Title
When do simple cues make citizens smart? : understanding the conditions under which cues improve decisions

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UNIVERSITY OF CALIFORNIA, SAN DIEGO

When Do Simple Cues Make Citizens Smart?

Understanding the Conditions Under Which Cues Improve Decisions

A Dissertation submitted in partial satisfaction of the requirements for the degree

Doctor of Philosophy

in

Political Science

by

Cheryl Boudreau

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Professor Mathew D. McCubbins, Chair
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2007

The Dissertation of Cheryl Boudreau is approved, and it is acceptable in quality and form for publication on microfilm.

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Chair

University of California, San Diego

2007

DEDICATION

For my dad, who taught me to work hard and dream big, and

for my mom, who taught me to have some fun along the way.

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When do cues work and for whom? This was the question I asked myself when I began working on my dissertation nearly two years ago. Since then, I have run hundreds of subjects through laboratory experiments designed to answer this question, and my results demonstrate that cues are not equally effective for all citizens in all contexts. Given these results, it became clear that particular cues are more fragile than previous research suggests and that the challenge for me in writing my dissertation was to identify conditions under which cues improve citizens' decisions. In what follows, I spell out several conditions for successful cue taking, emphasizing that whether cues help citizens depends upon citizens' levels of sophistication, as well as the context in which citizens receive the cue(s). The results, arguments, and conclusions contained within represent many hours of running experiments, analyzing data, and drafting chapters but, fortunately, I was not alone in this process.

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I also presented portions of my dissertation at several conferences and universities, where I received thoughtful comments and criticisms for which I am also grateful. Specifically, I thank all those who attended my talks at the University of California, Davis; the University of California, San Diego; Florida State University; Michigan State University; Stanford University; Claremont Graduate University; and Rice University. I received valuable feedback at each of my talks, and I appreciate the opportunity to present my work at these universities. I owe a special thanks to Paul Zak (Claremont Graduate University) and Rick Wilson (Rice University), who generously invited me to present my research to their departments before I went on the job market. At both universities, I gained valuable experience in presenting my work, and I also learned important lessons about how to frame my research best.

Undoubtedly, both of these opportunities contributed to my success on the job market, and I will forever be grateful for the kindness and interest that both Paul and Rick showed to a graduate student like myself.

I also owe a great deal of thanks to those who contributed to my intellectual development here at UCSD. Specifically, I thank the members of my dissertation committee—Mat McCubbins (Chair), Gary Cox, Jeff Elman, Skip Lupia, and Sam Popkin—for reading and commenting on my work and for challenging me to make each of my papers a better product. Both Sam Popkin and Jeff Elman encouraged me to think more about how my work relates to the cognitive science and psychology literatures, while Gary Cox and Skip Lupia challenged me to develop a better theoretical grounding for my work. Skip also, despite being at the University of Michigan, generously spent hours of his time meeting with me at conferences, talking with me on the phone about my research, listening to my practice job talk on the phone, as well as helping me to design an infinitely better job talk than the one that I started with. There are simply not enough words to convey how grateful I am for the many hours that Skip spent shaping the way that I think about political science and about the way that I present my work to others.

There are also not enough words to express my gratitude for all that my advisor, Mat McCubbins, has done for me during my four years at UCSD. As early as my first year of graduate school, Mat generously provided financial support for my experiments, and he graciously (and patiently!) taught me how to run experiments, design protocols, and even build partitions. As my research projects advanced, he offered invaluable comments on each of my papers, and as early as my second year in graduate school, he gave me many opportunities to present my work at conferences and seminars. But even more important to my development as a political scientist are the countless hours that he spent mentoring me, answering my questions, giving me advice, listening to my practice talks, and fielding my many phone calls. It is only through such interactions (which often occurred on weekends and evenings, during which he generously made time for me) that I learned how to think critically, write clearly, design research properly, and ask important questions.

In the process of shaping the way that I think about and do political science, Mat also contributed to my development as a person. He (thankfully) injected me with a healthy dose of skepticism and self-confidence, and he helped me to develop a much thicker skin than the one that I had when I first arrived at UCSD. He also taught me from a very early stage to speak up and ask questions at conferences, which paid dividends later on in my graduate career. This combination of intellectual and personal development that Mat effected has helped me in countless ways, and I fear that I will never be able to repay him fully for his teachings, his patience, and his friendship. I simply would not be where I am today without his support of me and my career as a political scientist.

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"What Statutes Mean: Interpretive Lessons from Positive Theories of Communication and Legislation," with Arthur Lupia, Mathew D. McCubbins, and Daniel B. Rodriguez, *San Diego Law Review* (forthcoming).

"Jurors are Competent Cue-Takers: How Institutions Substitute for Legal Sophistication." *International Journal of Law in Context* (forthcoming).

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ABSTRACT OF THE DISSERTATION

When Do Simple Cues Make Citizens Smart?

Understanding the Conditions Under Which Cues Improve Decisions

by

Cheryl Boudreau

Doctor of Philosophy in Political Science

University of California, San Diego, 2007

Professor Mathew D. McCubbins, Chair

It is widely known that citizens use cues—such as party labels, polls, candidates' appearances, and endorsements—when making political decisions. What is still in doubt, however, is whether and under what conditions particular cues help

different types of citizens to improve their decisions in different contexts. Indeed, given the multitude of cues that exist in the real world, the varied levels of sophistication among citizens, and the wide variety of situations in which citizens may find themselves, it seems unlikely that cues are equally effective for all citizens, at all times, and in all places.

In my dissertation, I identify conditions under which particular cues help citizens to improve their decisions. More concretely, I assess 1) whether and under what conditions a single cue helps both sophisticated and unsophisticated citizens to improve their decisions, 2) whether and under what conditions multiple cues work better than one cue alone, and 3) whether and under what conditions citizens can learn from the statements of multiple speakers. My theoretical and empirical results show that:

- The statements of an endorser only improve the decisions of both sophisticated and unsophisticated citizens when the endorser's incentives are clear. Once the endorser's incentives become less transparent, this cue no longer helps citizens to improve their decisions consistently, nor does it consistently close the gap between sophisticated and unsophisticated citizens.

- Two cues are not necessarily better than one cue. That is, when one cue enables citizens to achieve large improvements in their decisions, the presence of a second cue does not help citizens to improve their decisions further. However, when neither cue is particularly useful by itself, these cues help citizens to make better decisions than they make with only one cue.

- Competition between two experts does not necessarily induce both experts to make truthful statements; thus, citizens are unable to improve their decisions under these circumstances. Indeed, competition between experts only induces truthful statements and enables the citizen to learn once it is combined with institutions.

Taken together, my results demonstrate that cues are not equally effective for all citizens in all contexts.

Chapter 1

Citizen Competence and Cue Taking

One of the most hotly debated questions among political scientists is the question of whether citizens are competent to perform their duties. On the one hand, many scholars lament citizens' lack of factual knowledge about our government (Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960; Delli Carpini and Keeter 1996; Converse 1964; Bartels 1996; Neuman 1986). Indeed, ever since Berelson, Lazarsfeld, and McPhee (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; Neuman 1986; Bennett 1998; Delli Carpini and Keeter 1996). Based on this body of literature, many scholars question whether our democracy can possibly work, given that its citizens seem to lack the knowledge required for the decisions they must make. Indeed, if there are no ready substitutes for citizens' lack of political knowledge, then elections may not be decided by citizens, but rather by elites and the media.

Rather than condemn our democracy as unworkable, many other scholars emphasize that citizens can use cues to help them make informed political decisions (Popkin 1991; Conover and Feldman 1989; Lupia and McCubbins 1998; Lupia 1992, 1994; Kuklinski et al. 2001; Mondak 1993; Carmines and Kuklinski 1990; Mutz 1992; Lodge and Hamill 1986). Stated differently, these scholars suggest that regardless of citizens' levels of factual knowledge about politics, there are information shortcuts embedded in our political system that may help citizens to choose from the candidates and policies on the ballot in any given election. These scholars (like those discussed above) recognize that citizens may not possess detailed factual knowledge about politics. However, scholars in this camp suggest that citizens are able to use cues—such as party labels, polls, candidates' appearances, information about the economy, endorsements, etc.—as *substitutes* for factual knowledge about politics.

Although scholars convincingly demonstrate that citizens use cues when making political decisions, many of them do not assess the *conditions under which* cues help citizens to improve their decisions. Indeed, the implicit assumption throughout much of the literature on cue taking is that cues necessarily have beneficial effects and that they are equally effective for all citizens, at all times, and in all places. Given the vast differences among citizens and the wide variety of political contexts in which they may find themselves, it seems unlikely that cues are always effective substitutes for knowledge about politics. Indeed, recent research suggests that particular cues may be more or less effective under different circumstances and that the challenge for future researchers is to determine the conditions under which cues provide citizens with useful substitutes for detailed knowledge about politics (Sniderman, Brody, and Tetlock 1991; Kuklinski and Hurley 1994; Kuklinski and Quirk 2000; Lau and Redlawsk 2001; Kuklinski et al. 2001).

In my dissertation, I take steps toward identifying the conditions under which particular cues help citizens to improve their decisions. Specifically, I draw upon the cognitive science, psychology, economics, and political science literatures to develop a formal theory and conduct laboratory experiments that enable me to assess whether particular cues are equally effective for different types of citizens in different contexts.

My findings demonstrate that only under certain conditions do cues help citizens to improve their decisions. These conditions suggest that even though cues can be effective substitutes for detailed knowledge about politics, they are much more fragile than previous research suggests.

This introduction proceeds as follows. First, I briefly discuss the existing literature on cue taking. Then, I describe in detail the questions that the existing literature does not adequately address. Next, I discuss my approach to studying the conditions under which cues help citizens to improve their decisions. I conclude with a brief description of each chapter in my dissertation.

1. Existing Research on Cue Taking

One approach that scholars use to demonstrate that cues help citizens with their decisions is experimental in nature. That is, in the typical laboratory experiment on cue taking, scholars present subjects in an experiment with one particular cue, and they then examine the effect that the cue has on subjects' decisions, relative to the decisions of subjects that did not receive the cue. For example, many scholars use experiments to show that citizens use party labels as cues (Rahn 1993; Druckman 2001b; Lau and Redlawsk 2001; Tomz and Sniderman 2004), while several other scholars demonstrate experimentally that citizens use cues that the media and trusted endorsers provide (Lupia and McCubbins 1998; Druckman 2001a; Boudreau 2006, 2007). Still other scholars use experiments to show that citizens rely upon polls (Ceci and Kain 1982; Kaplowitz et al. 1983; Mutz 1992; Lau and Redlawsk 2001; Kam and Sommer 2006; Boudreau and McCubbins 2007) and candidates' appearances to help them make political choices (Kuklinski and Hurley 1994; Lau and Redlawsk 2001; Bailenson et al. 2006).

Another approach that scholars in the cue taking literature use is survey-based. Specifically, these scholars trade off the strong internal validity associated with laboratory experiments in order to gain the external validity that comes from asking random samples of the population to answer survey questions, such as those included on the American National Election Study's (ANES) survey and on the National Annenberg Election Survey (NAES). For example, using an online survey of over 1,000 nationally representative respondents, Ansolabehere and Jones (2005) demonstrate that citizens in the real world use party labels as cues. Similarly, Lupia (1994) and Mondak (1993) use survey evidence to demonstrate that trusted endorsers provide citizens with cues, while other scholars show that citizens rely upon polls (Bartels 1988; McAllister and Studlar 1991; West 1991; Hardy and Jamieson 2005), information about the state of the economy (Kinder and Kiewiet 1981), and candidates' appearances (Glass 1985) to help them make political choices. Given the remarkable consistency between these survey-based studies and the experimental studies that I discussed above, it is not surprising that it is now widely accepted that citizens use cues when making political decisions.

2. Gaps in Existing Research

As the preceding section illustrates, many scholars show that citizens use cues. Despite the richness of this literature and the many contributions that it has made to our understanding of how citizens make political decisions, it leaves several important questions either un-addressed or under-explored. Specifically, it does not address the following three questions, all of which must be answered if we are to understand fully the effects that cues have on citizens' decisions:

2.1 How do cues affect sophisticated versus unsophisticated citizens' decisions?

Several scholars assess whether cues are equally effective for both sophisticated and unsophisticated citizens (Rahn, Aldrich, and Borgida 1994; Lau and Redlawsk 2001; Sniderman, Brody, and Tetlock 1991; Kuklinski et al. 2001; see also Kam 2005), while others demonstrate that sophisticated and unsophisticated citizens tend to rely on different cues. For example, Sniderman, Brody, and Tetlock (1991) and Lau and Redlawsk (2001) suggest that sophisticated citizens are more likely to use cues that involve ideology and endorsements, while unsophisticated citizens are more likely to use cues that involve a candidate's appearance and party identification.

Although these scholars address the question of how cues affect sophisticated versus unsophisticated citizens, they have yet to converge upon a consistent account of how sophistication affects cue taking. For example, several scholars demonstrate that cues can close the gap between sophisticated and unsophisticated citizens (Kuklinski et al. 2001 and Rahn, Aldrich, and Borgida 1994). However, other scholars show that cues work best for the most sophisticated citizens and that they tend to have detrimental effects on unsophisticated citizens, thereby widening the gap between these two types of citizens (Lau and Redlawsk 2001). Thus, the question of whether and under what conditions cues close the gap between sophisticated and unsophisticated citizens remains hotly debated, yet unresolved.

2.2 Do multiple cues help citizens to improve their decisions, above and beyond the improvements that they achieve with only one of these cues?

Given the multitude of cues that are available to citizens, it is curious that relatively few scholars study how citizens use cues when there is more than one cue available. That is, much of the existing literature focuses on whether citizens can use a particular cue when it is presented to them *in isolation* and glosses over the question of whether and how citizens choose from and use the many different cues that exist in political contexts. Further, although several scholars do explore whether and how multiple cues affect citizens' decisions (see, e.g., Lau and Redlawsk 2001; Kuklinski and Hurley 1994; Huckfeldt et al. 2005), they do not address the question of whether and under what conditions two cues help citizens to improve their decisions, above and beyond the improvements that they achieve when only one cue is present and when they must make their decisions on their own. Answering this question is important not only because it will help us understand the choices that citizens make in the real world (where they do have multiple cues available to them), but also because it will allow us to identify the conditions under which particular cues (or combinations of cues) enable citizens to improve their decisions.

2.3 When can citizens learn from multiple speakers?

In many political, legal, and economic contexts, citizens must make decisions for which they are not fully informed. To do this, they must often rely upon the statements of others (Lupia and McCubbins 1998; Lupia 1992, 1994; Mondak 1993; Sniderman Brody and Tetlock 1991; Carmines and Kuklinski 1990). For example, when choosing among different candidates for office, uninformed citizens may rely upon the statements of politicians from competing political parties or upon the endorsements of trusted allies. Similarly, when learning about particular policy issues in deliberative settings, citizens are exposed to the statements of competing experts. When deciding a question at trial, jurors must rely upon the statements of competing attorneys and their witnesses, and when choosing among products, consumers often rely upon information provided by competing sellers and upon the opinions of trusted endorsers, such as Consumer Reports, the Better Business Bureau, and Good Housekeeping.

Given the frequency with which citizens are exposed to competing information sources in the real world, it is curious that relatively few scholars examine whether and under what conditions citizens can trust and learn from the statements of multiple speakers. Indeed, the vast majority of the theoretical and empirical literatures on this topic explore whether and under what conditions citizens can trust and learn from the statements of *one speaker* (Crawford and Sobel 1982; Calvert 1985; Lupia 1992, 1994; Mondak 1993; Austen-Smith 1994; Lupia and McCubbins 1998). Although there are, of course, many real world situations in which citizens only pay attention to and/or learn from one speaker, it is also important to understand whether and when they can learn from two competing speakers that they do not know. Indeed, given the large sums of money that scholars spend creating deliberative settings in which citizens are expected to learn from the statements of competing experts and politicians, it is crucial to analyze (both theoretically and empirically) whether and under what conditions such learning among citizens can occur.

3. My Research

I address each of these three questions in my dissertation. Specifically, in my first two chapters, I use laboratory experiments to assess the conditions under which particular cues help citizens to improve their decisions. Although laboratory experiments always involve a tradeoff between internal validity and external validity (Trochim 2001), they are they are in many ways ideally suited for the questions that I seek to answer.[1] Specifically, in survey-based research, scholars often do not know whether individuals receive particular cues, and they must also deal with the potentially confounding events that occur during the course of campaigns (see Hardy and Jamieson 2005 for further discussion). Thus, it is often difficult (if not impossible) to isolate the effects that cues alone have on citizens' decisions and to determine whether they help citizens to make better decisions than they would have made on their own. These limitations also often prevent scholars from analyzing which types of citizens (i.e. unsophisticated versus sophisticated) are better able to use particular cues.

In my third chapter, I develop a formal model of a communication game between two competing speakers, and I then test that model's predictions experimentally. By developing a formal model, I am able to analyze theoretically the

[1] Specifically, the main advantage of conducting my experiments in a controlled environment and randomly assigning subjects to treatment and control groups is that I am able to make internally valid causal inferences. For these very same reasons, however, my experiments are much weaker in external validity. In order to compensate for this, at a later date I plan to combine the results of these experiments with related quasi-experimental studies (which are weaker in internal validity but much stronger in external validity).

conditions under which two competing speakers have an incentive to make truthful statements to another individual (for example, a citizen), as well as the conditions under which a citizen can trust and then learn from these two speakers' statements. Then, by conducting laboratory experiments that correspond to key features of my model, I am able to assess the extent to which the model's predictions hold up empirically.

4. Overview of My Dissertation

My three empirical chapters build upon one another and explore the conditions under which cues help citizens to improve their decisions. Specifically, in Chapter 1, I analyze whether and under what conditions a *single cue* helps two different types of citizens (i.e. sophisticated versus unsophisticated) to improve their decisions. In Chapter 2, I examine whether *multiple cues* enable citizens to improve their decisions above and beyond the improvements they achieve with only one cue. In Chapter 3, I assess the conditions under which citizens can learn from *multiple speakers*. Although these three chapters build upon another, they are written as stand-alone articles. Thus, there are portions of text and one table that repeat across the three chapters. A brief description of each chapter is provided below.

<u>Chapter 1:</u> Closing the Gap: When Do Cues Eliminate Differences Between Sophisticated and Unsophisticated Citizens?

In this chapter, I analyze experimentally whether and under what conditions one particular cue (namely, the statements of an endorser) enables both sophisticated and unsophisticated citizens to make better choices than they would have made on their own. The results of my experiments demonstrate that this cue only improves the decisions of both sophisticated and unsophisticated citizens when the endorser's incentives are clear. Once the endorser's incentives become less transparent (as is often the case in the real world), this cue no longer helps citizens to improve their decisions consistently, nor does it consistently close the gap between sophisticated and unsophisticated citizens. Thus, my results demonstrate the fragility of this cue, as only under idealized circumstances does it consistently allow both sophisticated and unsophisticated citizens to improve their decisions.

Chapter 2: Are Two Cues Better Than One? Assessing Whether Multiple Cues Improve Citizens' Decisions

It is widely known that citizens use cues as substitutes for detailed knowledge about politics. However, given the multitude of cues that exist in the real world, it is curious that relatively few scholars study the effects of cues when there is more than one available to citizens. Thus, I conduct laboratory experiments in which I provide citizens with several different cues, and I assess whether multiple cues help citizens to improve their decisions, above and beyond the improvements that they achieve when only one of these cues is present. My results suggest that *two cues are not necessarily better than one*. That is, when one cue enables citizens to achieve large improvements in their decisions, the presence of a second cue does not allow citizens to improve their decisions further. However, when neither cue is particularly useful by itself, citizens use these cues in a way that enables them to make better decisions than they make when only one of these cues is present. Thus, only under certain conditions do multiple cues help citizens to improve their decisions.

<u>Chapter 3:</u> From Competition to Competence? Theory and Experiments Regarding Deliberation and Citizen Learning

Many scholars propose deliberation as a remedy for our politically uninformed citizenry. Specifically, these scholars emphasize that bringing citizens together, exposing them to the views of competing experts and politicians, and then letting them discuss those views among themselves will help citizens to learn about politics and develop more informed opinions about particular policy issues. In this chapter, I analyze whether and under what conditions competition between experts reveals truthful information and enables citizens to make informed choices. Specifically, I develop a formal model of a communication game between two competing experts and a citizen, and I test my model's predictions experimentally. The results of my model and experiments demonstrate that competition 1) does not necessarily induce both experts to send truthful signals to the citizen, 2) prevents the citizen from learning from the experts' signals, and 3) only induces truthful signals and enables the citizen to learn when it is combined with institutions, such as a penalty for lying or a threat of verification.

5. Conclusion

My dissertation builds upon existing research on cue taking by analyzing the conditions under which cues help different types of citizens to improve their decisions in different contexts. Taken together, my theoretical and empirical results cast doubt on an assumption (implicit in much of the literature on cue taking) that cues necessarily improve citizens' decisions. Indeed, my findings suggest that cues are *not* equally effective for all citizens, under all circumstances; rather, there appear to be limits to the extent to which cues help citizens to improve their decisions. By identifying some of these limits, my research provides a more complete account of how cues affect citizens' decisions and takes an initial step toward developing a theory of when citizens can use cues to improve their decisions.

Chapter 2

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

> "We take issue with the assumption, now prominent in the literature, that citizens' use of cues, specifically taking cues from political elites, serves the former well...[W]e need to identify the conditions under which taking cues from elites does and does not serve the interests of the electorate."
>
> --Kuklinski and Hurley 1994, p. 729-730

When do cues improve citizens' political decisions? Ever since Downs (1957) emphasized that it is irrational for citizens to invest time and effort to become politically informed, scholars have demonstrated time and again that cues help citizens to make reasoned choices at election time (Popkin 1991; Lupia 1992, 1994, forthcoming; Lupia and McCubbins 1998; Kinder and Kiewiet 1981; Druckman 2001c; Mutz 1992; Lodge and Hamill 1986; Mondak 1993). Although cues (such as party labels and the statements of trusted endorsers) certainly aid citizens with their political choices, it is unlikely that these cues improve the decisions of all citizens, in all places, and at all times. Indeed, recent research suggests that particular cues may be more or less effective under different circumstances and that the challenge for future researchers is to identify the *conditions under which* cues provide citizens with effective substitutes for detailed knowledge about politics (Sniderman, Brody, and Tetlock 1991; Kuklinski and Hurley 1994; Kuklinski and Quirk 2000; Lau and Redlawsk 2001; Kuklinski et al. 2001; Kam 2005).

I take up such a challenge in this paper. That is, rather than assume that cues necessarily have beneficial effects, I explore experimentally whether and when one particular cue—namely, the statements of an endorser—helps 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 his or her incentives are unclear (as is often the case in real world political settings[2]), and I also assess whether citizens can learn from an endorser's statements even when they lack the sophistication that is required for the decisions they must make.

Given that I cannot systematically manipulate the clarity of an endorser's incentives or citizens' levels of sophistication in real world electoral settings, I instead conduct laboratory experiments (extending Lupia and McCubbins's 1998 experimental design) in which I analyze whether and under what conditions citizens can learn from an endorser's statements and improve their decisions. Specifically, I obtain a pretest measure of subjects' levels of sophistication prior to my experiment. I then randomly assign subjects to either a treatment group or a control group, which enables me to manipulate whether subjects are exposed to the endorser's statements before they make their decisions. That is, subjects in my control group must make their decisions on their own, while subjects in my treatment group hear the endorser's statements before they must make the very same decisions that subjects in my control

[2] Although some endorsers' incentives may be clear to voters (i.e. many citizens know that the Sierra Club wants environmentally-conscious candidates to be elected), other endorsers' incentives may be less transparent. For example, when Tom Vilsack (the former governor of Iowa who recently dropped out of the 2008 presidential race) endorses Hillary Clinton, it is not clear whether his endorsement communicates which candidate is best for citizens or whether he is simply trying to further his own political ambitions.

group make. In this way, my experiments allow me to assess whether the endorser's statements help both sophisticated and unsophisticated citizens to make better decisions than they would have made on their own.

I next examine the conditions under which citizens are able to learn from the endorser's statements and, thereby, improve their decisions. To this end, I first give the endorser knowledge about which choice will make the other subjects better off. Then, I make the endorser's incentives clear to subjects, and I assess whether subjects can learn from the endorser's statements under such idealized conditions. Specifically, in my idealized experimental conditions, the endorser, in equilibrium, always has an incentive to tell the truth about the correct choice for subjects; thus, I expect subjects to trust the endorser's statements, learn from them, and improve their decisions. However, because an endorser's incentives may not be so transparent in real world settings, I next assess whether and when subjects can learn from the endorser's statements even though, in equilibrium, the endorser may have an incentive to lie. I also break my results down by subjects' levels of sophistication to examine whether this cue is equally effective for both sophisticated and unsophisticated subjects.

As my results demonstrate, when the endorser's incentives are clear (as is the case in my idealized experimental conditions), subjects are able to learn from the endorser's statements and improve their decisions. However, once the endorser's incentives become less transparent (which is the case in several of my other, more realistic experimental conditions), subjects are no longer able to improve their decisions consistently. In a similar manner, although both sophisticated and unsophisticated subjects are able to learn from the endorser's statements under idealized conditions, neither sophisticated nor unsophisticated subjects are able to do so consistently under more realistic conditions. In this way, my experimental results demonstrate the *fragility* of this cue, as only under idealized circumstances does it consistently allow both sophisticated and unsophisticated subjects to improve their decisions.

This paper proceeds as follows. I begin with a review of the literature on cues, emphasizing that although many scholars demonstrate that citizens use cues when making political choices, relatively few assess *the conditions under which* cues actually improve the decisions that citizens make. I then describe in detail the research design that I use in my experiments, and I propose a number of testable hypotheses. Next, I briefly describe the data source and statistical methods that I use, and I also comment on the generalizability of my results. I then present my experimental results, which demonstrate the conditions under which sophisticated and unsophisticated subjects can learn from an endorser's statements and improve their decisions. I conclude with a discussion of the implications that my research has for debates about cues, in general, and about the limitations of cues, in particular.

1. The Debate: Do Cues Improve Decisions?

1.1 Cues as Unconditional Substitutes for Sophistication

In response to scholars who lament citizens' lack of factual knowledge about politics, [3] many scholars argue that cues can substitute for factual knowledge about politics and enable citizens to make reasoned choices at election time (Popkin 1991; Page and Shapiro 1992; Conover and Feldman 1989; Lupia and McCubbins 1998; Lupia 1992, 1994; Kuklinski et al. 2001; Mondak 1993; Fiorina 1981; Kinder and Kiewiet 1981; Druckman 2001c; Mutz 1992; Lodge and Hamill 1986). For example, many scholars emphasize that party labels are cues that provide citizens with valuable information about candidates' policy preferences (Popkin 1991; Page and Shapiro 1992; Sniderman, Brody, and Tetlock 1991), while others suggest that citizens can learn what they need to know from the media, from polls, and from the statements of the trusted endorsers that often exist in political settings (Mondak 1993; Lupia 1994; Lupia and McCubbins 1998; Druckman 2001c; Lau and Redlawsk 2001). Although these scholars convincingly demonstrate that citizens use cues when making political decisions, many of them do not assess the *conditions under which* cues help citizens to make better decisions than they would have made on their own.[4] Indeed, the implicit assumption throughout much of this literature is that cues necessarily have beneficial effects and that they are equally effective for all citizens, at all times, and in all places.

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[3] See, e.g., Berelson, Lazarsfeld, and McPhee (1954), Campbell et al. (1960), Converse (1964), Neuman (1986), Delli Carpini and Keeter (1996), Bartels (1996), and Bennett (1998).

[4] For an important exception to this statement, see Lupia and McCubbins (1998).

1.2 The Conditionality of Cues

In contrast to those who assume that cues necessarily have beneficial effects, several scholars investigate the conditions under which cues help citizens to improve their decisions. Specifically, scholars in this camp address the question, “Who uses which cues when?” As for *who* can use cues, scholars primarily focus on whether cues are equally effective for both sophisticated and unsophisticated citizens (Lau and Redlawsk 2001; Sniderman, Brody, and Tetlock 1991; Kuklinski et al. 2001; see also Kam 2005). Similarly, when analyzing *which* cues citizens use, scholars demonstrate that sophisticated and unsophisticated citizens tend to rely on different cues. For example, sophisticated citizens are more likely to use cues that involve the use of abstract ideology and endorsements, while unsophisticated citizens are more likely to use cues that involve a candidate’s appearance and party identification (Sniderman, Brody, and Tetlock 1991; Lau and Redlawsk 2001). 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 et al. 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 analyze experimentally the conditions under which both sophisticated and unsophisticated citizens can learn from an endorser and improve their decisions. Although my research draws upon the experimental designs of Lupia and McCubbins (1998) and the insights of Kuklinski and his various coauthors, Lau and Redlawsk (2001), and Sniderman, Brody, and Tetlock (1991), it makes a number of new contributions to the literature and has several important advantages.

The first advantage of my design stems from the nature of the choices that subjects make in my experiments. Specifically, instead of asking subjects to vote for fictional candidates or policy issues, I ask subjects to make choices about math problems (that is, subjects are asked to 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 policy issues) provides a straightforward way to identify correct decisions and to assess whether and when the statements of an endorser induce an improvement in decision making. Stated differently, although it is often difficult to identify when citizens have chosen the "correct" candidate or policy,[5] 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 me with a valid and reliable measure of subjects' sophistication at making this type of decision. Indeed, although there does not exist an agreed upon measure of political sophistication (see, e.g., Luskin 1987; Krosnick 1990), 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

[5] Despite the difficulties associated with identifying when citizens vote for the "correct" candidate, Lau and Redlawsk (1997, 2001) and Payne, Bettman, and Johnson (1993) have developed several different measures that are designed to assess the correctness of citizens' votes.

to the experiment, which enables me to assess whether the endorser's statements help both sophisticated and unsophisticated subjects to improve their decisions.

The third advantage of my design is that math problems (even though they do not *look* like political decisions on the surface) are sometimes more similar to real world political decisions than are many of the tasks that scholars typically ask subjects to perform in a political experiment (e.g. voting for fictional candidates, predicting the outcomes of coin tosses, etc.). For example, in real world politics, citizens are not blank slates when they go to the ballot box; that is, they have some preexisting knowledge or beliefs about the candidates 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 correct choice because they have preexisting knowledge about how to solve math problems. The same cannot always be said for experiments that ask subjects to make choices about fictional candidates. Indeed, because experimenters invent the fictional candidates and their characteristics, it is often impossible for subjects to know or believe anything about them prior to making their choices in the experiment.

Further, in real world politics, there is something at stake for citizens when they make their decisions, but the stakes are not very large (see Hibbing and Alford 2004 and Hibbing and Theiss-Morse 2002 for a discussion of how typical political issues are low stakes games for most Americans). Similarly, there is something at stake for subjects in my experiments because they earn money if they make the correct choice and lose money if they make the incorrect choice. As in real world politics, the stakes in my experiments are not very large, as subjects either earn or lose 50 cents for each decision that they make. When voting for fictional candidates, however, there is often nothing at stake for subjects in the experiment. Indeed, because the candidates are fictional, they cannot be elected to office or affect the subjects in any real way. And, because scholars using fictional candidates often do not pay subjects for their choices (perhaps because it is difficult to identify "correct" choices in this type of experiment), there is nothing on the line for subjects when they make their decisions.[6]

Finally, several scholars suggest that real world political decisions have an emotional or affective component (Lupia and Menning 2006; Lodge and Taber 2000; Marcus et al. 2000; Rahn 2000). Although some real world political decisions are likely to be more emotional than others (i.e. national level elections may stimulate greater emotional or affective responses than local elections), the same is true of the math problems that I use in my experiments. Indeed, a great deal of work in psychology and education has demonstrated that individuals often have emotional responses when solving math problems and that the nature of the emotional response depends upon the problem, as well as the heuristic strategy that individuals use when solving the problem (McLeod and Adams 1989; McLeod 1989). The same cannot necessarily be said of voting for fictional candidates because there may not be anything emotional about choosing individuals who do not really exist and who cannot affect the subjects. Thus, although voting for fictional candidates *looks* like politics on the surface and captures some important elements of real world politics (for example, there may be some uncertainty when voting for fictional candidates, and there may also be subjects who vary in their levels of sophistication), this approach does not

[6] For an important exception to this statement, see Merolla (2004).

necessarily capture several key elements of real world politics that math problems do capture. (See Table 2.1 for a summary of how math problems are analogous to real world political decisions.)

Table 2.1. How Math Problems Are Analogous to Political Choices

Real World Politics	Math Problems	Fictional Candidates
Uncertainty	✓	✓
Varied Levels of Sophistication	✓	✓
Preexisting Knowledge	✓	
Small Stakes	✓	
Emotions and Affect	✓	

2. Research Design

In order to analyze the conditions under which citizens learn from the statements of an endorser, I design a two-group randomized experiment.[7] Specifically, I obtain a pretest measure of subjects' sophistication prior to the

[7] All of the experimental materials that were used in these experiments are available from the author. Please email Cheryl Boudreau at clboudreau@ucsd.edu if you would like to see copies of the scripts, handouts, and other materials that were used.

experiment (i.e. their SAT math scores), and I then randomly assign subjects to treatment and control groups. I next ask subjects to solve a series of binary choice math problems. The math problems that I use are drawn from an SAT math test and consist of many different types of problems (i.e. algebra, geometry, calculus) and several levels of difficulty. I tell subjects in both the treatment and control groups that they have 60 seconds to solve each math problem and that they will earn 50 cents for each problem that they answer correctly, that they will lose 50 cents for each problem that they answer incorrectly,[8] and that they will neither earn nor lose 50 cents if they leave a problem blank.[9]

The main difference between the treatment and control groups has to do with the conditions under which subjects solve the math problems. In the control group, subjects solve 24 math problems one at a time, with 60 seconds allotted for each problem. For each problem that subjects in the control group solve, I pay them according to whether they solve the problem correctly, incorrectly, or leave the problem blank.

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[8] Note that subjects earn money before they begin solving the math problems in my experiments. Specifically, all subjects receive 5 dollars for showing up on time to the experiment, and they also earn money by solving simple practice problems and by taking quizzes on the instructions that I read aloud. Also, just before subjects solve the math problems in the experiment, I tell them 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 that they can actually lose money in the experiment.

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

In the treatment group, subjects solve the very same 24 math problems one at a time, and they are also paid according to whether they solve the problem correctly, incorrectly, or leave the problem blank. What differs between the treatment and control groups, however, is that before subjects in the treatment group solve any of the math problems, the experimenter randomly selects one subject to act as "the endorser" in the remainder of the experiment. 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 still to solve the 24 math problems one at a time), the endorser is shown the correct answer to each math problem (that is, the endorser is given knowledge about the correct choice for subjects) and then makes a statement to the other subjects about the answer to the math problem.[10] After the endorser makes his or her statement, the other subjects in the treatment group are given 60 seconds to solve that particular math problem.

The key to this experiment is that both the endorser and the subjects know that the endorser can make any statement that he or she wishes. That is, the endorser can lie about the correct answer to the math problem or tell the truth; it is entirely up to him or her. The endorser's ability to make whatever statement he or she wishes is constant throughout this experiment and is designed to be an analogy to Crawford and

[10] Note that the endorser makes his or her statement by putting a checkmark beside the answer that he or she wishes to report. I, as the experimenter, then read that statement out loud to all of the subjects. This prevents the endorser's tone of voice from confounding the experiment. Similarly, throughout the experiment, the endorser sits behind a large partition so that the endorser's gender, race, and/or age do not affect the extent to which subjects listen to the endorser's statements. These personal characteristics of the endorser are, of course, interesting factors that I plan to vary in future experiments.

Sobel's (1982) and Lupia and McCubbins's (1998) models, as well as to many real world political situations.

Although the endorser can lie or tell the truth in all of my experimental conditions, I vary the conditions under which the endorser makes his or her statement. Specifically, I vary two things: 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 first analyze subjects' ability to solve the math problems when the endorser has *common interests* with them. This experimental 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. Then, in other experimental 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 experimental 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.

However, 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 when running my experiments. Specifically, I test the effects that different size penalties (namely, a $15 penalty, a $5 penalty, and a $1 penalty) and different probabilities of verification (specifically, a

100%, 90%, 70%, 50%, or 30% chance of verification) have on subjects' ability to solve the math problems correctly. Each of these experimental variations (i.e. common interests, penalties for lying, and verification) is common knowledge at the outset of each part of the experiment, and each alters how clear the endorser's incentives are to the subjects. (For a list of how each experimental condition affects the endorser's incentives and for a preview of how effective each condition is at improving subjects' decisions, see Table 2.2.)

Table 2.2. How Subjects Learn from the Endorser's Statement under Different Conditions

Endorser's Incentives:	Corresponding Experimental Condition:	Outcome (Relative to Control Group):
Clear (Endorser always has an incentive to tell the truth, in equilibrium)	*Common Interests*	Improved decisions
	$15 Penalty for Lying	Improved decisions
	100% Chance of Verification	Improved decisions
Unclear (Endorser may have an incentive to lie to subjects, in equilibrium)	*$5 Penalty for Lying*	No improvement
	$1 Penalty for Lying	Small improvement
	90% Verification	Small improvement
	70% Verification	No improvement
	50% Verification	Small improvement
	30% Verification	No improvement

So how do I vary the interests between the endorser and subjects and impose the institutions within the context of my experiments? In short, I vary both interests and institutions by manipulating the ways that the endorser 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 endorser is paid 50 cents for each subject who solves a particular math problem correctly. For example, if 11 subjects answer the 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).

For my next experimental variation, I establish conflicting interests between the endorser and subjects, and I then impose one of my institutional conditions. 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 and every 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 one of my institutional conditions—namely, a penalty for lying. So, in this segment of the experiment, 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 experimental condition) if he or she lies about the correct answer to the math problem.

For my other institutional condition—namely, verification—I again maintain conflicting interests between the endorser and the subjects. However, instead of imposing a penalty for lying upon the endorser, this time, 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 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 the 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 the 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. In this way, 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 in each of these experimental conditions 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 experimental condition, and I then compare the amounts of money that subjects earn in each experimental condition and in the control group. So, 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.

3. Hypotheses

3.1 Idealized Conditions

Several of the experimental conditions described above make the endorser's incentives clear to subjects and, therefore, yield predictions about the relative amounts of money that subjects in the treatment and control groups will earn. For example, in the presence of a knowledgeable endorser who has common interests with subjects, I predict that subjects will earn greater amounts of money, 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 knowledgeable speaker (who in my experiments is the endorser) and receivers (who in my experiments are the subjects) induce the speaker to tell the truth and the receivers to base their choices upon what the speaker says.

The reasoning behind this common interests prediction is perhaps best understood by considering the nature of my experiments. Recall that in the common interests condition, the endorser earns money each time a subject solves a particular problem correctly. I, therefore, expect the endorser to tell subjects the truth about the correct answer to the math problem, and I also expect subjects to trust the endorser's statement and base their choices upon it. Indeed, because subjects and the endorser are better off financially if the subjects solve the problems correctly, the endorser has a clear incentive to tell subjects the truth. Thus, I offer the following hypothesis:

Idealized Common Interests Hypothesis: Subjects exposed to a knowledgeable endorser who has common interests with them will earn greater amounts of money, relative to subjects in the control group.

In the $15 penalty for lying condition, I also expect subjects to earn greater amounts of money than do subjects in the control group. This prediction, again, 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 receivers trust and then learn from the speaker's statements.

In my experiments, the $15 penalty for lying is in fact "sufficiently large"—that is, it is large enough to ensure that the endorser always has an incentive to tell the truth and that the subjects know that the endorser has this incentive. To see why this is the case, consider the way that the endorser earns money under this condition when there are 11 subjects solving the math problems.[11] Given that there are conflicting interests between the endorser and the subjects, the endorser earns $5.50 if each and every subject answers a problem incorrectly. Although at first blush this might seem to give the endorser an incentive to lie, note that the $15 penalty for lying will reduce the endorser's gain of $5.50 down to a loss of $9.50. Further, if the endorser lies and all of the subjects happen to answer the problem correctly (perhaps realizing that the endorser is lying), then the endorser will lose $20.50 (i.e. a $15 loss because of the

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[11] Indeed, the typical number of subjects in each experimental session was 12 (11 subjects who solved the math problems and 1 endorser who made a statement to the other subjects).

penalty for lying and a $5.50 loss because 11 subjects answered the problem correctly).

If the endorser tells the truth, however, then the worst he or she can do is to lose $5.50 (which will happen if each and every subject answers the problem correctly), and the best that he or she can do is to earn $5.50 (which will happen if each and every subject answers the problem incorrectly). As these payoffs make clear, the endorser is always better off if he or she tells the truth about the correct answer to the math problem. Thus, I expect that the endorser will make a truthful statement about the correct answer to the math problem, and I also expect subjects to trust the endorser's statement. Hence, I advance the following hypothesis:

Idealized Penalty for Lying Hypothesis: Subjects exposed to a knowledgeable endorser who faces a $15 penalty for lying will earn greater amounts of money, relative to subjects in the control group.

In the 100% chance of verification condition, I also expect subjects to earn greater amounts of money, 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.[12] Thus, when the endorser is verified with certainty (as is the case in this idealized experimental condition), subjects should trust that the endorser's statement is correct and base their choices upon it. For this reason, I make the following prediction:

[12] Note that Austen-Smith (1994) also models verification, but he focuses on the endorser's inability to verify his or her lack of knowledge to receivers.

Idealized Verification Hypothesis: Subjects exposed to a knowledgeable endorser who is verified with certainty will earn greater amounts of money, relative to subjects in the control group.

3.2 More Realistic Conditions

As for the other, more realistic conditions that I impose in my experiment (i.e. a $5 penalty, a $1 penalty, and a 90%, 70%, 50%, or 30% chance of verification), I do not have any *ex ante* predictions about how they will affect the amounts of money that subjects earn. The reason for this lack of predictions is simply that, under each one of these conditions, the endorser's incentives are less transparent than in the idealized experimental conditions. Specifically, the endorser may earn more money if he or she lies about the correct answer to the math problem. Knowing this, subjects solving the math problems may or may not trust the endorser's statements.

Indeed, whether the endorser tells the truth in these experimental conditions and whether the subjects trust the endorser's statements hinges critically upon their beliefs about one another. For example, in the $1 penalty for lying condition, if the endorser believes that a particular math problem is easy (easy in the sense that most of the subjects will solve the math problem correctly on their own), then the endorser should tell the truth about the correct answer to avoid the $1 penalty. Similarly, if the subjects believe that the endorser believes that the math problem is easy, then they should trust the endorser's statement if they cannot solve the problem on their own. Because the endorser's and subjects' behavior in these experimental conditions depends upon their beliefs about each other and upon their beliefs about the difficulty of the math problems, I must simply observe *ex post* the amounts of money that subjects earn in these conditions.

3.3 Sophisticated versus Unsophisticated Subjects

I also do not have any *ex ante* predictions about the relative amounts of money that sophisticated versus unsophisticated subjects will earn in each experimental condition. Indeed, it is possible that the endorser's statements will be more helpful to sophisticated subjects because these subjects already know something (and in some cases a lot) about the decisions that they must make. On the other hand, it also seems possible that this cue will be more helpful to unsophisticated subjects, who might be more willing to listen to the endorser, as opposed to trying to make their decisions on their own.

Further, the existing literature has yet to converge upon a consistent account of how sophistication affects cue taking. For example, several scholars demonstrate that cues can close the gap between sophisticated and unsophisticated citizens (Kuklinski et al. 2001 and Rahn, Aldrich, and Borgida 1994). However, other scholars 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). Given these contradictory results (and in order to shed light on this debate), I break down my results to examine the amounts of money that sophisticated and unsophisticated subjects earn in each experimental condition.

4. Methodology

In order to test the above hypotheses and to assess the effects that the endorser's statements have on sophisticated and unsophisticated citizens' decisions, I conducted laboratory experiments at a large public university. When recruiting subjects, I posted flyers at various locations on campus (for example, in front of the library, in the cafeterias, in the dormitories, and in academic buildings), and I also sent out campus-wide emails to advertise the experiments. A total of 369 adults who were enrolled in undergraduate classes and who were of different genders, ages, races, and college majors participated in the experiments.

Because I use college undergraduates as my source of data and because I ask these undergraduates to make decisions about math problems, my results actually *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 in the experiment. 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, of course, 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 the undergraduates who attend this university.[13] It is these differences between the undergraduates that I use in my experiments and members of the general population that cause me to underestimate the effectiveness of this particular cue. Indeed, if I had used members of the general population in my experiments, then there would have been much more room for the endorser's statements to effect an improvement in subjects' decisions.[14]

When analyzing the data gleaned from these experiments, I simply compare the amounts of money that subjects earn in each experimental condition and in the control group. Specifically, I conduct difference of means tests to examine whether subjects who are exposed to the endorser's statements in each condition earn significantly more money than do subjects in the control group.[15] 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

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[13] That said, the college undergraduates who participated in my experiments vary greatly in their levels of sophistication. Specifically, their SAT math scores range from 450 (the 27th percentile) to 800 (a perfect score).

[14] Of course, laboratory experiments always involve a tradeoff between internal validity and external validity (Trochim 2001). Specifically, the main advantage of conducting my experiments in a controlled environment and randomly assigning subjects to treatment and control groups is that I am able to make internally valid causal inferences. For these very same reasons, however, my experiments are much weaker in external validity. In order to compensate for this, at a later date I plan to combine the results of these experiments with related quasi-experimental studies (which are weaker in internal validity but much stronger in external validity).

[15] Note that I also analyzed my data using nonparametric Wilcoxon Mann-Whitney tests. Unlike the standard difference of means test, the Wilcoxon Mann-Whitney test does not require the assumption that the differences between two samples are normally distributed. None of the results reported in this paper change if the Wilcoxon Mann-Whitney test is used instead of a difference of means test.

individuals to improve their decisions and, by extension, the amount of money that they earn.

5. Results

5.1 Improved Decisions under Idealized Conditions

As Figure 2.1 demonstrates, each of my predictions for my idealized experimental conditions is borne out in the data. That is, subjects who are exposed to an endorser who has 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, subjects in the control group earn, on average, only $0.13 per problem (N = 66), while subjects who are 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 = 68), while subjects who are exposed to an endorser who faces a 100% chance of verification earn, on average, $0.39 per problem (N = 74). Each of these differences between treatment and control group subjects is statistically significant (specifically, $p < 0.001$ for each of these comparisons).

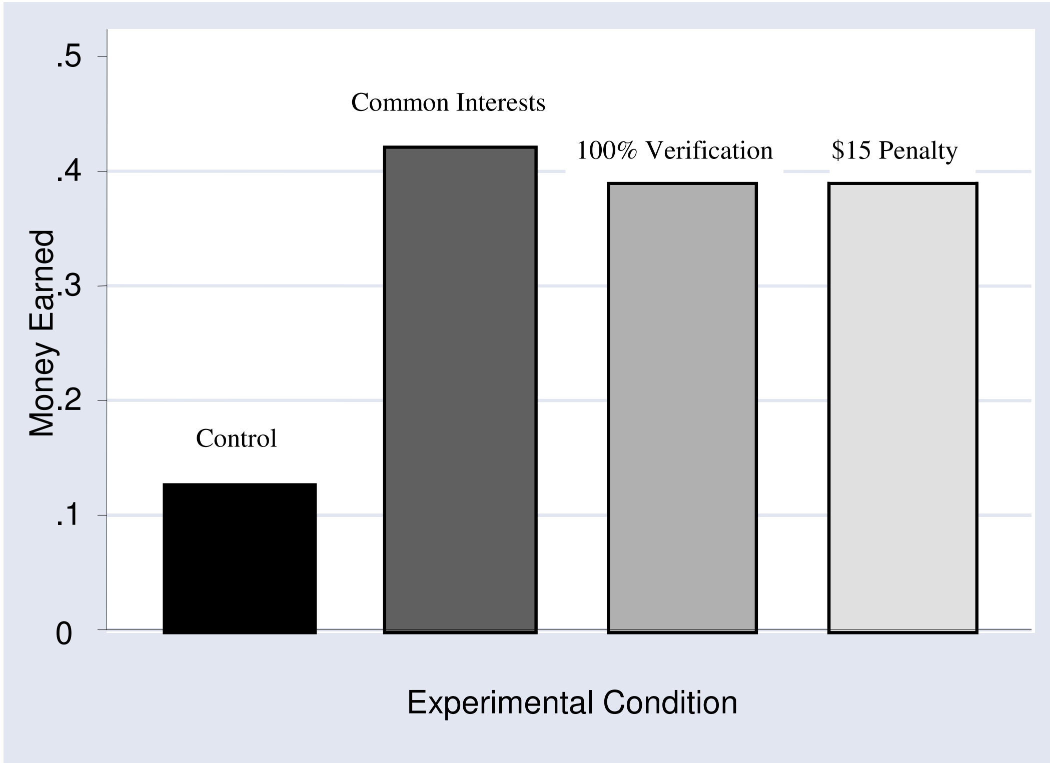

Figure 2.1 The Effects of Common Interests, a $15 penalty, and a 100% Chance of Verification on the Amount of Money Earned

5.2 Smaller Penalties, Smaller Improvements

Having confirmed each of my three hypotheses, I now examine the effects that smaller penalties for lying have on the amounts of money that subjects earn. Indeed, because penalties for lying in real world political settings may not always be large enough to ensure that endorsers tell the truth,[16] I investigate experimentally the effects that smaller penalties (which may make the expected value of lying exceed the

[16] For example, if an endorser believes that citizens will be fooled by a lie about a particular candidate or policy and if this endorser expects this lie to help him achieve a benefit (for example, getting his preferred candidate elected to office) whose value is greater than the small penalty that he will suffer (for example, a loss of reputation), then the endorser will have an incentive to lie to citizens.

expected penalty and, therefore, induce the endorser to lie[17]) have on the amounts of money that subjects earn.

As Figure 2.2 reveals, subjects in the smaller penalties for lying conditions do not consistently earn greater amounts of money than do subjects in the control group. Specifically, a difference of means test reveals that the $0.10 per problem that subjects earn in the $5 penalty for lying condition (N = 39) is not significantly greater than the $0.13 per problem that subjects in the control group earn. Although the $0.17 that subjects earn per problem in the $1 penalty for lying condition (N = 141) is significantly greater than the amount of money that subjects in the control group earn (p = 0.04), it is a much smaller improvement than the one that subjects in the $15 penalty for lying condition achieve (recall that subjects in the $15 penalty for lying condition earn, on average, $0.39 per problem). Thus, it appears that these smaller, but more realistic penalties for lying are not nearly as effective at improving the decisions that subjects make.

[17] For further discussion of this point, see Lupia and McCubbins (1998).

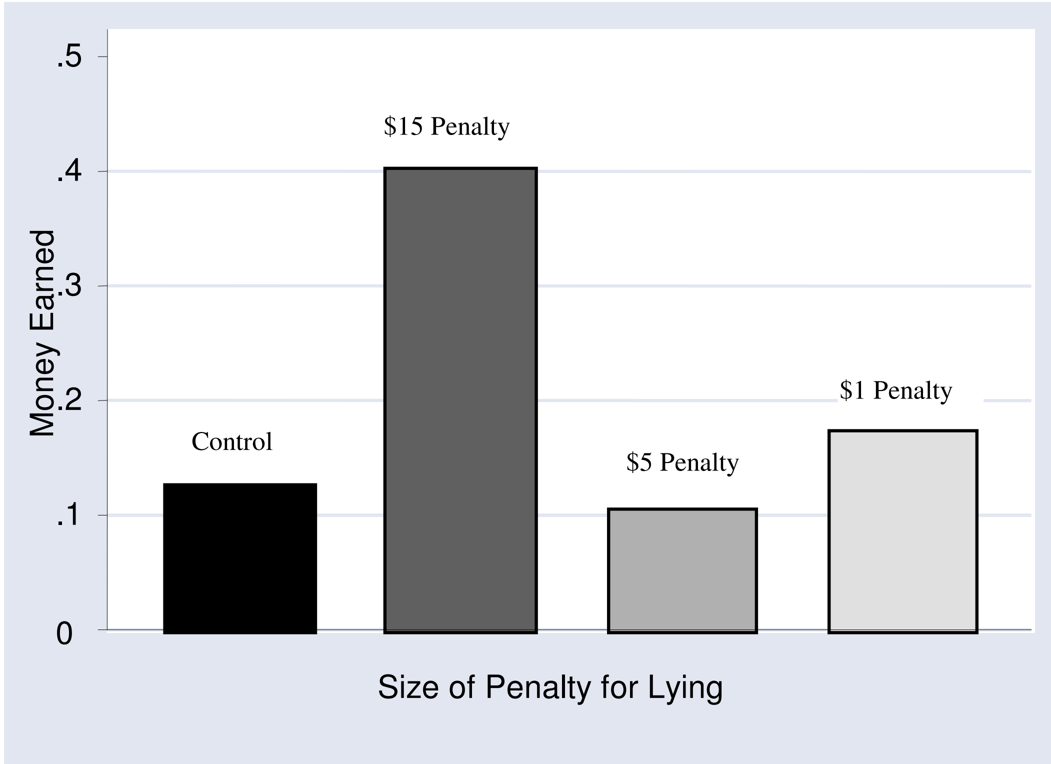

Figure 2.2. The Effects that Reducing the Penalty for Lying has on the Amount of Money that Subjects Earn

5.3 Smaller Probabilities of Verification, Smaller Improvements

Just as I examine the effects that reducing the size of the penalty for lying has on the amounts of money that subjects earn, so too do I analyze the effects of reducing the probability of verification. Indeed, given that a 100% chance of verification is unlikely to occur in real world political settings, it seems particularly useful to examine the extent to which subjects are able to learn from the endorser's statements when the probability of verification is reduced and when the endorser, therefore, may have an incentive to lie. Thus, I assess whether and to what extent 90%, 70%, 50%, and 30% chances of verification reduce the amounts of money that subjects earn.

As shown in Figure 2.3, subjects in the smaller chances of verification conditions do not consistently earn greater amounts of money than do subjects in the control group. Specifically, a difference of means test reveals that the $0.16 that subjects earn in the 70% chance of verification condition (N = 83) and the $0.08 that subjects earn in the 30% chance of verification condition (N = 85) are not significantly greater than the $0.13 that subjects in the control group earn. And, although the $0.25 that subjects earn in the 90% chance of verification condition (N = 33) and the $0.23 that subjects earn in the 50% chance of verification condition (N = 31) are significantly greater than the amount of money that subjects in the control group earn ($p < 0.001$ and $p = 0.002$, respectively), they are much smaller improvements than the one that subjects in the 100% chance of verification condition achieve (recall that subjects in the 100% chance of verification condition earn, on average, $0.39 per problem). Thus, it appears that these smaller chances of verification, like the smaller penalties for lying, are not nearly as effective at improving subjects' decisions.

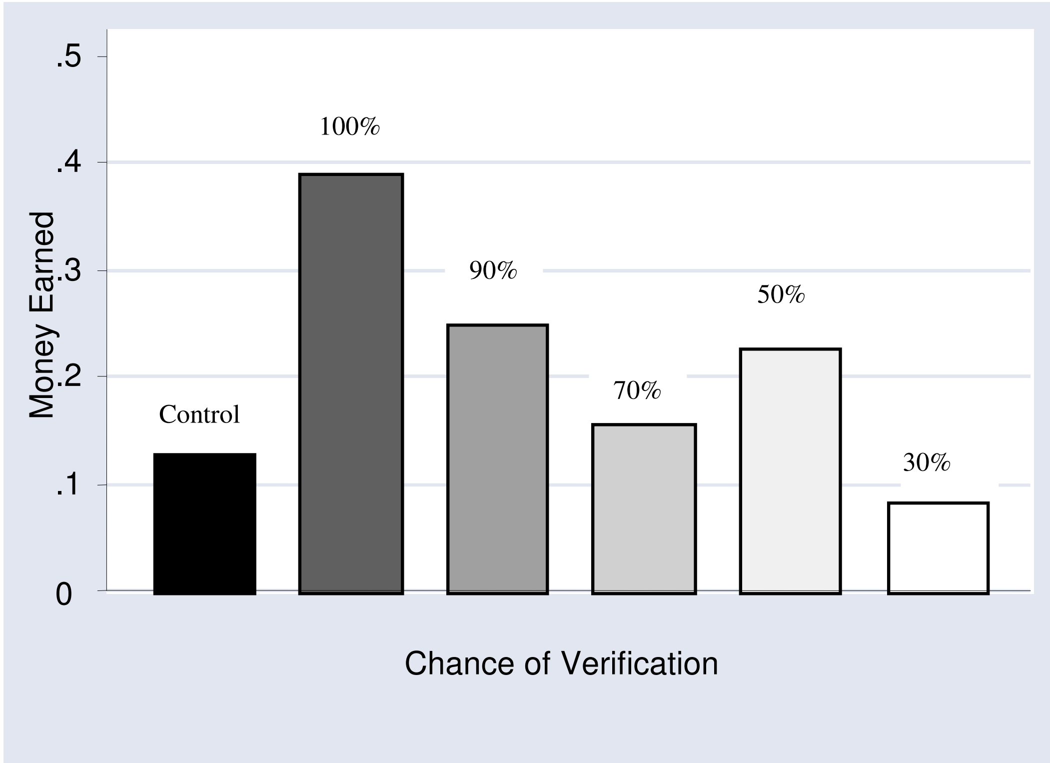

Figure 2.3. The Effects that Reducing the Chance of Verification has on the Amount of Money that Subjects Earn

5.4 How Well Do Sophisticated vs. Unsophisticated Citizens Use This Cue?

Having demonstrated that subjects earn significantly smaller amounts of money once the endorser's incentives become less clear, I now assess whether the endorser's statements help both sophisticated and unsophisticated subjects to improve their decisions. Specifically, I compare the effects that my idealized and more realistic experimental conditions have on subjects who are and who are not sophisticated. 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 (ETS) releases. Specifically, subjects whose SAT math scores fall in the 97th percentile or higher are considered sophisticated, while subjects whose SAT math scores fall in the 27th percentile through the 79th percentile are considered unsophisticated. Stated in terms of the scores associated with these percentile rankings, the sophisticated subjects' SAT math scores range from 740 points to 800 points, while the unsophisticated subjects' SAT math scores range from 450 points to 620 points.[18]

I first examine the effects that my idealized experimental conditions—i.e. common interests, a $15 penalty for lying, and a 100% chance of verification—have on the amounts of money that sophisticated and unsophisticated subjects earn. As I demonstrated in Figure 2.1, the endorser's statements in each of these conditions dramatically increases the amount of money that subjects earn, relative to the amount of money that subjects in the control group earn. The question that those results leave open, however, is whether these conditions have different effects on subjects who are and who are not sophisticated. Thus, I break down the results in Figure 2.1 to examine the amounts of money that sophisticated and unsophisticated subjects earn in each experimental condition.

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[18] Note that my results are highly robust to different definitions of mathematical sophistication. Indeed, defining mathematical sophistication as the top 1/3 or top 1/4 of all SAT math scores reported and defining the lack of mathematical sophistication as the bottom 1/3 or bottom 1/4 of all SAT math scores reported does not significantly change the results that I report in Table 3. My results also do not change if I instead consider subjects to be mathematically sophisticated if they score in the 98th percentile or higher on their SAT math test (i.e. have scores that range from 750 points to 800 points) and consider subjects to be mathematically unsophisticated if they score in the 27th percentile through the 70th percentile (i.e. have scores that range from 450 points to 580 points). My results also do not change if I instead obtain a measure of subjects' mathematical sophistication by predicting the percentage of questions that they will answer correctly and then rank subjects according to their predicted percentage of correct answers.

As the results in Table 2.3 reveal, the endorser's statements in the common interests, $15 penalty for lying, and 100% chance of verification conditions enable *both* sophisticated and unsophisticated subjects to increase the amount of money that they earn, relative to their sophisticated and unsophisticated counterparts in the control group. Further, in each of these experimental 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 experimental 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.

Table 2.3. The Effects that Common Interests, $15 Penalty for Lying, and 100% Chance of Verification have on Sophisticated and Unsophisticated Subjects*

Experimental Condition	Unsophisticated: Amount of Money Earned	Sophisticated: Amount of Money Earned	Significant Gap between Sophisticated and Unsophisticated?
Control Group	$0.07	$0.25	Yes
Common Interests	**$0.36 (p < 0.001)**	**$0.42 (p < 0.001)**	No Gap
$15 Penalty for Lying	**$0.36 (p < 0.001)**	**$0.43 (p < 0.001)**	No Gap
100% Verification	**$0.43 (p < 0.001)**	**$0.42 (p < 0.001)**	No Gap

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

Note also that the results in Table 2.3 reveal that there is a statistically significant difference between the performance of sophisticated and unsophisticated subjects in the control group. Specifically, the sophisticated subjects in the control group earn, on average, $0.25 per problem, while the unsophisticated subjects in the control group earn, on average, only $0.07 per problem ($p < 0.001$). Based on this finding, it appears that the endorser's statements can close the gap between sophisticated and unsophisticated citizens (see Kuklinski et al. 2001 and Rahn, Aldrich, and Borgida 1994 for discussions of how other environmental conditions can close the gap between sophisticates and novices). 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 (see the right hand column of Table 2.3).

But what about the amounts of money that sophisticated and unsophisticated subjects earn under more realistic conditions? In order to answer this question, I break down the results in Figures 2.2 and 2.3 to analyze differences in the amounts of money that sophisticated and unsophisticated subjects earn when the endorser faces a \$5 penalty, a \$1 penalty, and a 90%, 70%, 50%, or 30% chance of verification. As the results in Table 2.4 demonstrate, under these more realistic conditions, sophisticated subjects are never able to increase the amount of money that they earn, relative to their sophisticated counterparts in the control group. And, in two of the experimental conditions (specifically, the \$5 penalty and the 30% chance of verification conditions), the unsophisticated subjects are also unable to increase the amount of money that they earn, relative to their unsophisticated counterparts in the control group. Interestingly, unsophisticated subjects are able to increase the amount of money that they earn in the \$1 penalty, 90% chance of verification, 70% chance of verification, and 50% chance of verification conditions. Indeed, the unsophisticated subjects in these conditions earn amounts of money that are similar to the amounts of money that sophisticated subjects earn, which helps to close the gap between these two types of subjects. That said, the increases in the amounts of money that unsophisticated subjects earn are not nearly as large, nor as consistent, as the increases that I observed in my idealized experimental conditions.

Table 2.4. Amount of Money that Sophisticated and Unsophisticated Subjects Earn under Realistic Conditions*

Experimental Condition	**Unsophisticated: Amount of Money Earned**	**Sophisticated: Amount of Money Earned**	**Significant Gap between Sophisticated and Unsophisticated?**
Control Group	$0.07	$0.25	Yes
$5 Penalty for Lying	$0.06	$0.19	No Gap
$1 Penalty for Lying	**$0.13 (p = 0.04)**	$0.23	Yes
90% Verification	**$0.25 (p = 0.05)**	$0.27	No Gap
70% Verification	**$0.18 (p = 0.03)**	$0.15	No Gap
50% Verification	**$0.26 (p = 0.01)**	$0.25	No Gap
30% Verification	$0.02	$0.15	Yes

*Boldface indicates a statistically significant increase, relative to the control group, for each of the two levels of sophistication.

6. Conclusion

As the results of my experiments demonstrate, the mere presence of a particular cue is no guarantee that it will improve both sophisticated and unsophisticated citizens' decisions. Although subjects in my experiments had the opportunity to learn from the statements of an endorser who had knowledge about the correct answer to the math problem, only under very narrow circumstances did they trust the endorser's statements and improve their decisions. Specifically, my experiments demonstrate that only under idealized conditions (that is, conditions when, in equilibrium, the endorser is always better off if he or she tells the truth) were both sophisticated and unsophisticated individuals able to improve their decisions and increase the amounts of money that they earn. Once such idealized conditions were made more realistic (realistic in the sense that the endorser's incentives were less transparent), sophisticated and unsophisticated subjects were no longer able to improve their decisions consistently.

Because real world political settings often involve uncertainty about endorsers' incentives, my experimental results represent an important contribution to the literature on cues, in general, and to the literature on the limitations of cues, in particular. Indeed, although many scholars implicitly assume that cues necessarily improve citizens' decisions, my results reveal that the effectiveness of one particular cue is quite fragile; that is, citizens' ability to learn from the statements of an endorser hinges upon the clarity of the endorser's incentives. And, when an endorser's incentives are less than transparent, this cue may be less useful than the existing literature suggests.

More broadly, the results of my experiments suggest that scholars should continue to investigate the conditions under which citizens can use cues to help them with their decisions. In this study, I addressed the question of *who* can use a particular cue *when* by analyzing both sophisticated and unsophisticated citizens' ability to learn from an endorser's statements and by assessing the conditions under which this cue improved their decisions. Because I focused 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 work, and I emphasize that scholars should continue to investigate the conditions under which cues improve decisions.

Chapter 3

Are Two Cues Better Than One?
Assessing Whether Multiple Cues Improve Citizens' Decisions

> "[C]itizens use a multitude of cue devices in making sense of politics."
>
> --Huckfeldt et al. 1999, p. 891

> "Shortcuts for obtaining information at low cost are numerous."
>
> --Popkin 1991, p. 213

It is widely known that citizens use cues as substitutes for detailed knowledge about particular candidates or policies. For example, many scholars emphasize that party identification is a cue that helps citizens with their political choices (Popkin 1991; Page and Shapiro 1992; Sniderman, Brody, and Tetlock 1991), and others demonstrate that the statements of trusted endorsers enable even uninformed citizens to learn what they need to know (Downs 1957; Carmines and Kuklinski 1990; Lupia 1992, 1994; Mondak 1993; Lupia and McCubbins 1998; Boudreau 2006, 2007). In a similar manner, several scholars note that institutions provide citizens with valuable cues (Lupia and McCubbins 1998; Boudreau 2006), while others argue that citizens rely upon polls, results from early presidential primaries, information about the state of the economy, and candidates' appearances to help them make political choices (Popkin 1991; Fiorina 1981; Kinder and Kiewiet 1981; Lau and Redlawsk 2001; Bailenson et al. 2006; Boudreau and McCubbins 2007).

Given the multitude of cues that are available to citizens, it is curious that relatively few scholars study how citizens use cues when there is more than one cue available. That is, much of the existing literature focuses on whether citizens can use a particular cue when it is presented to them *in isolation* and glosses over the question of whether and how citizens choose from and use the many different cues that exist in political contexts. Although the question of whether citizens can use individual cues is undoubtedly important, it is equally important that scholars address the following questions: *Do multiple cues help citizens to improve their decisions, above and beyond the improvements that they achieve when only one of these cues is present? If so, under what conditions are two cues better than one cue?* Answering these questions is important not only because it will help us understand the choices that citizens make in the real world (where they do have multiple cues available to them), but also because it will allow us to identify the conditions under which particular cues (or combinations of cues) enable citizens to improve their decisions.

I address these questions by conducting laboratory experiments in which I provide citizens with several different cues. Although there are, of course, many cues that exist in the real world, I focus on the *institutional cues* that citizens may use when deciding whether to trust the statements of a speaker or endorser (c.f. Lupia and McCubbins 1998; Boudreau 2006). Such cues are embedded in the institutions of our political system, and they may help citizens to assess the veracity of a speaker's statements and to learn from them. For example, competition among candidates,[19] public debates, and the existence of the mass media all increase the probability that political statements will be verified and that any false statements will be revealed. Further, politicians may incur a penalty for lying (in the form of a loss of reputation or monetary sanctions) if they lie to their constituents or their colleagues (see Lupia and McCubbins 1998 for further discussion). In this way, both the threat of verification and penalties for lying provide citizens with cues that may help them to learn from the statements of a speaker who they do not know.

In order to incorporate these cues into my experiments, I give subjects the opportunity to learn from the statements of a knowledgeable speaker (who is analogous to an endorser, politician, or other political actor), and I impose *both* a penalty for lying and a threat of verification upon the speaker, thereby providing subjects with two different cues that might indicate how truthful the speaker's statements are.[20] Within each experimental condition, I also vary the size of the penalty (i.e. $1 or $15) and the chance of verification (i.e. 30%, 70%, 90%, or 100%) to create five unique combinations of these two cues.[21] In order to assess whether and

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[19] See Boudreau and McCubbins (2006) for an in depth discussion of the conditions under which competition induces trustworthy statements and enables citizens to learn from competing speakers.

[20] Note that, for the penalty for lying, both the speaker and the subjects know that the speaker will incur a penalty if he or she lies about the correct answer to the math problem. Similarly, for the threat of verification, both the speaker and the subjects know that the experimenter will verify the speaker's statement with some probability to make sure that it reveals the correct answer to the math problem.

[21] The specific combinations of cues that I examine in my experiments are: 1) a 30% chance of verification and a $1 penalty for lying, 2) a 70% chance of verification and a

under what conditions multiple cues help citizens to improve their decisions, I compare the decisions that subjects make when multiple cues are present with the decisions that subjects make when only one of these cues is available. For example, by comparing subjects' decisions 1) when a $1 penalty for lying is imposed upon the speaker, 2) when a 30% chance of verification is imposed upon the speaker, and 3) when a $1 penalty for lying *and* a 30% chance of verification are imposed upon the speaker, I am able to assess whether multiple cues help citizens to improve their decisions, relative to the improvements that citizens achieve when only one of these cues is present.

The results of my experiments suggest an interesting lesson about the conditions under which multiple cues help citizens to improve their decisions. Specifically, my results suggest that *two cues are not necessarily better than one*. That is, when one cue enables citizens to achieve large improvements in their decisions, the presence of a second, less useful cue does not help them to improve their decisions further. However, when neither cue is particularly useful by itself, citizens use these cues in a way that enables them to make significantly better decisions than they make when only one of these cues is present. Thus, it appears that, at least for the particular cues that I examine in this paper, only under certain conditions do multiple cues help citizens to improve their decisions further.

This paper proceeds as follows. I begin with a review of the literature on cues, emphasizing that although many scholars demonstrate that citizens use particular cues

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$1 penalty, 3) a 90% chance of verification and a $1 penalty, 4) a 100% chance of verification and a $1 penalty, and 5) a 30% chance of verification and a $15 penalty.

when making political choices, relatively few assess whether and how citizens use the many different cues that exist in political contexts. I then describe in detail the research design that I use in my experiments. Next, I briefly describe the data source and statistical methods that I use, and I also comment on the generalizability of my results. I then present my experimental results. I conclude with a discussion of the implications that my research has for debates about cues, in general, and about how citizens learn and make decisions, in particular. Specifically, I suggest that my experimental results are consistent with the cognitive science literature on learning, for it appears that subjects in my experiments are economizing on their scarce cognitive resources and using only as many cues as are necessary to improve their decisions.

1. The Debate: How Do Cues Affect Citizens' Decisions?

1.1 Assessing Cues One at a Time

In response to scholars who lament citizens' lack of factual knowledge about politics, [22] many scholars demonstrate that particular cues can substitute for factual knowledge about politics and enable citizens to make reasoned choices at election time (Popkin 1991; Page and Shapiro 1992; Conover and Feldman 1989; Lupia and McCubbins 1998; Lupia 1992, 1994; Kuklinski et al. 2001; Mondak 1993; Fiorina 1981; Kinder and Kiewiet 1981; Carmines and Kuklinski 1990; Druckman 2001a; Mutz 1992; Lodge and Hamill 1986). For example, many scholars demonstrate

[22] See, e.g., Berelson, Lazarsfeld, and McPhee (1954), Campbell et al. (1960), Converse (1964, 1975), Neuman (1986), Delli Carpini and Keeter (1996), Bartels (1996), and Bennett (1998).

experimentally that citizens use party labels as cues (Rahn 1993; Druckman 2001b; Tomz and Sniderman 2004), while others suggest that citizens can learn what they need to know from the media and from the statements of the trusted speakers or endorsers that exist in political settings (Carmines and Kuklinski 1990; Mondak 1993; Lupia 1994; Lupia and McCubbins 1998; Druckman 2001a). Although these scholars convincingly demonstrate that citizens use particular cues when making decisions, many of them study the effects of particular cues *in isolation* and do not address how citizens choose from and use the multiple cues that exist in political settings. While establishing that citizens can use particular cues is, of course, a worthwhile enterprise (indeed, this body of literature provides an important response to the argument that citizens are too uninformed about politics to make reasoned choices), it is equally important to understand whether and how citizens use the multiple cues that are available to them in nearly all political settings.

1.2 Analyzing Multiple Cues

Building upon the work of scholars who study particular cues in isolation, several other scholars investigate whether and how multiple cues affect citizens' decisions. For example, Lau and Redlawsk (2001) examine citizens' use of five different cues (namely, a candidate's partisanship, ideology, and appearance, as well as endorsements and polling information), and they find that both sophisticated and unsophisticated subjects rely upon several different cues when learning about candidates. In a similar manner, Kuklinski and Hurley (1994) assess the extent to which citizens use race and ideology as cues, and they demonstrate that, at least among African American citizens, race (not ideology) is the cue that determines whether they accept a particular politician's statement. Additionally, Huckfeldt et al. (2005) examine how citizens use different cues (namely, partisanship, ideology, and salient issues) when the information that these cues convey is contradictory, and they find that citizens do not simply combine all of these cues in a linear additive way.[23] Taken together, these scholars demonstrate the importance of assessing the conditions under which citizens use multiple cues.

It is this body of research that explores whether and how citizens use multiple cues that I build upon in this study. Specifically, I analyze experimentally the conditions under which multiple cues help citizens to achieve larger improvements in their decisions than they achieve when only one of these cues is present. Although my research draws upon the insights of those scholars discussed above, it makes a number of new contributions. For example, unlike previous scholars, I assess the conditions under which various institutions provide citizens with cues that help them to learn from the statements of a speaker and improve their decisions. Such a focus is important not only because citizens frequently have an opportunity to learn from others in real world political contexts, but also because it suggests a number of lessons about how we might design particular institutions (and combinations of institutions) to facilitate citizen learning.

Yet another advantage of my design stems from the nature of the choices that subjects make in my experiments. Specifically, instead of asking subjects to vote for

[23] The experimental results that I present in this paper also suggest that citizens do not necessarily combine cues in a linear manner.

fictional candidates or policy issues, I ask subjects to make choices about math problems (that is, subjects are asked to 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 policy issues) provides a straightforward way to identify correct decisions and to assess whether and when a particular cue (or combination of cues) induces an improvement in decision making. Stated differently, although it is often difficult to identify when citizens have chosen the "correct" candidate or policy,[24] it is very easy to tell when they have chosen the correct answer to a math problem.

The second advantage of my design is that math problems (even though they do not *look* like political decisions on the surface) are sometimes more similar to real world political decisions than are many of the tasks that scholars typically ask subjects to perform in a political experiment (e.g. voting for fictional candidates, predicting the outcomes of coin tosses, etc.). For example, in real world politics, citizens are not blank slates when they go to the ballot box; that is, they have some preexisting knowledge or beliefs about the candidates 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 correct choice because they have preexisting knowledge about how to solve math problems. The same cannot always be said for experiments that ask subjects to make choices about fictional candidates. Indeed, because

[24] Despite the difficulties associated with identifying when citizens vote for the "correct" candidate, Lau and Redlawsk (1997, 2001) and Payne, Bettman, and Johnson (1993) have developed several different measures that are designed to assess the correctness of citizens' votes.

experimenters invent the fictional candidates and their characteristics, it is often impossible for subjects to know or believe anything about them prior to making their choices in the experiment.

Further, in real world politics, there is something at stake for citizens when they make their decisions, but the stakes are not very large (see Hibbing and Alford 2004 and Hibbing and Theiss-Morse 2002 for a discussion of how typical political issues are low stakes games for most Americans). Similarly, there is something at stake for subjects in my experiments because they earn money if they make a correct choice and lose money if they make an incorrect choice. As in real world politics, the stakes in my experiments are not very large, as subjects either earn or lose 50 cents for each decision that they make. When voting for fictional candidates, however, there is often nothing at stake for subjects in the experiment. Indeed, because the candidates are fictional, they cannot be elected to office or affect the subjects in any real way. And, because scholars using fictional candidates often do not pay subjects for their choices (perhaps because it is difficult to identify "correct" choices in this type of experiment), there is nothing on the line for subjects when they make their decisions.[25]

Finally, several scholars suggest that real world political decisions have an emotional or affective component (Lupia and Menning 2006; Lodge and Taber 2000; Marcus et al. 2000; Rahn 2000). Although some real world political decisions are likely to be more emotional than others (i.e. national level elections may stimulate greater emotional or affective responses than local elections), the same is true of the math problems that I use in my experiments. Indeed, a great deal of work in

[25] For an important exception to this statement, see Merolla (2004).

psychology and education has demonstrated that individuals often have emotional responses when solving math problems and that the nature of the emotional response depends upon the problem, as well as the heuristic strategy that individuals use when solving the problem (McLeod and Adams 1989; McLeod 1989). The same cannot necessarily be said of voting for fictional candidates because there may not be anything emotional about choosing individuals who do not really exist and who cannot affect the subjects. Thus, although voting for fictional candidates *looks* like politics on the surface and captures some important elements of real world politics (for example, there may be some uncertainty when voting for fictional candidates, and there may also be subjects who vary in their levels of sophistication), this approach does not necessarily capture several key elements of real world politics that math problems do capture. (See Table 3.1 for a summary of how math problems are analogous to real world political decisions.)

Table 3.1. How Math Problems Are Analogous to Political Choices

Real World Politics	Math Problems	Fictional Candidates
Uncertainty	✓	✓
Varied Levels of Sophistication	✓	✓
Preexisting Knowledge	✓	
Small Stakes	✓	
Emotions and Affect	✓	

2. Research Design

In order to analyze the conditions under which multiple cues help citizens to improve their decisions, I design a two-group randomized experiment.[26] Specifically, I randomly assign subjects to treatment and control groups, and I next ask subjects to solve a series of binary choice math problems. The math problems that I use are drawn from an SAT math test and consist of many different types of problems (i.e. algebra, geometry, calculus) and several levels of difficulty. I tell subjects in both the treatment and control groups that they have 60 seconds to solve each math problem

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[26] All of the experimental materials that were used in these experiments are available from the author. Please email Cheryl Boudreau at clboudreau@ucsd.edu if you would like to see copies of the scripts, handouts, and other materials that were used.

and that they will earn 50 cents for each problem that they answer correctly, that they will lose 50 cents for each problem that they answer incorrectly,[27] and that they will neither earn nor lose 50 cents if they leave a problem blank.

The main difference between the treatment and control groups has to do with the conditions under which subjects solve the math problems. In the control group, subjects solve 24 math problems one at a time, with 60 seconds allotted for each problem. For each problem that subjects in the control group solve, I pay them according to whether they solve the problem correctly, incorrectly, or leave the problem blank.[28]

In the treatment group, subjects solve the very same 24 math problems one at a time, and they are also paid according to whether they solve the problem correctly, incorrectly, or leave the problem blank. What differs between the treatment and control groups, however, is that subjects in the treatment group are exposed to a speaker in a particular institutional context. Specifically, before subjects in the treatment group solve any of the math problems, the experimenter randomly selects one subject to act as "the speaker" in the remainder of the experiment. The speaker's

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[27] Note that subjects earn money before they begin solving the math problems in my experiments. Specifically, all subjects receive 5 dollars for showing up on time to the experiment, and they also earn money by solving simple practice problems and by taking quizzes on the instructions that I read aloud. Also, just before subjects solve the math problems in the experiment, I tell them 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 that they can actually lose money in the experiment.

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

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 still to solve the 24 math problems one at a time), the speaker is shown the correct answer to each math problem (that is, the speaker is given knowledge about the correct choice for subjects) and then makes a statement to the other subjects about the answer to the math problem.[29] After the speaker makes his or her statement, the other subjects in the treatment group are given 60 seconds to solve that particular math problem.

The key to this experiment is that both the speaker and the subjects know that the speaker can make any statement that he or she wishes. That is, 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 make whatever statement he or she wishes is constant throughout this experiment and is designed to be analogous to Crawford and Sobel's (1982) and Lupia and McCubbins's (1998) models.

Although the speaker can lie or tell the truth in all of my experimental conditions, I vary both the number and nature of the cues that are available to subjects. Specifically, in each experimental condition, the speaker and subjects have conflicting interests (i.e. the speaker earns money when the subjects lose money and vice versa), but I vary whether the speaker is subject to a *penalty for lying* (of either \$1 or \$15), a

[29] Note that the speaker makes his or her statement by putting a checkmark beside the answer that he or she wishes to report. I, as the experimenter, then read that statement out loud to all of the subjects. This prevents the speaker's tone of voice from confounding the experiment. Similarly, throughout the experiment, the speaker sits behind a large partition so that the speaker's gender, race, and/or age do not affect the extent to which subjects listen to the speaker's statements. These personal characteristics of the speaker are, of course, interesting factors that I plan to vary in future experiments.

threat of verification (i.e. there is either a 30%, 70%, 90%, or 100% chance of verification), or some *combination* of a particular penalty for lying and chance of verification. Thus, in some experimental conditions, the subjects have only one cue to rely upon, while in other experimental conditions, the subjects have multiple cues available to them. I then assess the effects that these cues (and combinations of cues) have on subjects' ability to solve the math problems correctly.

It is important to note that each of these experimental variations is common knowledge at the outset of each part of the experiment. Additionally, each experimental variation alters the usefulness of the particular cues that are available to subjects, where "usefulness" is defined as the extent to which a given cue enables subjects to make better decisions than they would have made without that cue. In order to measure the usefulness of each individual cue that I use in my experiments, I draw upon the results of Boudreau (2007), which show the extent to which various penalties for lying and chances of verification enable citizens to improve their decisions, relative to a control group that made the exact same decisions, but without a cue. As shown in Table 3.2, the cues that I use in this study vary greatly in their usefulness—that is, in the extent to which they enable citizens to improve their decisions.

Table 3.2. The Usefulness of Particular Institutional Cues[30]

Usefulness of the Cue:	**Corresponding Experimental Condition:**	**Percent Improvement (Relative to Control Group):**
Useful (Large improvements, relative to control group)	*$15 Penalty for Lying*	200%
	100% Verification	231%
	90% Verification	92%
Less useful (Little to no improvement, relative to control group)	*$1 Penalty for Lying*	31%
	70% Verification	23%
	30% Verification	-38%

So how do I establish conflicting interests between the speaker and the subjects and manipulate the cues that are available to subjects within the confines of my experiments? In short, I vary both interests and cues by manipulating the ways that the speaker and the subjects earn money. For example, to establish conflicting interests between the speaker and subjects, I pay subjects 50 cents for each math problem that they answer correctly. The speaker, on the other hand, earns 50 cents for each subject who gets the math problem *wrong*. So, for example, if 11 subjects answer the math problem correctly, the subjects earn 50 cents each, and the speaker

[30] This table is adapted from Boudreau (2007).

loses $5.50 (i.e. 50 cents for each of the 11 subjects who answer the problem correctly).[31]

Simultaneously, I also introduce one of my cues—namely, a penalty for lying. So, in this segment of the experiment, 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 or $1, depending on the experimental condition) if he or she lies about the correct answer to the math problem.

For my next experimental variation, I maintain conflicting interests between the speaker and the subjects, but instead of imposing a penalty for lying upon the speaker, this time, 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 and announce the correct answer to subjects. If the speaker chooses to make a true statement, then I simply read the speaker's statement to the 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 the math problem to the 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

[31] The typical number of subjects in each experimental session was 12 (11 subjects who solved the math problems and 1 speaker who made a statement to the other 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% and 30% chance of verification conditions proceed in a similar manner.

In order to assess whether and under what conditions multiple cues help citizens to improve their decisions further, I then impose *both* a penalty for lying and a threat of verification upon the speaker. Specifically, I examine the following five combinations of these two different cues: 1) a $1 penalty for lying and a 30% chance of verification, 2) a $1 penalty for lying and a 70% chance of verification, 3) a $1 penalty for lying and a 90% chance of verification, 4) a $1 penalty for lying and a 100% chance of verification, and 5) a $15 penalty for lying and a 30% chance of verification. Although there are, of course, many different combinations that I could examine, I choose these particular combinations for two reasons. First, they provide a range of differences in the relative usefulness of the two cues that are available to subjects. For example, as shown in Table 3.2, a 100% chance of verification is a much more useful cue than a $1 penalty for lying; that is, it enables subjects to achieve a 231% improvement in their decisions, while the $1 penalty for lying only enables subjects to improve their decisions by 31%. Similarly, a $15 penalty for lying is a much more useful cue than a 30% chance of verification (i.e. it enables subjects to improve their decisions by 200%, while a 30% chance of verification does not allow subjects to improve their decisions). However, a 70% chance of verification is roughly as useful as a $1 penalty for lying (that is, these cues enable subjects to improve their decisions by 23% and 31%, respectively).

Second, by using these combinations of cues, I am able to present subjects with several different combinations of useful and less useful cues, which gives me greater confidence that the usefulness of the cues (and not the particular cues themselves) is what is driving my results. Specifically, I present subjects with two different combinations of less useful cues (i.e. a 30% chance of verification plus a $1 penalty for lying and a 70% chance of verification plus a $1 penalty for lying), and I also provide them with three different combinations of a useful cue and a less useful cue (i.e. a 90% chance of verification plus a $1 penalty for lying, a 100% chance of verification plus a $1 penalty for lying, and a $15 penalty for lying plus a 30% chance of verification). In this way, I am able to assess whether changes in the relative usefulness of the two cues affect whether these cues allow subjects to achieve even larger improvements in their decisions.

3. Methodology

In order to assess whether multiple cues help citizens to improve their decisions, I conducted laboratory experiments at a large public university. When recruiting subjects, I posted flyers at various locations on campus (for example, in front of the library, in the cafeterias, in the dormitories, and in academic buildings), and I also sent out campus-wide emails to advertise the experiments. A total of 369 adults who were enrolled in undergraduate classes and who were of different genders, ages, races, and college majors participated in the experiments.

Because I use college undergraduates as my source of data and because I ask these undergraduates to make decisions about math problems, my results most likely *underestimate* the extent to which particular cues and combinations of cues help subjects to improve their decisions.[32] 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 in the experiment. 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, of course, 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 the undergraduates who attend this university.[33] It is these differences between the undergraduates that I use in my experiments and members of the general population that may cause me to underestimate the effectiveness of the particular cues that I examine in this paper. Indeed, if I had used members of the general population in my

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[32] It is, of course, possible that the undergraduates in my experiments are more adept at using multiple cues to improve their decisions than are members of the general population. If this is the case, then using college undergraduates as subjects would cause me to *overestimate* the effectiveness of multiple cues. In future work, I explore this possibility by examining whether there are differences in how sophisticated and unsophisticated subjects use multiple cues.

[33] That said, the college undergraduates who participated in my experiments vary greatly in their ability to solve math problems. Specifically, their SAT math scores range from 450 (the 27th percentile) to 800 (a perfect score).

experiments, then there would have been much more room for the cues to effect an improvement in subjects' decisions.[34]

Further, because subjects in each experimental condition earn money for answering problems correctly and lose money for answering problems incorrectly, my experiments yield a straightforward measure of whether multiple cues help subjects to improve their decisions, relative to the improvements they achieve when only one of these cues is available to them. Specifically, I calculate the average amount of money that subjects earn per math problem when there are two cues present, and I then compare this amount with the amounts of money that subjects earn when these same cues are presented separately. If multiple cues do in fact help subjects to improve their decisions further, then they should earn significantly more money when two cues are present than they earn when either of these cues is presented separately. Alternatively, if the addition of a second cue does not help subjects to achieve further improvements in their decisions, then I should not observe a significant difference between the amounts of money that subjects earn when there are two cues present and the amounts of money that subjects earn when only one of the cues is present.

In order to examine whether there are in fact differences in the amounts of money that subjects earn when they are presented with one versus two cues, I conduct

[34] Of course, laboratory experiments always involve a tradeoff between internal validity and external validity (Trochim 2001). Specifically, the main advantage of conducting my experiments in a controlled environment and randomly assigning subjects to treatment and control groups is that I am able to make internally valid causal inferences. For these very same reasons, however, my experiments are much weaker in external validity. In order to compensate for this, at a later date I plan to combine the results of these experiments with related quasi-experimental studies (which are weaker in internal validity but much stronger in external validity).

difference of means tests.[35] Specifically, I examine whether subjects who are exposed to two cues earn significantly more money than do subjects who are exposed to only one of those cues. And, in order to gain a sense of how much these individual cues and combinations of cues enable citizens to improve their decisions, I also assess whether subjects in each of my experimental conditions earn significantly more money than do subjects in the control group (recall that subjects in the control group must solve the problems on their own, i.e. without any cues).

4. Results

4.1 When Two Cues Are Better Than One

As the results in Figure 3.1 demonstrate, multiple cues help citizens to improve their decisions even when, individually, those cues are not particularly useful. Specifically, the results from the experimental conditions in which the speaker is subject to 1) a 30% chance of verification plus a $1 penalty for lying and 2) a 70% chance of verification plus a $1 penalty for lying demonstrate that, although these cues are not very useful when presented separately, they lead to large improvements in the amounts of money that subjects earn when they are presented together. For example, when the speaker is subject to a 30% chance of verification only, subjects earn, on average, $0.08 per problem (N = 85). When the speaker is subject to a $1 penalty for

[35] Note that I also analyzed my data using nonparametric Wilcoxon Mann-Whitney tests. Unlike the standard difference of means test, the Wilcoxon Mann-Whitney test does not require the assumption that the differences between two samples are normally distributed. None of the results reported in this paper change if the Wilcoxon Mann-Whitney test is used instead of a difference of means test.

lying only, subjects earn, on average, \$0.17 per problem ($N = 141$). However, when both of these cues are presented together, the subjects are able to improve their decisions dramatically. That is, they earn, on average, \$0.26 per problem ($N = 31$), which is a statistically significant improvement relative to the 30% chance of verification condition ($p < 0.001$), the \$1 penalty for lying condition ($p < 0.005$), and the control group, where subjects earn, on average, \$0.13 per problem ($N = 66$; $p < 0.001$). Thus, for this particular combination of cues, multiple cues enable subjects to achieve even larger improvements in their decisions, relative to subjects who have only one of these cues available to them and relative to subjects who do not have any cues available to them (i.e. subjects in the control group).

The results from the experimental condition in which the speaker is subject to a 70% chance of verification and a \$1 penalty for lying provide further evidence that multiple cues help subjects to improve their decisions even when, individually, the cues are not particularly useful. Indeed, as shown in Figure 3.1, when the speaker is subject to a 70% chance of verification only, subjects earn, on average, \$0.16 per problem ($N = 83$). When the speaker is subject to a \$1 penalty for lying only, subjects earn, on average, \$0.17 per problem ($N = 141$). Once again, when both of these cues are presented together, subjects are able to achieve large improvements in the amount of money that they earn. Specifically, subjects earn, on average, \$0.29 per problem when both cues are present ($N = 33$), which is a statistically significant improvement relative to the 70% chance of verification condition ($p > 0.001$), the \$1 penalty for lying condition ($p < 0.001$), and the control group ($p < 0.001$). Taken together, these results also indicate that multiple cues help citizens to achieve further improvements in their decisions. Indeed, the amount of money that they earn when two cues are present is larger than the amounts of money that they earn when only one of these cues is present.

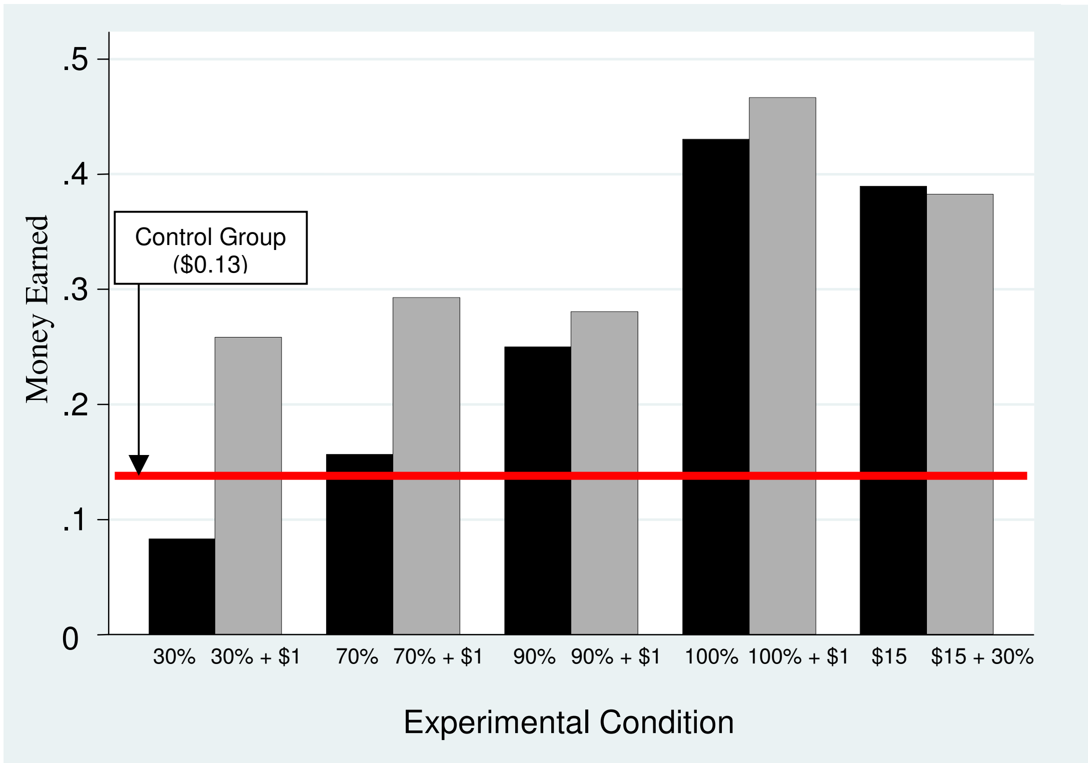

Figure 3.1. The Effects of Individual versus Multiple Cues on the Amounts of Money that Subjects Earn

4.2 When Two Cues Are *<u>Not</u>* *Better Than One*

Although multiple cues help subjects to improve their decisions when those two cues, individually, are not very useful, the results in Figure 3.1 suggest that multiple cues do *not* induce further improvements in decision making when one of those cues, by itself, already allows subjects to achieve large improvements. Stated differently, when subjects have available to them both a useful cue and a less useful cue (as defined in Table 3.2), they do not make better decisions than they do when only the useful cue is present. For example, as shown in Table 3.2, when subjects have the opportunity to learn from a speaker who is subject to a 90% chance of verification, they are able to achieve a 92% improvement in their decisions, relative to subjects in the control group. Indeed, subjects in the 90% chance of verification condition earn, on average, \$0.25 per problem (N = 33), which is significantly larger than the \$0.13 per problem that subjects in the control group earn ($p < 0.001$).

However, unlike the results described in section 4.1, when a \$1 penalty for lying (i.e. a less useful cue) is presented together with a 90% chance of verification, subjects are not able to achieve an even larger improvement in their decisions.[36] That is, when both a \$1 penalty for lying and a 90% chance of verification are imposed upon the speaker, subjects earn, on average, \$0.28 per problem (N = 33), which is not significantly different from the \$0.25 that subjects earn when the speaker is subject to a 90% chance of verification only ($p = 0.2$). Because the addition of a \$1 penalty for lying does not make a difference for subjects' decisions and because the amounts of money that subjects earn in the 90% chance of verification condition and in the 90% chance of verification plus a \$1 penalty for lying condition are statistically

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[36] Note that even though a 90% chance of verification, by itself, is a useful cue, there is still plenty of room for subjects to improve their decisions further with a second cue. Stated differently, my findings that demonstrate that two cues are not better than one cue are not simply the result of a ceiling effect. Indeed, in all of my experimental conditions, the second cue has the potential to improve the amounts of money that subjects earn, but sometimes it does not.

indistinguishable, my results suggest that, when it comes to one useful cue plus one less useful cue, two cues are not better than one.[37]

The results from the experimental condition in which the speaker is subject to a 30% chance of verification and a \$15 penalty for lying provide further evidence that when one cue is useful, by itself, the addition of a second, less useful cue does not allow for further improvements in decision making. For example, as shown in Table 3.2, a \$15 penalty for lying is an extremely useful cue, for it enables subjects to achieve a 200% improvement in their decisions, relative to subjects in the control group. Specifically, subjects in the \$15 penalty for lying condition earn, on average, \$0.39 per problem ($N = 68$), which is significantly larger than the \$0.13 per problem that subjects in the control group earn ($p < 0.001$).

However, when a \$15 penalty for lying is presented together with a less useful cue (i.e. a 30% chance of verification), subjects again are not able to achieve an even larger improvement in their decisions. That is, when both a \$15 penalty for lying and a 30% chance of verification are imposed upon the speaker, subjects earn, on average, \$0.38 per problem ($N = 33$), which is not significantly different from the \$0.39 that subjects earn when the speaker is subject to a \$15 penalty for lying only ($p = 0.4$). Because the addition of a 30% chance of verification does not make a difference for

[37] Note that subjects in the 90% chance of verification plus a \$1 penalty for lying condition are not simply relying upon the \$1 penalty for lying. Indeed, as I noted in section 4.1, a \$1 penalty for lying, by itself, enables subjects to earn only \$0.17 per problem. This amount is significantly lower than the \$0.28 that subjects earn in the 90% chance of verification plus a \$1 penalty for lying condition ($p < 0.001$). It is also significantly lower than \$0.25 that subjects earn per problem in the 90% chance of verification condition ($p < 0.05$).

subjects' decisions and because the amounts of money that subjects earn in the $15 penalty for lying condition and in the $15 penalty for lying plus a 30% chance of verification condition are not statistically different, my results again suggest that two cues are not necessarily better than one.[38]

In a similar manner, the results from the 100% chance of verification plus a $1 penalty for lying condition also indicate that two cues do not allow for further improvements in decision making when one of those cues is useful, by itself. As shown in Table 3.2, a 100% chance of verification is another extremely useful cue because it enables subjects to achieve a 231% improvement in their decisions, relative to subjects in the control group. Specifically, subjects in the 100% chance of verification condition earn, on average, $0.43 per problem ($N = 30$), which is significantly larger than the $0.13 per problem that subjects in the control group earn ($p < 0.001$).

However, when a 100% chance of verification is presented together with a less useful cue (i.e. a $1 penalty for lying), subjects, again, do not achieve an even larger improvement in their decisions. That is, when both a 100% chance of verification and a $1 penalty for lying are imposed upon the speaker, subjects earn, on average, $0.47 per problem ($N = 30$), which is not significantly different from the $0.43 that subjects earn when the speaker is subject to a 100% chance of verification only ($p = 0.12$).

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[38] Again, subjects in the $15 penalty for lying plus a 30% chance of verification condition are not simply relying upon the 30% chance of verification when making their decisions. Specifically, as I noted in section 4.1, a 30% chance of verification, by itself, enables subjects to earn only $0.08 per problem. This amount is significantly lower than the $0.38 that subjects earn in the $15 penalty for lying plus a 30% chance of verification condition ($p < 0.001$). It is also significantly lower than $0.39 that subjects earn per problem in the $15 penalty for lying condition ($p < 0.001$).

6. Conclusion

As the results of my experiments demonstrate, whether multiple cues help citizens to improve their decisions depends upon how useful the various cues are. Specifically, when one of the cues is particularly useful in that it alone enables subjects to achieve large improvements in their decisions, the addition of a second, less useful cue does not allow subjects to achieve even larger improvements in their decisions. However, when two less useful cues are present, subjects use both of these cues in a way that enables them to make better decisions than they did when only one of these cues was present. Taken together, these results demonstrate that *two cues are not necessarily better than one*. That is, although there are conditions under which multiple cues help citizens to improve their decisions further, there are also conditions under which additional cues do not enable citizens to improve their decisions beyond what they achieved with only one cue.

Because citizens in real world political contexts have many different cues to draw upon, my experimental results are an important contribution to the literature on cues. Specifically, much of the existing literature examines whether citizens can use particular cues when they are presented in isolation (Rahn 1993; Druckman 2001b; Tomz and Sniderman 2004; Mondak 1993; Lupia 1994; Carmines and Kuklinski 1990), and my study builds upon this literature by assessing the conditions under which multiple cues help citizens to improve their decisions further. Although I am, of course, not the first to study the use of multiple cues (see, e.g., Lau and Redlawsk 2001; Huckfeldt et al. 2005; Lupia and McCubbins 1998; Kuklinski and Hurley 1994), I am unique in that I suggest conditions under which multiple cues help citizens to improve their decisions and conditions under which the addition of a second cue does not allow citizens to improve their decisions further. And, as the results of my study suggest, whether two cues are better than one appears to depend upon how useful the various cues are.

More broadly, my experimental results lend support to a cognitive science perspective on how citizens learn. Specifically, there is a large body of literature in cognitive science and a growing literature in political science that suggest that humans have limited time, attention, and cognitive resources (for a survey, see Lupia and McCubbins 1998, Chapter 2). What these limitations imply, of course, is that whether citizens pay attention to particular stimuli in their environment depends upon whether the benefits of paying attention to a given stimulus exceed the costs (for further discussion, see Lupia and McCubbins 1998). The results of my experiments are consistent with these studies, for they suggest that when one cue, by itself, enables subjects to achieve large improvements in their decisions, subjects use only this more useful cue and ignore the other, less useful cues. That subjects appear to use only as many cues as are necessary to improve their decisions, suggests that they are economizing on their scarce cognitive resources, as previous research suggests.

Chapter 4

From Competition to Competence?
Theory and Experiments Regarding Deliberation and Citizen Learning

> "A national random sample is brought to a single place where its deliberations, in small group sessions and with competing experts and politicians, can be broadcast nationally and where it can arrive at considered judgements."
>
> --Fishkin 1996, p. 132

> "The moderators [in deliberative settings] also help ensure that competing arguments are heard."
>
> --Luskin, Fishkin, and Jowell 2002, p. 459

Does deliberation promote learning among citizens and improve their decisions? It is widely known that citizens are uninterested in politics and that they must often make political decisions for which they have little information (Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960; Delli Carpini and Keeter 1996; Bartels 1996).[39] Viewing citizens' lack of political knowledge as a threat to our democracy, many scholars argue that we must improve citizens' levels of political knowledge through deliberation. Specifically, these scholars suggest that by bringing together random samples of citizens, exposing them to competing experts' and

[39] In response to these prominent findings, many scholars argue that cues can substitute for detailed factual knowledge about politics (see, e.g., Popkin 1991; Lupia and McCubbins 1998; Lupia 1992, 1994, forthcoming; Kuklinski et al. 2001; Mondak 1993; Fiorina 1981; Kinder and Kiewiet 1981; Druckman 2001a, 2001b, 2001c; Mutz 1992; Lodge and Hamill 1986; Sniderman, Brody, and Tetlock 1991; Boudreau 2007).

politicians' views about particular policy issues, and then encouraging them to discuss these different views, citizens will learn more about politics and develop more informed opinions about particular policy issues (Fishkin 1991; Fishkin 1996; Gastil and Dillard 1999; Luskin, Fishkin, and Jowell 2002; Ackerman and Fishkin 2004; Fishkin and Luskin 2005). Based upon these scholars' influential suggestions, there have been more than 17 instances of deliberation carried out in the United States, Britain, Denmark, and Australia (Luskin, Fishkin, and Jowell 2002).

Despite the widespread enthusiasm for deliberation and the many promises that proponents have made on its behalf, there is reason to question whether one of its core features (that is, exposing citizens to the statements of competing experts and politicians) actually promotes learning and improves decision making. Indeed, there is a large body of literature in cognitive science and accumulating work in political science and economics that suggest that competition between experts does not necessarily produce beneficial effects. Specifically, advocates of deliberation assume that the competing experts and politicians will be truthful, and they also assume that citizens will listen to the competing experts and politicians, as well as respect and learn from the information and opinions that they offer. As the cognitive science literature reminds us, however, listening and learning are costly behaviors; that is, when citizens speak or listen to one person, they must forego the opportunity to do something else (Jackendoff 1980; Lupia and McCubbins 1998; Lupia 2002; McCubbins and Rodriguez 2006). Further, because humans have limited energy, they are able to pay attention to and remember only a small fraction of the information available to them (Schacter 2001; Lupia 2004; Lupia and McCubbins 1998). And, as the political science and economics literatures demonstrate, “talk is cheap”; that is, without some cost associated with making false statements, there is no way to ensure that competing experts and politicians tell the truth (Crawford and Sobel 1982; Lupia and McCubbins 1998).

What concerns us here is whether competition between experts in deliberative settings leads to truth telling by the experts and enables citizens to discern and use the truth when making decisions. In order to capture what scholars seem to have in mind when they advocate exposing citizens to “competing experts and politicians” and “balanced panels” of experts (Fishkin 1996; Luskin, Fishkin, and Jowell 2002), we examine the case where these experts have diametrically opposed interests, so that one person’s gains are the other’s losses.[40] To this end, we provide a formal model of a static communication game between two competing experts and a citizen,[41] and we then test our model’s predictions experimentally.

The result that emerges from our communication game and in our experiments contradicts much of the existing literature, which extols the virtues of deliberation, in general, and competition between experts, in particular (Fishkin 1991; Fishkin 1996; Gastil and Dillard 1999; Luskin, Fishkin, and Jowell 2002; Ackerman and Fishkin 2004; Fishkin and Luskin 2005). Specifically, we find that competition 1) does not necessarily induce both experts to send truthful signals to the citizen and 2) prevents the citizen from being able to learn from the experts’ signals. In this way, our model

[40] Note that this also excludes the possibility of collusion.

[41] For an example of a dynamic, multi-stage communication game, see Sobel (1985).

and experiments suggest that, rather than leading to the revelation of truthful information, competition (absent institutional constraints) instead creates a situation in which the citizen cannot trust or learn from the experts' signals.

Following Lupia and McCubbins (1998), we next extend our basic model and our experiments to incorporate two institutions that have direct analogues in many real world competitive contexts, but that are largely absent in the deliberative settings that scholars advocate:[42] penalties for lying and the threat of verification. For example, in political contexts, citizens know that competing politicians may incur a penalty for lying (in the form of a loss of reputation or monetary sanctions) if they lie to their constituents or their colleagues, and in legal contexts, jurors know that witnesses for both the prosecution and the defense face penalties for perjury if they lie on the stand. Similarly, attorneys' cross-examinations (which are a form of verification) often reveal when witnesses have made false statements, and the media often reveals when politicians have not been truthful. The results of these extensions to our basic model (and their corresponding experiments) demonstrate that when competition is coupled with one of these institutions, it induces both experts to send truthful signals to the citizen and enables the citizen to learn from these signals and make a correct choice. In this way, these two institutions (which are core features of our representative democracy) ensure that truth emerges from competition.

[42] Indeed, it is not clear that penalties for lying or the threat of verification exist in deliberative settings. As far as we are aware, the experts who speak to citizens in deliberative settings face little (if any) consequences for misrepresenting the truth, and they are rarely (if ever) verified.

This paper proceeds as follows. We begin by describing our basic model of competition. We then prove that competition between experts, by itself, does not necessarily lead to the revelation of truthful information and does not allow the citizen to learn from the experts' signals. We next present two extensions to our basic model (one that imposes a penalty for lying upon the two competing experts and one that imposes a threat of verification upon the two competing experts), and we demonstrate the conditions under which these institutions induce both experts to make truthful statements to the citizen and, therefore, enable the citizen to make a correct choice. We then describe the experiments that we use to test our model's predictions, and we present our experimental results, which confirm these predictions. We conclude with a discussion of how competition in deliberative settings must be coupled with one or more institutions in order to lead to the revelation of truthful information and to enable citizens to learn from competing experts.

1. The Debate: Does Competition between Experts Help Citizens Learn?

1.1 The Virtues of Deliberation

For decades, scholars have extolled the virtues of deliberation, arguing that we should incorporate more deliberative practices into our political system. Specifically, scholars have argued that electoral processes should be made more deliberative by bringing citizens together, exposing them to the views of competing experts and politicians, and then having them discuss those views among themselves (Fishkin 1991; Ackerman and Fishkin 2004; Fishkin and Luskin 2005). Indeed, for many scholars, deliberative settings are seen as a procedural "cure all" for an uninformed citizenry (Gutmann and Thompson 1996; Fishkin 1991; Manin 1987; Gastil and Dillard 1999; Fishkin and Luskin 1996), for the oppression of minorities (Petit 2000), for social fragmentation (Gutmann and Thompson 1996; Cohen 1997), and for low levels of confidence in our government (Dryzek 2001; Cohen 1997; Ackerman and Fishkin 2004; Manin, Stein, and Mansbridge 1987), among other ills.

1.2 The (Im)possibility of Deliberation

In contrast to those who emphasize the virtues of deliberation, many scholars caution that deliberation is beneficial *only under narrow conditions* and that it may even have negative effects (Mansbridge 1983; Knight and Johnson 1994; Sanders 1997; Mendelberg 2002; Lupia 2002, 2004; Sunstein 2003; Posner 2004; McCubbins and Rodriguez 2006). For example, Lupia (2002, 2004) argues that because citizens have limited time, attention, and cognitive resources, deliberation is unlikely to effect positive outcomes. Building upon Lupia's arguments, McCubbins and Rodriguez (2006) demonstrate experimentally that, when there are costs associated with speaking and listening, deliberation brings about significant declines in social welfare and does not encourage citizens to speak, listen, and learn from one another. Indeed, in their experiments, citizens actually make worse decisions when they converse with one another than when they do not communicate at all.

It is this research on the difficulties of deliberation that we build upon in this study. Specifically, we draw upon the insights of Lupia and McCubbins (1998),[43] and we demonstrate both formally and experimentally that a key facet of the deliberative settings that scholars advocate (that is, competition between two experts) does not induce truthful statements or promote learning among citizens. Indeed, we find that only when competition between experts is combined with one or more institutions (such as a penalty for lying or a threat of verification) does it lead both experts to make truthful statements and allow the citizen to trust these statements, learn from them, and make a correct choice.

2. The Basic Model

We model competition between experts in a deliberative setting as a game between three players: expert 1 (denoted e1), expert 2 (denoted e2), and the citizen (denoted z). The citizen chooses one of two alternatives, which are called *a* and *b*, and the experts each send a signal to the citizen about his choice. The extensive form of this game is shown in Figure 4.1.

[43] Specifically, Lupia and McCubbins (1998) emphasize that competition only leads to the revelation of truthful information and promotes learning if the competing information provider is both knowledgeable and trustworthy.

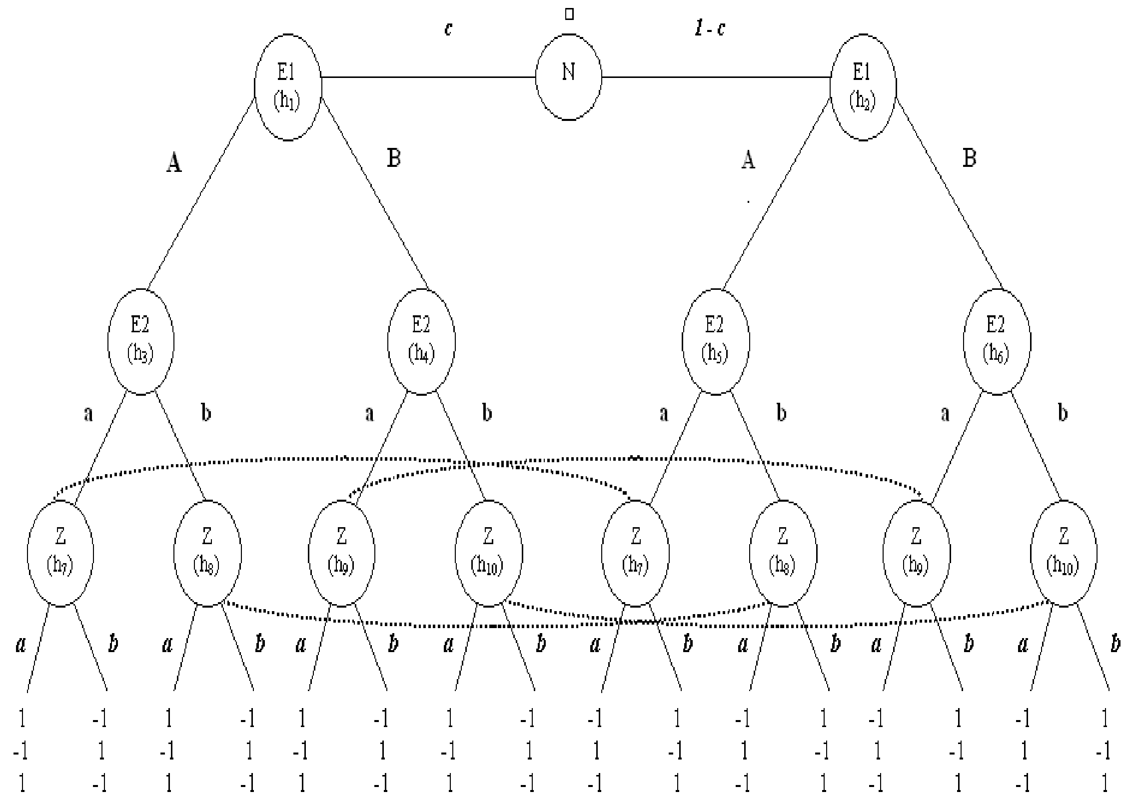

Figure 4.1. The Extensive Form of our Basic Model

The sequence of events in this game is as follows. First, a move by Nature determines the "state of the world." We denote this choice as $n_c \in$ {*correct, incorrect*}. This choice determines whether option *a* is the *correct* choice or the *incorrect* choice for the citizen. Nature chooses the state of the world n_c = *correct* with probability $c \in [0, 1]$ and the state of the world n_c = *incorrect* with probability 1 – *c*. If n_c = *correct*, then *a* is the correct choice (and *b* is the incorrect choice) for the citizen. Similarly, if n_c = *incorrect*, then *a* is the incorrect choice (and *b* is the correct choice) for the citizen. Note also that both expert 1 and expert 2 observe n_c, which makes them knowledgeable about whether option *a* or option *b* is the correct choice for the citizen. The citizen, on the other hand, does not observe n_c, but he does have prior beliefs about the "state of the world," which are represented by the probability *c*.

After the move by Nature, the game between the two experts and the citizen begins. First, expert 1 sends a signal to the citizen. We denote this signal $s_1 \in \{A, B\}$. If $s_1 = A$, then expert 1 states that option *a* is the correct choice (and that option *b* is the incorrect choice) for the citizen. If $s_1 = B$, then expert 1 states that option *b* is the correct choice (and that option *a* is the incorrect choice) for the citizen.[44] Expert 1 selects which of these two signals to send and may either lie or tell the truth. Additionally, expert 1 is assumed to have common interests with the citizen,[45] the implications of which we will discuss below.[46]

After expert 1 sends his signal, expert 2 observes expert 1's signal and then sends a signal of his own to the citizen. We denote expert 2's signal as $s_2 \in \{a, b\}$. If $s_2 = a$, then expert 2 states that option *a* is the correct choice (and that option *b* is the incorrect choice) for the citizen. If $s_2 = b$, then expert 2 states that option *b* is the correct choice (and that option *a* is the incorrect choice) for the citizen. As was true

[44] We follow Calvert (1985) and many others by modeling the experts' signals in this way. Indeed, Calvert (1985, p. 534) notes that the basic nature of any speaker's advice is a "distillation of complex reality into a simple recommendation."

[45] There are many interesting models of the effects that varying interests between an expert and a citizen have on incentives and behavior (see, e.g., Crawford and Sobel 1982, Austen-Smith 1990, and Lupia and McCubbins 1998).

[46] We could also model the interests between the experts and the citizen by having Nature select, with some probability, whether expert 1 or expert 2 has common interests with the citizen.

for expert 1, expert 2 selects which of these two signals to send and may either lie or tell the truth. Further, expert 2 is assumed to have conflicting interests with the citizen, the implications of which we will also discuss below.

Upon hearing both of the signals that expert 1 and expert 2 send, the citizen then chooses *a* or *b*. By assumption, the citizen does not know whether it is expert 1 or expert 2 who has common interests with him. Specifically, the citizen believes that there is a 50% chance that expert 1 has common interests with him (and that expert 2 has conflicting interests with him) and that there is a 50% chance that expert 2 has common interests with him (and that expert 1 has conflicting interests with him).[47] Although the citizen does not know the interests of the two experts, he does know that expert 1 and expert 2 are adversaries (i.e. what one expert gains, the other loses), and he also knows the order in which the two experts send their signals (i.e. if the citizen obtains the signal A, b, then he knows that expert 1 sent the signal A and that expert 2 sent the signal b). After the citizen chooses *a* or *b*, the game ends, and all players receive their utility payoffs.

Payoffs for the players are determined by three parameters: n_c, the citizen's choice of *a* or *b*, and the assumed interests of expert 1 and expert 2. As for the citizen,

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[47] We model the citizen's prior beliefs in this way because we are interested in analyzing the conditions under which citizens can learn from competing experts *who they do not know*. This aspect of our model corresponds to the deliberative settings that many scholars advocate. Indeed, when learning from competing experts in deliberative settings, citizens may not know which expert's interests are aligned with their own. That said, we could, of course, incorporate into our model more informative prior beliefs for the citizen. Indeed, if we were interested in analyzing the conditions under which citizens can learn from competing experts *who they know something about*, we could assume, for example, that the citizen believes that there is a 70% chance that expert 2 has conflicting interests with him.

if n_c = *correct* and he chooses *a*, then he receives a payoff of 1. However, if n_c = *correct* and he chooses *b*, then he receives a payoff of –1. Similarly, if n_c = *incorrect* and the citizen chooses *a*, then the citizen earns a payoff of –1, and if n_c = *incorrect* and the citizen chooses *b*, then the citizen earns a payoff of 1. Payoffs are described in Figure 4.1.

As mentioned above, expert 1 and expert 2's payoffs depend upon whether they have common or conflicting interests with the citizen and whether the citizen earns positive or negative utility. Specifically, if the citizen earns a payoff of 1, then expert 1 (who is assumed to have common interests with the citizen) also earns a payoff of 1. Similarly, if the citizen earns a payoff of –1, then expert 1 also earns a payoff of –1. On the other hand, expert 2 (who is assumed to have conflicting interests with the citizen) earns a payoff of –1 when the citizen earns a payoff of 1, and expert 2 earns a payoff of 1 when the citizen earns a payoff of –1. In this way, expert 1's incentives are aligned with those of the citizen, while expert 2's incentives are not. Note also that these payoffs induce zero sum competition between the two experts (i.e. when one expert gains 1 util, the other expert loses 1 util).

3. Defining Information Sets and Terms

We use the vector π_{e1} to denote expert 1's component of strategy profile π. π_{e1} has two elements, one for each of expert 1's information sets $h_{e1} \in \{h_1, h_2\}$. These information sets, as well as those of the other players, are shown in Figure 4.1. Note that expert 1's information sets are completely determined by Nature's choice. Further, each element $\pi_{e1}(s_1; h_j)$, where j = 1 or 2, is the probability that expert 1 signals $s_1 \in \{A, B\}$ if he is at information set h_j. These probabilities must sum to 1 for each information set.

We use the vector π_{e2} to denote expert 2's component of π. π_{e2} has four elements, one for each of expert 2's information sets $h_{e2} \in \{h_3, h_4, h_5, h_6\}$. Note that expert 2's information sets are determined by Nature's choice, as well as by expert 1's choice. Note also that we define each element $\pi_{e2}(s_2; h_j)$, where $j = 3 \ldots 6$, as the probability that expert 2 signals $s_2 \in \{a, b\}$ if he is at information set h_j. These probabilities also must sum to 1 for each information set.

As for the citizen, we use the vector π_z to denote the citizen's component of π. π_z has four elements, one for each of the citizen's information sets $h_z \in \{h_7, h_8, h_9, h_{10}\}$. The citizen's information sets are determined by expert 1's choice and by expert 2's choice; that is, the citizen observes an ordered signal from the two experts (i.e. he observes the signal A, then b; A, then a; B, then a; or B, then b), but he is uncertain about Nature's choice. Note that we define each element $\pi_z(a; s_1, s_2)$ as the probability that the citizen chooses *a* after observing the signals $s_1 \in \{A, B\}$ and $s_2 \in \{a, b\}$. Similarly, we define $1 - \pi_z(a; s_1, s_2)$ as the probability that the citizen chooses *b* after observing the same signal. These probabilities also must sum to 1 for each information set.

4. Uninformative Equilibria

This basic model yields an interesting result: Namely, competition does not necessarily induce both experts to send truthful signals to the citizen, and it also prevents the citizen from being able to learn from the experts' signals. Specifically, because the citizen has uninformative prior beliefs about whether expert 1 or expert 2 shares common interests with him, he cannot know which (if any) expert sent a truthful signal about whether *a* or *b* is the correct choice. For example, if the citizen obtains the signal A, b, he does not know whether expert 1 (who signaled A) or expert 2 (who signaled b) has common interests with him; therefore, he cannot learn anything from this signal. Similarly, if the citizen obtains the signal A, a, he still cannot learn from this signal because he does not know whether both expert 1 and expert 2 sent truthful signals or whether both expert 1 and expert 2 sent false signals. Because much research in cognitive science demonstrates that people ignore stimuli that they do not expect to help them with their decisions (for a survey of this body of literature, see Lupia and McCubbins 1998),[48] we assume that the citizen will ignore the experts' signals and base his choice upon his prior beliefs about the state of the world, *c*.

Given that the citizen ignores the experts' signals, both expert 1 and expert 2 will, in equilibrium, send either a truthful signal or a false signal to the citizen. Indeed, because both experts know that the citizen will ignore their signals, they can send either a truthful signal or a false signal to the citizen and not have an incentive to deviate from the signal they choose to send. Thus, there exist multiple equilibria to

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[48] Summarizing this body of research in cognitive science, Lupia and McCubbins (1998, p. 50) emphasize that "[P]eople *ignore* stimuli that they do not expect to facilitate reasoned choices…[P]ersuasion requires the [citizen] to believe that the expert's statement will help her avoid costly mistakes. That is, persuasion does not occur if the [citizen] believes that the expert is likely to have conflicting interests. If, however, the [citizen] believes that common interests are more likely, then persuasion is possible." Because the citizen in our model does not know which expert has common interests with him, our assumption that the citizen ignores the experts' statements (and that persuasion does not occur) is innocuous.

this game: In one equilibrium, both expert 1 and expert 2 send false signals and the citizen bases his choice upon his prior beliefs, *c*. In another equilibrium, expert 1 sends a truthful signal, expert 2 sends a false signal, and the citizen bases his choice upon *c*. In still another equilibrium, expert 1 sends a false signal, expert 2 sends a truthful signal, and the citizen bases his choice upon *c*, and in yet another equilibrium, both expert 1 and expert 2 send truthful signals and the citizen bases his choice upon *c*. Alternatively, expert 1 and/or expert 2 can play a mixed strategy, and the citizen still bases his choice upon *c*.

More formally, the equilibrium strategies for each player can be stated as follows:[49]

Expert 1's equilibrium strategies:

$$\pi_{e1}(A; h_1) = 1; \pi_{e1}(A; h_2) = 1$$

$$\pi_{e1}(A; h_1) = 1; \pi_{e1}(A; h_2) = 0$$

$$\pi_{e1}(A; h_1) = 0; \pi_{e1}(A; h_2) = 1$$

$$\pi_{e1}(A; h_1) = 0; \pi_{e1}(A; h_2) = 0$$

Expert 2's equilibrium strategies:

$$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 1$$

$$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 1$$

$$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 1$$

$$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 1$$

$$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 1$$

[49] Due to space constraints, we list formally only the pure strategies for expert 1, expert 2, and the citizen.

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 1$

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 1$

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 1$

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 1; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 0; \pi_{e2}(a; h_5) = 1; \pi_{e2}(a; h_6) = 0$

$\pi_{e2}(a; h_3) = 0; \pi_{e2}(a; h_4) = 1; \pi_{e2}(a; h_5) = 0; \pi_{e2}(a; h_6) = 0$

Citizen’s equilibrium strategies:

$\pi_z(a; \text{A}, \text{b}) = 1$ if $c > 0.5$; otherwise $\pi_z(a; \text{A}, \text{b}) = 0$

$\pi_z(a; \text{B}, \text{a}) = 1$ if $c > 0.5$; otherwise $\pi_z(a; \text{B}, \text{a}) = 0$

$\pi_z(a; \text{A}, \text{a}) = 1$ if $c > 0.5$; otherwise $\pi_z(a; \text{A}, \text{a}) = 0$

$\pi_z(a; \text{B}, \text{b}) = 1$ if $c > 0.5$; otherwise $\pi_z(a; \text{B}, \text{b}) = 0$

Proof: We assume that the citizen has uninformative prior beliefs about which expert shares common interests with him. Given this assumption, we further assume that the citizen ignores the experts’ signals and bases his choice upon his prior beliefs about the state of the world, c. That is, if $c > 0.5$, then the citizen chooses a, regardless of the signals that the experts sent. If $c < 0.5$, then the citizen chooses b, regardless of the signals that the experts sent. If $c = 0.5$, then the citizen randomly chooses a or b.

From these assumptions about the citizen's behavior, it follows logically that expert 1 and expert 2 can send either truthful signals or false signals in equilibrium. Indeed, for any signals that expert 1 and expert 2 send, they can never be made better off by switching to a different signal because the citizen is simply basing his decision upon his prior beliefs, *c*.

5. Extension #1: Penalties for Lying

Having demonstrated that competition, by itself, is not sufficient to induce both experts to send truthful signals to the citizen, we now couple competition between two experts with an institutional constraint that often exists in the real world—namely, penalties for lying. The addition of this institutional constraint changes the structure of the basic model in only one way. Specifically, if an expert sends a false signal to the citizen, he must pay a penalty, $p \geq 0$, which gets subtracted from his utility. So, for example, if $n_c = correct$, if expert 2 lies about the true state of the world (i.e. sends the signal b), and if the citizen nonetheless chooses *a*, then expert 2 earns utility = $-1 - p$. Similarly, if $n_c = correct$, if expert 2 lies about the true state of the world (i.e. sends the signal b), and if the citizen chooses *b*, then expert 2 earns utility = $1 - p$. The payoffs for expert 1 are determined in a similar manner, and if either expert sends a truthful signal to the citizen, then their payoffs remain the same as in the basic model.

This extension to our basic model yields an encouraging result: Namely, if $p > 2$, then, both expert 1 and expert 2 have a dominant strategy to send truthful signals to the citizen, and the citizen will, therefore, obtain a unified, truthful set of signals and will make a correct choice.

More formally, when $p > 2$, the equilibrium strategies for each player are:

Expert 1's equilibrium strategy:

$\pi_{e1}(A; h_1) = 1$; $\pi_{e1}(A; h_2) = 0$

Expert 2's equilibrium strategy:

$\pi_{e2}(a; h_3) = 1$; $\pi_{e2}(a; h_4) = 1$; $\pi_{e2}(a; h_5) = 0$; $\pi_{e2}(a; h_6) = 0$

Citizen's equilibrium strategies:

$\pi_z(a; A, a) = 1$

$\pi_z(a; B, b) = 0$

$\pi_z(a; A, b) = 1$ if $c > 0.5$; otherwise $\pi_z(a; A, b) = 0$

$\pi_z(a; B, a) = 1$ if $c > 0.5$; otherwise $\pi_z(a; B, a) = 0$

Proof: When $p > 2$, expert 1 has a dominant strategy to send a truthful signal to the citizen. Specifically, if expert 1 lies at either of his information sets when $p > 2$, then expert 1 either earns a payoff that is less than –1 (which occurs if the citizen makes a correct choice) or earns a payoff that is less than –3 (which occurs if the citizen makes an incorrect choice). However, if expert 1 tells the truth at either of his information sets, then he receives a payoff of 1 if the citizen makes a correct choice or a payoff of –1 if the citizen makes an incorrect choice. As these payoffs make clear, expert 1 can never be made better off by lying to the citizen when $p > 2$. Therefore, when $p > 2$, expert 1 will tell the truth in equilibrium.[50]

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[50] Imposing an institution (such as a penalty for lying) upon expert 1 is a bit redundant, given that this expert has common interests with the citizen. Stated differently, as long as the penalty for lying is large enough to ensure that expert 2 (the expert that has conflicting interests with the citizen) tells the truth at each of his information sets, then expert 1 will also tell the truth. The reason for this is that expert 1 has common

When $p > 2$, expert 2 also has a dominant strategy to send a truthful signal to the citizen. Specifically, if expert 2 lies at any of his information sets when $p > 2$, then expert 2 either earns a payoff that is less than –1 (which occurs if the citizen makes an incorrect choice) or earns a payoff that is less than –3 (which occurs if the citizen makes a correct choice). However, if expert 2 tells the truth at any of his information sets, then he receives a payoff of –1 if the citizen makes a correct choice or a payoff of 1 if the citizen makes an incorrect choice. As these payoffs make clear, expert 2 can never be made better off by lying to the citizen when $p > 2$. However, when $p \leq 2$, then the equilibrium strategies for expert 1 and expert 2 are the same as in the basic model (i.e. they either lie or tell the truth).

Given the incentives and equilibrium strategies of expert 1 and expert 2 when $p > 2$, the citizen will obtain unified, truthful signals. Thus, in equilibrium, the citizen will base his choice on the experts' signals and make a correct choice. If $p \leq 2$, then the equilibrium strategy for the citizen is the same as in the basic model.

6. Extension #2: Verification

Having demonstrated that the addition of a sufficiently large penalty for lying leads to the revelation of truthful information, we now couple competition between two experts with the threat of verification. The addition of this institutional constraint changes the structure of the basic model in only one way. Specifically, after both experts send their signals to the citizen (but before the citizen makes his choice), we

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interests with the citizen; that is, he earns positive utility only when the citizen makes a correct choice and earns positive utility.

add a second probabilistic move by Nature, which we denote $n_v \in \{n_c\ s_2,\ s_1\ n_c,\ s_1\ s_2,\ n_c\ n_c\}$.[51] That is, if either expert sends a false signal, then with probability $v \in [0, 1]$ Nature replaces that false signal (or false signals, if both experts lie) with n_c (the true state of the world) *and* imposes a conditional cost, $k \geq 0$, upon the lying expert(s). (Note that the cost, k, is conditional because it will only be imposed if verification occurs). With probability $1 - v$, Nature does not replace any false signals before the citizen makes his choice and does not impose any costs upon the expert(s). Note also that none of the players observe n_v. Thus, the experts do not know whether their signals will be verified before they send them, and the citizen does not know whether the signals that he obtains are the experts' signals or whether they have been verified by Nature.

This extension to the basic model also yields a promising result: Specifically, when v and k are sufficiently large,[52] then, in equilibrium, both experts have a

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[51] Note that Austen-Smith (1994) also models verification, but he focuses on one expert's inability to verify his or her lack of knowledge to citizens.

[52] Specifically, v and k must be large enough to ensure that expert 2 (i.e. the expert with conflicting interests) has a dominant strategy to tell the truth at each of his information sets. More formally, expert 2's expected benefit of lying (i.e. signaling b) and his expected benefit of telling the truth (i.e. signaling a) at h_3 can be defined as follows:

$$EU_{e2}(a; h_3) = -v[\pi_z(a; A, a)] + v[\pi_z(b; A, a)] + (-1 + v)[\pi_z(a; A, a)] + (1 - v)[\pi_z(b; A, a)]$$

$$EU_{e2}(b; h_3) = (-v - vk)[\pi_z(a; A, b)] + (v - vk)[\pi_z(b; A, b)] + (-1 + v)[\pi_z(a; A, b)] + (1 - v)[\pi_z(b; A, b)]$$

dominant strategy to send truthful signals to the citizen, and the citizen will, therefore, obtain a unified, truthful set of signals and will make a correct choice

More formally, the equilibrium strategies for each player when *v* and *k* are sufficiently large can be stated as follows:

Expert 1's equilibrium strategy:

$\pi_{e1}(A; h_1) = 1$; $\pi_{e1}(A; h_2) = 0$

Expert 2's equilibrium strategy:

$\pi_{e2}(a; h_3) = 1$; $\pi_{e2}(a; h_4) = 1$; $\pi_{e2}(a; h_5) = 0$; $\pi_{e2}(a; h_6) = 0$

Citizen's equilibrium strategies:

$\pi_z(a; A, a) = 1$

$\pi_z(a; B, b) = 0$

$\pi_z(a; A, b) = 1$ if $c > 0.5$; otherwise $\pi_z(a; A, b) = 0$

$\pi_z(a; B, a) = 1$ if $c > 0.5$; otherwise $\pi_z(a; B, a) = 0$

Proof: When *v* and *k* are sufficiently large, expert 1 has a dominant strategy to send a truthful signal to the citizen. Specifically, if expert 1 lies at either of his information sets when *v* and *k* are sufficiently large, then expert 1 earns a payoff that is less strictly less than the payoff that he earns when he tells the truth. Thus, expert 1 can never be made better off by lying to the citizen when *v* and k are sufficiently large, and he will tell the truth in equilibrium.

When the values of *v* and *k* are such that $EU_{e2}(a; h_3) \geq EU_{e2}(b; h_3)$, then, in equilibrium, expert 2 will tell the truth (i.e. send the signal a) at h_3. A similar logic holds for expert 2's expected benefit of lying and telling the truth at h_4, h_5, and h_6.

When *v* and *k* are sufficiently large, expert 2 also has a dominant strategy to send a truthful signal to the citizen. Specifically, if expert 2 lies at either of his information sets when *v* and *k* are sufficiently large, then expert 2 also earns a payoff that is less strictly less than the payoff that he earns when he tells the truth. Thus, expert 2 can never be made better off by lying to the citizen when *v* and k are sufficiently large, and he will also tell the truth in equilibrium. However, when *v* and *k* are not sufficiently large, then the equilibrium strategies for expert 1 and expert 2 are the same as in the basic model (i.e. they either lie or tell the truth).

Given the equilibrium strategies of expert 1 and expert 2 when *v* and *k* are sufficiently large, the citizen will obtain a unified, truthful set of signals. Thus, in equilibrium, the citizen will base his choice on the experts' signals and make a correct choice. If *v* and *k* are not sufficiently large, then the equilibrium strategy for the citizen is the same as in the basic model.

7. Experimental Tests of Our Theory

In order to test the theoretical results stated in the preceding sections, we conduct laboratory experiments that analyze whether and under what conditions competition between two experts induces trustworthy statements and enables citizens to learn from these statements and make correct choices.[53] To this end, we extend Lupia and McCubbins's (1998) and Boudreau's (2007) experimental designs, and we

[53] The experimental materials that we used in these experiments are available from the authors (Cheryl Boudreau at clboudreau@ucsd.edu or Mat McCubbins at mmccubbins@ucsd.edu).

randomly assign subjects (who are analogous to the citizen in our model) to treatment and control groups. We then ask subjects in both groups to solve binary choice math problems (as in our model, the subjects are asked to choose either *a* or *b* as the correct choice).[54] The math problems that we use are drawn from an SAT math test and consist of several different types of problems and various levels of difficulty. We tell subjects in both the treatment and control groups 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,[55] and neither earn nor lose 50 cents if they leave a problem blank.[56]

The main difference between the treatment and control groups has to do with the conditions under which subjects solve the math problems. In the control group, subjects solve 6 math problems one at a time, with 60 seconds allotted for each problem. For each problem that subjects in the control group solve, we pay them according to whether they solve the problem correctly, incorrectly, or leave the problem blank. The purpose of the control group is to establish a baseline for how well subjects perform on the math problems when they must make their choices based

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[54] For a discussion of why math problems are good to use in experiments about political decision making, see Boudreau (2007).

[55] Due to the financial costs of running these experiments, we reduced the payoffs for making correct and incorrect choices from plus or minus 1 dollar (util) to plus or minus 50 cents. Such a reduction in payoffs does not change the logic or the results of our model.

[56] It makes no difference for the results of our model if citizens simply choose *a* or *b* or if citizens first decide whether to answer or not answer and then, if they decide to answer, choose *a* or *b*.

only upon their prior beliefs about the correct answer (i.e. without an opportunity to learn from two competing experts' statements).

In the treatment group, subjects solve the very same 6 math problems one at a time, and they are also paid according to whether they solve the problem correctly, incorrectly, or leave the problem blank. Subjects in the treatment group, however, are exposed to two competing experts. Specifically, before each math problem that subjects in the treatment group solve, the experimenter randomly selects two subjects to act as "expert 1" and "expert 2" for that particular math problem, thereby creating a series of "one-shot" decisions for both the experts and the subjects.[57]

The experts' 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 still to solve the 6 math problems one at a time), the experts are shown the correct answer to a particular math problem and are then allowed to make a statement to the other subjects about the answer to that math problem. Specifically, expert 1 (who, like expert 1 in our model, has common interests with subjects) states whether *a* or *b* is the correct answer to the problem.[58] After expert 1 makes his or her statement, then expert 2 (who, like expert 2 in our model, has conflicting interests with subjects) makes a statement about whether *a* or *b* is the correct answer to the problem.

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[57] We design the experiment as a series of six "one-shot" decisions in order to obtain a greater number of observations from a single experiment. Conducting the experiment in this manner in no way compromises the correspondence between our model and our experiments.

[58] Note that the experimenter reads both experts' statements aloud to the subjects in order to prevent subjects from learning anything about the experts from the sound of their voices.

After expert 1 and expert 2 make their statements sequentially (which corresponds to the sequential nature of our model), the other subjects in the treatment group are then given 60 seconds to solve that particular math problem and to decide whether *a* or *b* is the correct choice. After 60 seconds have passed, we then randomly select two more subjects to act as expert 1 and expert 2 for the next math problem. We select two new experts for each math problem in order to ensure that our experiments are analogous to the static communication game that we model and to avoid any repeat play effects.

The key to our experimental design is twofold: First, as in our model, subjects know that one expert has common interests with them and that one expert has conflicting interests with them, but they do not know whether it is expert 1 or expert 2 who has common interests with them. Stated differently, subjects know that the experts are adversaries, but *they do not know which expert's interests are aligned with their own*. Second, both experts and the subjects know that the experts can make any statements that they wish. That is, the experts can lie about the correct answer to the math problem or tell the truth; it is entirely up to them. The experts' ability to make whatever statement they wish is constant throughout this experiment and is designed to be analogous to our model.

Although the experts can lie or tell the truth in all of our experimental conditions, we vary the institutional context in which the experts make their statements. Specifically, we first analyze the truthfulness of the experts' statements and the subjects' ability to choose the correct answer to the math problems when the experts are engaged in *competition*. We then alter this basic competition condition by imposing one of two institutions upon the competing experts: namely, a *penalty for lying* or a *threat of verification*. Each of these experimental variations is common knowledge at the outset of each part of the experiment, and each variation corresponds to our basic model and its extensions.

So how do we induce competition between the two experts and impose the institutional conditions within the context of our experiments? In short, we induce competition and impose the institutions by manipulating the ways that the experts and the subjects earn money. For example, in the competition condition, subjects are paid 50 cents for each and every math problem that they answer correctly. Similarly, expert 1 is paid 50 cents for each and every subject who solves a particular math problem correctly, and expert 2 is paid 50 cents for each and every subject who solves a particular math problem *incorrectly*. So, for example, if 11 subjects answer the math problem correctly, they earn 50 cents each, expert 1 earns $5.50 (i.e. 50 cents for each of the 11 subjects who answer the problem correctly), and expert 2 loses $5.50 (i.e. 50 cents for each of the 11 subjects who answer the problem correctly).[59] Similarly, if 11 subjects answer the math problem incorrectly, then they lose 50 cents each, expert 1 loses $5.50, and expert 2 earns $5.50. In this way, the interests of the two competing experts are adversarial, or zero sum.

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[59] Although our model analyzes the interaction between two competing experts and one citizen, we ran our experiments with two competing experts and 11 citizens (subjects). We ran the experiments in this way because we wanted a more cost-effective method of generating data on citizens' decisions. That said, we ensured that the experts and citizens in our experiments had the exact same incentives that they would have had if there had been only two experts and one citizen (for more on this point, see Lupia and McCubbins 1998).

For our next experimental variation, we maintain the competition between the two experts, and we then impose a penalty for lying upon both experts. So, in this segment of the experiment, the experts are again engaged in competition, but we announce to the experts and the subjects that both experts will incur a $15 penalty if they lie about the correct answer to the math problem. Note that we impose a $15 penalty because that amount is large enough to ensure that expert 1 and expert 2 both have a dominant strategy to tell the truth and that the subjects know this.

To see why this is the case, consider the way that expert 2 (i.e. the expert with conflicting interests) earns money under this condition: Given that there are conflicting interests between expert 2 and the subjects, expert 2 earns $5.50 if each and every subject answers a problem incorrectly. Although at first blush this might seem to give expert 2 an incentive to lie, note that the $15 penalty for lying will reduce expert 2's gain of $5.50 down to a loss of $9.50. Further, if expert 2 lies and all of the subjects happen to answer the problem correctly, then expert 2 will lose $20.50 (i.e. a $15 loss because of the penalty for lying and a $5.50 loss because 11 subjects answered the problem correctly).

If expert 2 tells the truth, however, then the worst he or she can do is to lose $5.50 (which will happen if each and every subject answers the problem correctly), and the best that he or she can do is to earn $5.50 (which will happen if each and every subject answers the problem incorrectly). As these payoffs make clear, expert 2 is always better off if he or she tells the truth about the correct answer to the math problem. The same is, of course, true for expert 1, who has common interests with subjects.

For our other institutional condition—namely, verification—we again maintain the competition between the two experts. However, instead of imposing a penalty for lying upon the experts, this time, we verify both experts' statements to make sure that they are true statements before they are read to subjects. Specifically, in the 100% chance of verification condition, if either expert chooses to make a false statement about the correct answer to the math problem, then we charge that expert $2 and announce the correct answer to subjects. If either expert makes a true statement, then we simply read that expert's statement to the subjects. Note that we use a 100% chance of verification and a $2 cost because these values are large enough to ensure that both expert 1 and expert 2 have a dominant strategy to tell the truth.

Because subjects earn money for answering problems correctly and lose money for answering problems incorrectly, our experiments yield a straightforward measure of whether competition (with or without the addition of institutions) enables subjects to learn from the experts' statements and make correct choices. Specifically, we calculate and then compare the average amounts of money that subjects earn per math problem in each of our experimental conditions and in the control group. Although our model, of course, does not make explicit predictions about the amounts of money that subjects will earn, it does predict when subjects will learn from the two competing experts' statements and when subjects will be forced to rely on their prior beliefs when making their choices. Because subjects in the control group do not have an opportunity to learn from the competing experts' statements, this condition provides a baseline for the amount of money that subjects earn when solving the problems based solely on their prior beliefs. Given that we randomly assign subjects to treatment and control groups, any improvements that we observe relative to the control group can be attributed to subjects learning from the two competing experts' statements.

8. Methodology

In order to test our model's predictions, we conducted laboratory experiments at a large public university. When recruiting subjects, we posted flyers at various locations on campus (for example, in front of the library, in the cafeterias, in the dormitories, and in academic buildings), and we also sent out campus-wide emails to advertise the experiments. A total of 144 adults who were enrolled in undergraduate classes and who were of different ages, races, genders, and college majors participated in the experiments.

When analyzing the data gleaned from these experiments, we conduct two different types of comparisons. First, we simply compare the percentage of times that expert 1 and expert 2 make true and false statements under each experimental condition. Such an analysis enables us to test the predictions that our model makes about the conditions under which the experts will (and will not) tell the truth. Second, we compare the average amounts of money that subjects earn in each experimental condition and in the control group. To this end, we conduct difference of means tests to examine whether subjects who are exposed to the two competing experts in each experimental condition earn significantly more money than do subjects in the control group, who must make their choices based solely upon their prior beliefs about the correct answer to the math problem.

9. Results

9.1 Competition Condition

As our results for the competition condition demonstrate, each of our predictions is borne out in the data. Specifically, Table 4.1 reveals that, as we predicted, expert 1 and expert 2 made both truthful statements and false statements to subjects. That is, in ten out of twelve trials (83% of the time), expert 1 told subjects the truth about the correct answer to the math problem, and in two out of twelve trials (17% of the time), expert 1 lied to subjects about the correct answer to the math problem. Further, in three out of twelve trials (25% of the time), expert 2 made a truthful statement to subjects, and in nine out of twelve trials (75% of the time), expert 2 lied to subjects about the correct answer to the math problem.[60]

[60] Although there exist multiple equilibria to this communication game, we are not surprised that subjects in our experiments typically converged on the equilibrium in which expert 1 tells the truth and expert 2 lies.

Table 4.1. The Experts' Statements by Experimental Condition[61]

Condition:	Expert Number:	% True Statements:	% False Statements:
Competition	*Expert 1*	83% (10/12)	17% (2/12)
	Expert 2	25% (3/12)	75% (9/12)
Penalty for Lying	*Expert 1*	100% (12/12)	0% (0/12)
	Expert 2	92% (11/12)	8% (1/12)
Verification	*Expert 1*	100% (12/12)	0% (0/12)
	Expert 2	100% (12/12)	0% (0/12)

Because subjects in our experiments (like the citizen in our model) do not know whether expert 1 or expert 2 has common interests with them, we expect them to ignore the competing experts' signals and base their decisions upon their prior beliefs about whether *a* or *b* is the correct choice. This prediction is also confirmed by our data, as subjects in this experimental condition were not able to increase the amount of money that they earn, relative to the amount of money that subjects in the control group earn. As shown in Figure 4.2, subjects in the control group earn, on average, \$0.13 per problem (N = 66), and subjects in the competition condition earn, on

[61] A total of 72 subjects acted as either expert 1 or expert 2 in our experiments. This yields 12 observations for each expert in each experimental condition.

average, $0.19 per problem (N = 78). This difference between treatment and control group subjects is not statistically significant at conventional levels (specifically, p = 0.2).

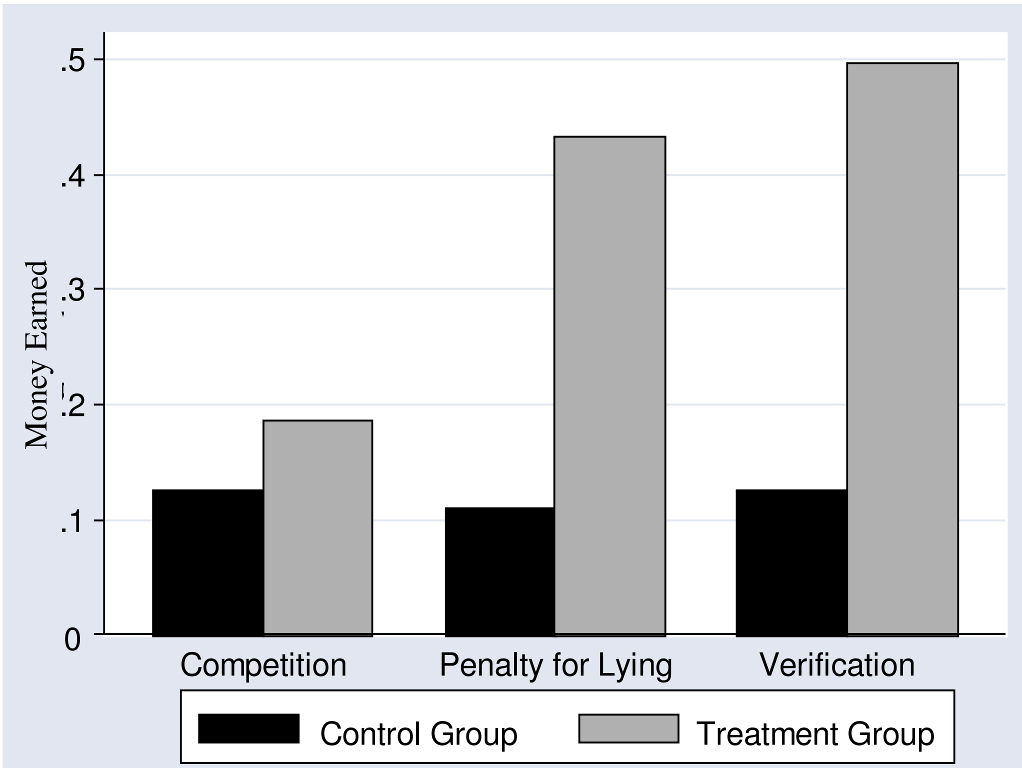

Figure 4.2. Money Earned in Our Treatment and Control Groups[62]

9.2 Penalty for Lying Condition

When we impose a $15 penalty for lying upon both of our competing experts, our predictions are again confirmed by the data. Specifically, Table 4.1 shows that expert 1 always makes a truthful statement to subjects; that is, in twelve out of twelve

[62] In this graph, we compare the treatment group's performance on the two math problems that they solved in each experimental condition with the control group's performance on these *exact same two math problems*. Because we break down the six math problems in this way, we obtain slightly different results for the control group depending upon the experimental condition.

trials (100% of the time), expert 1 told subjects the truth about the correct answer to the math problem, which is perfectly consistent with our predictions. Further, expert 2 also makes truthful statements to subjects about the correct answer to the math problem. Indeed, in eleven out of twelve trials (92% of the time), this expert told subjects the truth about the correct answer to the math problem.

Because subjects in our experiments know that the penalty for lying is large enough to induce both experts to tell the truth, we expect them to trust and then learn from the competing experts' statements. This prediction is also confirmed by our data, as subjects were able to learn from the competing experts' statements and increase the amount of money that they earn, relative to the amount of money that subjects in the control group earn. Specifically, Figure 4.2 shows that subjects in the control group earn, on average, \$0.11 per problem ($N = 66$), and subjects in the penalty for lying condition earn, on average, \$0.43 per problem ($N = 78$). This difference between treatment and control group subjects is statistically significant (specifically, $p < 0.001$).

9.3 Verification Condition

When we impose a 100% chance of verification upon both of our competing experts, our predictions are again confirmed by the data. Specifically, Table 4.1 shows that expert 1 always makes a truthful statement to subjects; that is, in twelve out of twelve trials (100% of the time), expert 1 told subjects the truth about the correct answer to the math problem. Further, expert 2 also always makes a truthful statement to subjects about the correct answer to the math problem. Indeed, in twelve out of twelve trials (100% of the time), this expert also told subjects the truth about the correct answer to the math problem.

Because expert 1 and expert 2 behave exactly as our model predicts (i.e. they always tell the truth about the correct answer to the math problem), subjects in this experimental condition always received unified, truthful signals about the correct answer to the math problem. Knowing that there exists a 100% chance of verification, subjects were able to learn from the competing experts' signals and increase the amount of money that they earn, relative to the amount of money that subjects in the control group earn. Specifically, Figure 4.2 shows that subjects in the control group earn, on average, \$0.13 per problem (N = 66), and subjects in the verification condition earn, on average, \$0.50 per problem (N = 78). This difference between treatment and control group subjects is statistically significant (specifically, $p < 0.001$).

10. Conclusion

In this paper, we developed a formal model and an experimental test of a simple communication game between two experts (who are competing with each other) and a citizen. The result that emerges in our model and in our experiments, however, is anything but simple. Indeed, it suggests that *competition between experts, by itself, does not necessarily reveal truthful information*. Rather, in equilibrium, it prevents the citizen from being able to learn from the experts' signals, and it, therefore, induces him to ignore the experts' signals and base his decision upon his prior beliefs. Knowing that the citizen will ignore their signals and make a decision based upon his prior beliefs, both expert 1 and expert 2 can either lie or tell the truth in equilibrium. Indeed, there exist multiple equilibria to this game, many of which involve one or both of the experts lying to the citizen about whether *a* or *b* is the correct choice.

However, once competition is coupled with institutional constraints (constraints that are common in real world competitive contexts, but largely absent in the deliberative settings that scholars advocate), then the citizen's ability to learn from the experts' statements and make a correct choice improves. Specifically, when both experts face a sufficiently large penalty for lying, then, in equilibrium, both experts send truthful signals to the citizen, and the citizen then obtains a unified, truthful set of signals and makes a correct choice. This prediction is largely confirmed by our experimental results. Similarly, when both experts face a sufficiently large probability of verification and conditional cost, then, in equilibrium, both experts send truthful signals to the citizen, and the citizen again obtains a unified, truthful set of signals and makes a correct choice. This prediction is also confirmed by our experimental results.

As for the implications that our findings have for debates about deliberation, we emphasize that, at a minimum, our results suggest that competition between experts and politicians in deliberative settings must be coupled with institutions that establish the conditions for trustworthy communications.[63] Absent such institutions,

[63] We acknowledge that our theory and experiments analyze only one aspect of the deliberative settings that scholars advocate. That is, they focus exclusively on the conditions under which citizens can learn from competing experts, and they do not assess whether and under what conditions citizens will speak, listen, and learn from one another after they have heard the experts' statements. For an experimental assessment of this latter aspect of deliberation, see McCubbins and Rodriguez (2006).

the deliberative settings that scholars advocate are unlikely to induce both experts to tell the truth and allow citizens to trust and learn from their statements. In the extreme, the results of our model and experiments suggest that perhaps deliberation is not the best way to promote citizen learning, for the institutions that may ensure trustworthy communications (such as penalties for lying and the threat of verification) are already in place in many political, legal, and economic contexts. Thus, rather than recruiting citizens, experts, and politicians to converse with one another in deliberative settings, scholars might instead develop ways to bolster our existing institutions so that they better facilitate citizen learning.

Conclusion

It is widely known that citizens use cues when making political decisions. Indeed, given the plethora of experimental and survey-based research demonstrating that citizens use cues such as party labels, polls, candidates' appearances, and endorsements, there is little doubt that cues play an important role in our political system. What is still in doubt, however, is whether and under what conditions particular cues help different types of citizens to improve their decisions in different contexts. Specifically, given the varied levels of sophistication among citizens and the wide variety of situations in which they may find themselves, it seems unlikely that cues are equally effective for all citizens, at all times, and in all places.

In my dissertation, I began to identify conditions under which particular cues help citizens to improve their decisions. More concretely, I asked 1) whether and under what conditions a single cue (namely, the statements of an endorser) helps both sophisticated and unsophisticated citizens to improve their decisions, 2) whether and under what conditions multiple cues work better than one cue alone, and 3) whether and under what conditions citizens can learn from the statements of multiple speakers. My theoretical and empirical results show that:

- The statements of an endorser only improve the decisions of both sophisticated and unsophisticated citizens when the endorser's incentives are clear. Once the endorser's incentives become less transparent (as is often the case in the real world), this cue no longer helps citizens to improve their decisions consistently, nor does it consistently close the gap between sophisticated and unsophisticated citizens.

- Two cues are not necessarily better than one cue alone. That is, when one cue enables citizens to achieve large improvements in their decisions, the presence of a second cue does not help citizens to improve their decisions further. However, when neither cue is particularly useful by itself, these cues help citizens to make better decisions than they make when only one of these cues is present.

- Competition between two experts does not necessarily induce both experts to make truthful statements; thus, citizens are unable to learn from the experts' statements and improve their decisions under these circumstances. Indeed, competition between experts only induces truthful statements and enables the citizen to learn once it is combined with institutions, such as a penalty for lying or a threat of verification.

Taken together, my results demonstrate that particular cues are not equally effective for all citizens in all contexts. Although there is, of course, much more work that needs to be done to identify the conditions under which cues improve citizens' decisions, I have taken an initial step toward this goal in my dissertation. In other work, I examine the effectiveness of different cues (for example, polls) in still other contexts (for example, when cues send conflicting signals about the correct choice for citizens). This work, combined with my dissertation, provides a more nuanced picture of how citizens use cues, and it also indicates that cues are much less effective than previous research suggests.

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