Closing the Gap: When Do Cues Eliminate Differences between Sophisticated and Unsophisticated Citizens?

Cheryl Boudreau University of California, Davis

Relatively few scholars assess the conditions under which cues improve citizens' decisions. I analyze experimentally the conditions under which one cue (the statements of an endorser) enables both sophisticated and unsophisticated citizens to improve their decisions. My results demonstrate that the effectiveness of this cue depends upon the endorser's incentives and citizens' levels of sophistication. Specifically, I find that under idealized conditions (i.e., when the endorser always has an incentive to make truthful statements), this cue dramatically improves the decisions of (and closes the gap between) sophisticated and unsophisticated subjects. When the endorser's incentives are more realistic (i.e., the endorser may have an incentive to lie), this cue affects sophisticated versus unsophisticated subjects differently: sophisticated subjects do not improve their decisions, whereas unsophisticated subjects typically improve their decisions enough to make them comparable to sophisticated subjects. Thus, even under more realistic conditions, the gap between sophisticated and unsophisticated subjects closes.

Can uninformed citizens make good political decisions? Ever since Aristotle and Plato raised this question in age-old debates about rhetoric, scholars have investigated whether and when a speaker's statements can help uninformed citizens to improve their decisions. According to Aristotle, statesmen can use rhetoric to persuade citizens to make good decisions, particularly if citizens perceive that the speaker is of good personal character (*Rhetoric* 1356a, 1377b–1378a). But even if citizens in ancient times were capable of correctly perceiving the character of speakers, modern citizens (who also rely upon speakers' statements when making political decisions) often do not know one another well enough to determine the character of particular speakers. Thus, citizens in modern democracies must often use factors other than a speaker's personal character when deciding whether to trust a speaker's statements about the decisions they should make.

Knowing that modern citizens often lack information about speakers' personal character, political scientists have identified a variety of cues that can substitute for knowledge about character and help citizens to make good decisions. Specifically, many scholars argue that a speaker's party identification provides citizens with information about whether that speaker's interests are aligned with their own and, therefore, whether they should trust his or her statements (Druckman 2001b; Popkin 1991; Sniderman, Brody, and Tetlock 1991; Tomz and Sniderman 2004). In a similar manner, scholars suggest that a speaker's gender and race can convey whether a speaker shares common interests with citizens (Iyengar et al. 1997; McDermott 1997, 1998). Further, other scholars suggest that when particular institutions are imposed upon a speaker, they can provide citizens with valuable cues about whether they should trust the speaker's statements (Lupia and McCubbins 1998). Taken together, this body of research establishes that there are cues that can substitute for knowledge about a speaker's personal character embedded in our political system.

What is not well established, however, are the *conditions under which* these cues help uninformed citizens to improve their decisions. That is, although scholars have demonstrated time and again that cues help citizens to make informed decisions, it is unlikely that these cues improve the decisions of all citizens in all contexts. Indeed, recent research suggests that particular cues may be more or less effective under different circumstances and for different types of citizens (Kam 2005; Kuklinski and Hurley 1994; Kuklinski and Quirk 2000; Kuklinski et al. 2001; Lau

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and Redlawsk 2001; Sniderman, Brody, and Tetlock 1991). Thus, the challenge for future researchers is to identify the conditions under which cues provide citizens with effective substitutes for knowledge about politics and for knowledge about a speaker's personal character.

I take up this challenge in this paper. That is, I use laboratory experiments to assess the conditions under which an endorser's statements help both sophisticated and unsophisticated citizens to improve their decisions. Specifically, I analyze whether citizens are able to learn from an endorser's statements even when the endorser does not clearly have an incentive to be truthful (as is often the case in real-world political settings). I also assess whether citizens can learn from an endorser's statements even when they lack the sophistication that would help them with their decisions.

My results demonstrate that under idealized conditions (i.e., when the endorser always has an incentive to tell the truth about which choice will make subjects better off), both sophisticated and unsophisticated subjects are able to learn from the endorser's statements and achieve very large improvements in their decisions. And, the even larger improvements that unsophisticated subjects achieve help close the gap between them and the sophisticated subjects. However, when the endorser's incentives are more realistic (in that the endorser may have an incentive to lie to subjects about the welfare-improving choice), subjects are no longer able to improve their decisions consistently. Additionally, these more realistic conditions affect sophisticated and unsophisticated subjects in different ways. Specifically, sophisticated subjects do not improve their decisions under these conditions, but unsophisticated subjects typically improve their decisions enough to make them comparable to sophisticated subjects. Thus, even though sophisticated and unsophisticated subjects do not both achieve large improvements in their decisions under more realistic conditions, the gap between sophisticated and unsophisticated subjects still closes.

When do Cues Improve Decisions?

In response to scholars who lament citizens' lack of knowledge about politics (see, e.g., Bartels 1996; Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960; Converse 1964; Delli Carpini and Keeter 1996), many scholars argue that cues help citizens to make informed decisions (Downs 1957; Druckman 2001a, 2001b; Kinder and Kiewiet 1981; Kuklinski et al. 2001; Mondak 1993). For example, many scholars argue that party labels are cues that provide citizens with information about candidates' interests and policy preferences (Druckman 2001b; Popkin 1991; Sniderman, Brody, and Tetlock 1991; Tomz and Sniderman 2004). Others suggest that citizens can learn what they need to know from the media, from polls, and from the statements of trusted endorsers (Boudreau 2006; Druckman 2001a; Lau and Redlawsk 2001; Lupia 1994; Lupia and McCubbins 1998; Mondak 1993). Although these scholars demonstrate that citizens use cues when making political decisions, many do not assess the conditions under which cues help citizens to improve their decisions.

Given that cues are unlikely to be equally effective for all citizens, at all times, and in all contexts, several scholars investigate the conditions under which cues help citizens to improve their decisions. Specifically, scholars in this camp address the question, "Who can use which cues when?" As for *who* can use cues, scholars primarily focus on whether cues are equally effective for both sophisticated and unsophisticated citizens (Kuklinski et al. 2001; Lau and Redlawsk 2001; Sniderman, Brody, and Tetlock 1991; see also Kam 2005). Similarly, when analyzing *which* cues citizens use, scholars find that sophisticated and unsophisticated citizens tend to rely on different cues (Lau and Redlawsk 2001; Sniderman, Brody, and Tetlock 1991). As for *when* citizens tend to use cues, many scholars suggest that citizens are more likely to rely upon cues when faced with complex information and difficult decisions (Bodenhausen and Lichtenstein 1987; Lau and Redlawsk 2001; Merolla, Stephenson, and Zechmeister 2005). Taken together, these scholars illustrate the importance of assessing the conditions under which cues enable sophisticated and unsophisticated citizens to improve their decisions.

It is this body of research on the conditionality of cues that I build upon in this study. Specifically, I use experiments to assess the conditions under which both sophisticated and unsophisticated citizens can learn from an endorser's statements and improve their decisions. Although my research draws upon the theoretical framework and experimental design of Lupia and McCubbins (1998) and the insights of Lau and Redlawsk (2001), it makes a number of new contributions to these studies and has several important advantages.

Specifically, my research sheds new light on an important, but unresolved, debate over how cues affect sophisticated versus unsophisticated individuals. While some scholars find that cues can eliminate differences between sophisticated and unsophisticated citizens (Kuklinski et al. 2001; Rahn, Aldrich, and Borgida

1994), others show that cues 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 one particular cue (endorsements) are *more helpful to unsophisticated citizens, thereby closing the gap between their decisions and the decisions of sophisticated citizens*. Thus, my results indicate that this cue helps unsophisticated citizens to improve their decisions and behave as if they are sophisticated.

Importantly, the existing experimental research on which I build (i.e., Lau and Redlawsk 2001; Lupia and McCubbins 1998) does not directly address the conditions under which endorsements close the gap between sophisticated and unsophisticated citizens. Although Lupia and McCubbins (1998) systematically vary the incentives of an endorser, 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 of subjects' sophistication. Further, although Lau and Redlawsk (2001) measure subjects' levels of sophistication, they do not systematically manipulate or measure the trustworthiness of the endorsements to which subjects are exposed. Thus, my experiments build on Lupia and McCubbins's and Lau and Redlawsk's research by analyzing how endorsers with varying incentives to be truthful 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 (subjects are asked to choose whether answer "a" or answer "b" is the correct answer to a given problem). One reason 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 an endorser's statements induce an improvement in decision making. Stated differently, although it is often difficult to identify when citizens have chosen the "correct" candidate or policy,[1] it is very easy to tell when they have chosen the correct answer to a math problem.

The second advantage is that asking subjects to solve math problems provides a valid and reliable measure of subjects' sophistication at making this type of decision. Indeed, although an agreed upon measure of political sophistication does not exist (see, e.g., Krosnick 1990; Luskin 1987), there does exist 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 the endorser's statements 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 the 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) or as an index of political knowledge and various forms of political interest (see, e.g., Lau and Redlawsk 2001). Measures of this nature, however, may not have a strong relationship to the tasks that subjects perform in an experiment (for example, voting in mock elections or stating a preference for a candidate) 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 overcome this limitation and use a measure of sophistication that is directly related to the task that subjects perform in my experiment.[2]

Connecting Math Problems to Politics

Although math problems do not *look* like political decisions on the surface, they capture several key elements

[1]Lau and Redlawsk (1997, 2001) develop measures that assess the correctness of citizens' votes.

[2]One could criticize my measure of sophistication for being too closely related to the task that subjects perform, as both my sophistication and performance measures use SAT math problems. To address this concern, I replicate my results using subjects' college majors as a measure of sophistication (subjects majoring in math-related disciplines are considered sophisticated, while subjects not majoring in these disciplines are considered unsophisticated). I obtain the same results when I use this alternative measure of sophistication—a measure that is distinct from the SAT math problems used to measure subjects' performance, but that is still related to the task that subjects perform. See the online appendix at http://journalofpolitics.org/ for this replication.

of the psychological processes used by voters in real-world political contexts. Thus, they can tell us a great deal about how citizens in the real world make political choices. At the most basic level, citizens making political decisions often choose between two options (i.e., 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 (see Fowler and Kam 2006). Similarly, subjects in my experiments choose between two options ("a" versus "b") that also have different effects on their future welfare. Indeed, because subjects (1) earn money for correct choices and lose money for incorrect choices, (2) are not paid for their decisions until the end of the experiment, and (3) are not given feedback about their decisions until the completion of the experiment, the choices that they make affect their welfare down the road.

Further, just as citizens in the real world can choose not to make political decisions, so too can subjects in my experiments choose not to make decisions about particular math problems. For example, citizens might choose to leave blank the portions of their ballots that pertain to particular initiatives or state and local candidates if they do not know what choice to make. Similarly, subjects in my experiments can choose to leave certain math problems blank if they do not know which choice to make.[3]

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.[4] Specifically, in real-world politics, citizens are not blank slates when they go to the ballot box; that is, they may have preexisting knowledge or beliefs 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 solve particular problems.[5]

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 and referenda, 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 when making their decisions 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 for them. And, just as citizens in the real world vary greatly in their levels of sophistication, so too do subjects in my experiments, as their SAT math scores range from 450 (the 27th percentile) to 800 (a perfect score).

Further, some types of political decisions either implicitly or explicitly involve solving math problems. For example, ballot initiatives regarding school funding policies, property tax policies, and other economic policies often involve math problems that citizens must solve to determine if particular policies benefit them and what the net impact of these policies is. Similarly, evaluating politicians' statements about the consequences of social security privatization involves calculations about whether and when private accounts will yield a higher rate of return than the current system (see Jerit 2008). In the real world (as in my experiments), these decisions can be difficult not only because the problem is complex, but also because an endorser may have an incentive to misrepresent what the "correct" solution is. Given the many similarities between real-world political decisions and decisions about math problems, there is a close mapping between the psychological processes that occur in my experiments and the psychological

[3]Because the math problems that I use have only two choices, subjects (if they are risk neutral) could break even by guessing randomly on all of the problems. What I observe, however, is that subjects are risk averse and frequently leave problems blank.

[4]Despite the many similarities between math problems and political decisions, one could question how a nonpartisan, nonpolitical task like solving math problems maps onto party cues. Interestingly, research in political science demonstrates that party labels affect citizens' behavior even in experiments and surveys that are not explicitly political or partisan (see, e.g., Druckman 2001b; Fowler and Kam 2007; Squire and Smith 1988). Taken together, these studies show that the effects of party labels can be meaningfully studied in nonpolitical or nonpartisan settings. Thus, in future work, I plan to replicate and extend my experiments by revealing the endorser's party identification to subjects before they make their decisions. Thus, before receiving the endorser's statement, subjects would be told whether the subject acting as the endorser is a Democrat, Republican, or Independent. Similar to Fowler and Kam (2007), I would also collect each subject's party identification to assess whether subjects who are Democrats are more likely to trust the statements of an endorser who is also a Democrat. These experiments would provide an assessment of whether and to what extent party cues affect subjects' willingness to trust even nonpolitical statements made by a partisan endorser.

[5]This aspect of my experiments is different from Lupia and McCubbins's (1998) and Lau and Redlawsk's (1997, 2001) experiments, where subjects did not have much (if any) preexisting knowledge about their options.

processes that occur in real-world politics (Aronson, Wilson, and Brewer 1998).[6]

Research Design

In order to analyze the conditions under which an endorser's statements help citizens to improve their decisions, I conduct a randomized laboratory experiment. Specifically, I obtain a pretest measure of subjects' sophistication prior to the experiment (i.e., their SAT math scores), and I then randomly assign subjects to treatment and control groups.[7] I next ask subjects to solve a series of math problems. The math problems that I use are drawn from an SAT math test and consist of many different types of problems[8] and several levels of difficulty.[9] I tell subjects in the treatment groups and subjects in the control group that they have 60 seconds to solve 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,[10] and neither earn nor lose 50 cents if they leave a problem blank. Each subject participates in only one experiment, but makes multiple decisions in that experiment (see the online appendix).

The difference between the treatment groups and the control group is that subjects in the control group solve the problems on their own, while subjects in the treatment groups hear the statements of an endorser before they solve the same problems that subjects in the control group solve. Specifically, before subjects in the treatment groups solve the math problems, the experimenter randomly selects one subject to act as "the endorser."[11] The endorser'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 solve the math problems), the endorser is shown the correct answer to each math problem (that is, the endorser 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.[12] After the endorser makes his or her statement, the other subjects in the treatment group are given 60 seconds to answer that particular math problem.

The key to this experiment is that both the endorser and the subjects know that the endorser can lie about the correct answer to the math problem

[6]There are, of course, disadvantages associated with using math problems. Specifically, the main disadvantage is that math problems are low in mundane realism; that is, on the surface, they do not resemble political events in everyday life (Aronson, Wilson, and Brewer 1998). For example, subjects in my experiments hear statements about the answers to math problems, not statements about political issues. Similarly, subjects in my experiments choose "a" or "b," not one candidate or policy over another. That said, I argue that the psychological realism of my experiments (i.e., that the psychological processes involved in my experiments are similar to the psychological processes involved in real-world politics) makes up for what my experiments lack in mundane realism (Aronson, Wilson, and Brewer 1998). Indeed, as Brewer states, "An experimental setting may have little mundane realism but still capture processes that are highly representative of those that underlie events in the real world" (2000, 12).

[7]Demographic data indicates that random assignment worked quite well. Specifically, there is no significant difference in the average SAT math scores or grade point averages of subjects in the various treatment groups and in the control group. There is also no difference between the treatment and control groups with respect to the percent of subjects who are math majors, who have taken a college math class, and who are female.

[8]Specifically, the problems involve algebra, geometry, trigonometry, logarithms, exponents, functions, factorials, modular arithmetic, and systems of equations.

[9]Subjects in the control group (who solve the problems on their own) provide an objective measure of the difficulty of each problem. These results show that some problems are easy (almost all control group subjects answer these problems correctly), some are difficult (few control group subjects answer these problems correctly), and some fall in between. That said, there is no difference in the difficulty of the problems across treatment conditions and the control group. Thus, the varying difficulty of the problems does not confound my results.

[10]Subjects earn money before they begin solving the problems. All subjects receive $5 for showing up and earn money by solving practice problems and taking quizzes on the instructions. I then tell subjects that they will either keep the money that they have in front of them, lose it, or have more added to it. Thus, subjects know they can lose money in the experiment.

[11]One could question whether heterogeneous preferences for fairness among subjects acting as the endorser are biasing my results. The data indicates that the random selection of subjects to act as endorsers was in fact random; thus, preferences for fairness among subjects chosen to act as the endorser (as well as other differences between these subjects) were evenly distributed across conditions. Specifically, there is no difference in the percent of economics majors acting as endorsers in each treatment condition, nor is there a significant difference in the average SAT math scores or the average grade point averages of subjects acting as endorsers in each condition. There is also no difference in the percent of subjects who have taken a college math class or the percent of subjects who are math majors among subjects acting as endorsers in each condition.

[12]The endorser makes his or her statement by checking the answer that he or she wishes to report. The experimenter then reads that answer aloud to subjects to prevent the endorser's voice from confounding the experiment. The endorser also sits behind a partition so that the endorser's gender, race, and age do not affect subjects' propensity to listen to the endorser's statements.

or tell the truth; it is entirely up to him or her. The endorser'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 situations.

Although the endorser can lie or tell the truth in all treatment groups, I vary the conditions under which the endorser makes his or her statement. Specifically, I vary the interests between the endorser and subjects, as well as the institutional context in which the endorser makes his or her statement. When varying the interests between the endorser and subjects, I analyze subjects' ability to answer the math problems correctly when the endorser shares *common interests* with them. This treatment condition is analogous to many real-world political situations, such as when citizens who are concerned about the environment look to the Sierra Club for guidance on how to vote in a given election. In other treatment conditions, I make the endorser and subjects have *conflicting interests*, at which point I vary the institutional context by imposing a *penalty for lying* or a *threat of verification* upon the endorser. These treatment conditions also have real world analogues, as endorsers 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) or by the chance that another individual or organization (such as political watchdog groups) will verify their statements.

Because the nature of institutions is likely to vary in real-world settings, I manipulate both the size of the penalty for lying and the probability of verification that is imposed upon the endorser. Specifically, I test the effects that different penalties (namely, a $15, $5, and $1 penalty) and different probabilities of verification (specifically, a 100%, 90%, 70%, 50%, and 30% chance of verification) have on subjects' ability to answer the math problems correctly. Each of these variations is common knowledge and is explicitly explained to subjects. Further, each variation alters how clear the endorser's incentives are to subjects. Specifically, in the common interests, $15 penalty for lying, and 100% chance of verification conditions, the endorser always has an incentive to tell the truth, in equilibrium; thus, his or her incentives are most clear to subjects. However, in the smaller penalties for lying and smaller chances of verification conditions, the endorser may have an incentive to lie, in equilibrium; thus, his or her incentives are less clear to subjects in these conditions.[13]

So how do I vary the interests between the endorser and subjects and impose the institutions within the context of these experiments? In short, I vary both interests and institutions by manipulating the ways that the endorser and the subjects earn money. Specifically, in the common interests condition, subjects are paid 50 cents for each math problem that they answer correctly. Similarly, the endorser is paid 50 cents for each subject who solves a particular math problem correctly. So, if 11 subjects (the typical number used in these experiments) answer a math problem correctly, each subject earns 50 cents, and the endorser earns $5.50 (i.e., 50 cents for each of the 11 subjects who answer the problem correctly).

In another treatment condition, I establish conflicting interests between the endorser and subjects, and I then impose an institution—namely, a penalty for lying. To induce conflicting interests between the endorser and subjects, I merely alter the way that the endorser gets paid. That is, unlike the common interests condition, the endorser in the conflicting interests condition earns 50 cents for each subject who gets the math problem *wrong*. Subjects, on the other hand, still earn 50 cents for solving the math problems correctly. Simultaneously, I also impose a penalty for lying. So, in this treatment condition, the endorser and subjects have conflicting interests, but I announce to both the endorser and subjects that the endorser will incur a penalty (of either $15, $5, or $1, depending on the treatment condition) if he or she lies about the correct answer to the math problem.

For the other institutional condition—namely, verification—I maintain conflicting interests between the endorser and the subjects. However, instead of imposing a penalty for lying upon the endorser, I verify the endorser'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 endorser chooses to make a false

[13]To ensure that subjects understand the endorser's incentives, the experimenter gives them a short quiz after reading the instructions for a given treatment condition (see the online appendix for the instructions and quizzes). Subjects, by and large, answer all of the quiz questions correctly. Further, during post-experiment conversations with subjects, none expressed confusion about the endorser's incentives. Thus, I am confident that subjects understand that in the common interests, $15 penalty for lying, and 100% chance of verification conditions, the endorser always has an incentive to tell the truth. I am also confident that subjects understand that in the smaller penalties for lying and slimmer chances of verification conditions, the endorser may have an incentive to lie.

statement about the correct answer to the math problem, then I charge the endorser two dollars and announce the correct answer to subjects. If the endorser chooses to make a true statement, then I simply read the endorser's statement to subjects. However, in the 90% chance of verification condition, I roll a 10-sided die before the endorser 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 endorser's statement, charge him or her two dollars if he or she chooses to make a false statement, and announce the correct answer to the math problem to subjects. If the die lands on 10, however, then I simply announce the answer that the endorser chooses to report, regardless of whether it is correct or incorrect. I explicitly explain this verification process in detail to subjects. Thus, in the 90% chance of verification condition, subjects know that there is a 90% chance that the endorser will be verified, but they do not know whether the endorser has been verified on any particular problem. The 70%, 50%, and 30% chance of verification conditions proceed in a similar manner.

Because subjects earn money for answering problems correctly and lose money for answering problems incorrectly, my experiments yield a straightforward measure of whether the endorser's statements help subjects to improve their decisions. Specifically, I calculate the average amount of money that subjects earn per math problem in each treatment condition, and I then compare the amounts of money that subjects earn in each treatment condition and in the control group. For example, if the endorser's statements enable subjects to improve their decisions, then I should observe higher average payoffs for subjects in my treatment groups, relative to the average payoffs of subjects in the control group, who solved the math problems without the opportunity to learn from the endorser's statements. By contrast, if the endorser's statements do not help subjects to make better decisions, then there should be no differences between the amounts of money that subjects in the treatment and control groups earn. I also break down these results by subjects' levels of sophistication to determine whether and when the endorser's statements close the gap between the performance of sophisticated and unsophisticated subjects.

Hypotheses

The treatment conditions described above yield predictions about when the gap between the earnings of sophisticated and unsophisticated subjects will close. For example, when the endorser shares common interests with subjects, I predict that there will be no difference in the amounts of money that sophisticated and unsophisticated subjects earn. This prediction is derived from Lupia and McCubbins's (1998) and Crawford and Sobel's (1982) models, which demonstrate that common interests between a knowledgeable speaker (i.e., the endorser) and listeners (i.e., the subjects) induce the speaker to tell the truth and the listeners to base their choices upon what the speaker says. Because it is common knowledge that the endorser shares common interests with subjects in my experiments, all subjects (regardless of their levels of sophistication) know that the endorser always has an incentive to make truthful statements. Indeed, to ensure that subjects understand that the endorser always has an incentive to tell the truth in this treatment condition, I give subjects a quiz at the beginning of the experiment that asks them to state how much money the endorser earns under various circumstances. Subjects, by and large, answer the quiz questions correctly. If a subject answers a quiz question incorrectly, I explain to that subject why the answer that he or she chose is incorrect and reveal the correct answer to that subject. In this way, I am certain that all subjects understand the endorser's incentives. Thus, I expect both sophisticated and unsophisticated subjects to trust the endorser's statements and base their decisions upon them, which should close the gap between sophisticated and unsophisticated subjects' decisions. I also expect subjects, in the aggregate, to earn greater amounts of money in this treatment condition than in the control group.

In the $15 penalty for lying condition, I also predict that there will be no difference in the amounts of money that sophisticated and unsophisticated subjects earn. The logic behind 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, a speaker never has an incentive to lie, and the subjects trust the speaker's statements. In my experiments, the $15 penalty is "sufficiently large"—that is, it is large enough to ensure that the endorser has a dominant strategy to tell the truth and that all subjects (regardless of their levels of sophistication) know this.[14]

[14]The reason for this is that the endorser's expected value of lying does not exceed the penalty in this condition. As in the common interests condition, I ensure that subjects understand that the endorser always has an incentive to tell the truth by giving them a quiz that asks them to state how much money the endorser earns under various circumstances.

Thus, I expect both sophisticated and unsophisticated subjects to trust the endorser's statements and base their decisions upon them, which should close the gap between these two types of subjects. I also expect subjects, in the aggregate, to earn greater amounts of money in this treatment condition than in the control group.

When a 100% chance of verification is imposed upon the endorser, I also predict that there will be no difference in the amounts of money that sophisticated and unsophisticated subjects earn. This prediction again stems from Lupia and McCubbins's (1998) model, which demonstrates that increasing the probability of verification decreases the probability that a speaker benefits from making a false statement. Thus, when there is a 100% chance that the endorser's statements will be verified, subjects are certain to receive truthful statements about the correct choice. Thus, subjects should trust the endorser's statements, base their choices upon them, and make better decisions as a result—again, regardless of their levels of sophistication.[15] I also expect these subjects, in the aggregate, to earn greater amounts of money than subjects in the control group.

In the other, more realistic treatment conditions (i.e., a $5 penalty, a $1 penalty, and a 90%, 70%, 50%, or 30% chance of verification), I expect to observe a gap between the earnings of sophisticated and unsophisticated subjects. Specifically, I expect sophisticated subjects to earn more money than unsophisticated subjects in these treatment conditions. This prediction is derived from Lupia and McCubbins's (1998) model, which demonstrates that an endorser does not necessarily have an incentive to make truthful statements when smaller penalties for lying and slimmer chances of verification are in place. If the endorser does not necessarily have an incentive to make truthful statements,[16] then it follows logically that subjects cannot simply trust the endorser's statements (as they should in the idealized conditions). Rather, subjects must rely upon their own knowledge to determine whether the endorser is telling the truth about the answers to particular problems and whether and how to answer those problems. Because sophisticated subjects possess greater preexisting knowledge about how to answer math problems (and are, therefore, less dependent upon the endorser, who may have an incentive to lie), I expect them to earn greater amounts of money than unsophisticated subjects in these conditions. I also predict that subjects, in the aggregate, should earn smaller amounts of money in these more realistic conditions than in the idealized conditions.

Methodology

In order to assess the effects that the endorser's statements have on sophisticated and unsophisticated subjects' decisions, I conducted laboratory experiments at a large public university. When recruiting subjects, I posted flyers on campus and sent out campus-wide emails to advertise the experiments. A total of 381 adults who were enrolled in undergraduate classes and who were of different genders, ages, and college majors participated.

Because I use college undergraduates as my source of data and because I ask these undergraduates to make decisions about math problems, my results may underestimate the extent to which the endorser's statements help both sophisticated and unsophisticated citizens to improve their decisions. The reason for this is that subjects in my experiments know something (and, in some cases, a lot) about the choices that they are asked to make. That is, all subjects know something about how math problems should be solved, and they may also have beliefs about whether answer "a" or answer "b" is the correct choice for a particular problem. Further, all subjects who participate in my experiments have taken the SAT math test and have experience solving math problems in short amounts of time. This is not the case for many members of the general population, who may have never taken the SAT math test or who are likely to have SAT math scores that are much lower than those of college undergraduates. These differences between the undergraduates that I use in my experiments and members of the general population may cause me to underestimate the effectiveness of this particular cue. Indeed, if I had used members of the general population in my experiments, there would have been much more room for the endorser's statements to effect an improvement in subjects' decisions.

[15]Again, I give subjects a quiz to ensure that they understand how this treatment condition will proceed. Thus, I am certain that sophisticated and unsophisticated subjects understand that the endorser will be verified with certainty in this condition.

[16]If the endorser believes that a particular math problem is difficult, then he or she may have an incentive to lie about the correct answer to the problem. The endorser has this incentive because only the most sophisticated subjects should be able to solve the difficult problems on their own, thereby verifying the endorser's statements and recognizing when the endorser is lying. The unsophisticated subjects, however, may be fooled by the endorser's lie or choose to leave the problem blank if they do not know which choice to make.

When analyzing the data gleaned from these experiments, I conduct difference of means tests to examine whether subjects who are exposed to the endorser's statements in each treatment condition earn significantly more money than subjects in the control group.[17] I also break my results down by subjects' levels of sophistication to assess whether and under what conditions the endorser's statements enable both sophisticated and unsophisticated subjects to improve their decisions and, by extension, the amount of money that they earn.

Aggregate Results

As predicted, subjects who are exposed to an endorser who shares common interests with them, who faces a \$15 penalty for lying, or who faces a 100% chance of verification earn significantly more money than do subjects in the control group. Specifically, Table 1 shows that subjects in the control group earn, on average, only \$0.13 per problem (N = 66), while subjects exposed to an endorser who has common interests with them earn, on average, \$0.42 per problem (N = 62). Subjects who are exposed to an endorser who faces a \$15 penalty for lying earn, on average, \$0.39 per problem (N = 79), and subjects who are exposed to an endorser who faces a 100% chance of verification earn, on average, \$0.40 per problem (N = 85). Each of these differences between treatment and control group subjects is statistically significant.

Because penalties for lying and chances of verification in real-world political settings may not always be large enough to ensure that endorsers tell the truth, I also analyze the effects that smaller penalties and slimmer chances of verification have on the amounts of money that subjects earn. As shown in Table 2, subjects in the smaller penalties for lying and slimmer chances of verification conditions do not consistently earn greater amounts of money than do subjects in the control group. Specifically, difference of means tests show that the amounts of money that subjects earn in the \$5 penalty for lying, 70% chance of verification, and 30% chance of verification conditions are not significantly greater than the amount of money that subjects in the control group earn. Although subjects do earn significantly more money than control group subjects in the \$1 penalty for lying,[18] 90% chance of verification, and 50% chance of verification conditions, these are much smaller amounts of money than the amounts of money that subjects earn in the idealized treatment conditions. Thus, as expected, these more realistic conditions are not as effective as the idealized conditions at improving subjects' decisions in the aggregate.

Results for Sophisticated versus Unsophisticated Citizens

I now test my hypotheses regarding whether and when the endorser's statements close the gap between sophisticated and unsophisticated subjects' 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. Specifically, 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).[19]

I first examine the effects that idealized treatment conditions have on the amounts of money that sophisticated and unsophisticated subjects earn. As shown in Table 1, the endorser's statements in the common interests, \$15 penalty for lying, and 100% chance of verification conditions enable *both*

[17] I also estimate my results using OLS and random effects GLS regressions. As I note above, my experimental design balances the difficulty of the math problems across conditions and random assignment ensures that there is no difference in subjects' SAT math scores across conditions. That said, as a robustness check, I estimate regressions in which I control for problem difficulty, subjects' sophistication, and the order in which subjects answer the math problems. The random effects model is used to account for subjects making multiple choices in each experiment. See the online appendix for these regression results.

[18] Although subjects in the \$5 penalty for lying condition do not earn more money than subjects in the control group, I find that, as expected, endorsers are more likely to tell the truth in the \$5 penalty for lying condition than in the \$1 penalty for lying condition. Thus, the larger \$5 penalty induces more truth telling than the \$1 penalty and does not "crowd out" subjects' motivations for honesty (Frey 1998). Also, there is not a significant difference between the amounts of money that subjects earn in the \$5 penalty and \$1 penalty conditions.

[19] My results are robust to different specifications of sophistication. Specifically, my results do not change if I alter the high and low SAT math scores that I use to classify subjects as sophisticated or unsophisticated. They also do not change if I use an alternative measure of sophistication—namely, whether subjects' college majors are in a math-related discipline or not.

Table 1 The Effects that Common Interests, a $15 Penalty for Lying, and a 100% Chance of Verification have on Sophisticated and Unsophisticated Subjects*

Experimental Condition	All Subjects: Money Earned	Unsophisticated: Money Earned	Sophisticated: Money Earned	Significant Gap b/t Sophisticated and Unsophisticated?
Control Group	$0.13 (N = 66)	$0.07 (N = 20)	$0.21 (N = 25)	Yes (Difference = $0.14; Std. err. = 0.03)
Common Interests	**$0.42 (N = 62; Difference = $0.29; Std. err. = 0.02)**	**$0.42 (N = 31; Difference = $0.35; Std. err. = 0.03)**	**$0.43 (N = 27; Difference = $0.22; Std. err. = 0.02)**	*No Gap* (Difference = $0.01; Std. err. = 0.03)
$15 Penalty for Lying	**$0.39 (N = 79; Difference = $0.26; Std. err. = 0.02)**	**$0.36 (N = 36; Difference = $0.29; Std. err. = 0.03)**	**$0.39 (N = 26; Difference = $0.18; Std. err. = 0.03)**	*No Gap* (Difference = $0.03; Std. err. = 0.03)
100% Verification	**$0.40 (N = 85; Difference = $0.27; Std. err. = 0.02)**	**$0.38 (N = 40; Difference = $0.31; Std. err. = 0.04)**	**$0.41 (N = 32; Difference = $0.20; Std. err. = 0.03)**	*No Gap* (Difference = $0.03; Std. err. = 0.04)

*Boldface indicates a statistically significant improvement, relative to the control group, for all subjects and for each of the two levels of sophistication.

sophisticated and unsophisticated subjects to increase the amount of money that they earn, relative to their sophisticated and unsophisticated counterparts in the control group. As predicted, in each of these treatment conditions, *there is no statistically significant difference in the amounts of money that sophisticated and unsophisticated subjects earn*. Indeed, both sophisticated and unsophisticated subjects earn, on average, over $0.35 per problem in the common interests, $15 penalty for lying, and 100% chance of verification conditions. Taken together, these results suggest the power of these idealized conditions, for regardless of subjects' initial endowments of sophistication, the endorser's statements enable subjects to earn significantly greater amounts of money than do their sophisticated and unsophisticated counterparts in the control group.

Note also that the results in Table 1 reveal that there is a statistically significant difference between the performance of sophisticated and unsophisticated subjects in the control group. Specifically, sophisticated subjects in the control group earn, on average, $0.21 per problem, while unsophisticated subjects in the control group earn, on average, only $0.07 per problem. Based on this finding, it appears that the endorser's statements close the gap between sophisticated and unsophisticated subjects. Indeed, although there is a gap between sophisticated and unsophisticated subjects in the control group, there is no gap between the amounts of money that sophisticated and unsophisticated subjects earn in the common interests, $15 penalty for lying, and 100% chance of verification conditions.

What about the amounts of money that sophisticated and unsophisticated subjects earn under more realistic conditions? The results in Table 2 demonstrate that, contrary to expectations, the gap between sophisticated and unsophisticated subjects closes in all but two of these more realistic treatment conditions. Specifically, there is not a statistically significant difference in the amounts of money that sophisticated and unsophisticated subjects earn in the $5 penalty for lying, 90% chance of verification, 70% chance of verification, and 50% chance of verification conditions.

The reason for why the gap closes under these realistic conditions is different from why the gap closes under idealized conditions. Under realistic conditions, sophisticated subjects do not increase the amount of money that they earn, relative to their sophisticated counterparts in the control group. Unsophisticated subjects, however, typically achieve modest increases in the amount of money that they earn, which makes their decisions comparable to those of sophisticated subjects. The modest increases that unsophisticated subjects achieve are somewhat surprising, given that the endorser may have an incentive to lie under these more realistic conditions. That said, the data show that these modest increases

TABLE 2 Effects of More Realistic Conditions on Sophisticated and Unsophisticated Subjects*

Experimental Condition	All Subjects: Money Earned	Unsophisticated: Money Earned	Sophisticated: Money Earned	Significant Gap b/t Sophisticated and Unsophisticated?
Control Group	$0.13 (N = 66)	$0.07 (N = 20)	$0.21 (N = 25)	Yes (Difference = $0.14; Std. err. = 0.03)
$5 Penalty for Lying	$0.15 (N = 48; Difference = $0.02; Std. err. = 0.03)	$0.11 (N = 27; Difference = $0.04; Std. err. = 0.03)	$0.19 (N = 19; Difference = $0.02; Std. err. = 0.06)	*No Gap* (Difference = $0.08; Std. err. = 0.06)
$1 Penalty for Lying	**$0.17 (N = 141; Difference = $0.04; Std. err. = 0.019)**	**$0.14 (N = 65; Difference = $0.07; Std. err. = 0.03)**	$0.20 (N = 67; Difference = $0.01; Std. err. = 0.02)	Yes (Difference = $0.06; Std. err. = 0.026)
90% Verification	**$0.25 (N = 33; Difference = $0.12; Std. err. = 0.03)**	**$0.23 (N = 14; Difference = $0.16; Std. err. = 0.06)**	$0.26 (N = 17; Difference = $0.05; Std. err. = 0.03)	*No Gap* (Difference = $0.03; Std. err. = 0.07)
70% Verification	$0.16 (N = 83; Difference = $0.03; Std. err. = 0.03)	**$0.16 (N = 44; Difference = $0.09; Std. err. = 0.04)**	$0.13 (N = 34; Difference = $0.08; Std. err. = 0.04)	*No Gap* (Difference = $0.03; Std. err. = 0.04)
50% Verification	**$0.23 (N = 31; Difference = $0.10; Std. err. = 0.03)**	**$0.20 (N = 14; Difference = $0.13; Std. err. = 0.05)**	$0.25 (N = 15; Difference = $0.04; Std. err. = 0.04)	*No Gap* (Difference = $0.05; Std. err. = 0.06)
30% Verification	$0.09 (N = 85; Difference = $0.04; Std. err. = 0.025)	$0.04 (N = 45; Difference = $0.03; Std. err. = 0.04)	$0.13 (N = 36; Difference = $0.08; Std. err. = 0.03)	Yes (Difference = $0.09; Std. err. = 0.04)

*Boldface indicates a statistically significant improvement, relative to the control group, for all subjects and for each of the two levels of sophistication.

are driven by two factors. First, unsophisticated subjects exposed to the endorser's statements are less likely to leave the problems blank (and are more likely to answer the problems correctly) than are unsophisticated subjects in the control group. Second, even though the endorser may have an incentive to lie under these realistic conditions, the endorser more often than not chooses to tell the truth. Thus, when unsophisticated subjects base their decisions on the endorser's statements in these conditions, the statements they receive tend to be truthful.

Conclusion

My experiments identify conditions under which an endorser's statements help sophisticated and unsophisticated subjects to improve their decisions. Specifically, I find that under idealized conditions (i.e., when the endorser is always better off if he or she tells the truth), both sophisticated and unsophisticated subjects achieve large improvements in their decisions. Such improvements effectively close the gap between the performance of sophisticated and unsophisticated subjects. However, once such idealized conditions are made more realistic, sophisticated and unsophisticated subjects are no longer able to improve their decisions consistently. Further, these more realistic conditions affect sophisticated versus unsophisticated subjects in different ways. That is, sophisticated subjects do not improve their decisions under these conditions, but unsophisticated subjects typically improve their decisions enough to make them comparable to sophisticated subjects. Thus, even though sophisticated and unsophisticated subjects do not both achieve large improvements in their decisions, the gap between sophisticated and unsophisticated subjects typically closes under realistic conditions.[20]

Because endorsers in real-world political settings may have an incentive to lie and because citizens in the real world vary in their levels of sophistication, my results contribute to the theoretical and empirical literatures on cues, in general, and the limitations of cues, in particular. Indeed, my results reveal that the effectiveness of endorsements depends not only upon the endorser's incentives (as Lupia and McCubbins 1998 suggest), but also upon the sophistication levels of citizens. Specifically, when an endorser does not necessarily have an incentive to be truthful, this cue may be less useful to citizens. That said, even under these conditions, this cue may still close the gap between sophisticated and unsophisticated citizens by boosting the performance of unsophisticated citizens up to the level of sophisticated citizens. Interestingly, unsophisticated citizens achieve these improvements because, even when the endorser is not necessarily trustworthy, the endorser more often than not makes truthful statements, and unsophisticated citizens are more likely to make a decision (as opposed to abstaining). In this way, my results are consistent with existing research demonstrating that cues close the gap between sophisticated and unsophisticated citizens, but my results suggest two different explanations for how this closing of the gap occurs under different circumstances.

More broadly, my results have implications for the age-old question that modern political theorists and empirical political scientists continue to debate: can uninfomed citizens make good decisions? In contrast to Aristotle's emphasis on how a speaker's personal character can persuade citizens to make better decisions, I demonstrate that a speaker's ability to persuade is influenced by institutions that affect the speaker's incentive to tell the truth (see also Lupia and McCubbins 1998). Specifically, in my experiments, the endorser is able to persuade subjects because they know that a sufficiently large penalty for lying or threat of verification gives the endorser an incentive to be truthful. Indeed, subjects in my experiments know nothing about the endorser's personal character when they decide whether to base their decisions upon his or her statements because the endorser's identity and personal attributes are not revealed to subjects. In this way, my experiments demonstrate that even in the absence of information about a speaker's personal character, institutions can induce a speaker to be truthful and help him or her to persuade others. Thus, my approach is more akin to the institutional solutions offered by Madison (see Hamilton, Jay, and Madison [1787–88] 1961), who argues that we can design institutions (such as having ambition counteract ambition) that increase the likelihood that an agent will act in accordance with the principals' preferences and the common good, even apart from the good motives of agents.

Taken together, this discussion suggests that scholars should continue to investigate the conditions under which endorsers can persuade citizens, as well as the conditions under which citizens can use cues to help them with their decisions. In this study, I address the question of *who* can use a particular cue *when* by analyzing sophisticated and unsophisticated citizens' ability to learn from an endorser's statements and by assessing the conditions under which this cue improves their decisions. Because I focus on one particular cue, however, my experiments leave open the question of *which* cues are most effective for different citizens in different contexts. I take up this question in future experiments, where I analyze citizens' decisions of which, if any, cue(s) to use when the problems they face are more or less difficult.

[20]Elsewhere, I identify conditions under which an endorser helps subjects to improve their decisions even when their decisions are difficult.

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Cheryl Boudreau is assistant professor of political science, University of California, Davis, Davis, CA 95616.