

# Politics from Anarchy to Democracy

*Rational Choice in Political Science*

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Stanford University Press  
Stanford, California

2004

*Can Political Institutions Improve  
Citizens' Competence?*

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MANY CITIZENS base political decisions on limited information about the consequences. If lacking information causes citizens to choose differently than if they knew these consequences, then it is common to say that they are incompetent with respect to the tasks at hand. Many observers are concerned about citizens' competence. While some react by chiding the masses for the apparent qualities of their decisions, others attempt to structure political institutions in ways that increase competence (i.e., they try to help citizens make the same choices they would have made if they were more knowledgeable about the consequences).<sup>1</sup>

As a general matter, it is correct to assert that providing more information to decision makers can increase their competence. It is also correct to assert that political institutions can be designed in ways that increase the amount of information available to decision makers. In what follows, however, I argue that the correspondence between an institutional design that provides more information and the competence of the citizens to whom that information is directed depends on how citizens process the information. In short, institutional designs improve competence only if citizens process information in particular ways.

Consider, for example, the plight of citizens as jurors. Jurors are asked to distill verdicts from competing claims about contentious issues. They hear testimony from many witnesses. Their ability to render a competent verdict—the verdict they would deliver given knowledge of relevant events—depends on which testimony they choose to believe.

When making such decisions, jurors may want to consider witnesses' motives. If jurors know witnesses to be motivated by their desire to tell the truth, they gain an incentive to believe the testimony. But such knowledge is

an ideal. In reality, witnesses and jurors often know little about each other, making motives difficult to discern.

Institutional attributes can help jurors overcome their lack of knowledge about witnesses' motives, which, in turn, can help them use testimony more effectively. The ability to cross-examine witnesses and the threat of penalties for perjury, for example, can affect which witnesses jurors believe. When an institutional attribute induces witnesses to tell the truth at the same time that it induces jurors to believe what they hear, then it makes competent verdicts more likely.

More generally, if someone wants to design an institution in order to increase citizens' competence, then they must be reasonably correct about how citizens use information. I have argued elsewhere (Lupia 2002) that mistakes about how citizens use information are why many well-intentioned efforts to improve civic competence—such as public interest Web sites, deliberative schemes, and civic education campaigns—fail to have the effects that their advocates anticipate.<sup>2</sup> Here, I use a formal model and two experiments to reveal a general principle that makes such efforts more effective.

The model, from Lupia and McCubbins 1998, clarifies the conditions under which an instrumentally motivated speaker can persuade an instrumentally motivated receiver to change the latter's beliefs and behaviors. Persuasion—defined as causing a change in beliefs—is critical to the question at hand, as the only way that a speaker's advice can increase a receiver's competence is if it changes the receiver's beliefs. In the model, the receiver must choose one of two alternatives and is uncertain about which one provides higher utility. The speaker advises the receiver about which alternative is better, but the receiver is uncertain about whether his claim is true. If the receiver ignores the signal, she may not have sufficient information to make an optimal choice. But if she believes the speaker, and if the speaker gives false advice, then her choice is also suboptimal.

I use the model to show how variations in a common institutional attribute, *the threat of verification*, affect how citizens process information. In the model, verification comes in the form of subsequent information that allows the receiver to verify whether the speaker's signal is true or false. I show that increasing the verification threat increases the receiver's competence (i.e., causes her to make the utility-maximizing choice) only if

- The receiver perceives the speaker to know more than she does about which alternative is better.
- The increased verification threat is high enough to induce the receiver to follow the speaker's advice.
- The increased verification threat actually induces the speaker to make a truthful statement.

If any of these conditions fails, then the increased verification threat is not sufficient to increase competence.

I then use two experiments to evaluate the model's conclusions. In both experiments, some subjects (receivers) are initially uncertain about which of two alternatives will yield a higher payoff. Other subjects (speakers) send signals about which alternative pays them more. The key experimental variation is a change in the verification threat. The experimental data reinforce the multipart conclusion stated above.

Moreover, it is worth noting that the two experiments are very different. One was conducted in a laboratory; the other was not. In one, I paid people for their choices. In the other, I did not. I conducted one experiment on a self-selected sample of undergraduates; the other was conducted on a randomly selected sample of Americans. In one experiment, I had great control over the subject's environment; in the other, I had little control. That key aspects of the correspondence between verification and competence appear in both experimental contexts reinforces the model's empirical relevance.

In sum, this chapter demonstrates that institutional design can be an important part of efforts to increase citizens' competence, but only if certain conditions are met. To see how the stated conditions can aid future attempts to improve citizens' competence, note that the negation of each condition reveals a reason why such attempts fail. First, information is often provided to people who ignore it. Second, the information is often provided by a source that is not sufficiently credible in the eyes of the intended recipients. Third, people are induced to use information that is not helpful to them. Put another way, to increase citizens' competence it is necessary to get their attention, gain their trust, and then deliver the information that will help them better understand the consequences of their actions. To gain trust, receivers must believe that speakers either have their best interests at heart *or are operating in an institutional context that clarifies speaker motivations and makes truth-telling incentive compatible.*

### *Theory*

Citizens must make decisions about things they cannot experience directly. For voters, the task is to choose candidates whose future actions in office cannot be experienced in advance of the election. For jurors, the task is to determine who is responsible for a crime they did not witness.

Relying on others for information in such circumstances can be an efficient way to acquire knowledge. In political situations, however, the strategy can be fraught with risk. Many people who provide political information (e.g., campaign organizations, lobbyists, courtroom witnesses) do so out of self-interest, which can induce false or misleading presentations.

For citizens who rely on others for information (e.g., witnesses, friends, the media), their competence depends on how they choose whom to believe. If they believe people who provide accurate information, they can make better decisions. *When can institutions help citizens make such choices more effectively?*

In this section, I derive conditions under which a particular kind of institutional design has this desirable attribute. The model integrates and builds from relevant insights in social psychology and economics. Psychology's contributions include Richard Petty and John Cacioppo's (1986) and Alice Eagly and Shelly Chaiken's (1993) work on the ways in which persuasion can occur. From economics I draw insights on how organizational designs affect credible commitment (see, e.g., North 1994) and incentives to convey information (see, e.g., Laffont and Tirole 1993). The model's most direct ancestors, however, are economic signaling and cheap talk models.

The seminal *signaling model* focuses on the plight of an employer who needs to hire a new worker (Spence 1973). While the employer prefers to hire a skilled applicant, she cannot observe skill levels in advance. However, she knows that skilled applicants can purchase a formal education with less effort than can unskilled applicants. Moreover, she can observe whether an applicant has a degree. The model's conclusion is that the degree persuades the employer of the applicant's skill level when unobservable skill levels and observable education levels are highly correlated.

The seminal *cheap talk model* has similar dynamics (Crawford and Sobel 1982).<sup>3</sup> The model features a speaker and a receiver. The receiver's job is to make a choice. Before the receiver chooses, the speaker—who is fully informed—signals the receiver about the consequences of her choice. The model's conclusion is that persuasion requires a speaker and receiver to have *common interests*.<sup>4</sup> The intuition underlying this result is straightforward; if choices that are good for the receiver also benefit the speaker, then the speaker has an incentive to reveal what he knows and the receiver has an incentive to believe what she hears. By contrast, if what is good for a speaker is bad for a receiver, then the speaker has an incentive to reveal nothing and the receiver has an incentive to ignore everything. Collectively, signaling and cheap talk models clarify how the costs of communication (i.e., the purchase of a good in signaling models or opportunity costs in cheap talk models) affect what people say and believe.

The model presented below generates different conclusions about learning in political settings. It does so by relaxing key assumptions about what the speaker and receiver know. In particular, we allow the receiver to be uncertain about what a speaker knows and whether the speaker's interests conflict with her own. In what follows, I describe the part of the model that clarifies when increasing a verification threat boosts the receiver's competence.

## MODEL SPECIFICS

In Lupia and McCubbins 1998, communication is modeled as an interaction between two players, a *speaker* and a *receiver*. The *speaker* makes a statement about which of two alternatives,  $x$  and  $y$ , offers higher utility to the receiver. The receiver then chooses one of these alternatives. Unless stated otherwise, all elements of the game are common knowledge.

The model's key feature is that it contains three distinct sources of uncertainty. The first source of uncertainty pertains to which of the two alternatives is better for (i.e., gives higher utility to) the receiver. The receiver has beliefs about, but need not know, which alternative is better. Lupia and McCubbins 1998 represents these beliefs with the parameter  $b \in [0, 1]$ , which is the probability that  $x$  is better for the receiver.

The second source of uncertainty pertains to the speaker's knowledge. With probability  $k \in [0, 1]$ , the speaker knows which alternative is better for the receiver. With probability  $1 - k$ , he knows only  $b$ . In other words, *the speaker need not know which alternative is better for the receiver* (i.e.,  $k$  need not equal 1), and *the receiver can be uncertain about how much the speaker knows* (i.e., the receiver knows  $k$ , the probability that the speaker is knowledgeable, but does not know the speaker's actual knowledge).

The third source of uncertainty pertains to interests. The speaker and receiver have *common* interests if the speaker benefits when the receiver makes a utility-maximizing decision. The speaker and receiver have *conflicting* interests if the speaker earns negative utility when the receiver earns positive utility and vice versa. In this model, players have *common* interests with probability  $c \in [0, 1]$  and *conflicting* interests with probability  $1 - c$ . In other words, the receiver can be uncertain about the speaker's interests with  $1 - c$  and  $c$  being the receiver's prior belief about the probability that she and the speaker have common interests.

Next, the speaker makes one of two statements, "better" or "worse." The statement "better" means "I assert that  $x$  is better than  $y$  for the receiver." The statement "worse" means "I assert that  $x$  is worse than  $y$  for the receiver." The speaker selects which statement to make and *need not tell the truth*. Then, the receiver chooses  $x$  or  $y$ . After she does so, the game ends and both players receive a utility payoff.

The model also features verification threats as one of several institutional attributes that it analyzes. It represents verification as follows—after the speaker speaks, but before the receiver chooses, nature reveals to the receiver whether  $x$  is better or worse for her. Verification occurs with probability  $0 < \nu < 1$ . For example, when  $\nu = .3$ , the speaker believes that there is a 30 percent chance that the receiver will know the truth when she

makes her choice and a 70 percent chance that she will have only the speaker's advice and her own prior beliefs to go on.

The direct effect of an increase in the model's verification threat is to reduce the expected return to the speaker of lying to the receiver (Lupia and McCubbins 1998, 56–57). This effect can change the speaker's signal and the receiver's incentive to believe it. For example, if the receiver knows that the increased verification threat dissuades the speaker from lying, then the threat gives her a greater incentive to base her choice on the signal. In cases where an increased threat reduces the receiver's uncertainty about the speaker's willingness to lie, her incentive to believe the speaker can also increase. For a more precise description of the verification effect, see Lupia and McCubbins 1998, 250–51.

The following proposition paraphrases the aspects of theorems 3-1 to 3-3 from Lupia and McCubbins 1998 that are relevant to how verification threats affect competence.<sup>5</sup> Below the proposition, I present an explanation that follows the proposition's sequence of claims.

Proposition:

- a. If  $\nu = 0$ , perceived common interests ( $c > .5$ ) and perceived speaker knowledge ( $k > 0$ ) are each necessary, but not sufficient, for persuasion.
- b. If  $\nu > 0$ , the extent to which perceived common interests and perceived speaker knowledge are required decreases when  $\nu$  changes. In other words, with respect to persuasion, the threat of verification can substitute for perceived common interests.
- c. Persuasion also requires that the receiver's prior beliefs,  $b$ , not be too strong relative to her beliefs about speaker attributes and the verification threat.
- d. Increasing the receiver's competence requires persuasion, the receiver initially lacking sufficient information to choose the utility-maximizing alternative, and a correct postverification perception of the statement's veracity.

I begin by explaining the conditions for persuasion, absent a verification threat (part a of the proposition). If the receiver is *certain* that the speaker knows no more than she does about which alternative is better for her ( $k = 0$ ), then persuasion is impossible. This is true even if the receiver is certain that she and the speaker share common interests ( $c = 1$ ). By contrast, if the receiver believes that the speaker *might* possess the knowledge she requires ( $k > 0$ ), then persuasion is possible. Similarly, if the receiver is certain that she and the speaker have conflicting interests ( $c = 0$ ), then persuasion is impossible. By contrast, if the receiver believes that the speaker has common interests (e.g.,  $c > .5$ ), then persuasion is possible. A corollary reveals further implications.

Corollary: Actual common interests are neither necessary nor sufficient for persuasion. Actual speaker knowledge is neither necessary nor sufficient for persuasion.

So, if the receiver is uncertain about the speaker's actual knowledge or interests—a plausible assumption for many political contexts—then *perceived* speaker knowledge and *perceived* common interests (as opposed to *actual* speaker knowledge and *actual* common interests) drive persuasion. As a result, a knowledgeable speaker who shares common interests with a receiver will fail to persuade if the receiver does not accurately perceive these speaker attributes. In Vincent Crawford and Joel Sobel's (1982) model, by contrast, "equilibrium signaling is more informative when agents' preferences become more similar" (1431). There are two reasons for this difference. One is that the speaker in our model need not know which alternative is better for the receiver. The other is that the receiver in our model need not know the speaker's actual interests or knowledge.

In sum, part a of the proposition reveals that persuasion in the model requires the receiver to be initially uncertain about which alternative is better for her and to believe that the speaker has such knowledge. Parts b and c state additional requirements. Part b reveals that increasing verification threats reduces the extent to which a receiver must be certain about the commonality of her interests with the speaker. In other words, increased verification threats generally act as a substitute for knowledge of common interests—they give receivers a reason to believe people whom they would not regard as credible absent the threats. Part c reveals that the extent to which perceived common interests or verification threats are needed depends on the strength of the receiver's prior beliefs about which alternative is better. If a receiver initially thinks that alternative  $x$  is almost surely better for her than alternative  $y$ , the magnitude of interest commonality or verification threat required to induce her to change her mind is much higher than would be the case if she was initially indifferent between  $x$  and  $y$ . When we combine parts b and c, we find that persuasion requires that the receiver believe that the speaker has an incentive to reveal what he knows—regardless of whether the incentive emanates from perceptions of speaker attributes or the threat of verification—and that the incentives are sufficiently strong to counter her prior beliefs about which alternative is better.

Part d of the proposition reveals that for the verification threat to increase the receiver's competence, it must also be the case that the receiver's new perception of speaker incentives is accurate. For if persuasion occurs with inaccurate perceptions (i.e., the receiver believes that a verification threat,  $v = .8$ , is sufficient to dissuade many speakers from lying but, in actuality, the speaker with whom she is dealing is one of the rare types not dissuaded),



then the receiver has a reason for believing that the statement is true when, in fact, it is false. Put another way, the threat of verification is most helpful to the receiver when—without it—she lacks information sufficient to determine the speaker's interests and—with it—she can correctly infer the speaker's incentive to speak truthfully.

In sum, the model predicts that an increased verification threat increases the receiver's competence only if the following three conditions are true: the receiver lacks so much information about her choice's consequence that she has an incentive to attend to the speaker's advice, the probability of common interests or the verification threat are sufficiently high to induce the receiver to follow the speaker's advice, and the threat and actuality of verification are sufficient to induce truthful advice.

My experiments address two questions about these claims. First, are verification threats sufficient to increase decision makers' competence? Second, are the conditions under which I observe such effects consistent with those derived from the model? In what follows, the answer to both questions is yes.

### *Experiment 1*

In this experiment (from Lupia and McCubbins 1998, 135–39), as in the model described above, the receiver chooses one of two alternatives, while the speaker advises the receiver about his or her choice. Specifically, the receiver predicts whether an unobserved coin toss landed on heads or tails and the speaker advises “heads” or “tails.”

We ran these experiments on undergraduates at the University of California, San Diego. We recruited subjects by posting flyers on the campus.<sup>6</sup> When subjects came for their appointments, we paid them a nominal amount (two dollars) for showing up. We then asked subjects to read and sign a standard consent form. The form told them that they would be in an experiment on decision making.

In each trial that followed, we paid the receiver one dollar for a correct prediction and nothing for an incorrect prediction. The key experimental variation lies in how we compensated the speaker. In some cases, the speaker earned one dollar when the receiver predicted correctly (i.e., the speaker and receiver had common interests). In other cases, the speaker earned fifty cents when the receiver made an *incorrect* prediction (i.e., the receiver and speaker had conflicting interests).<sup>7</sup> And, as we describe below, the receivers were often uncertain about which compensation scheme was in place (i.e., they were uncertain about whether or not they and the speaker had common interests).

Most experiments featured one speaker and ten to twelve receivers. The

receivers had identical information and payoff schedules. Therefore, to the speaker, their situation was no different than if they were speaking to just one receiver—as is the case in the model.

To isolate the effect of verification, we took steps to limit what the subjects could learn about each other. For example, we used partitions to prevent visual contact. We also asked the speaker to signal “heads” or “tails” by checking an appropriately labeled box on a sheet of paper. A graduate assistant relayed the paper signal to me via hand signal. Then, I, not knowing the outcome of the coin toss or whether the speaker’s statement was true or false, announced the speaker’s signal. These steps prevented subjects from basing their decisions on information not present in the model. Table 6.1 summarizes our observations.

In our experiments’ initial trials, we observed what receivers would choose if fully informed about the coin toss outcomes. In these trials, receivers made correct predictions almost always (97 percent, 376/389). Then we examined what receivers would choose if they were unable to observe the coin tosses and received no further information. In these trials, correct predictions occurred at about the rate of chance (48 percent, 377/780).

In all subsequent trials reported below, receivers did not observe the coin toss outcomes and were uncertain about the speaker’s compensation scheme. Specifically, we rolled an eight-sided die once per trial. If the die landed on one or two, then we paid the speaker for each correct prediction a receiver made; otherwise, we paid the speaker for each incorrect prediction. We informed the speaker of the outcome, so the speaker knew whether or not he or she had common or conflicting interests with the receivers. The receivers, by contrast, knew only that for each trial there was a 25 percent chance of common interests with the speaker and a 75 percent chance of conflicting interests.

In the control condition (without verification), the receivers’ predictions matched the speaker’s advice 56 percent (63/112) of the time and the receivers made correct predictions 46 percent (51/112) of the time. The receivers’ behaviors in this variation mimicked the behaviors in incomplete information trials described above. In other words, the receivers made decisions as if they ignored the speakers’ advice—even if the speaker did, in fact, share common interests with them and tell the truth. Without verification, the speaker’s advice did not improve the receivers’ competence (which we measure here in terms of correct predictions).

In the treatment conditions, we introduced the threat of verification. We expect this change to have two related effects. First, it should reduce the value of lying for speakers. That is, speakers who have conflicting interests with receivers and face the threat of verification should expect to gain less from lying than is the case under no such threat. Second, verification gives

TABLE 6.1  
Summary of Observations—Laboratory Experiments

Condition	Observe Coin Toss	Message Sent	$V > 0$	Actual Verification	Persuasion	Competence
Full info	Yes	No	No	No	n/a	97%
No info	No	No	No	No	n/a	48%
$V = 0$	No	Yes	Yes	No	56%	46%
$V > 0$	No	Yes	Yes	Yes	86%	86%
$V > 0$	No	Yes	Yes	No	97%	97%
$V > 0$ (total)	No	Yes	Yes	Both	88%	88%

receivers a stronger reason to believe what they hear. In particular, if receivers believe that verification can keep speakers from lying, then verification gives them a greater incentive to base their prediction on the advice they receive.

We instituted the verification threat by rolling a ten-sided die. In the trials reported below, if the die landed on one through seven, then we verified; otherwise, we did not. Put another way, for every trial, there was a 70 percent chance that we would report the true coin toss outcome instead of the speaker's action.<sup>8</sup>

Three aspects of this experimental design were particularly important. First, the speaker did not know before making his or her statement whether or not verification would occur. The speaker and receiver knew only that there was a 70 percent chance of verification in each trial. Second, the receivers had no way to know whether the message they heard came directly from the speaker or was our verification—the receivers merely heard “heads” or “tails.” We instituted verification in this manner to replicate the model's incentives for speakers in the experiment without revealing to the experiment's receivers whether the speaker had true or false signals in the past. This procedure allowed us to run multiple trials without reputation effects spoiling the data.

Third, we also told everyone that the speaker would have to pay two dollars for the right to make any statement. We expected speakers to respond by making a statement only if they shared common interests with the receivers. This follows because the 70 percent verification threat made statements unprofitable, in expectation, for speakers with conflicting interests. For example, given ten receivers, a two-dollar fee for making a statement is equivalent to twenty cents per receiver. Given this information, the speaker's expected payoff per receiver, if the receivers base their choice on the signal,

is  $(.7) \times (-\$0.20) + (.3) \times (\$.50 - \$0.20) = -\$0.05$ . This amount is less than the expected payoff of \$0 from not making a statement. Therefore, when the speaker had conflicting interests, it was better not to make a statement.

Because we expected the increased verification threat to affect speaker incentives in the manner described above, we expected receivers to base their predictions on the signals they heard and to make more competent choices as a result. Table 6.1 shows that these expectations were realized. In trials where the 70 percent verification threat existed and verification occurred, the receivers followed the advice they heard and made correct predictions approximately 86 percent (187/217) of the time. In trials where the threat existed but *no verification occurred*, and the speaker actually had common interests, we had the same expectations. In these trials, the speaker had an incentive to make a statement and the receivers had an incentive to believe what they heard. In fact, from the receivers' vantage point these trials were observationally equivalent to the trials where verification occurred. In these trials, receivers' predictions matched the signals approximately 97 percent (55/57) of the time, and receivers made correct predictions approximately 97 percent (55/57) of the time. Recall that with no verification threat the percent of correct predictions was less than 50 percent.

Introducing the verification threat into the experiment increases competence. With this competence gain in mind, it is worth noting that in the trials just described three conditions were met:

1. The receivers lacked so much information about the consequences of their choices that they had an incentive to attend to the speaker's advice.
2. The probability of verification was sufficiently high to induce the receiver to follow the speaker's advice.
3. The threat or actuality of verification was sufficiently high that the speaker indeed had an incentive to speak truthfully.

The formal model describes these conditions as sufficient to increase competence—and that they did. Introducing the verification threat in these trials raised the probability of a correct prediction from the level of chance to over 90 percent.

In other trials, we made sure that at least one of these conditions was violated. For example, in trials where there was a verification threat, no verification actually occurred, and the speaker had conflicting interests, we did not expect the speaker to make a statement. In almost all cases he or she did not. On two occasions, however, the speaker did make a statement—contrary to our expectation—and one of the two signals was truthful. The receivers' predictions matched these signals 90 percent (18/20) of the time, which is as we predict given the high verification threat. These receivers, however, made correct predictions only 40 percent (8/20) of the time—they

followed bad advice and did not make payoff-maximizing choices. These trials were ones where the verification threat was not high enough to give the speaker an incentive to speak truthfully. These trials, and others like them, clarify when verification threats do and do not increase competence.

### *Experiment 2*

A laboratory experiment is designed to evaluate a causal hypothesis. The control that facilitates causal evaluations, however, can increase the difference between experiments and the substantive settings in which scholarly audiences are interested. Audiences sometimes want to see laboratory findings replicated elsewhere. Such demands motivate my use of general population experiments, which allow researchers to retain some experimental control while interacting with subjects in a setting that differs from the typical lab.

This experiment was part of the *Second Multi-Investigator Study on Political Persuasion and Attitude Change*. The eligible subject population consisted of all English-speaking adults, eighteen years of age or older, residing in households with telephones, within the forty-eight contiguous U.S. states. Professional interviewers conducted all interviews between June 21, 1998, and March 7, 1999. The interviewers randomly contacted 1,913 households using computer-assisted telephone interviewing technology. Of these households, 725 refused to participate, 73 were never at home, and 48 were unable to participate. The remaining 1,067 households constitute the sample.

General population experiments such as this present experimental stimuli to a nationwide sample of randomly selected subjects. As in a phone survey, people are contacted in their homes. In a departure from most such surveys, they are randomly assigned to different experimental groups. I evaluate hypotheses by comparing group reactions.

An advantage of general population experiments is that their subjects are likely to be different from laboratory subjects. Laboratory subjects typically self-select into experiments by taking certain courses or responding to advertisements. They tend to be undergraduates or people who live near universities. If a national sample of subjects need not respond to experimental stimuli in the same way as people who attend or live near universities, then we can use general population experiments to evaluate the extent to which laboratory findings apply more broadly (see, e.g., Sniderman and Grob 1996).

I designed the experiment to evaluate the following null hypothesis: "A statement *is not* more likely to affect subject predictions if it is described as occurring in a context where the verification threat is high and lying is punished." The format of the experiment is depicted in Figure 6.1. The ques-

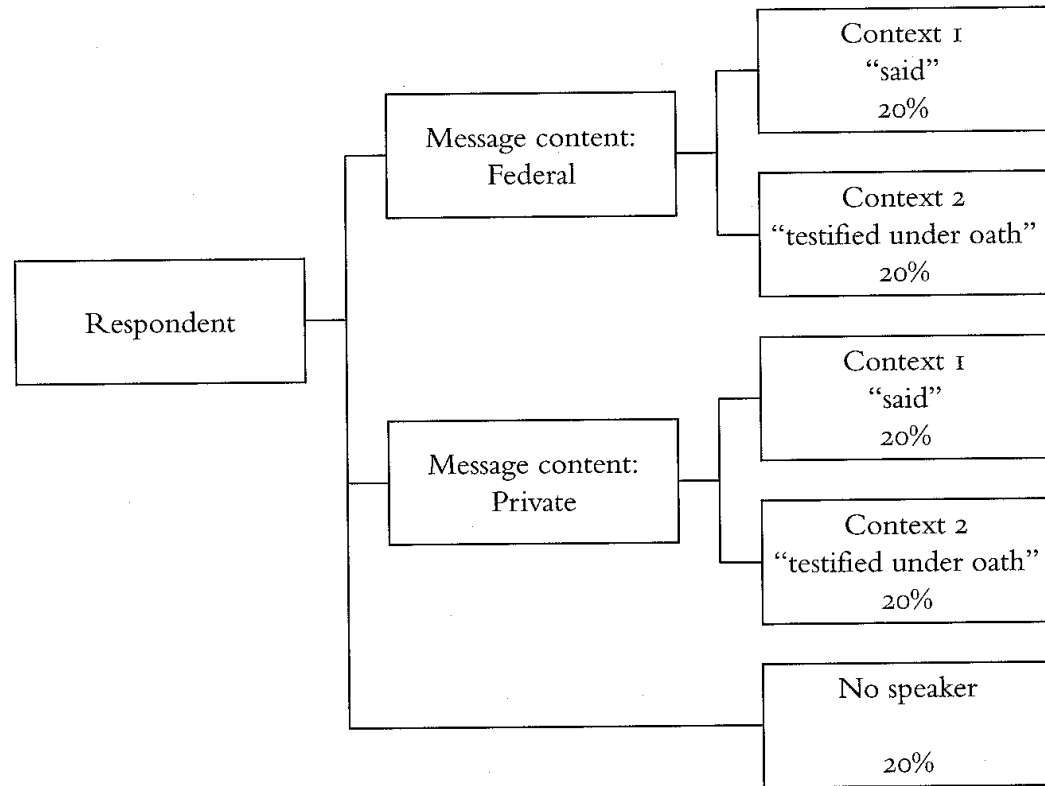


FIGURE 6.1 The random assignment strategy

tions used to conduct this experiment are as follows, with the key experimental variation in *italic*:

Our next questions focus on some issues being discussed in Washington, D.C., these days. One important debate concerns how best to promote airline safety. One proposal is to allow private companies to manage air traffic control stations. The other is to allow the Federal Aviation Administration to continue to manage air traffic control stations.

[No treatment group (20 percent of the sample, randomly selected):]

- Looking ahead to one year from now, who do you think will be managing air traffic control—private companies or the federal government?

[Treatment group (80 percent of the sample; random selection makes all subjects equally likely to hear each version):]

- A safety expert who works for [a private company / the federal government] [*said* / *testified under oath*] that the government should [remain in charge of / allow private companies to manage] air traffic control. Looking ahead to one year from now, who do you think will be managing air traffic control—private companies or the federal government?

In each case, the variation of interest is the switch between the word "said" and the phrase "testified under oath." This switch is meant to represent a change from a setting in which the verification threat is unstated to one where it is widely known to be high. We draw inferences about the effect of the switch by comparing the responses of those who hear different versions of the question. As in the laboratory experiment, we ask subjects to *predict* what will happen. We do this, rather than ask what government should do, in an attempt to filter people's beliefs about the future from their moral position on the issue. The correct prediction was federal control, an outcome about which there was virtual certainty among experts at the time that this experiment was conducted.<sup>9</sup>

#### EXPECTATIONS

How we expect subjects to respond to the experimental treatment is tempered by two differences between laboratory experiments and general population experiments. First, subjects in a general population experiment receive no evidence that they are in an experiment. The interview occurs in the subject's home, just like a phone survey, and the questions sound like typical survey questions. Subjects are not aware that others are answering slightly different versions of the same questions. Second, subjects in our general population experiment were not compensated for their participation. Unlike subjects in most economic experiments, who receive behavior-based pay, or subjects in most psychology experiments, who often receive course-relevant credit, our subjects received nothing from us for participating.

These two differences imply that subjects in the general population experiments have less motivation to respond to any particular stimulus than do laboratory subjects. The model described above, however, suggests that a decrease in motivation should affect subject behaviors in a limited fashion. Less motivated subjects are equivalent to receivers whose utility gradients are relatively flat. As a result, we expect to observe an effect of verification in the general population experiments that is smaller in magnitude than that observed in the laboratory.

Caveats aside, if the switch from "said" to "testified under oath" is a sufficient representation of an increased verification threat, then we predict that treatment groups can be rank ordered by the probability that subjects will predict "federal control." The predicted ranking is as follows:

- Subjects who heard someone *testify* that the *federal* government should remain in charge of air traffic control are most likely to predict federal control.
- Subjects who heard someone *say* that the *federal* government should remain in charge of air traffic control are second-most likely to predict federal control.

TABLE 6.2  
Raw Data for Key Experimental Groups

	Fraction predicting "federal" control	
Subjects who heard "testified federal"	161/182	88%
Subjects who heard "said federal"	197/227	87%
Subjects who heard "said private"	170/200	85%
Subjects who heard "testified private"	171/217	79%

- Subjects who heard someone *say* that *private* companies should control air traffic control are third-most likely to predict federal control.
- Subjects who heard someone *testify* that *private* companies should control air traffic are least likely to predict federal control.

This is a strong prediction. To see why, consider that there are twenty-four possible orderings of these four groups. As a result, if subjects made predictions independent of what they heard, then the likelihood of realizing any particular ordering is  $1/24$ , or just under 5 percent (.0417).

Were the institutional variations described within the questions irrelevant to subjects' beliefs or behaviors, we would not expect to see any particular ordering of experimental groups. Observing this particular ordering, by contrast, provides strong support for the notion that institutional variations have systematic and predictable effects on individual behavior—even for subjects with low motivation.

#### RESULTS

Table 6.2 provides the raw numbers. It shows that the ordering of subjects by treatment groups is precisely as predicted above. Of the four groups, the subjects most likely to predict federal control were the subjects who heard "testimony" to this effect, followed by those who heard the same claim without mention of an oath. The subjects least likely to predict federal control were the subjects who heard contrary testimony, followed by those who heard the same contrary claim without an oath.

The finding in Table 6.2 is not trivial. The likelihood of seeing this ordering if subjects ignore what they hear is less than 5 percent. Moreover, the finding was achieved using a minimal representation of verification threats on subjects who lacked the material incentives given to most subjects in laboratory experiments. Since many political decision makers render judgments in similar low-intensity situations, the finding provides evidence that verification threats affect beliefs in ways that the model predicts even for citizens whose motivation is low.<sup>10</sup>



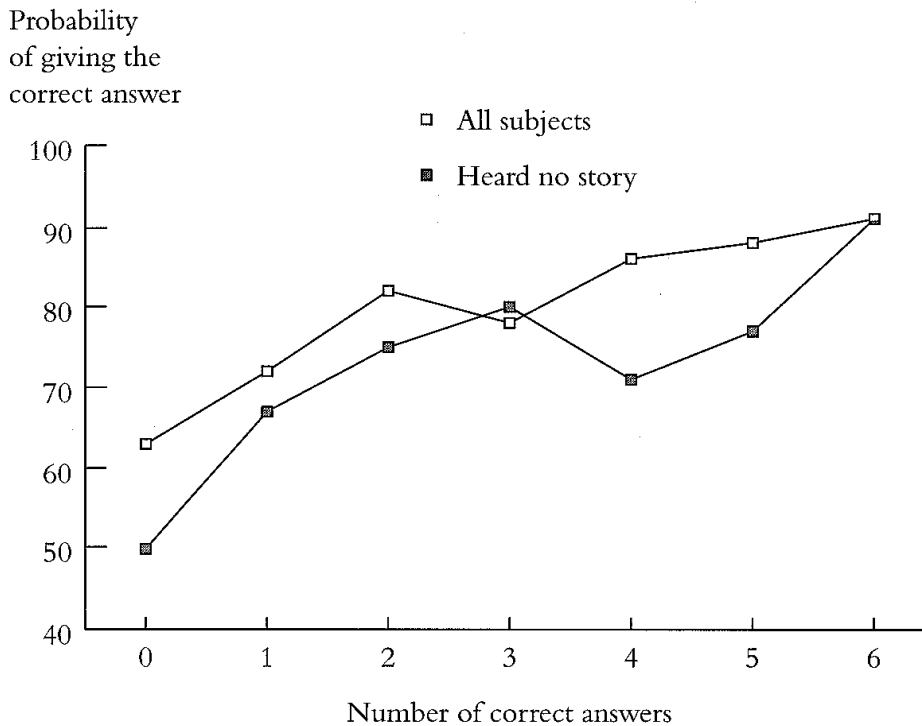


FIGURE 6.2 How awareness corresponds to predicting correctly. The correct answer was “federal control.”

While the differences in Table 6.2 are as predicted, they may be the result of a spurious correlation. There is, after all, no a priori reason to expect this very diverse group of subjects to have the same prior beliefs about the future of air traffic control or the same reactions to new information about it. As a result, I looked for other factors that explain the observed variations. I found only one, a measure of political awareness, whose effect is not trivial.

Like respondents on most political surveys, our subjects were asked seven political information questions. While I do not agree with the common interpretation of such questions as a reliable indicator of citizen competence with respect to concrete tasks such as voting (see Lupia n.d. for an elaboration), I do adopt John Zaller's (1992, 21) interpretation of data drawn from these questions as a measure of *political awareness*, “the extent to which an individual pays attention to politics and understands what he or she has encountered.” We should expect more aware citizens to have stronger prior beliefs about the likely future of air traffic control (i.e., people who follow news of airline regulation are more likely to know that the federal government has controlled air traffic for decades and that no attempt to change control has advanced far in Congress).

Figure 6.2 reports the relationship between subject predictions and awareness.<sup>11</sup> It shows that as awareness increases, subjects are more likely to

TABLE 6.3  
Regressions

	Prediction	Model 1 All Subjects		Model 2 Treatment Only		Model 3 ~ Treatment Only	
		$\beta$	$p$	$\beta$	$p$	$\beta$	$p$
Testified federal	Largest	.88*	3.09	.67*	2.32		
Said federal	Second- largest	.71*	2.76	.50	1.90		
Said private	Second- smallest	.57*	2.20	.36	1.35		
Testified private	Smallest	.21	.09				
Awareness		.26*	5.12	.28*	4.75	.20*	2.05
Constant		.29	.23	.44	1.81	.49	1.35
N		1047		826		221	
Initial LL		-478.91		-354.49		-120.58	
Final LL		-457.56		-338.89		-118.43	
$\chi^2$ (df)		42.68 (5)		31.20 (4)		4.29 (1)	

NOTE: Dependent variable: 1 if subject predicts federal, 0 if subject predicts private.

\* =  $p$ -value < .05.

predict federal control. This is true not only for subjects who heard no statement, but also for subjects as a whole.

Given the relative sizes of the effects observed in Figure 6.2, the ordering of treatment groups observed in Table 6.2 may, in fact, be driven by a spurious correlation with awareness. Table 6.3 displays multivariate tests of this hypothesis.

Table 6.3 presents three logistic regressions. In all cases, the dependent variable is the subject's prediction. A prediction of federal control is assigned a value of 1, and a prediction of private control is assigned the value 0. Positive coefficients imply that the named factor corresponds to an increase in the likelihood that a subject predicts federal control. The first regression is for all subjects, the second regression is for subjects who were in one of the four treatment groups, and the third regression is for subjects who were not in a treatment group.

As in Figure 6.2, awareness has a strong effect in all of the regressions. Extracting from analyses of first differences, we can see that each additional awareness question answered correctly corresponds to a five- to nine-percentage-point increase in the likelihood that a subject predicts federal control.

TABLE 6.4  
The Effect of Institutions

Message	Aware	Number Predicting Federal Control				Effect of "Testified"	
		"Said"		"Testified"		Predicted	Actual
		Number	%	Number	%		
Federal	Less	20/29	69	19/23	83	+	+14
Private	Less	21/28	75	22/35	63	-	-12
Federal	More	177/198	89	142/159	89	+	0
Private	More	149/172	87	149/182	82	-	-5

Table 6.3 also shows that shifting from "said" to "testified" had a substantial effect. In both regressions where the treatment groups are included, the ordering predicted by the theory and witnessed in Table 6.2 not only survives after accounting for awareness but prospers. After awareness is accounted for, and again extracting from analyses of first differences, subjects hearing testimony advocating federal control were approximately seventeen percentage points more likely to predict federal control than were those who heard testimony to the contrary. By contrast, a shift in the content of the message without the verification threat ("said federal" versus "said private") corresponds to a shift in subject predictions, but the difference is neither as large as that seen "under oath" nor is it significant.

Having demonstrated that subjects respond to the institutional shift in the way the theory predicts, I turn to showing when the shift increases competence. Recall the theoretical result that verification increases competence only if the receiver lacks so much information about the consequence of his choice that he has an incentive to attend to the speaker's advice, the probability of verification is sufficiently high to induce the receiver to follow the speaker's advice, and the verification threat is sufficiently high that the speaker does indeed make a truthful statement. Table 6.4 breaks down the data in a way that helps us evaluate the applicability of this result.

The top half of Table 6.4 shows the effect of the experimental treatment on subjects who could answer no more than one awareness question correctly; the bottom half provides the same data for all other subjects in the treatment groups. A comparison of the top and bottom of Table 6.4 shows that the impact of the verification threat was greater for the least aware. This correspondence is consistent with the idea that those who have less information are more likely to attend to new information.

Table 6.4 also divides subjects by the content of the statement they heard.

Note that half of the subjects in the treatment groups heard advice that turned out to be accurate (statement content = continued federal control), while the other half heard inaccurate advice (statement content = private control). For the least aware among the group hearing accurate advice, the condition “the verification threat is sufficiently high that the speaker does indeed make a truthful statement” is effectively satisfied and, as predicted, institutions increase competence. For the least aware among the group who heard bad advice, however, the condition is not satisfied. As expected, institutions do not increase competence for them.

In sum, this experiment demonstrates three things. First, even subjects with low motivation use a minimal description of the institutional context when choosing what to believe. Indeed, subjects were more likely to follow advice that came from a context that many perceive to have a high verification threat. Second, the effect is largest for the least aware. And third, for the institutional change to increase competence (a correct prediction about the regulation), the perceived attributes of the institution (under oath) must match its actual attributes (an accurate statement). All told, the experiment demonstrates when a verification threat does and does not increase competence.

*Conclusion: The New Institutionalism Should Become More Behavioral*

Over the last quarter century, institutions reemerged as an explanatory juggernaut in political science. The new institutionalism has made its most important contributions when focusing on elite behavior (see, e.g., North 1994). While its applications have been broad (see, e.g., Levi 1997; Stinchcombe 1997; Thelen 1999), many of its most notable teachings pertain to formal legislative bodies, such as the U.S. House of Representatives (see, e.g., Kiewiet and McCubbins 1991; Aldrich and Shepsle 1997).

This new institutionalism is divisible into rational choice and historical camps. The rational choice camp has demonstrated that institutions matter because they alter individual incentives. Historical institutionalists, by contrast, view institutions as affecting beliefs directly—often through a process of path dependency.

Both camps adopt the idea that institutions provide new information about the future consequences of current actions. For the rational choice camp, the information is about incentives. For the historical camp (also known as the cultural approach), the information is about precedent and norms. As Peter Hall and Rosemary Taylor (1996, 939) report,

Those who adopt a [rational choice] approach focus on those aspects of human behaviour that are instrumental and based on strategic calculation. . . . What do insti-

tutions do, according to the calculus approach? Institutions affect behaviour primarily by providing actors with greater or lesser degrees of certainty about the present and future behaviour of other actors. More specifically, institutions provide information relevant to the behaviour of others, enforcement mechanisms for agreements, penalties for defection, and the like. The key point is that they affect individual action by altering the expectations an actor has about the actions that others are likely to take in response to or simultaneously with his own actions. . . . Contrast this with a "cultural approach" to such issues. The latter stresses the degree to which behaviour is not fully strategic but bounded by an individual's worldview. . . . From this perspective, institutions provide moral or cognitive templates for interpretation or action.

Both camps' arguments depend on certain assumptions about how institutions affect beliefs. These assumptions, however, are often unstated or untested, a point on which Guy Peters (1996, 211) has criticized the historical camp:

While valuable in attempting to reunite political science with some of its roots, in theoretical terms the approach may encounter some problems. In particular, knowing how a particular policy has developed over time it may be difficult to imagine any other sequence of development. Thus, refutation of the institutionalist approach may be difficult. In overly simplified terms, the argument appears to be that there was a set institution, there was a policy outcome, and the two must be linked.

The rational choice camp is also vulnerable to this critique. While some work in this tradition examines how structural variations affect people's willingness to convey information to others, the correspondence between institutional design and information processing has not been this camp's primary focus. The rational choice camp focuses on incentives (the effect of external forces on actions), not persuasiveness (the effect of external forces on beliefs)—and the two are not always equivalent.

Simultaneously, research on political behavior focuses on individual attributes as determinants of how people react to information (see Sniderman, Brody, and Tetlock 1991 and Zaller 1992 for leading examples). Together with related work on persuasion in social psychology (Petty and Cacioppo 1986, Eagly and Chaiken 1993), this literature clarifies how individual differences cause variations in how stimuli affect behaviors. For all of its virtues, however, the literature on political behavior has evolved with minimal attention to institutions. As Sniderman (2000, 68) notes,

Initially, we asked how citizens effectively can simplify political choices so as to make them coherently. Putting the question this way led us, like virtually everyone else, to start the explanatory process by focusing on the characteristics of citizens. How much attention do they pay to politics? What do they know about it? . . . Answer these questions, and we should be in a position to figure out how citizens make political choices. Or so it seemed then. Now, I am persuaded, we had the order of

things wrong. Citizens do not operate as decision makers in isolation from political institutions. If they are in a position to overcome their informational shortfalls by taking advantage of judgmental shortcuts, it is because public choices have been organized by political institutions in ways that lend themselves to these shortcuts.

The chasm between research on institutions and research on political behavior hampers our discipline's ability to explain when institutions can change beliefs (e.g., about whether a particular affirmative action program is necessary or sufficient to change behaviors toward certain racial groups).<sup>12</sup> As a result, it hinders our ability to inform the decisions of the many well-intentioned public and private interests who seek to build institutions that increase civic competence.

The way forward is, in this regard, for political science to pursue a more *behavioral institutionalism*—research which ignores neither the incentive effects of common political contexts nor the aspects of our physical endowments that lead us to acquire and process information in particular ways. Like behavioral economics, such an effort would strengthen “the accuracy and empirical reach of [institutional] theory by incorporating information from neighboring social science disciplines, especially psychology and sociology” (Russell Sage Foundation 1999). Formal theory should be a critical part of this endeavor, as the logical rigor it requires helps document the complex interactions between cognitive endowments and institutional attributes.

The research described in this chapter takes a small step in the direction of behavioral institutionalism. Using a formal model and two experiments, I find that the extent to which institutions increase competence depends on what information subjects lack, what incentives they have to change their existing behaviors, and the extent to which the context clarifies the credibility of the advice they receive. The key factor in all experiments is *the interaction* between what institutions reveal about the quality of the subjects' information and the behavioral attributes that make subjects differ in the extent that they are willing to seek and accept new information.

### Notes

I thank the National Science Foundation for supporting this research (SBR-9422831 for laboratory experiments and SBR-9633743 for general population experiments); Sean A. Cain for research assistance; James N. Druckman, Karen Garrett, Mathew D. McCubbins, and the Survey Research Center at the University of California–Berkeley for advice on experimental design; and Elisabeth R. Gerber, James H. Kuklinski, Jennifer Jerit, Howard Lavine, David P. Myatt, Kenneth A. Shepsle, and seminar participants at the University of California–San Diego, the University of Maryland, the 2000 Annual Meetings of the Midwest and American Political Science

Associations, and the volume's editors for comments on previous versions of this manuscript. I also acknowledge the support of the Center for Advanced Study in the Behavioral Sciences.

1. Following Lupia and McCubbins 1998, 24–25, I define knowledge as the ability to make accurate predictions and information as data. Knowledge requires information because accurate predictions require data—at a minimum you need some data to verbalize the prediction you are making. By contrast, you can know a long list of facts and fail to put them together in a way that allows you to make accurate predictions. Thus, while you cannot have knowledge without having information, you can have information without having knowledge. I define competence as the ability to accomplish a concrete task. The kind of task that motivates the present study is “Can voters make the same decision they would have made if knowledgeable on issue positions *a* through *z*?” If they can, we say they are competent; if they cannot, we say that they are incompetent. Therefore, competence requires sufficient knowledge, which requires sufficient information.

2. Reviews of evidence on the pathologies of deliberation in the context of group or team decision making include National Research Council 1994, chapter 7, and Van Avermaet 1996.

3. A cheap talk model is a signaling model in which speech does not directly affect payoffs. For example, in the seminal signaling model, a speech act was the purchase of a formal education that imposed direct costs on the speaker. In cheap talk models, a speech act does not require the purchase of any such good.

4. Vincent Crawford and Joel Sobel (1982) find that “equilibrium signaling is more informative when agents' preferences are more similar” (1431). In their model, all equilibria are partition equilibria, which means that all equilibria can be stated in terms that describe the accuracy of the speaker's statements (i.e., the message space is partitioned and the more segments a message space contains, the more persuasive the message). They conclude that “the more nearly [the speaker's and receiver's] interests coincide—the finer partition there can be. . . . As [the distance in their interests goes to infinity], [the number of partitions] eventually falls to unity and only the completely uninformative equilibrium remains” (1441).

5. This proposition describes the most informative equilibrium. Like many strategic communication models, this model also yields an equilibrium in which no informative communication occurs (i.e., a babbling equilibrium). Henceforth, I follow convention and describe only the nonbabbling equilibrium. See Lupia and McCubbins 1998, 245–46, for more information.

6. Our flyers gave prospective subjects a number to call for an appointment. Our research assistants fielded these calls, verified the callers' age (eighteen years or older) and undergraduate standing, and assigned experiment appointment times to eligible callers. Typically, we scheduled more subjects than we needed in a given experiment because of an expected 20 percent no-show rate. When extra subjects arrived, we admitted only the number needed for the experiment into the laboratory on a first-come-first-admitted basis. We then paid the extras five dollars and invited them to sign up for another experiment. No person was a subject in our experiments more than once.

7. This design also has the virtue of presenting subjects with simple and familiar situations. To ensure that subjects saw these situations as simple and familiar, we began each experimental session with simple explanations and examples of the experiment's sequence of events, what information subjects would have, and how subjects would earn money. After we gave a set of instructions, we administered a brief quiz on the instructions. Most subjects achieved perfect scores on these quizzes. That subjects understood the instructions so well gives us confidence that they, like the speaker and receiver in our model, understood the situation they were in. As a result, we were confident about our ability to use the results from our experiments to draw meaningful inferences about the theoretical hypotheses.

8. Lupia and McCubbins 1998, 135-39, 145-46, reports other variations of this experiment in which the effects of verification threats are consistent with the model's substantive predictions.

9. Without data on subjects' priors about the relative credibility of private companies and the federal government on the topic of air traffic control, we have no a priori reason to expect that variation in the identity of the speaker would correspond to variations in observed behaviors. We chose not to collect data on subject priors because we did not want to prime subjects about the questions that were to come. However, we allowed variation in the speaker's identity to ensure that the speaker did not have an amorphous identity, which could vary among subjects and affect responses in ways that we would then be unable to explain. My analyses reveal no independent effect of speaker identity.

10. Subjects who heard no statement about the future of air traffic control were the least likely to predict federal control (169/221), 76 percent. This pattern of behavior, while not directly relevant to our hypothesis test, is curious. The fact that all treatment groups were higher implies that being primed to such a conversation led people to make more accurate predictions, regardless of what they heard. This conclusion is, however, speculative. In addition, eighteen subjects replied "don't know" and two refused to answer.

11. The six questions used (with percentage responding correctly in parentheses) are: Which party has the most members in the House of Representatives? (79), How much of a majority is required for the U.S. Senate and House of Representatives to override a presidential veto? (59), Would you say Democrats are more conservative than Republicans, or Republicans are more conservative than Democrats? (68), Whose responsibility is it to determine if a law is constitutional or not? (74), How many four-year terms can the president of the United States serve? (92); and How many members of the Supreme Court are there? (64). I also ran analyses using six dummy variables, each of which indicated whether subjects gave a correct response to individual awareness questions. While there was a high correlation between these dummy variables, each had a similar correspondence to subject predictions (i.e., answering any question correctly corresponded to predicting federal control).

12. Such chasms are not unique to political science, as Chip Heath, Richard Larrick, and Joshua Klayman (1998, 3) explain: "On the one side, research in cognitive psychology has largely treated individual learners as 'rugged individuals' who face a



difficult environment alone, equipped only with their own, flawed cognitive strategies. On the other side, organizational research has largely ignored the literature on individual cognition, focusing instead on issues of motivation or incentives."

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