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ORIGINAL PAPER

# Making Citizens Smart: When do Institutions Improve Unsophisticated Citizens' Decisions?

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**Abstract** Many scholars lament citizens' lack of political sophistication, while others emphasize that information shortcuts can substitute for sophistication and help citizens with their political choices. In this paper, I use experiments to assess whether and under what conditions institutions can substitute for sophistication and enable even unsophisticated citizens to make informed decisions. The results of my experiments demonstrate that institutions, such as a penalty for lying or a threat of verification, can help both sophisticated and unsophisticated citizens to make more informed decisions. Further, my results suggest that institutions may, under certain conditions, level the playing field between sophisticated and unsophisticated citizens.

**Keywords** Institution · Sophistication · Decision-making · Voter · Experiment · Trust · Cue

Are citizens competent to perform their duties? While various scholars demonstrate that citizens use information shortcuts to guide their political decisions (Downs 1957; Popkin 1991; Lupia 1994; Lupia and McCubbins 1998; Sniderman et al. 1991; Druckman 2001a, b), many others lament citizens' lack of factual knowledge about our government (Berelson et al. 1954; Campbell et al. 1960; Converse 1964). Still other scholars demonstrate that citizens (particularly those who are unsophisticated) are susceptible to how the media frames political issues (Iyengar and Kinder 1987; Zaller 1992). Based upon this body of literature, many scholars question whether our democracy can possibly work, given that citizens seem to lack the sophistication required for the decisions they must make. Indeed, if there are no

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substitutes for citizens' lack of sophistication, then elections may not be decided by citizens, but rather by elites and the media.

In this paper, I demonstrate experimentally that *institutions can substitute for sophistication* and enable even unsophisticated citizens to make informed decisions. Stated differently, regardless of citizens' levels of factual knowledge about politics, there are institutions embedded in our political system that may help citizens to assess the veracity of political actors' statements and to learn from them. For example, competition among candidates, public debates, and the existence of the mass media all increase the probability that political actors' statements will be verified and that any false statements will be revealed. Further, campaigns, advertising, and party labels typically highlight the interests and incentives of candidates (for example, an advertisement might reveal that a particular candidate has common interests with a group of voters; see Lupia and McCubbins 1998). As I demonstrate in this paper, when conditions such as these are met, even unsophisticated citizens can learn from political actors' statements and make informed decisions.

Because I cannot systematically manipulate institutions, political actors' statements, or citizens' levels of sophistication in real world political settings, I instead conduct laboratory experiments in which I analyze whether and under what conditions institutions can substitute for sophistication. The results of these experiments demonstrate that both sophisticated and unsophisticated subjects are able to make more informed decisions after receiving information from a speaker who shares common interests with them, who faces a penalty for lying, or who is verified by a third party. Interestingly, these results show that even small penalties for lying and slim chances of verification help sophisticated and unsophisticated subjects to achieve similar (and significant) improvements in their decisions. Based on these results, it appears that institutions enable even unsophisticated citizens to improve their decisions and, under certain conditions, help level the playing field between sophisticated and unsophisticated citizens.

This paper proceeds as follows. I begin with a review of the literature on citizen competence, and I demonstrate that there is much disagreement among political scientists about whether citizens are sophisticated enough to make informed political decisions. I then suggest that even unsophisticated citizens are capable of making informed decisions because there are substitutes for sophistication embedded in the institutions of our political system. Next, I describe the experimental design that I use to test this proposition, and I suggest a number of hypotheses regarding subjects' ability to make informed decisions when different institutions are in place. I then briefly describe the data source and statistical methods that I use. I next present my experimental results. I conclude with a discussion of the implications that my research has for debates about citizen competence, in general, and about how to improve citizens' levels of political sophistication, in particular.

## Can Citizens Make Informed Political Decisions?

The literature on citizen competence focuses on the question: Do citizens need factual knowledge about politics to make informed decisions? On the one hand, many political scientists answer this question in the affirmative, suggesting that successful democracy requires that citizens be well informed about political matters (Berelson et al. 1954; Campbell et al. 1960; Converse 1964; Delli Carpini and Keeter 1996; Bartels 1996). Indeed, ever since Berelson et al. (1954) emphasized that voters fall far short of democratic standards, scholars have documented time and again that citizens in our country are ignorant of even the most basic political facts (Campbell et al. 1960; Delli Carpini and Keeter 1996; Somin 1998, 2004; Bennett 1988, 2003; Bartels 2005; Kinder et al. 2005). For example, Somin (2004); Bennett (1988) ask citizens to answer factual questions about politics, and they find that many respondents possess little if any politically relevant knowledge. Similarly, Bennett (2003) shows that even citizens who believe that a particular issue (i.e. the economy) is important are unable to answer basic questions about that issue correctly. Based on these findings and others like them, scholars have questioned whether our democracy can possibly work, given that citizens are almost completely uninformed about politics.

In contrast to those who doubt the competence of citizens, many scholars suggest that citizens, despite their lack of sophistication, can make informed political choices. These scholars (like those discussed above) recognize that citizens may not be inherently knowledgeable about politics. However, rather than condemn our democracy as unworkable, these scholars suggest that information shortcuts (such as party labels) and contextual factors (such as institutions) can help citizens to make informed decisions (Popkin 1991; Mondak 1993; Gordon and Segura 1997; Lupia and McCubbins 1998; Lupia 1994; Kuklinski et al. 2001; Lau and Redlawsk 2001; Kinder and Kiewiet 1981; Druckman 2001a, b; Berggren 2001; Boudreau 2006; Boudreau and McCubbins 2008; Boudreau et al. 2008). For example, many scholars argue that party labels, polls, and endorsements can substitute for knowledge about politics and help citizens to behave as though they are sophisticated (Popkin 1991; Sniderman et al. 1991; Mondak 1993; Lupia 1994; Lupia and McCubbins 1998; Druckman 2001a, b; Boudreau and McCubbins 2007).[1] Several other scholars suggest that electoral institutions and political party systems can affect citizens' levels of sophistication (Gordon and Segura 1997 and Berggren 2001). Specifically, Gordon and Segura (1997) and Berggren (2001) argue that particular institutions make the acquisition of political information more or less costly to citizens and, therefore, can explain citizens' levels of sophistication. Berggren (2001) also demonstrates that certain institutional contexts can help level the playing field between sophisticated and unsophisticated citizens.

[1] Although information shortcuts often help citizens with their decisions, there are circumstances under which they may lead citizens astray. Indeed, it is unlikely that information shortcuts improve the decisions of all citizens in all contexts. Thus, scholars have rightly begun to question the conditions under which information shortcuts help citizens to improve their decisions (see, e.g., Bodenhausen and Lichtenstein 1987; Sniderman et al. 1991; Kuklinski and Quirk 2000; Kuklinski et al. 2001; Lau and Redlawsk 2001; Kam 2005; Merolla et al. 2005; Somin 2006; Friedman 2006; Kinder 2006; Boudreau 2008).

It is this body of research on how information shortcuts and institutions affect citizens' levels of sophistication that I build upon in this study. Specifically, I analyze experimentally whether and when institutions can substitute for sophistication and enable even unsophisticated citizens to make informed decisions. Although my research draws upon the insights of Gordon and Segura (1997) and Berggren (2001), as well as the theoretical model and experimental design of Lupia and McCubbins (1998), it makes a number of new contributions to the literature and has several important advantages.

Specifically, my research sheds new light on an important, but unresolved, debate over how sophistication and the decision-making environment interact. While some scholars find that the decision-making environment can reduce differences between sophisticated and unsophisticated citizens (Kuklinski et al. 2001; Rahn et al. 1994; Berggren 2001), others show that information shortcuts work best for the most sophisticated citizens and that they tend to have detrimental effects on unsophisticated citizens (Lau and Redlawsk 2001). In contrast to this latter finding, I identify conditions under which institutions help even unsophisticated citizens with their decisions. Interestingly, my results indicate that even small penalties for lying and slim chances of verification can help unsophisticated citizens to improve their decisions.

Importantly, the existing experimental research on which I build (i.e. Lupia and McCubbins 1998) does not directly address the effects that institutions have on sophisticated and unsophisticated citizens. Indeed, although Lupia and McCubbins (1998) systematically vary the nature of institutions, they are not able to compare the decisions of sophisticated versus unsophisticated subjects. The reason for this is that subjects in their experiments predict the outcomes of unseen coin tosses. Thus, subjects cannot be more or less sophisticated at this task, and Lupia and McCubbins do not have a measure subjects' sophistication. Thus, my experiments build on Lupia and McCubbins's research by analyzing how various institutions (such as different size penalties for lying and chances of verification) affect sophisticated and unsophisticated subjects' decisions.

In addition to these substantive contributions, my experimental design has several important advantages. The first advantage stems from the nature of the choices that subjects make in my experiments. Specifically, instead of asking subjects to vote for fictional candidates or policies, I ask subjects to make choices about math problems (that is, subjects choose whether answer "a" or answer "b" is the correct answer to a given math problem). One reason that this type of decision is advantageous is that solving math problems (unlike voting for fictional candidates or policies) provides a straightforward way to identify correct decisions and assess whether and when institutions induce an improvement in decision-making. Stated differently, although it is often difficult to identify when citizens have chosen the "correct" candidate or policy, it is easy to tell when they have chosen the correct answer to a math problem.

The second advantage is that asking subjects to answer math problems provides a valid, reliable, and agreed upon measure of subjects' sophistication at making this type of decision. Indeed, political scientists measure political sophistication in many different ways, which reflects a disagreement about how this concept should be measured.[2] (For a survey of the many different measures of political sophistication that political scientists use, see Luskin 1987, pp. 865–885). In contrast to these many different measures of political sophistication, there exists an agreed upon, widely used, and straightforward measure of mathematical sophistication—namely, SAT math scores. For this reason, I collect subjects' SAT math scores prior to the experiment, which enables me to assess whether institutions help both sophisticated and unsophisticated subjects to improve their decisions.

Third, subjects' SAT math scores provide a measure of sophistication that is directly related to the task that subjects perform in my experiment (i.e., solving math problems). This also represents an improvement upon existing research because scholars often use a measure of sophistication that is *not* directly related to the task they seek to study. Specifically, scholars frequently measure political sophistication as the ability to answer factual questions about politics (see, e.g., Delli Carpini and Keeter 1996). Measures of this nature, however, may not have a strong relationship to the tasks that subjects perform in an experiment (for example, voting in a mock election, expressing an attitude about a particular policy, etc.) or to the tasks that citizens perform in the real world (i.e., voting for candidates or policies). By using SAT math scores as a measure of sophistication, I am able to use a measure of sophistication that directly relates to the task that subjects perform in my experiment.

The fourth advantage of my design is that math problems, even though they do not *look* like political decisions on the surface, capture many key characteristics of political decisions; thus, they can tell us a great deal about how citizens in the real world make political choices. Indeed, because political scientists conducting experiments cannot have subjects actually cast their votes for real candidates during their experiments, we must develop tasks that are analogous to real world political decisions that subjects can perform (and that we can observe) in laboratory settings. In an effort to provide a close link between political decisions and the decisions that subjects make in an experiment, many scholars have developed tasks that look a great deal like real world political decisions (for example, asking subjects to vote for fictional candidates in fictitious elections during an experiment).

Other tasks that scholars have developed *look* less similar to real world political contexts, but the psychological processes that occur as subjects perform these tasks closely relate to psychological processes that occur in the real world (stated differently, they have a great deal of psychological realism; see Aronson et al. 1998). Examples of this type of task include: (1) using abstract questions about gains versus losses to investigate framing effects and their limitations (Kahneman and Tversky 1979; Druckman 2001b), (2) asking subjects to predict the outcomes of coin tosses to assess how endorsements affect decisions made under uncertainty (Lupia and McCubbins 1998), (3) using spelling tests to analyze citizens' productivity and preferences for redistribution (Frohlich and Oppenheimer 1990;

[2] For example, while some political scientists advocate fact-based measures of political sophistication (see, e.g., Luskin 1987; Delli Carpini and Keeter 1996), others measure political sophistication as an index of political knowledge and various forms of political interest (see, e.g., Lau and Redlawsk 1997, 2001). Still other scholars measure political sophistication as the distance between a citizen's placement of political parties on a 10-point ideological scale and the mean placement of those parties by other citizens (Gordon and Segura 1997).

Esarey et al. 2007), and (4) giving subjects the opportunity to pass money to another subject to assess investment and trust (Berg et al. 1995; Eckel and Wilson 2004; Zak et al. 2004; Wilson and Eckel 2006). These examples (and many others like them) demonstrate that even when an experimental task does not look like real world politics, it can still tell us a great deal about how citizens in the real world make political choices. As I argue below, math problems do exactly this because they capture the following elements of real world political decisions: (1) citizens must choose between two options that affect their future welfare, (2) there are often small stakes associated with citizens' decisions, (3) citizens have preexisting knowledge about their choices, (4) citizens may be uncertain about their decisions, and (5) citizens vary in their levels of sophistication.

## Connecting Math Problems to Politics

At the most basic level, citizens making political decisions often choose between two options (for example, voting "yes" or "no" on an initiative, voting for the incumbent or the challenger, etc.) that will have different effects on their welfare in the future (Fowler and Kam 2006). Similarly, subjects in my experiments must choose between two options (i.e., "a" or "b") that also have different effects on their future welfare. Indeed, because subjects (1) earn money if they make a correct choice and lose money if they make an incorrect choice, (2) are not paid for their decisions until the end of the experiment, and (3) are not given feedback about their decisions until the end of the experiment, the choices that they make affect their welfare down the road.

Although most political decisions affect citizens' welfare (and, thus, there is something at stake for citizens when they make their political decisions), the stakes are often not perceived to be large. Indeed, Hibbing and Alford (2004) and Hibbing and Theiss-Morse (2002) indicate that typical political issues are low stakes games for most Americans. Thus, in my experiments, I ensure that there is something at stake for subjects (money), but that, as in real world politics, the stakes are not large (i.e., subjects either earn or lose 50 cents for each decision they make).

Another similarity between making decisions about math problems and making decisions about politics pertains to the preexisting knowledge that citizens in the real world and subjects in my experiments possess. Specifically, in real world politics, citizens are not blank slates when they go to the ballot box; that is, they may have preexisting knowledge about the candidates and policies that they are choosing from. Similarly, subjects in my experiments are not blank slates when they make their decisions about whether "a" or "b" is the best choice for them because they may have preexisting knowledge about how to answer a particular math problem.

That said, citizens in the real world might be uncertain about their decisions; that is, they may not know which candidate will make them better off. This is especially true when party labels are not attached to the options from which voters must choose, as is the case in nonpartisan elections, ballots containing initiatives, etc. Similarly, subjects in my experiments may be uncertain about whether choosing "a" or "b" will make them better off. As in the real world, the uncertainty that subjects experience depends, in part, upon their levels of sophistication. Indeed, just as unsophisticated citizens in the real world may be more uncertain about which choice will make them better off, so too may unsophisticated subjects in my experiments be more uncertain about whether “a” or “b” is the best choice. And, just as citizens in the real world vary in their levels of sophistication, so too do subjects, as their SAT math scores range from 450 (the 27th percentile) to 800 (a perfect score). Given the many similarities between real world political decisions and decisions about math problems, there is a close mapping between the psychological processes of subjects in my experiments and the psychological processes of voters in real world political contexts.

## Research Design

In order to assess whether institutions enable even unsophisticated citizens to make informed decisions, I design a randomized laboratory experiment. Specifically, I obtain a pretest measure of subjects’ sophistication prior to the experiment (i.e. their SAT math scores). I then randomly assign subjects to treatment and control groups. I next ask subjects to answer a series of binary choice math problems that are drawn from an SAT math test and consist of many different types of problems and several levels of difficulty. I tell subjects in both the treatment and control groups that they have 60 s to answer each math problem and that they will earn 50 cents for each problem that they answer correctly, lose 50 cents for each problem that they answer incorrectly, and neither earn nor lose 50 cents if they leave a problem blank.

The difference between the treatment and control groups is that subjects in the control group answer the problems on their own, while subjects in the treatment group hear the statements of a speaker before they answer the problems. Specifically, before subjects in the treatment group answer the math problems, the experimenter randomly selects one subject to act as “the Speaker.” The Speaker’s role in the experiment is far different from that of the other subjects. That is, unlike the other subjects (whose role in the experiment is to answer the math problems), the Speaker is shown the correct answer to each math problem (that is, the Speaker is given knowledge about the best choice for subjects) and then makes a statement to the other subjects about the answer to the math problem.[3]

[3] The Speaker makes his or her statement by putting a checkmark beside the answer that he or she wishes to report. The experimenter then reads that statement aloud to the other subjects. This prevents the Speaker’s tone of voice from confounding the experiment. Similarly, throughout the experiment, the Speaker sits behind a partition so that the Speaker’s gender, race, and/or age do not affect the extent to which subjects trust the Speaker’s statements. Although this communication process is stripped of contextual factors (such as tone of voice, physical appearance, rhetorical skills, etc.) that we know are important in politics, it is necessary for eliminating confounding variables that limit the inferences that can be drawn from these experiments. Indeed, if subjects are able to see, hear, or talk to one another, then it is difficult (if not impossible) to determine whether particular institutions cause subjects to trust the Speaker’s statements or whether some other factor (such as the Speaker’s gender, tone of voice, or rhetorical skill) causes subjects to trust the Speaker. Stated differently, uncontrolled contextual factors reduce the strong internal validity associated with laboratory experiments and make it difficult to isolate the effects of particular treatment conditions. Other scholars use similar communication processes in their experiments, and they also recognize that anonymous, controlled communication environments are essential for isolating the effects of particular treatments and eliminating confounding variables (see, e.g., Wilson and Sell 1997; Lupia and McCubbins 1998; McCubbins and Rodriguez 2006; Ahn et al. 2007).

After the Speaker makes his or her statement, the other subjects in the treatment group must decide within 60 s whether to answer the problem, and if they choose to answer, whether to pick “a” or “b.”

The key to this experiment is that both the Speaker and the subjects know that the Speaker can lie about the correct answer to the math problem or tell the truth; it is entirely up to him or her. The Speaker’s ability to lie or tell the truth is constant throughout this experiment and is designed to resemble Crawford and Sobel’s (1982) and Lupia and McCubbins’s (1998) models, as well as many real world political settings.

Although the Speaker can lie or tell the truth about the answer to each math problem, I vary the conditions under which the Speaker makes his or her statement. Specifically, I vary (1) the interests between the Speaker and subjects and (2) the institutions that are imposed upon the Speaker. When varying the interests between the Speaker and subjects, I first analyze subjects’ ability to answer the math problems correctly when the Speaker shares *common interests* with them. This experimental condition is analogous to many real world political situations, such as when citizens look to members of their own political party for guidance on how to vote in a given election.[4] Then, I make the Speaker and subjects have conflicting interests, at which point I impose institutions,[5] such as a *penalty for lying* or a *threat of verification,* upon the Speaker. These experimental conditions also have real world analogues, as speakers in political settings often have incentives to misrepresent the truth, but may be deterred from doing so by sufficiently large penalties (such as a loss of reputation or monetary sanctions)[6] or by the chance that

[4] Speakers (such as political parties, candidates, friends, and family members) frequently make statements to citizens about what choice they should make at election time. When deciding whether to trust a speaker’s statement, citizens can use a speaker’s party identification to determine whether the speaker shares common interests with them (Lupia and McCubbins 1998). Importantly, a speaker’s party identification is a widely used cue—one whose use is not limited to only sophisticated citizens (see, e.g., Brady and Sniderman 1985; Popkin 1991; Sniderman et al. 1991; Lau and Redlawsk 2001; Tomz and Sniderman 2004; Kam 2005). Thus, the common interests condition is met in real world politics whenever citizens use a speaker’s party identification as a basis for trusting his or her statements.

[5] Throughout this paper, I draw upon North’s (1990) definition of institutions, as well as Lupia and McCubbins’s (1998) discussion of how penalties for lying and the threat of verification can be conceived of as institutions. Specifically, North (1990, p. 3) defines institutions as “humanly devised constraints that shape human interaction…In consequence, [institutions] structure incentives.” Lupia and McCubbins (1998) also emphasize that institutions alter the incentives of political, legal, and economic actors. They then describe how penalties for lying and the threat of verification alter the incentives of speakers in electoral, legal, and other political contexts (see Lupia and McCubbins 1998, pp. 205–227).

[6] Speakers in political and legal settings frequently incur penalties if they are caught lying. For example, Lupia and McCubbins (1998) emphasize that jurors know that witnesses for both the prosecution and the defense face penalties for perjury if they lie on the witness stand. Similarly, in political contexts, citizens know that a candidate who is caught lying may suffer a penalty in the form of losing a valuable reputation for honesty (Gerber and Lupia 1995). Importantly, citizens need not be highly politically informed to know that a candidate or witness will suffer a loss of reputation or face monetary sanctions if he or she is caught lying. Candidates and witnesses also know that they can incur a penalty if they are caught lying. Thus, the existence of penalties for lying can be common knowledge in the real world, just as it is in my experiments.

another individual or organization (such as political watchdog groups) will verify their statements.[7]

Because the nature of institutions is likely to vary in real world political settings, I manipulate both the size of the penalty for lying and the probability of verification. Specifically, I test the effects that different size penalties (namely, a $15 penalty, a $5 penalty, and a $1 penalty)[8] and different probabilities of verification (specifically, a 100, 90, 70, 50, and 30% chance of verification)[9] have on sophisticated and unsophisticated subjects' ability to answer the math problems correctly. Each variation (i.e. common interests, conflicting interests, penalties for lying, and verification) is common knowledge.

So how do I vary the interests between the Speaker and subjects and impose these institutions in my experiments? In short, I vary both interests and institutions by manipulating the ways that the Speaker and the subjects earn money. For example, in the common interests condition, subjects are paid 50 cents for each math problem that they answer correctly. Similarly, the Speaker is paid 50 cents for each subject who answers a particular math problem correctly. So, if 11 subjects answer a math problem correctly, they earn 50 cents each, and the Speaker earns $5.50 (50 cents for each of the 11 subjects who answer the problem correctly).[10]

In another experimental condition, I establish conflicting interests between the Speaker and subjects, and I simultaneously impose an institution—namely, a penalty for lying—upon the Speaker. To induce conflicting interests between the Speaker and subjects, I merely alter the way that the Speaker earns money. That is, unlike the common interests condition, the Speaker in this experimental condition earns 50 cents for each subject who answers the math problem *wrong*. Subjects, on

[7] Speakers in political and legal settings are often subject to a threat of verification. For example, attorneys' cross-examinations of witnesses are a form of verification that can reveal, with some probability, when witnesses have made false statements (Lupia and McCubbins 1998). Similarly, in political settings, other candidates, interest groups, debate moderators, political watchdog groups, and other interested parties can verify a candidate's statements and reveal when he or she has not been truthful (Lupia and McCubbins 1998). Note also that most citizens know that candidates do not make statements in a vacuum, but rather in an environment in which it is possible for their statements to be verified by others. Most candidates also know all too well that their statements can be verified. Thus, the existence of a threat of verification can be common knowledge in the real world, just as it is in my experiments.

[8] I vary the size of the penalty because penalties for lying in real world political and legal settings also vary in how large they are, relative to what is at stake for a speaker in a particular context. For example, the $15 penalty for lying is a very large penalty, relative to what is at stake for the Speaker in these experiments. The $1 penalty, however, is small relative to the money the Speaker can gain in these experiments. Indeed, in the $1 penalty for lying condition, the Speaker can gain as much as $5.50 if 11 subjects choose the wrong answer to a problem and only lose $1 for lying about the correct answer. Similarly, the penalties for lying that speakers in real world political and legal settings face can be large or small, depending on the nature of the election or trial, the stakes involved, and the value that a speaker places on his or her reputation.

[9] Speakers' false statements are not likely to be detected or punished with certainty (or with any one probability) in the real world. Rather, the probability of verification is likely to vary depending on the nature of the political or legal context in which a speaker makes a statement. For example, verification may be more likely in high profile court cases with experienced attorneys and in campaigns for national office, as opposed to local office. Thus, I examine the effects of different chances of verification.

[10] The typical number of subjects in each experiment is 12 (11 subjects who answer the math problems and 1 Speaker).

the other hand, still earn 50 cents for answering the math problem correctly. Simultaneously, I also impose a penalty for lying upon the Speaker. So, in this experimental condition, the Speaker and subjects have conflicting interests, but I announce to both the Speaker and subjects that the Speaker will incur a penalty (of either $15, $5, or $1, depending on the experimental condition) if he or she lies about the correct answer to the math problem.

For the other institution—namely, verification—I maintain conflicting interests between the Speaker and subjects. However, instead of imposing a penalty for lying upon the Speaker, I verify the Speaker's statement with some probability to make sure that it is a true statement before it is read to subjects. In the 100% chance of verification condition, if the Speaker chooses to make a false statement about the correct answer to the math problem, then I charge the Speaker two dollars[11] and announce the correct answer to subjects. If the Speaker chooses to make a true statement, then I simply read the Speaker's statement aloud to subjects. However, in the 90% chance of verification condition, I roll a 10-sided die before the Speaker makes his or her statement. If the die lands on 1, 2, 3, 4, 5, 6, 7, 8, or 9, then I silently verify the Speaker's statement, charge him or her two dollars if he or she chooses to make a false statement, and announce the correct answer to subjects. If the die lands on 10, however, then I simply announce the answer that the Speaker chooses to report, regardless of whether it is correct or incorrect. In this way, subjects know that there is a 90% chance that the Speaker will be verified, but they do not know whether the Speaker has been verified on any particular problem. The 70, 50, and 30% chance of verification conditions proceed in a similar manner.

## Hypotheses

Several of the experimental conditions described above yield predictions about subjects' ability to answer the math problems correctly. For example, in the presence of a Speaker who shares common interests with subjects, I predict that subjects will be more likely to answer the math problems correctly, relative to subjects in the control group. This prediction stems directly from Lupia and McCubbins's (1998) and Crawford and Sobel's (1982) models, which demonstrate that, in equilibrium, common interests between a Speaker and listeners (who in my experiments are the subjects) induce the Speaker to tell the truth and the listeners to base their choices upon what the Speaker says.

In the $15 penalty for lying condition, I also expect subjects to be more likely to answer the math problems correctly, relative to subjects in the control group. This prediction also stems from Lupia and McCubbins's (1998) model, which demonstrates that when a penalty for lying is sufficiently large, then, in equilibrium, the Speaker never lies and the listeners trust the Speaker's statements. In my experiments, the $15 penalty for lying is in fact "sufficiently large"—that is, it is

[11] I include a small cost in all verification conditions for two reasons. First, in real world settings, speakers must often pay a cost (be it a loss of reputation or monetary sanctions) if they are verified and found to be misrepresenting the truth. Second, the original experiments that tested the effects of verification also included a small cost (Lupia and McCubbins 1998).

large enough to ensure that the Speaker always has an incentive to tell the truth and that subjects know the Speaker has this incentive. The reason for this is that the Speaker's expected value of lying does not exceed the expected penalty in this experimental condition. Thus, I expect that the Speaker will make truthful statements and that subjects will trust the Speaker's statements and answer the problems correctly.

In the 100% chance of verification condition, I also expect subjects to be more likely to answer the math problems correctly, relative to subjects in the control group. As Lupia and McCubbins (1998) note, increasing the probability of verification decreases the probability that a Speaker can benefit from making a false statement. Thus, when the Speaker is verified with certainty, subjects should trust that the Speaker's statement is correct and base their decisions upon it.

## Smaller Penalties for Lying, Slimmer Chances of Verification

When smaller penalties for lying and slimmer chances of verification are imposed upon the Speaker, the Speaker may earn more money if he or she lies about the correct answer to the math problem. Knowing this, subjects may or may not trust the Speaker's statements. Thus, in these experimental conditions, I do not have *ex ante* predictions about how the Speaker's statements will affect subjects' ability to answer the math problems correctly.

The lack of predictions for these experimental conditions stems from key differences between the model on which my experiments are based (namely, Lupia and McCubbins's 1998 signaling model) and the task that subjects perform in my experiments. Specifically, although Lupia and McCubbins predict that subjects will not improve their decisions when the penalty for lying and chance of verification are not sufficiently large, it may be possible for subjects in my experiments to improve their decisions under these conditions. The reason for this is that subjects (and the Speaker) in my experiments have preexisting knowledge about the decisions they must make. Thus, depending on what subjects and the Speaker believe about one another and about the difficulty of the problem, the Speaker may have an incentive to be truthful and subjects may base their decisions upon the Speaker's statement, even when the penalty for lying or chance of verification is small.[12]

For example, in the $1 penalty for lying condition, if the Speaker believes that a particular math problem is easy (in the sense that most subjects will answer the problem correctly on their own), then the Speaker should tell the truth about the correct answer to avoid the $1 penalty. Similarly, if subjects believe that the Speaker believes that the math problem is easy, then they should trust the Speaker's statement if they cannot solve the problem on their own. Such circumstances may lead to improved decision-making among subjects. On the other hand, if the Speaker believes that a particular problem is difficult (in the sense that most subjects will not

[12] Although there is a significant decrease in the Speaker's propensity to tell the truth when the 100% chance of verification is reduced, the Speaker still makes truthful statements more often than not even when the chance of verification is as low as 30%. Thus, subjects should never simply assume that the Speaker is lying. Rather, they should base their decisions upon their beliefs about the Speaker, their preexisting knowledge about the math problem, and their beliefs about the difficulty of the problem.

be able to answer the problem correctly on their own), then the Speaker may have an incentive to lie about the correct answer to the problem. If subjects also believe that the problem is difficult, then they may choose to leave the problem blank if they cannot tell whether the Speaker is being truthful.[13] Such circumstances are unlikely to induce improvements in subjects' decisions. Because the Speaker's and subjects' behavior in these experimental conditions depends upon their beliefs about each other and the difficulty of the problems, I must simply observe *ex post* subjects' ability to answer the problems correctly in these conditions.

### Sophisticated Versus Unsophisticated Subjects

I also do not have *ex ante* predictions about how institutions will affect sophisticated and unsophisticated subjects' ability to answer the math problems correctly. Indeed, the model on which my experiments are based does not distinguish between sophisticated versus unsophisticated individuals (Lupia and McCubbins 1998). Further, it is possible that institutions will be more helpful to sophisticated subjects because these subjects already know something (and in some cases a lot) about the decisions they must make. On the other hand, it is also possible that institutions will be more helpful to unsophisticated subjects, who might be more willing to listen to the Speaker, as opposed to trying to make their decisions on their own (as sophisticated subjects might do).

Also, the existing literature has yet to converge upon a consistent account of how sophistication and the decision-making environment interact. While several scholars find that the decision-making environment can level the playing field between sophisticated and unsophisticated citizens (Kuklinski et al. 2001; Rahn et al. 1994; Berggren 2001), others show that information shortcuts work best for the most sophisticated citizens and that they tend to have detrimental effects on unsophisticated citizens (Lau and Redlawsk 2001). In order to shed light on this debate, I break down my results to examine sophisticated and unsophisticated subjects' ability to answer the math problems correctly in each experimental condition and in the control group.

## Methodology

In order to assess whether and when institutions help even unsophisticated citizens to make informed decisions, I conducted laboratory experiments at a large public university. When recruiting subjects, I posted flyers at various locations on campus, and I also sent out campus-wide emails to advertise the experiments. A total of 381 adults who were enrolled in undergraduate classes participated.

When examining the data gleaned from these experiments, I conduct three different types of analyses. First, I assess the effect that each experimental condition

[13] Subjects solving the math problems are significantly more likely to leave the difficult problems blank.

has on subjects' propensity to answer the math problems correctly. To this end, I estimate a random effects logistic regression in which I regress a dummy variable for whether a subject answers a given math problem correctly (coded 1 if a subject chooses the correct answer and 0 otherwise) on: (1) a dummy variable for each experimental condition (i.e. the *Common Interests* variable is coded 1 for the common interests condition and 0 otherwise), (2) a variable that controls for the difficulty of the math problems,[14] (3) a variable that controls for subjects' levels of sophistication,[15] and (4) a variable that controls for the order in which subjects answer the math problems.[16] The omitted category in this regression is the control group. (Recall that subjects in the control group answer the problems on their own, i.e. without a Speaker). Thus, positive and significant coefficients indicate that a particular experimental condition increases the probability that subjects answer the math problems correctly, relative to subjects in the control group. A random effects regression is used to account for the fact that subjects make multiple decisions in the experiment.

Second, I use Wald tests to compare the coefficients for each experimental condition. This analysis allows me to test whether particular institutions are significantly different from one another with respect to the extent to which they help subjects to improve their decisions. For example, I assess whether subjects are significantly more likely to answer the problems correctly in the large (i.e. $15) penalty for lying condition, relative to the smaller (i.e. $5 or $1) penalty for lying conditions. I also assess the effect that reducing the chance of verification has on the probability that subjects answer the problems correctly.

Third, I break my results down by subjects' levels of sophistication to assess whether institutions enable even unsophisticated subjects to make informed decisions. When classifying subjects as sophisticated or unsophisticated, I use subjects' SAT math scores, as well as the nationwide SAT math percentile rankings that the Educational Testing Service releases. Subjects whose SAT math scores fall above the median score for my sample are considered sophisticated, while subjects whose SAT math scores fall below the median are considered unsophisticated. In terms of the scores associated with these classifications, sophisticated subjects' scores range from 680 to 800 (the 91st percentile and higher), while unsophisticated subjects' scores range from 450 to 660 (the 27th percentile through the 87th percentile).[17]

[14] The *Difficulty* variable reflects the percentage of control group subjects that answer each problem correctly; thus, higher values of this variable indicate an easier problem.

[15] The *Sophistication* variable is based on subjects' SAT math scores and, thus, their levels of sophistication. Specifically, this variable is coded 1 for sophisticated subjects (i.e. subjects whose SAT math scores range from 680 to 800) and 0 for unsophisticated subjects (i.e. subjects whose SAT math scores range from 450 to 660). Note that the results do not change if I use a continuous measure of sophistication or if I alter the cutoffs used to classify subjects as sophisticated or unsophisticated.

[16] The *Question Order* variable captures the order in which the problems were presented to subjects.

[17] My results are robust to different specifications of sophistication. Specifically, my conclusions do not change if I alter the high and low SAT math scores that I use to classify subjects as sophisticated or unsophisticated.

## Aggregate Results

As shown in Table 1, subjects who are exposed to a Speaker who shares common interests with them, who faces a $15 penalty for lying, or who faces a 100% chance of verification are significantly more likely to answer the math problems correctly, relative to subjects in the control group. Specifically, Table 2 demonstrates that subjects in the control group have only a 32% chance of answering the math problems correctly, while subjects who are exposed to a Speaker who shares common interests with them have an 81% chance of answering the math problems correctly (i.e. a 49% increase over the control group). Subjects who are exposed to a Speaker who faces a $15 penalty for lying have a 91% chance of answering the math problems correctly, while subjects who are exposed to a Speaker who faces a 100% chance of verification have a 94% chance of answering the math problems correctly.[18] These differences between the treatment and control groups are statistically significant ($p < 0.05$).

Because penalties for lying and chances of verification in real world political settings may not always be large enough to ensure that speakers tell the truth, I also analyze the effects that smaller penalties for lying and slimmer chances of verification have on the probability that subjects answer the problems correctly. Not surprisingly, the results demonstrate that there is a significant decrease in the probability that subjects answer the math problems correctly when the $15 penalty

[18] Interestingly, subjects do not answer every math problem correctly in the 100% chance of verification condition. This seemingly anomalous result reflects the fact that, when using human subjects in experiments, there is always a certain amount of noise in the data and unexpected behavior. I am confident that this result does not stem from confusion about the experiment because, after reading the instructions for a given treatment condition, the experimenter gives subjects a short quiz that asks them about their own incentives, the incentives of the Speaker, and the way that a particular treatment condition works. On the quiz for the 100% chance of verification condition, subjects are asked what percentage of the time the experimenter will verify the Speaker's statements and announce the correct answer to the math problem (the correct answer to this quiz question is "100%"). To motivate performance on the quiz, the experimenter tells subjects that they earn $0.25 for each quiz question that they answer correctly. After subjects finish taking the quiz, the experimenter corrects subjects' quizzes in front of them, explains any wrong answers to them, and pays them the money that they earned for their performance on the quiz. Subjects, by and large, answer all of the quiz questions correctly. The experimenter also conducts informal post-experiment conversations with subjects, asking them about their understanding of the Speaker's incentives, as well as their own incentives and the way that verification worked. In these conversations, no subjects expressed confusion about the treatment condition.

What the informal post-experiment conversations with subjects reveal, however, is that subjects who behave anomalously in the 100% chance of verification condition (i.e. do not trust the Speaker's statements) are either bored, make a mistake, or want to see what happens if they do not behave as expected. Unfortunately, boredom, making a mistake, and wanting to see how the experimenter reacts to anomalous behavior are factors that play a role in nearly all laboratory experiments. Indeed, I have replicated the 100% chance of verification treatment condition in other, slightly different experiments, and I never observe behavior that is perfectly consistent with my predictions. For example, in other experiments, I show that when a 100% chance of verification is imposed upon two competing Speakers, subjects do not earn the maximum amount of money possible (i.e. they earn, on average, only $0.40, not the maximum $0.50 that I would expect, given that the Speakers' statements are always verified by the experimenter). Perhaps more astonishingly, Lupia and McCubbins (1998) pay subjects to predict that the outcome of an unseen coin toss is heads (regardless of whether that prediction is correct), and they find that subjects predict heads only 86% of the time (which is significantly less than the expected 100%).

**Table 1** The effect of each experimental condition on the accuracy of subjects' decisions

| Independent variables | Dependent variable = whether subjects answer a problem correctly |
|---|---|
| Common interests | 2.240* |
| | (0.273) |
| $15 penalty for lying | 3.084* |
| | (0.224) |
| $5 penalty for lying | 1.054* |
| | (0.226) |
| $1 penalty for lying | 0.626* |
| | (0.178) |
| 100% verification | 3.554* |
| | (0.226) |
| 90% verification | 2.332* |
| | (0.267) |
| 70% verification | 1.733* |
| | (0.200) |
| 50% verification | 1.836* |
| | (0.269) |
| 30% verification | 1.128* |
| | (0.198) |
| Difficulty | 0.052* |
| | (0.002) |
| Sophistication | 0.639* |
| | (0.127) |
| Question order | −0.005 |
| | (0.010) |
| Constant | −2.848* |
| | (0.237) |
| Rho | 0.212 |
| Log likelihood | −2569.31 |
| *N* | 5298 |
| Groups | 365 |

This table displays the coefficients from a random effects logistic regression. The control group is the omitted category

Standard errors in parentheses

* $p < 0.05$

for lying is reduced to $5 or $1 ($p < 0.001$).[19] Similarly, there is a significant decrease in the probability that subjects answer the math problems correctly when the 100% chance of verification is reduced to 90, 70, 50, or 30% ($p < 0.001$). Interestingly, as shown in Tables 1 and 2, subjects in each of the smaller penalty for

[19] Subjects in the $5 penalty for lying condition are significantly more likely to answer the problems correctly than are subjects in the $1 penalty for lying condition ($p < 0.05$).

**Table 2** Increases in the probability that all subjects, sophisticated subjects, and unsophisticated subjects answer correctly in each experimental condition

| Experimental condition | All subjects: Increase in probability of a correct answer, relative to control | Unsophisticated subjects: Increase in probability of a correct answer, relative to control | Sophisticated subjects: Increase in probability of a correct answer, relative to control |
|---|---|---|---|
| Common interests | **0.49** (0.404, 0.584) | **0.51** (0.411, 0.603) | **0.46** (0.372, 0.542) |
| $15 penalty | **0.59** (0.524, 0.652) | **0.62** (0.557, 0.684) | **0.53** (0.456, 0.600) |
| $5 penalty | **0.25** (0.151, 0.359) | **0.24** (0.141, 0.348) | **0.26** (0.154, 0.358) |
| $1 penalty | **0.15** (0.068, 0.230) | **0.14** (0.062, 0.215) | **0.16** (0.070, 0.240) |
| 100% verification | **0.62** (0.558, 0.680) | **0.66** (0.601, 0.718) | **0.55** (0.479, 0.622) |
| 90% verification | **0.51** (0.421, 0.593) | **0.52** (0.431, 0.613) | **0.47** (0.384, 0.551) |
| 70% verification | **0.41** (0.325, 0.488) | **0.41** (0.324, 0.488) | **0.39** (0.307, 0.468) |
| 50% verification | **0.43** (0.323, 0.529) | **0.43** (0.319, 0.538) | **0.40** (0.308, 0.499) |
| 30% verification | **0.27** (0.183, 0.363) | **0.26** (0.175, 0.351) | **0.27** (0.183, 0.361) |
| Control group: Baseline probability of a correct answer | 0.32 | 0.27 | 0.41 |

The results in this table convert the coefficients from the random effects logistic regression reported in Table 1 to probabilities. The results in the "All Subjects" column reflect first differences with all other treatment variables set to zero and the control variables held constant at their mean values (King et al. 2000). The results in the "Unsophisticated Subjects" and "Sophisticated Subjects" columns were generated in the same way for unsophisticated and sophisticated subjects. To arrive at the probability of answering correctly in each experimental condition, I add the increase in the probability of a correct answer in each experimental condition to the baseline probability of answering correctly in the control group. For example, subjects in the common interests condition have an 81% chance of answering the math problems correctly. This percentage was obtained by adding the 49% increase in the probability of answering correctly in the common interests condition to the 32% chance of answering correctly in the control group

Confidence intervals in parentheses. Boldface indicates that the 95% confidence interval does not contain zero, signifying a statistically significant increase over the control group for all subjects, unsophisticated subjects, and sophisticated subjects

lying and slimmer chance of verification conditions still have a significantly higher probability of answering the math problems correctly than do subjects in the control group.

## How Do Institutions Affect Sophisticated Versus Unsophisticated Individuals?

I next examine the effects that common interests, penalties for lying, and the threat of verification have on the probability that sophisticated and unsophisticated subjects answer the math problems correctly. As shown in Tables 1 and 2, each of these experimental conditions increases the probability that subjects, in the aggregate, answer the math problems correctly, relative to the probability of answering correctly in the control group. The question that those aggregate results leave open, however, is whether institutions have different effects on subjects who are and who are not sophisticated. Thus, I break down these results to examine the probability that sophisticated and unsophisticated subjects answer the problems correctly in each experimental condition.

As the results in Table 2 reveal, the common interests, $15 penalty for lying, and 100% chance of verification conditions enable *both* sophisticated and unsophisticated subjects to improve their performance on the math problems, relative to their sophisticated and unsophisticated counterparts in the control group. Specifically, unsophisticated subjects in the control group have only a 27% chance of answering the math problems correctly, while sophisticated subjects in the control group have a 41% chance of answering the math problems correctly. Interestingly, both sophisticated and unsophisticated subjects have over a 77% chance of answering the math problems correctly in the common interests, $15 penalty for lying, and 100% chance of verification conditions, which is a significant improvement over their sophisticated and unsophisticated counterparts in the control group ($p < 0.05$).

What about sophisticated and unsophisticated subjects' decisions when the Speaker faces a smaller penalty for lying or a slimmer chance of verification? In order to answer this question, I analyze the probability that sophisticated and unsophisticated subjects answer the problems correctly when the Speaker faces a $5 penalty, a $1 penalty, and a 90, 70, 50, or 30% chance of verification. As the results in Table 2 demonstrate, sophisticated and unsophisticated subjects in these experimental conditions are also significantly more likely to answer the problems correctly, relative to their sophisticated and unsophisticated counterparts in the control group ($p < 0.05$). That said, the increases that sophisticated and unsophisticated subjects achieve in these experimental conditions are typically not as large as the increases that I observe in the $15 penalty for lying or 100% chance of verification conditions.

Taken together, these results reflect the power of institutions. Regardless of subjects' initial endowments of sophistication, the institutions make them significantly more likely to make correct decisions. Further, Table 2 shows that the increases in the probability of answering correctly that unsophisticated subjects achieve are similar to the increases that sophisticated subjects achieve in each experimental condition. Thus, it appears that institutions can "level the playing field" between sophisticated and unsophisticated subjects; that is, the institutions can, under certain conditions, reduce differences in the probability that sophisticated and unsophisticated subjects answer the problems correctly.

## Conclusion

My experimental results demonstrate that institutions can substitute for sophistication and enable even unsophisticated citizens to make informed decisions. Indeed, the institutions imposed in this experiment made subjects more likely to answer the math problems correctly, relative to subjects in the control group. Further, these institutions helped even unsophisticated subjects to increase their probability of answering the problems correctly. Taken together, these results demonstrate the powerful, positive effects that institutions can have on otherwise unsophisticated individuals.

These results have a number of implications for debates within political science. Indeed, within the political science literature a debate has raged over whether citizens need "encyclopedic" information to make informed decisions or whether information shortcuts are sufficient for citizens to learn what they need to know. The results of my experiments largely support scholars in the information shortcuts camp, for my results suggest that institutions can help citizens make informed decisions even when they are unsophisticated. Specifically, in my experiments, many subjects lacked the ability to answer particular math problems, but even without such ability, the institutions made them more likely to answer the problems correctly. In this way, otherwise unsophisticated subjects were able to make more informed decisions.

For this reason, my results suggest that scholars' lamentations about citizens' lack of political sophistication are premature. Although many scholars argue that we need to improve citizens' levels of political sophistication, my results suggest that it is possible for existing institutions to promote learning and improve decision-making even in the face of an unsophisticated citizenry. Although my experimental results suggest that the efficacy of such institutions is fragile (recall the declines in the probability of answering the problems correctly once the penalty for lying or the probability of verification is reduced), they also indicate that, at least under certain conditions, large improvements can be achieved. I will have more to say on the fragility of institutions in future work, but suffice it to say for now that sophistication does not appear to be a prerequisite for learning and making informed decisions. Thus, rather than advocate educational remedies aimed at increasing citizens' levels of political sophistication, scholars should instead seek substitutes for citizens' lack of sophistication in the institutions of our democracy.

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