treatment effects.

Table 1 summarizes the effect of providing a monetary incentive on the number of correct answers. As the top of the table shows, the incentive increased correct answers from 4.5 to 5.0 on average. This 11 percent increase is statistically significant at $p < .05$.[6] We thus reject our first null hypothesis: Since an incentive for correctly answering knowledge questions increases the number of correct answers, it follows that conventional mass opinion surveys underestimate how much political information respondents hold in memory. *Simply paying respondents a small amount for answering questions correctly yields a significant increase in performance*. This result suggests that standard survey practice does not provide sufficient incentives for respondents to thoroughly search their declarative memory.

(Table 1 goes here)

The distribution of knowledge in the population is also consequential. Table 1 presents the effect of the monetary incentive for different demographic and attitudinal subgroups. For several groups, the experimental effect was far larger than the average 11 percent increase. Among respondents with a moderate interest in politics, for example, the monetary incentive increased correct answers by 32 percent. Men, white Americans, and those between 35 and 59 years of age also improved their performance disproportionately in the "one minute with pay" condition.

Such differences are relevant because they indicate if knowledge gaps in the population widen or narrow when political knowledge is measured in a more accurate fashion. Our results suggest, for example, that gender and race differences in political knowledge are larger than commonly believed. In the control condition, men responded correctly to about more 0.8

[6] The experimental effect remains robust when we control for the impact of common demographic and attitudinal predictors of political knowledge (political interest, education, gender, age, race, and employment status).

questions than women. The monetary incentive increases this difference to 1.3. This outcome suggests that under conventional survey conditions, women search their declarative memories more effectively than men. When we offer compensation, the gender gap widens.

Racial differences in political knowledge increase even more dramatically in the "one minute with pay" condition. White Americans do better by about 0.8 items without an incentive. The monetary incentive expands the race gap to 2.3 items. The monetary incentive improved whites' performance very robustly, but had no effect at all on non-whites. (In fact, the experimental effect on non-whites is negative, although not statistically significant.) Our sample size prevents us from drawing precise conclusions about the experimental effects on specific non-white groups, but separate analyses of Blacks, Hispanics, and other groups reveal effects of similar magnitude.

To examine if these group-level differences are robust to the inclusion of relevant demographic variables, we estimate multivariate models of political knowledge in the two experimental conditions. The OLS estimates are shown in Table 2. If the coefficients for a particular attribute are significantly different in the "one minute with pay" condition than they are in the "control" condition, then we can conclude that our treatment changes the effect of this attribute on our respondents' political knowledge scores. (With only about 300 respondents in each condition, we consider differences with p-values of less than .10 as sufficiently precise.)

(Table 2 goes here)

The results in Table 2 confirm the difference in the effect of political interest in the two conditions. When politically uninterested people search their memories more thoroughly, they do not find much more than respondents in the control group. Those with moderate interest, on the other hand, know quite a bit more than they tell us in a typical survey interview. Among the most

interested, the experimental effect is in the same direction, but smaller. These findings suggest that traditional survey procedure fails to motivate moderately and (to a lesser extent) strongly interested citizens to try as hard as they can on political knowledge assessments. As a result, past survey-based studies of political knowledge have likely underestimated the impact of political interest on knowledge.

Control variables slightly reduce the race and gender differences found above. The difference between men and women is only marginally larger in the "one minute with pay" condition than in the control condition. Substantively, the effects remain large. The monetary incentive *more than doubles* both the gender gap and the race gap in political knowledge.

When respondents are encouraged to exert extra effort in answering knowledge questions, men, white Americans, and the politically more interested increase their performance disproportionately. The difference between moderately interested white men and uninterested non-white women is little over one item using traditional survey procedures, but surges to more than four items when thorough memory search are encouraged. The difference between these two groups increases from one third of a standard deviation on the knowledge index to four thirds of a standard deviation with a monetary incentive. Therefore, conventional survey measures likely not only underestimate political knowledge, but also underestimate the inequality in the distribution of political knowledge on several key demographics.

**The Effect of Extra Time**

According to Null Hypothesis #2, providing survey respondents with extra time should simply reproduce results obtained from previous knowledge measures. Our alternative hypothesis is that political knowledge and political learning skills may represent different paths to informed decision-making. Since we used a between-subjects experimental design, we cannot

evaluate this hypothesis simply by estimating the relationship between political knowledge and political learning skills. Instead, we compare the performance of different demographic and attitudinal subgroups on the two tasks. If no significant subgroup differences emerge, we can conclude that political learning skills simply replicate political knowledge. If, on the other hand, some groups of people are better (or worse) political learners than their political knowledge would suggest, political learning skills capture a separate dimension.

We document the effect of extra time as it appeared in two distinct experimental treatments. In one treatment, randomly selected respondents were given extra time to answer questions and no compensation for answering correctly. Another randomly selected group received extra time *and* a monetary incentive. Our rationale for using a monetary incentive in the measurement of political knowledge was to reduce distortions from differential effort that respondents devote to memory searches. The same rationale applies to the political learning task, so we offered some (again, randomly selected) respondents in the "24 hour" treatments the same monetary incentive. This "24 hours with pay" condition indicates best how well respondents' can educate themselves about politics when they are at least modestly motivated to do so by us. The "24 hours, no pay" condition documents how well respondents do on the learning task without extrinsic motivation.

Figure 3 graphs the distributions of responses in the control group (one minute, no pay) and the two "24 hour" conditions. Extra time shifts the distribution of correct responses markedly to the right. While 28 percent of the respondents in the control condition answered less than three questions correctly, that share drops to 15 percent in the "24 hours with pay" condition. With extra time, but without the monetary incentive, the share is 18 percent. Only 10 percent get more than eight items right in the control condition, compared to almost twice that (19 percent) in the "24 hours with pay" condition.

(Figure 3 goes here)

Table 3 expresses the effect of extra time more succinctly by comparing mean performance in the three experimental conditions. The number of correct responses is significantly higher when respondents have 24 hours to complete the knowledge questions. Compared to the control group, average performance increases by 18 percent without a monetary incentive and by 24 percent with the incentive.

However, higher averages alone are not sufficient to show that political learning skills capture an element of informed decision-making that is distinct from that captured by "instant recall." By giving respondents more time, we have changed the meaning of the scale, so the knowledge scale is not directly comparable to the learning skills scale (even though both scales use the same unit, the number of correct answers.) Put more simply, even if knowledge and learning skills were perfectly correlated, individual respondents would not necessarily receive the same score on both scales. To determine if political knowledge and political learning skills (at least as measured in our experimental domains) are different, we examine the relative performance of different groups.

(Table 3 goes here)

Table 3 shows the experimental effects of extra time (relative to the "control" condition) for the same set of demographic as did Table 1. Extra time has a disproportionately large effect on less interested respondents, respondents without a college degree, older respondents, and white

respondents. These findings are our first indication of important differences between political knowledge and political learning skills.[7]

Moving from bivariate comparisons to multivariate models, we present OLS estimates in Table 4. The table shows the same model for all four experimental conditions (repeating, for ease of comparison, the results from Table 2.) The most important contrast in Table 4 is between the "one minute with pay" condition, which represents our best measure of political knowledge, and the "24 hours with pay" condition, which assesses political learning skills with less interference from differential survey motivation than the "24 hours, no pay" condition. We can reject our second null hypothesis if one or more regression coefficients in the fourth column are significantly different from the same coefficients in the second column. Moreover, if the absolute value of a coefficient is greater in the "24 hours with pay" conditions than in the "one minute with pay" group, differences in political learning skills amplify differences in political knowledge. If, in contrast, a variable has a smaller absolute effect in the former condition, the distribution of political learning skills attenuates the effect of the variable on the capacity to reach informed decisions.

(Table 4 goes here)

[7] Because of the conceptual difference between political knowledge and political learning skills, we examine the effects of the two experimental factors separately. They can of course also be evaluated in one model. Analysis of variance confirms that both experimental factors significantly increase knowledge scores (Time: $F[1,1216] = 17.2$, $p < .0001$; Pay: $F[1,1216] = 5.5$, $p < .02$). The interaction of the two factors, on the other hand is not significant (Time × Money: $F[1,1216] = .43$, n.s.), indicating that each factor works independently.

Four factors in Table 4 are significantly different in the second and fourth columns. Another difference (for gender effects) approaches statistical significance. Hence, our experimental treatments do more than boost overall performance and stretch the knowledge scale. They also provide a second dimension for observing who in the population exhibits a higher potential for informed decision-making than traditional emphases on political knowledge would reveal.

For example, people aged 60 and older do slightly worse on the instant knowledge task than people under 35. Given time to figure out the answers, however, they answer between 1 and 1.5 more questions correctly. So, while young and old Americans are equally knowledgeable, seniors are far more likely to figure out the answers to the questions we posed. Here, providing the opportunity for political learning amplifies the advantages of age.

Another large difference between political knowledge and political learning skills occurs for people who left college without a degree. This segment of the population is barely more knowledgeable than those who did not go to college at all, but when given an opportunity to look up the answers (in the "24 hours with pay" condition) their performance parallels that of the college graduates. This result suggests that college attendees who left without a degree may not store as much political information in their declarative memory, but they have acquired skills relevant to answering political questions. Their political decisions may thus be better informed than conventional political knowledge measures suggest.

Extra time also narrows the gender difference observed earlier. When we give them extra time, men and women are equally good learners. The distribution of learning skills along gender lines thus attenuates the impact of gender on the ability to answer our questions correctly. Women may not carry as much political information in declarative memory as do men, but our results suggest that it would be premature to infer from such data that women know less than

men when we give each an opportunity to learn. Fully employed people, too, take advantage of the opportunity to learn and answer questions far more effectively than standard political knowledge scores would suggest.

The most remarkable difference between political knowledge and political learning skills, however, concerns the role of political interest. Political interest has a very large impact on our measure of political knowledge (see column two). When respondents are motivated by the prospect of a small material reward for answering correctly, but have no opportunity to draw on their procedural memory, the most politically interested among them do better than those who are moderately interested, and the moderately interested, in turn, do better than the uninterested. These differences are far smaller when it comes to political learning skills (see column four). Politically uninterested people have considerable learning skills (by comparison to the more interested). The learning skills of politically very interested people are still significantly greater, but this difference is barely half as big as the equivalent difference for political knowledge. The significantly smaller coefficients for political interest in the fourth column indicate that the distribution of political learning skills (as exhibited in our experiment) attenuates inequalities in the potential for informed-decision making that would arise from declarative knowledge differences between more and less interested citizens.[8]

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[8] When political interest is not entered in the model as a series of dummies, but as a 4-point scale (ranging from 0 to 3), the OLS coefficient is .91 in the "one minute with pay" condition, which implies a knowledge difference of 2.7 items between the least and most interested respondents. In the "24 hours with pay" condition, that coefficient is only .48—almost 50 percent smaller. In terms of political learning skills, the least and most interested respondents are only 1.4 items apart.

We illustrate this difference graphically in Figure 4 by comparing our political knowledge and political learning skills measures for different levels of political interest. The figure plots the predicted number of correct answers in the "one minute with pay" and the "24 hours with pay" conditions for a married, white, female college graduate between 45 and 59 with mean income and full-time employment. The solid line illustrates that political interest has a large impact on political knowledge. The difference between the least and most interested respondents is more than two items on the knowledge scale, a very large effect by comparison to the effects of other variables. The same difference in political interest has a much smaller effect on political learning (dotted line), amounting to just over one item. Political learning does not require much political interest. Age and a few years of college attendance, not political interest, are the main factors that explain who figured out the answers to our questions.

(Figure 4 goes here)

The finding that politically uninterested Americans have such learning skills illustrates the usefulness of distinguishing between political knowledge and political learning skills. Many people who are intrinsically motivated to follow politics acquire political information regularly and regardless of whether a decision is impending. They are knowledgeable when we ask them fact-based questions on surveys. Others who do not enjoy politics as much are less likely to carry such information in their declarative memories. When survey interviewers contact them without warning, these people do not perform well. But it would be a mistake to infer from this observation that they are incapable of answering the questions. In the absence of an professional or intrinsic motivation to learn about politics, it may be more reasonable for them to "study" only before a political decision. To be sure, we do not claim that every uninterested American behaves in this fashion. But according to our results, a good number of them appear to be more

capable of informed decision-making than extant research often claims. The fact that most uninterested Americans carry little political information around with them does not necessarily imply that their decisions are made in an uninformed way.

## Conclusion

Many observers are concerned about what citizens know about politics because a knowledgeable population is thought to make decisions that are more beneficial to themselves, their families, and the communities in which they live. In this study, we have suggested two methods to improve survey-based measures of political knowledge: The measurement of knowledge using incentives to encourage greater effort and the conceptualization of relevant knowledge as both declarative and procedural. Our results show that people store, and know how to find, more political information than previous research suggests.

That a small monetary incentive elicits more correct answers to knowledge questions demonstrates that people process and store more political information than commonly thought. Establishing whether or not citizens hold certain political facts in memory is, in our view, a more meaningful measure of political knowledge than the conventional approach of assessing whether or not citizens know the facts *and* are motivated to tell us so in a survey interview. To the extent that we are truly interested in learning about what citizens do and do not know, we should construct political knowledge surveys to limit the effects of satisficing and the lack of trying more generally.

Survey-based knowledge measures also ignore procedural memory, even though people rely on it regularly in their professional and everyday lives. In politics those who cannot instantly recall a particular fact often have opportunities to ask someone else or look up the answer. Traditional surveys, while having many virtues, prevent or inhibit exactly the kinds of search

activities that are in fact strongly encouraged by people who want others to make more informed decisions. To convert this critique into a constructive basis for improved understanding of what citizens know, we removed typical time restrictions on some respondents, thus transforming a political knowledge pop-quiz into an evaluation of respondents' abilities to find answers. In so doing, we find that some people, in particular people who report being less interested politically, are more capable than their traditionally-measured political knowledge levels would suggest.

By pointing to political learning skills as an underappreciated path to informed decision-making, we do not mean to downplay the importance of political knowledge. There are situations when citizens are called to act or make political decisions with little advance warning. In those situations, political learning skills are of little help and political information stored in memory is all citizens can draw on. What people know at those moments is important. But in many other situations, including elections, people can collect relevant information and reach more informed political decisions. In those situations, political knowledge *and* political learning skills can contribute to informed decision-making.

Political learning skills indicate a *potential* for informed decision-making. To what extent individuals realize this potential is a separate question. Unfortunately, it is practically impossible to answer this question directly for a large sample of people. Surveys do not occur at the time when respondents reach political decisions, and political knowledge at the time of the interview may not be a good proxy for people's political knowledge when they make political decisions or develop political opinions. Surveys therefore underestimate political knowledge levels if respondents have either not yet acquired or already forgotten information they used (or will use) in their decision-making. The percentage of respondents who answer knowledge questions correctly increases as an election approaches (e.g., Johnston, Hagen, and Jamieson 2004),

indicating that many people acquire political information in anticipation of their vote decision. But once respondents have reached a particular political decision, it may be cognitively inefficient for them to retain the facts that were helpful in making the decision, but are unlikely to be relevant in the future. As a result, early deciders may already have forgotten some of the information that affected their decision (Lodge, McGraw, and Stroh 1989; Lodge, Steenbergen, and Braun 1995). As different people decide at different times, it becomes virtually impossible to interview all respondents when they make their decisions. Since people's knowledge when they are interviewed need not indicate how well-informed their decision actually was (or will be), assessing their political learning skills may provide a more reliable indication of their competence.

Both of the survey procedures we propose to better understand the role of information in political decision-making can be refined and extended. For example, our results demonstrate that it does not take much to induce respondents to approach political knowledge questions in a manner that improves their responses. Introducing a small financial incentive ($1 per correct answer) was sufficient to increase performance significantly. While our study dismisses the null hypothesis that typical survey procedures elicit all that respondents know, it does not establish how much more thorough their memory searches become when we offer monetary incentives. Alternative calibrations of the monetary incentive could clarify the motivational push necessary to get different kinds of respondents to report what they know. The effects of non-monetary incentives also merit attention.

To sum up, we have shown that conventional political knowledge scales suffer from two problems. First, they confound respondents' recall of political information and their motivation to engage the survey question. Second, conventional measures of knowledge cannot assess how

good respondents are at accessing political information that is not stored in memory at the time of the survey. We have addressed the first problem by extrinsically motivating respondents to search their memory for the correct answer. We show that people store more political information than past research has indicated. The second problem led us to measure political learning skills directly. We found that some less knowledgeable people are quite skilled at finding political information when they have an opportunity to do so. Our results provide a new and distinct reason for being skeptical when analysts use existing knowledge measures as the basis for broad generalizations about what citizens do not know. Both of the innovations we propose reveal greater capacity for informed decision-making than traditional knowledge tests suggest.

Table 1: Effect of a Monetary Incentive on the Number of Correct Responses to Knowledge Questions

| | No Pay | Pay | Percent Increase |
|---|---|---|---|
| Mean Correct | 4.5 | 5.0* | **+ 11** |
| Standard Deviation | 2.78 | 2.95 | |
| N | 312 | 306 | |
| Follows politics… | | | |
| "most of the time" (N=205) | 6.2 | 6.5 | + 4 |
| "some of the time" (N=222) | 3.9 | 5.2** | **+ 32** |
| "only now and then" or "hardly at all" (N=189) | 3.5 | 3.3 | - 4 |
| College Degree (N=182) | 6.1 | 6.5 | + 7 |
| No College Degree (N=436) | 3.9 | 4.5* | **+ 15** |
| Female (N=321) | 4.1 | 4.5 | + 8 |
| Male (N=297) | 4.9 | 5.8* | **+ 17** |
| Age | | | |
| 18 - 34 (N=150) | 4.6 | 4.4 | - 4 |
| 35 - 59 (N=291) | 4.5 | 5.4* | **+ 20** |
| 60 - (N=177) | 4.6 | 5.1 | + 10 |
| White (N=477) | 4.7 | 5.6** | **+ 17** |
| Non-whites (N=141) | 3.9 | 3.3 | - 13 |
| Works full time (N=341) | 4.5 | 5.1 | + 11 |
| Does not work full time (N=277) | 4.5 | 5.0 | + 11 |
| Married (N=371) | 4.8 | 5.3 | + 9 |
| Not married (N=247) | 4.1 | 4.7 | + 15 |

* $p < .05$, ** $p < .01$ (two-tailed t-test)

*Note*: All respondents had one minute to complete each knowledge question. For significant experimental effects, the percent increase is bolded.

Table 2: Predictors of Political Knowledge With and Without a Monetary Incentive

| | No Pay | Pay |
|---|---|---|
| Follows politics "some of the time" | **.03**<br>**(.34)** | **1.50****<br>**(.36)** |
| Follows politics "most of the time" | 1.94**<br>(.36) | 2.26**<br>(.39) |
| High school degree only | .25<br>(.39) | .59<br>(.43) |
| Some college | .51<br>(.46) | .89*<br>(.45) |
| College or graduate degree | 1.83**<br>(.42) | 1.73**<br>(.46) |
| Female | -.38<br>(.26) | -.89**<br>(.28) |
| Age: 35 - 44 | -.29<br>(.38) | -.37<br>(.41) |
| Age: 45 - 59 | -.23<br>(.39) | .38<br>(.41) |
| Age 60 - | -.63<br>(.42) | -.33<br>(.44) |
| Racial/Ethnic Minority | **-.84***<br>**(.34)** | **-1.81****<br>**(.34)** |
| Income (1-19) | .18**<br>(.04) | .12**<br>(.04) |
| Full-time employment | -.73*<br>(.32) | -.57<br>(.31) |
| Married | -.08<br>(.28) | -.01<br>(.29) |
| Constant | 2.47**<br>(.56) | 3.02**<br>(.59) |
| $R^2$ | .36 | .39 |
| N | 312 | 306 |

** $p < .01$, * $p < .05$ . Cell entries are OLS coefficients with standards errors in parentheses. All respondents had one minute to complete each knowledge question. For comparisons between columns, bolded coefficients are statistically different from each other at $p < .10$.

Table 3: Effect of Extra Time on the Number of Correct Responses to Knowledge Questions

| | Control | 24 hours, no pay | % change | 24 hours, $1/correct | % change |
|---|---|---|---|---|---|
| Mean Correct | 4.5 | 5.4** | **+ 18** | 5.6** | **+ 24** |
| Standard Deviation | 2.78 | 2.93 | | 3.10 | |
| N | 312 | 302 | | 300 | |
| Follows politics "most of the time" (N=314) | 6.2 | 6.3 | + 2 | 6.8 | + 10 |
| "some of the time" (N=340) | 3.9 | 5.3** | **+ 36** | 5.4** | **+ 38** |
| "only now and then" or "hardly at all" (N=256) | 3.5 | 4.2 | +20 | 4.6** | **+ 31** |
| College Degree (N=276) | 6.1 | 6.4 | + 5 | 6.7 | + 10 |
| No Degree (N=638) | 3.9 | 4.9** | **+ 26** | 5.2** | **+ 33** |
| Female (N=457) | 4.1 | 5.1** | **+ 24** | 5.3** | **+ 29** |
| Male (N=457) | 4.9 | 5.7* | **+ 16** | 5.9** | **+ 20** |
| Age | | | | | |
| 18 - 34 (N=230) | 4.6 | 4.4 | - 4 | 5.0 | + 9 |
| 35 - 59 (N=420) | 4.5 | 5.3* | **+ 18** | 5.9** | **+ 31** |
| > 60 (N=264) | 4.6 | 6.3** | **+ 37** | 5.9** | **+ 28** |
| White (N=705) | 4.7 | 5.7** | **+ 21** | 6.0** | **+ 28** |
| Non-whites (N=209) | 3.9 | 4.2 | + 8 | 4.2 | + 8 |
| Works full time (N=515) | 4.5 | 5.3* | **+ 18** | 5.6** | **+ 27** |
| Does not (N=399) | 4.5 | 5.5** | **+ 22** | 5.6** | **+ 24** |
| Married (N=561) | 4.8 | 5.7** | **+ 19** | 6.0** | **+ 25** |
| Not married(N=353) | 4.1 | 4.8 | + 17 | 5.0* | **+ 22** |

* $p < .05$, ** $p < .01$ (two-tailed t-test). For both 24 hour conditions, differences and percent changes are calculated relative to the "1 minute, no pay" condition. For significant experimental effects, the percent increase is bolded.

Table 4: Comparing Experimental Effects to Other Correlates of Political Knowledge

| | One Minute | | 24 Hours | |
|---|---|---|---|---|
| | No Pay | Pay | No Pay | Pay |
| Follows politics "some of the time" | .03 (.34) [a] | 1.50** (.36) [ab] | .86* (.40) | -.01 (.41) [b] |
| Follows politics "most of the time" | 1.94** (.36) | 2.26** (.39) [ab] | 1.26** (.42) [a] | 1.08* (.45) [b] |
| High school degree only | .25 (.39) | .59 (.43) | .52 (.47) | .43 (.50) |
| Some college | .51 (.46) [a] | .89* (.45) [b] | .76 (.50) [c] | 2.39** (.57) [abc] |
| College or graduate degree | 1.83** (.42) | 1.73** (.46) | 1.80** (.48) | 2.20** (.57) |
| Female | -.38 (.26) | -.89** (.28) | -.47 (.32) | -.32 (.32) |
| Age: 35 - 44 | -.29 (.38) | -.37 (.41) | .26 (.47) | .44 (.47) |
| Age: 45 - 59 | -.23 (.39) | .38 (.41) | .55 (.46) | .63 (.46) |
| Age 60 - | -.63 (.42) [ac] | -.33 (.44) [bd] | 1.55** (.50) [ab] | 1.05* (.50) [cd] |
| Racial/Ethnic Minority | -.84* (.34) [ab] | -1.81** (.34) [a] | -1.08** (.37) | -1.92** (.41) [b] |
| Income (1-19) | .18** (.04) | .12** (.04) | .11* (.04) | .12* (.05) |
| Full-time employment | -.73* (.32) | -.57 (.31) | .04 (.38) | -.15 (.38) |
| Married | -.08 (.28) | -.01 (.29) | .33 (.33) | .16 (.36) |
| Constant | 2.47** (.56) | 3.02** (.59) | 2.26** (.75) | 2.81** (.70) |
| $R^2$ | .36 | .39 | .23 | .26 |
| N | 312 | 306 | 302 | 300 |

** $p < .01$, * $p < .05$. Cell entries are OLS coefficients with standards errors in parentheses. For comparisons between columns, coefficients with common superscript letters are statistically different from each other at $p < .10$.

Appendix Table 1: Knowledge Questions

| Question ID | Question wording | Response options (Correct response in bold) |
|---|---|---|
| Senate term | How long is the term of office for a U.S. Senator? | **open-ended, correct: 6** |
| Reps in Senate | Of the 100 members of the U.S. Senate, how many are members of the Republican party? | **open-ended, correct: 51, accepted range: 51-59**. This range reflects two key points: a Republican majority and its inability to prevent filibusters. |
| Closeness in 2000 | What was the outcome of the 2000 Presidential Election in the state in which you now live?<br><br>[The correct answer depends on the respondent's residence.] | • Bush won by more than 5 percentage points<br>• Bush won by less than 5 percentage points<br>• Gore won by less than 5 percentage points<br>• Gore won by more than 5 percentage points |
| Striving Readers | President Bush proposed a "Striving Readers initiative" to help high school students who are not reading as well as they should be for their age. What is the status of the Striving Readers program? | • The program was implemented in 2002 and has already led to a 1.3 increase in functional literacy among high school students.<br>• **President Bush has proposed to fund this program at $100 million in his 2005 budget.**<br>• President Bush proposed this program, but did not include any funding for it in his 2005 budget.<br>• The program started last year, but in his 2005 budget President Bush proposed to cut its funding by $200 million. |
| Iraq authorization | In the key Senate vote on October 11, 2002, how many Democratic Senators voted to give President Bush the authority to attack Iraq? | • None of them<br>• Two Democratic senators<br>• About a quarter of all Democratic senators<br>• **A majority of all Democrats in the Senate, but not all of them**<br>• All Democratic senators |
| Line-item | A line-item veto allows the president to sign a budget bill | • President Bush and Senator Kerry both oppose the |

| | | |
|---|---|---|
| veto | while cutting specific spending items and tax expenditures that he disapproves. The Supreme Court recently ruled one version of the line-item veto unconstitutional. Other versions of the line-item veto are less likely to be overruled by the court. Which of the following statements best describes the presidential candidates' positions on new versions of the line item veto? | line-item veto.<br>• President Bush supports a line-item veto, while Senator Kerry opposes it.<br>• Senator Kerry supports a line-item veto, while President Bush opposes it.<br>• **President Bush and Senator Kerry both support a line-item veto.** |
| Al-Qaeda connection | As you may know, a special government commission—called the "9/11 Commission," investigated the circumstances surrounding the September 11 attacks and recently issued its final report. Which statement most accurately represents the Commission's conclusions about the relationship between Iraq and al Qaeda? | • They had no connection at all<br>• **A few al-Qaeda individuals visited Iraq or had contact with Iraqi officials**<br>• Iraq gave substantial financial support to al-Qaeda, but was not involved in the September 11th attacks<br>• Iraq was directly involved in carrying out the September 11th attacks |
| Taxes compared to Europe | Compared with the citizens of Western European countries, do you think Americans pay a higher percentage of their income in taxes, a smaller percentage of their income in taxes, or about the same percentage of their income in taxes? | • A higher percentage<br>• **A smaller percentage**<br>• About the same percentage |
| Unemployment rate | The U.S. Bureau of Labor Statistics counts a person as unemployed if they are not employed at any job and are looking for work. By this definition, what percentage of Americans was unemployed in August of 2004? | • around 11 percent<br>• around 9 percent<br>• around 7 percent<br>• **around 5 percent**<br>• around 3 percent |
| Estate tax | There is a federal estate tax – that is, a tax on the money people leave to others when they die. What percentage of Americans leaves enough money to others for the federal estate tax to kick in? | • About 95 of all Americans<br>• About 70 of all Americans<br>• About 50 of all Americans<br>• About 25 of all Americans<br>• **Less than 5 of all Americans** |

| | | |
|---|---|---|
| Uninsured Americans | In August 2004, the United States Census Bureau reported an estimate of the number of Americans without health insurance. The Census Bureau classified people as uninsured if they were not covered by any type of health insurance at any time in 2003. By this definition, what percentage of Americans did not have health insurance in 2003? | **open-ended, correct: 15.6, accepted range: ±6 points** |
| Federal Debt | The outstanding public debt of the United States is the total amount of money owed by the federal government. Every year the government runs a deficit, the size of the public debt grows. Every year the government runs a surplus, the size of the public debt shrinks. In January of 2001, when President Bush took office, the outstanding public debt of the United States was approximately 5.7 trillion dollars. Which of the following responses is closest to the outstanding public debt today? | • Less than 3.5 trillion dollars<br>• 4.5 trillion dollars<br>• 5.5 trillion dollars<br>• 6.5 trillion dollars<br>• **7.5 trillion dollars**<br>• 8.5 trillion dollars<br>• More than 9.5 trillion dollars |
| Kerry tax proposal | John Kerry says that he would eliminate the Bush tax cuts on families making how much money? | • Over 50,000 a year<br>• Over 100,000 a year<br>• Over 150,000 a year<br>• **Over 200,000 a year**<br>• Over 500,000 a year |
| Poverty rate | In August 2004, the Census Bureau reported how many Americans live in poverty. The poverty threshold depends on the size of the household. For example, a person under age 65 is considered to live in poverty if his or her 2003 income was below $9,573 and a family of four is considered to live in poverty if its 2003 income was below $18,810. By this definition, what percentage of Americans lived in poverty in 2003? | **open-ended, correct 12.5, accepted range: ±6 points** |

Figure 1: The Experimental Design

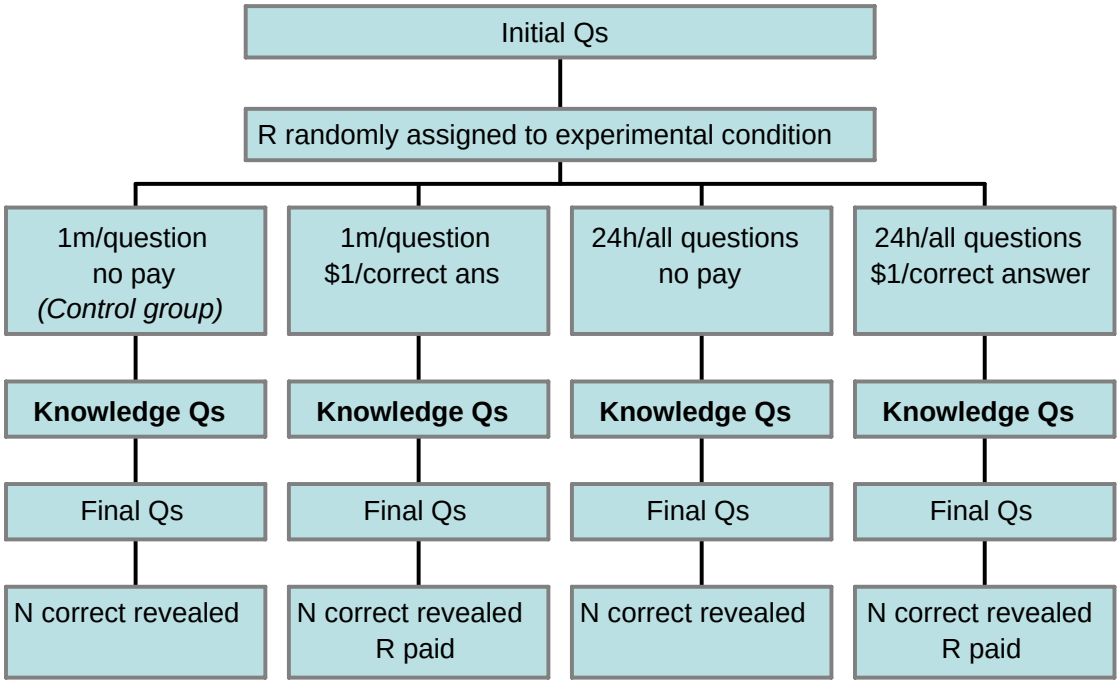


Figure 2: Number of Correct Responses with and without Monetary Incentive

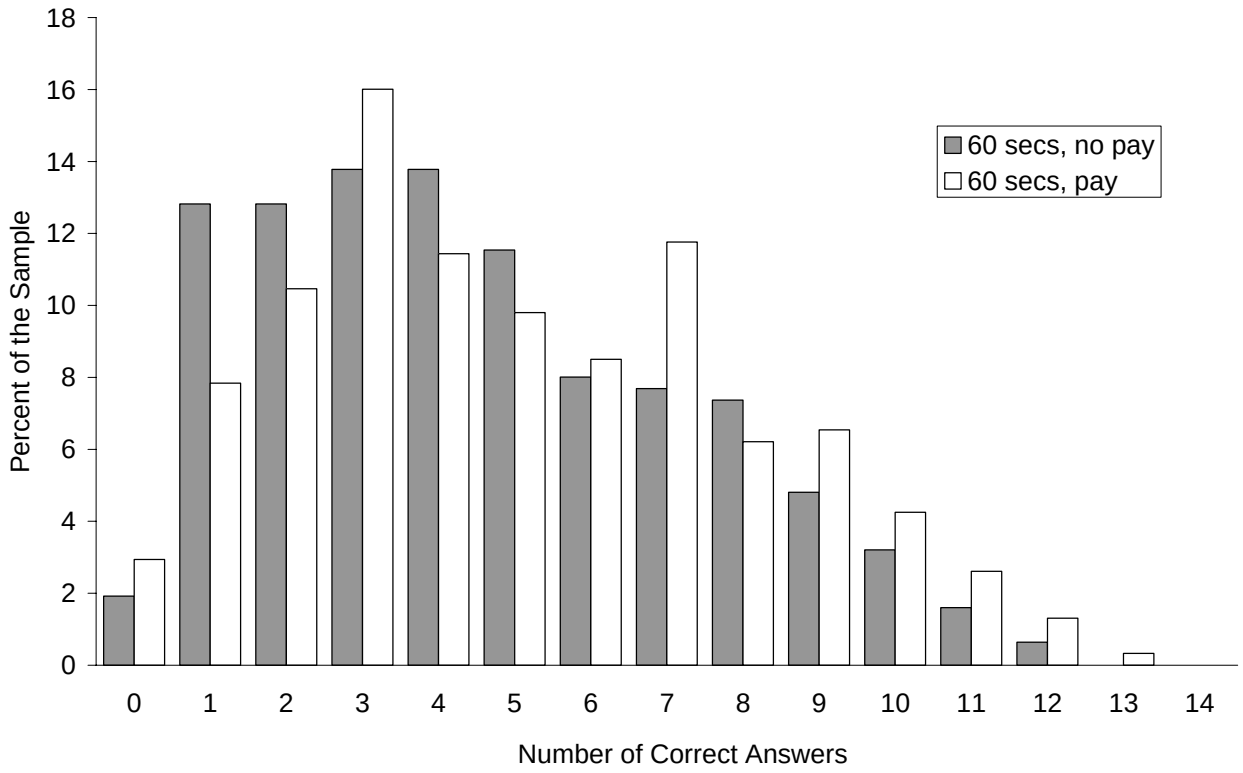


Figure 3: The Effect of Time on the Number of Correct Responses

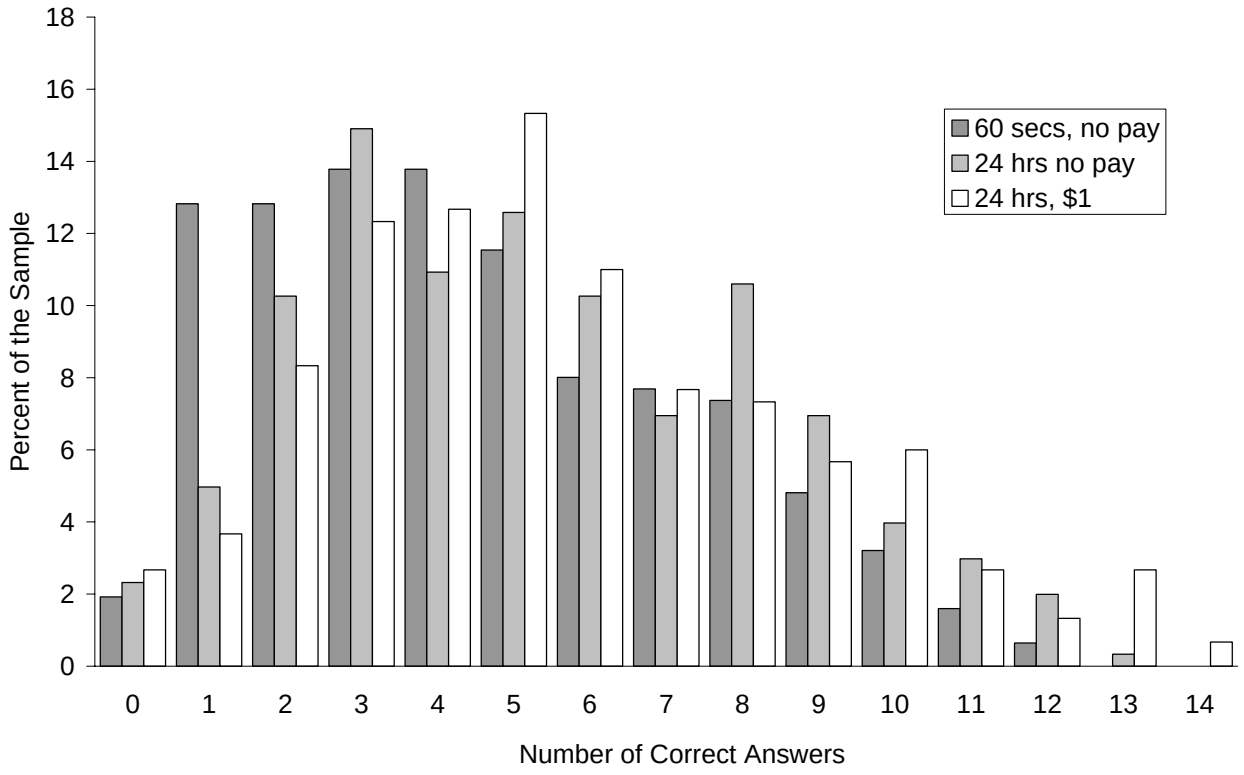
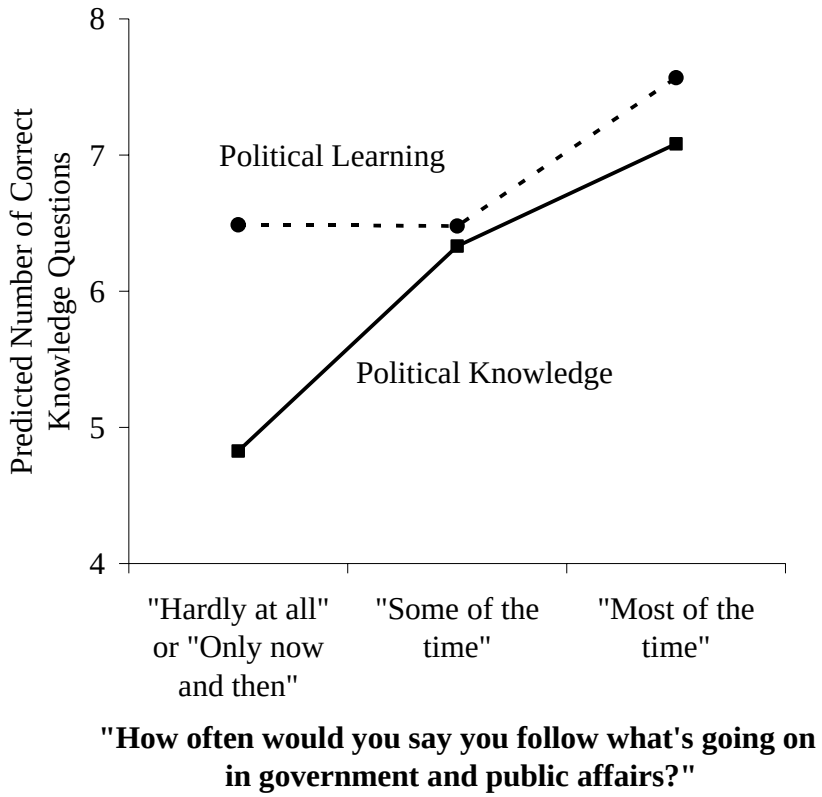


Figure 4: The Effect of Political Interest on Political Knowledge and Political Learning




Note: This figure plots the predicted number of correctly answered knowledge questions by levels of political interest in the “one minute with pay”condition (political knowledge) and in the “24 hours with pay” condition (political learning).

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