Learning More from Political Communication Experiments: Pretreatment and Its Effects

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Research on political communication effects has enjoyed great progress over the past 25 years. A key ingredient underlying these advances is the increased usage of experiments that demonstrate how communications influence opinions and behaviors. Virtually all of these studies pay scant attention to events that occur prior to the experiment—that is, in "pretreatment events." In this article, we explore how and when the pretreatment environment affects experimental outcomes. We present two studies—one where we control the pretreatment environment and one where it naturally occurred—to show how pretreatment effects can influence experimental outcomes. We argue that, under certain conditions, attending to pretreatment dynamics leads to novel insights, including a more accurate portrait of the pliability of the mass public and the identification of potentially two groups of citizens—what we call malleability reactive and dogmatic.

Over the last 25 years, scholars have made remarkable progress in understanding how mass communications shape the public's opinions. The field has moved from being "one of the most notable embarrassments of modern social science" (Bartels 1993, 267) to introducing "compelling" concepts that have "had a major impact in political science and communications scholarship" (Iyengar 2010, 190). Indeed, researchers no longer ask whether communications shape opinions, but rather when and how.

A bulk of the research on mass communication effects comes from experiments. A typical study randomly exposes some respondents to one message (e.g., a description of a hate group rally request framed as a free speech issue) and other respondents to a different message (e.g., a rally description using a public safety frame). When opinions of the groups differ, it is taken as evidence that communications affect opinions (see Nelson, Bryner, and Carnahan 2011).[1] But just how much do these experiments—which have been conducted with a wide range of people on innumerable topics—reveal about the influence of political communication?

One notable problem concerns timing and, specifically, what occurred *before* the experimental treatments (i.e., "*pretreatment*"). If the experiment explores a communication that regularly occurs in "reality," then reactions in the experiment might be contaminated by those "regular" occurrences prior to the experiment. For example, it could be that the aforementioned free speech frame registers no effect because it already moved the respondents before the experiment (i.e., pretreatment), and one more exposure in the experiment does little. Given that many, if not most, researchers design experiments aimed at capturing "real world" political communications, the likelihood of pretreatment contamination is substantial (Gaines, Kuklinski, and Quirk 2007). Despite

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[1] Experiments enable researchers to know with near certainty the communications to which respondents were exposed and that respondents did not themselves select those communications. Nelson, Bryner, and Carnahan state that experimentation and the study of communication effects "seem made for each other" (2011, 202).

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the potential consequences of pretreatment effects, there has been virtually no explicit work on the topic (although see Slothuus 2011).[2]

In this article, we provide what we believe is the first conclusive evidence of a pretreatment dynamic. More importantly, we identify conditions under which pretreatment effects occur. We test our expectations with two studies, one in a laboratory environment that controls the pretreatment environment and the other in the context of an Election Day exit poll that observes pretreatment experiences. Our findings reveal what can be learned by attending to pretreatment dynamics. Specifically, our results suggest the possibility of two types of citizens—what we call *malleably reactive* and *dogmatic*. Moreover, it may be that the aggregation of experimental political communications studies overstates effects on attitudes that tend to be malleable and fleeting (also see Barabas and Jerit 2010).

Psychology of Pretreatment Effects

We follow much prior work on mass communication effects by focusing on framing effects. A framing effect occurs when a communication changes people's attitudes toward an object by changing the relative weights they give to competing considerations about the object (Druckman 2001, 226–31). The aforementioned hate group rally experiment is a classic example of a framing effect experiment (Nelson, Clawson, and Oxley 1997).[3] The *pretreatment environment* refers to the context prior to exposing experimental participants to the frame. A *pretreatment effect* occurs when a prior event (e.g., a mass communication) shapes attitudes about the rally that persist at the time of the experiment and condition responses to the experimental stimuli.[4] Individuals who were recently exposed to discussions of the issue might react differently to the treatment than those encountering the issue for the first time (or for the first time in a long time).

Our previous example posited that experimental participants had been (repeatedly) exposed to media coverage using the free speech frame which moved opinions. The one additional exposure in the experiment might not further contribute to this movement, leading to the conclusion, based on the experiment, that the free speech frame has no (or a small) effect, when in fact, it had a large effect prior to and outside of the experimental context.[5] This possibility is routinely ignored by experimenters who "implicitly assume . . . that respondents enter the survey as clean slates . . . [despite the fact that] there is inevitably some possibility that respondents enter the experiment having already participated in a similar experiment, albeit one occurring in the real world" (Gaines, Kuklinski, and Quirk 2007, 17, 13).[6] Let us be clear that we focus exclusively on the impact of prior communications or other short-term factors. Of course longer-term predispositions will condition experimental response, but such long-term factors are distinctive from pretreatment, which refers to stimuli (i.e., communications) in the environment prior to treatment (e.g., Brewer 2003; Malhotra and Margalit 2010).

We posit the presence of three conditions as leading to pretreatment effects. First, prior to the experiment, respondents must be exposed and attentive to a communication akin to the treatment. Absent exposure and attention, there is no (pre)treatment. Second, the pretreatment communication must influence the respondents' opinions. Third, that effect must sustain until the time of the experiment.

A key element in these conditions concerns pinpointing when a pretreatment effect endures so as to influence

[2]Some recent work has explored the related phenomenon of posttreatment over-time effects (e.g., Chong and Druckman 2010; Lecheler and de Vreese 2010; Matthes and Schemer 2010). On first glance this may appear to be the same topic, simply applying distinct labels. For example, in that work, scholars explore how a communication effect at time 1 endures to time 2 and possibly affects reactions to another stimulus at time 2. One could simply relabel time 2 as time 1 and call the initial time 1 stimulus "pretreatment." Yet, this would be deceiving. First, as we will discuss, the pretreatment environment involves more than a single stimulus as has been the focus in posttreatment work (i.e., there is a larger context). Second, we will define the necessary conditions for a pretreatment effect which this other work has not considered. Third, the psychology of pretreatment and posttreatment could theoretically differ, in part, because increased repetition of pretreatment exposure may matter. Finally, unlike the aforementioned work, a focus on pretreatment brings with it different implications, particularly regarding the inferences that can be drawn from any cross-sectional experimental study. We imagine it is for these reasons that Gaines, Kuklinski, and Quirk (2007) and Chong and Druckman (2010, 664) draw a sharp conceptual distinction between pretreatment and posttreatment dynamics.

[3]See Chong and Druckman (2007, 115) and Druckman, Kuklinski, and Sigelman (2009) for discussion of how framing effects are indistinguishable from what many scholars call priming.

[4]We focus on "mass" communications since that is typically what is being emulated in these experiments (e.g., as opposed to interpersonal communications).

[5]Gaines, Kuklinski, and Quirk explain that while the pretreatment effect may often cause a "downward bias" (2007, 15–16), it is also possible in some circumstances that it will lead to an upward bias depending on the type of prior exposure.

[6]While randomization of subjects should, on average, evenly distribute those with prior message exposure between experimental groups, it does not prevent under- or overestimates of message exposure effects.

experimental responses.Indeed, if the pretreatment has no impact in the first place, it seems plausible that the analogous experimental stimulus also will be impotent (although see Barabas and Jerit 2010). The endurance of a pretreatment effect likely increases when individuals form/update their attitudes in ways that enhance *attitude strength* (see Chong and Druckman 2010 for elaboration).[7] A strong attitude, by definition, persists and resists change (Krosnick and Smith 1994; Miller and Peterson 2004; Visser, Bizer, and Krosnick 2006). If individuals process pretreatment communications in a manner producing stronger attitudes, this will increase the persistence of those attitudes.[8] Consequently, a similar communication in the later experiment may register no effect on individuals with previously formed strong attitudes (i.e., formed in response to the earlier pretreatment communication).

On the flip side, these strong attitude respondents may reject an experimental communication if it runs counter to those received in the pretreatment environment. Strong attitudes often lead to motivated reasoning where individuals avoid, ignore, or reject information that is inconsistent with their prior opinions (e.g., Druckman and Bolsen 2011; Druckman et al. 2010; Kunda 2001; Lodge and Taber 2000; Redlawsk 2002; Rudolph 2006). Those with stronger attitudes are substantially more likely to engage in motivated reasoning not only because their attitudes reflect cumulative exposure over time to information, but also because they increasingly resist new information that might change those attitudes (see Lodge and Taber 2000, 211). In short, those who process information in a way that produces strong attitudes during the pretreatment period may subsequently be immune to the experimental stimulus either because yet another consistent communication has little effect or because they reject a contrary communication.

In contrast, for individuals who process pretreatment communications in ways that generate weak attitudes, the effect of those communications will decay and respondents will enter the experiment as virtual clean slates. As a result, regardless of the direction of the survey stimulus, an effect will likely occur.

We focus, here, on two dynamics affecting attitude strength that may enhance pretreatment effects. First, individuals may form and update their attitudes using varying degrees of either online or memory-based processing of information. When individuals process a message about an issue online, they routinely integrate the various considerations contained in the message into an overall issue evaluation. Individuals then store the summary evaluation in memory, possibly forgetting the original considerations that contributed to the tally. When asked subsequently for their attitude toward the issue, individuals retrieve and report their overall online tally rather than reconstruct and evaluate the specific pieces of information that comprise this summary (see, e.g., Druckman and Lupia 2000; Hastie and Park 1986; Lodge, Steenbergen, and Brau 1995).[9] Importantly, these attitudes—which reflect the pretreatment message—will sustain (i.e., they are strong) and thus condition response in the experiment (they maintain inertia). This differs from individuals who use memory-based information processing—they store considerations about the issue in memory (without necessarily forming an overall judgment) and subsequently retrieve and evaluate accessible considerations when asked their opinion about the issue (Bizer et al. 2006, 646). These individuals are much more likely to forget the specific earlier pretreatment messages (i.e., attitudes are weak) when they reconstruct their attitudes later, and thus, they are less likely to experience pretreatment effects (see Briñol and Petty 2005, 583; Tormala and Petty 2001, 1600).

Second, individuals vary in their tendency to form spontaneous evaluations when processing information. An individual's "need-to-evaluate" (NE) is a trait reflecting one's "propensity to engage in evaluation" (Jarvis and Petty 1996, 172). People with a high NE develop stronger attitudes and more opinions on subjects ranging from personally relevant matters to more remote topics (Bizer et al. 2004, 998). As Hermans, De Houwer, and Eelen explain, individuals with a high NE "possess stronger object-evaluation associations due to their chronic evaluative responding" (2001, 159; also see Bizer et al. 2006, 1000; McGraw and Dolan 2007; Tormala and Petty 2001).[10]

In sum, we hypothesize that pretreatment effects (e.g., leading to no experimental stimulus effect) will be more likely to occur when individuals (a) are exposed

[7] Attitude strength is distinct from Chong and Druckman's (2007) concept of "frame strength."

[8] Individual and circumstantial factors also contribute to the formation of stronger opinions. Attitudes increase in strength, for example, when they are more extreme, more accessible, and deemed more personally important (e.g., Krosnick and Smith 1994). We focus on the processes by which the attitude is formed and updated, as that is most relevant to exploring overtime dynamics.

[9] Processing mode creates variation in the opinions expressed at any moment (e.g., McGraw and Dolan 2007), but less noted is its effect on the durability of opinions (Mitchell and Mondak 2007).

[10] NE reflects a highly "stable dispositional characteristic of individuals" across contexts and time and is "distinct from various frequently studied personality traits" (Bizer et al. 2004, 999). NE is only weakly correlated with, and therefore can be differentiated from, ideology and other constructs reflecting cognitive engagement (e.g., knowledge; Bizer et al. 2004).

and attentive to earlier communications similar to the experimental stimuli *and* (b) form/update their attitudes in ways that promote strength. This occurs among online processors and high-NE processors.[11]

Study 1

Our first study employs an experimental approach, which allows for exogenous control over the conditions posited to affect the size of a pretreatment effect: pretreatment exposure/attention, pretreatment influence (of which we can maximize the likelihood by using strong/effective frames), and modes of attitude formation. We recruited 647 participants to take part in a four-part study, in the political science laboratory at Northwestern University, conducted in Spring 2010. Most, but not all, respondents were individuals enrolled in classes who, in exchange for their participation, received extra credit and entry into a lottery where several people won $50. While some may worry about the student sample, an increasing amount of evidence suggests results from such samples widely generalize (e.g., Druckman 2004; Chong and Druckman 2007; Miller and Krosnick 2000; for more general discussion, see Druckman and Kam 2011).

There were three notable elements of our design. First, we focused on two distinct issues: a publicly funded casino and the U.S. Patriot Act. The former involves a government-owned gambling casino where revenue can be used to ease tax burdens and subsidize programs (e.g., education). The latter refers to a piece of legislation enacted shortly after the September 11, 2001, terrorist attacks to increase the powers of law enforcement agencies to monitor communications, records, and financial transactions in an effort to identify terror threats.[12] We believe these issues are representative in the sense of being periodically salient (and fortunately from a design perspective, neither issue received sustained media coverage during our experiments). The issues nicely touch on both economic and social dimensions, with the casino issue concerning taxes, debt, and social addictions and the Patriot Act revolving around the proper balance between national security and civil liberties. Opinions on these issues are liable to change (e.g., Best and McDermott 2007), which allows us to test hypotheses about the moderating effects of attitude strength before people have developed crystallized opinions.[13] Our dependent variables are the extent to which an individual opposes or supports a state-owned and -operated gambling casino and the extent to which an individual opposes or supports the Patriot Act (both measured on 7-point scales with higher scores indicating increased support). All respondents were assigned to conditions on both issues and always received information regarding the casino issue first.

The second key design element is that we used pretests to select two competing "strong" frames on each of our issues—these were the frames to which respondents were exposed.[14] For the casino, our Pro frame emphasized the economic benefits from the casino (e.g., tax relief and education funding) while the Con frame focused on social costs, including addiction and debt. For the Patriot Act, our Pro frame revolved around protecting citizens from acts of terrorism while our Con frame concerned civil liberties violations.[15] We presented these frames in mock news editorials.[16] Examples of the casino economic frame and Patriot Act civil liberties frame appear in Appendix A; the other frames are analogous and are available from the authors.

Third, our controlled attitude-formation process involves induced memory-based (MB) versus online (OL) processing—recall OL processors are expected to form stronger attitudes and thus exhibit larger pretreatment effects. We used a standard procedure to manipulate the strength of attitudes formed in response to frames by

[11]Time between pretreatment exposure and the experiment also will likely matter; as the time increases, the pretreatment effect will decrease (contingent on attitude strength).

[12]The Act contains a number of other elements, such as redefining terrorism so as to include domestic incidents. The actual name of the Act is the "USA PATRIOT Act," which stands for "Uniting and Strengthening America by Providing Appropriate Tools Required to Intercept and Obstruct Terrorism."

[13]Opinions about the Patriot Act divide more sharply along partisan lines, but we found that on neither issue does partisanship cause immunity to strong arguments.

[14]Chong and Druckman (2007) show that, when all frames are received concurrently, stronger frames influence opinions more than weaker frames. We follow prior work by identifying strong frames via pretests that ask respondents to rate the frame's "effectiveness." Details are available from the authors.

[15]In their content analysis of *New York Times* coverage of the Act, Chong and Druckman (2011a) report these are among the most frequently appearing frames on each issue.

[16]In constructing the editorials, we strived for realism by using content analysis of news coverage and prior research on the Patriot Act and public-funded casinos to identify how different frames were presented in public discussions of these issues. Our restriction of the experimental design to a small number of competing frames is both methodologically practicable and realistic. Chong and Druckman's (2011a) content analysis finds that, over the course of coverage, arguments will be repeated with varying frequencies, but each side very quickly tends to concentrate on a small number (one or two) of frames that are presumed to be stronger or more effective arguments.

exogenously inducing either MB or OL processing of messages (e.g., Bizer et al. 2006; Hastie and Park 1986; Mackie and Asuncion 1990). Participants read a news editorial containing the relevant frame. For the OL manipulation, designed to produce stronger attitudes, respondents were instructed to evaluate each article's paragraph according to the extent to which it decreased or increased their support for the casino/Patriot Act.[17] As is typical, respondents in the OL condition were also told they would be asked to report their attitudes at a later point in time (but we did not ask them to report opinions after each period, waiting only until the final survey, so as to minimize possible demand effects; see Hastie and Park 1986). In the MB manipulation, intended to produce weaker attitudes, respondents were asked to rate each paragraph according to the extent it seemed "dynamic" (i.e., used more action-oriented words); these respondents were not informed that they would be asked for their opinion on the issue. Examples of the manipulations appear in Appendix A.

All respondents participated in four waves, each separated by approximately five days.[18] In the first wave, respondents completed a brief background questionnaire of demographics. They then were assigned into one of 15 conditions that varied three elements: (1) pretreatment environment, (2) processing mode, and (3) survey frame. We manipulated pretreatment environment by assigning individuals to receive (a) no relevant articles (i.e., no pretreatment), (b) a Pro frame article, or (c) a Con frame article at *each* of Waves 1, 2, and 3.[19] While the articles at each wave used the same frame, they were written so as to be distinctive from one another.[20] This approach ensures that respondents received a directionally uniform pretreatment environment. We manipulated processing mode at each wave as noted above; for example, those in the OL conditions rated the extent to which the paragraph affected their opinions, at Waves 1, 2, and 3.

Finally, at Wave 4, respondents were randomly assigned to receive a survey question employing the No frame, the Pro frame, or the Con frame. These conditions mimic those typically found in survey experiments. For example, the No frame casino question asked: "A proposal is being considered for the Illinois state government to operate a land-based gambling casino. What do you think—do you oppose or support the proposal for a state-run gambling casino? Choose one number on the following 7-point scale." The Con version asked the same question but also stated, " . . . Some say that a state-run casino will have severe social costs, such as addiction and debt. . . ." The Pro question instead included, ". . . Some say that the revenue from the casino would provide tax relief and help to fund education. . . ." The Patriot Act items appear in Appendix B; higher scores indicate increased support for the Patriot Act. As mentioned, the answers to these support questions constitute our main dependent variables.

The full set of conditions, along with the Ns, appears in Table 1.[21] The first set of conditions (1–3) resembles the typical experiment where the pretreatment environment is ignored and processing mode is not manipulated. The other conditions involve manipulating the processing mode as well as introducing a pretreatment environment.[22] Specifically, Conditions 4–9 involve OL processing and then mix the nature of the pretreatment environment and the survey frame, while Conditions 10–15 do the same for MB processing.[23] We expect that the survey frames will exhibit scant effects in the OL conditions, either because exposure to a survey frame consistent with the pretreatment context will have minimal added

[17]We asked respondents to rate each paragraph so as to more closely resemble the processing manipulations used conventionally in psychology (e.g., where the OL manipulation requires statement-by-statement assessments).

[18]We sent up to three reminders to participants. The general response rate by wave was near 100%, which is not surprising given that completion of each wave was required for compensation.

[19]Those assigned to the no-pretreatment conditions read articles irrelevant to the issues. Also, respondents were assigned to the same frame direction on each issue (e.g., those who received Pro casino articles also received Pro Patriot Act articles).

[20]Articles were pretested for readability and to ensure they employed the frames we believed they did.

[21]The unequal Ns across some of the conditions stem from inequities in potential subjects' initial participation across conditions; particular experimental sessions assigned to some conditions (e.g., Condition 3) happened to have unexpectedly low initial turnout. The differences do not reflect variation in attrition over the stages of the experiment. As mentioned, we had scant attrition.

[22]In the no-processing manipulation cases, we exclude conditions that introduce/manipulate pretreatment context. While such conditions would provide insight into how the "average" person processes information when nonmanipulated, they are not needed to test our hypotheses (and would require the addition of six more conditions on top of the already large number of 15 conditions). Moreover, we explore this type of situation in Study 2. In the processing manipulation conditions (OL or MB), we excluded conditions with no pretreatment environment (i.e., there is always a pretreatment context). We have no basis to expect processing mode to matter in overall opinion when there are no pretreatments (either in terms of overall main effect or reaction to the frames). That is, the processing-mode manipulation should only matter in terms of how new information is used at later points in time.

[23]Individuals assigned to a given pretreatment scenario read the exact same articles regardless of the processing manipulation (i.e., individuals in Conditions 4 and 10 read the same articles). Likewise, those assigned to the same processing mode experienced the same manipulation (i.e., individuals in Conditions 4 and 8 experienced the same processing-mode manipulation). The full slate of articles and manipulations is available from the authors.

Table 1 Study 1 Conditions

	No Survey Frame	Pro Survey Frame (Economic/ Terrorism)	Con Survey Frame (Social Costs/ Civil Liberties)
No Processing Manipulation			
No Pretreatment	(Condition 1) N = 46	(2) N = 39	(3) N = 32
Online Processing Manipulation			
Pro Pretreatment (Economic/ Terrorism)	(4) N = 61	(5) N = 36	(6) N = 36
Con Pretreatment (Social Costs/ Civil Liberties)	(7) N = 42	(8) N = 39	(9) N = 43
Memory-Based Processing Manipulation			
Pro Pretreatment (Economic/ Terrorism)	(10) N = 44	(11) N = 44	(12) N = 62
Con Pretreatment (Social Costs/ Civil Liberties)	(13) N = 41	(14) N = 47	(15) N = 37

effect or because contrary survey frames are rejected due to a tendency to engage in motivated reasoning. In contrast, the survey frames should drive opinions in the MB conditions.

Results

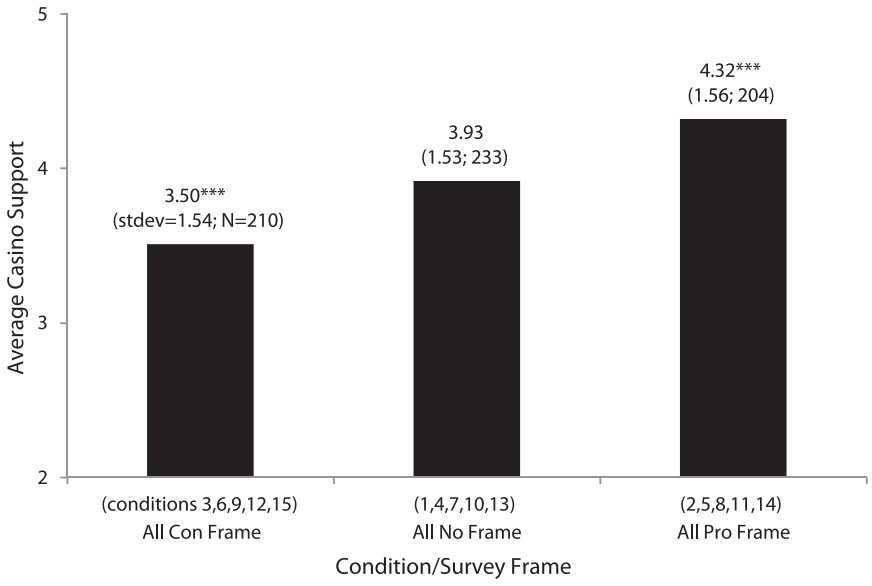
We present the results by comparing mean support scores across relevant conditions. The casino and Patriot Act results mimic one another and thus we report the findings in tandem for the issues.

We begin in Figures 1 and 2 by displaying the mean scores based on exposure to the survey frame—the Con frame (social costs, civil liberties), the No frame, or the Pro frame (economic, terrorism). We merge conditions regardless of processing mode and pretreatment exposure. Asterisks refer to significance for *t*-statistic comparisons to the no survey frame conditions; we use one-tailed tests throughout given our directional predictions (see Blalock 1979, 163; Druckman and Nelson 2003; Nelson, Clawson, and Oxley 1997). We see very strong survey framing effects for both issues. For example, Figure 1 shows that those exposed to the Con social-costs frame on the casino issue reported an average support score of 3.50, which is significantly lower than the 3.92 score of those not exposed to a frame ($t_{442} = 2.87$; $p \leq .01$). On the flip side, the 4.32 average support score for those receiving the Pro economic frame significantly exceeds the No frame group ($t_{437} = 2.71$; $p \leq .01$). Figure 2 displays nearly identical dynamics for the Patriot Act. In short, if we treated these data as cross-sectional, as is typical in the experimental literature—ignoring pretreatment exposure and processing mode—we would find fairly notable framing effects on both issues.

We next delve deeper by separating out the results by processing mode; Figures 3 and 4 display mean levels of support, again by survey frame, separately for each processing mode. Within each processing mode, we merge pretreatment environment conditions. Thus, the figures display basic survey framing effects by processing mode. Asterisks again denote statistical significance, this time relative to the no survey frame condition *within* each processing mode.

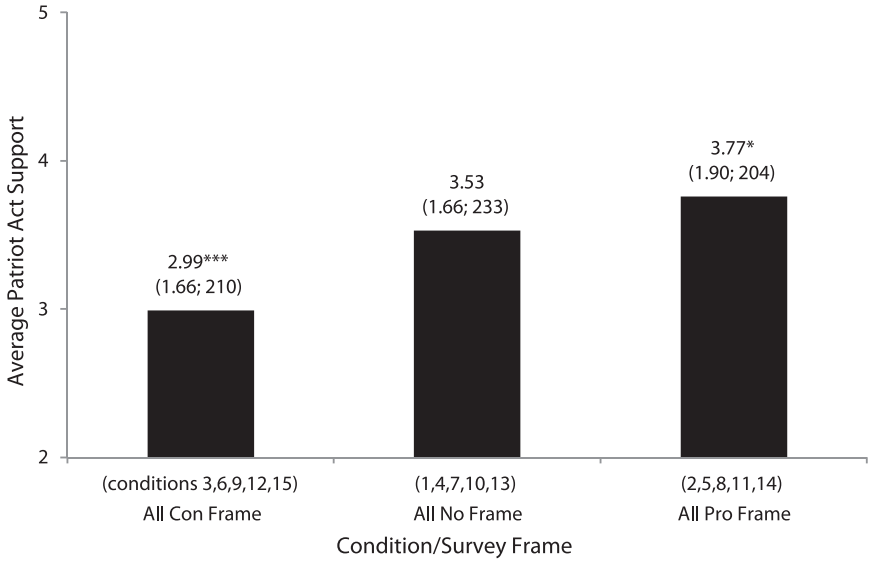
Interestingly, for both issues, we see significant survey framing effects for the nonmanipulated group and the memory-based (MB) processors. We do *not* see survey framing effects, however, for the online (OL) processors. For example, for the casino issue, nonmanipulated processors exposed to the No frame report an average score of 3.96, which significantly differs from nonmanipulated individuals exposed to the Con social-costs frame (average score of 3.06) or the Pro economic frame (average score of 4.50; respectively, $t_{76} = 2.63$; $p \leq .01$; $t_{83} = 1.64$; $p \leq .05$). We see similar differences in the MB group—significant survey framing effects in both directions. We see the same for the Patriot Act issue. When we turn to the OL group for each issue, though, the survey framing effect ceases to exist, and in fact the mean scores across survey frames are nearly identical for the casino issue (i.e., 3.81, 3.84, and 3.93) and fairly similar for the Patriot Act. The survey framing effect evident in the merged data thus reflects only an effect among nonmanipulated and MB

FIGURE 1 Casino Survey Framing Effects (Merging Pretreatment and Processing Conditions)



(***p < .01; **p < .05; *p < .1 for one-tailed tests, versus "All No Frame")

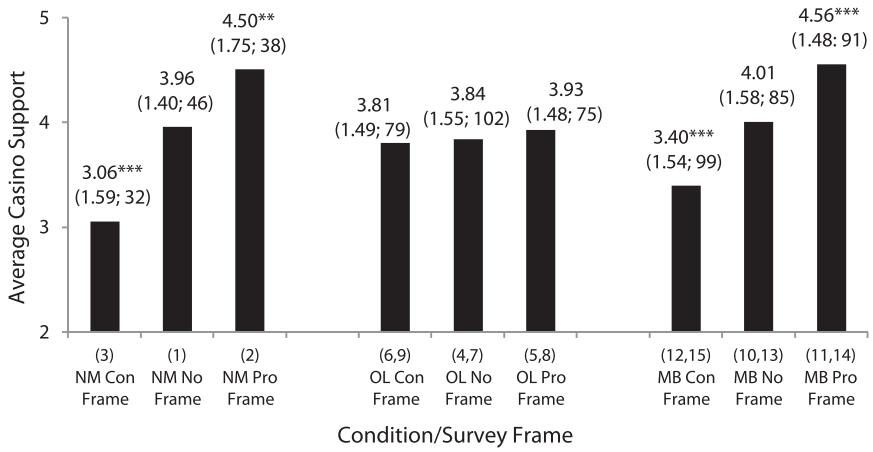
FIGURE 2 Patriot Act Survey Framing Effects (Merging Pretreatment and Processing Conditions)



(***p < .01; **p < .05; *p < .1 for one-tailed tests, versus "All No Frame")

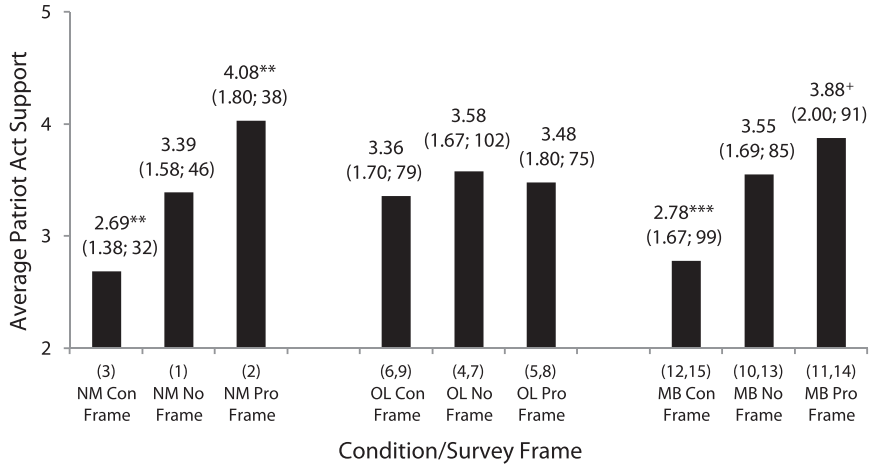
processors. There are three implications. First, on these issues, nonmanipulated individuals appear to process in an MB fashion, given the similarities between the two groups (see Chong and Druckman 2010). Second, the nonresult for the OL processors shows that extant survey results may reflect effects present only among the subgroups of the population with weak attitudes. When individuals form strong opinions about a particular issue, no experimental

FIGURE 3 Casino Survey Framing Effects Occur among MB But Not OL Processors (Merging Pretreatment Conditions)



(***p < .01; **p < .05; *p < .1 for one-tailed tests, versus "No Frame" within each processing mode)

FIGURE 4 Patriot Act Survey Framing Effects Occur among MB But Not OL Processors (Merging Pretreatment Conditions)

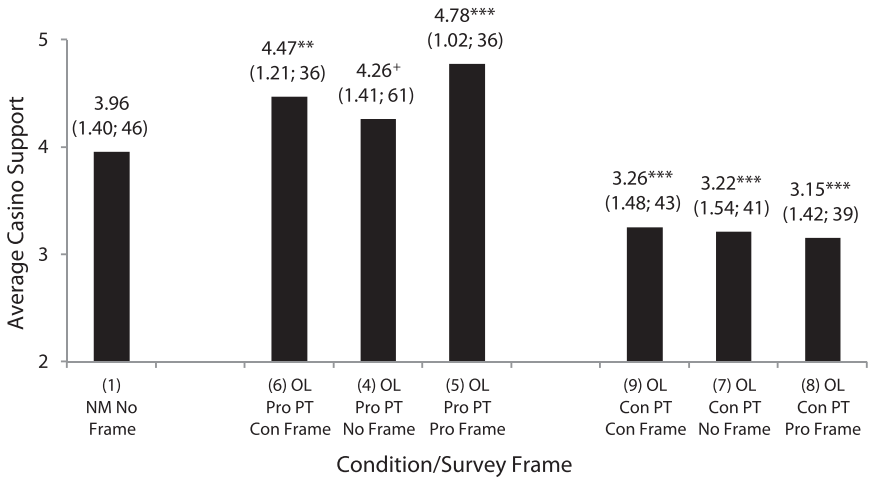


(***p < .01; **p < .05; *p < .1; +p < .13; for one-tailed tests, versus "No Frame" within each processing mode)

effects will appear. Third, effect sizes may be understated for influenced subgroups (i.e., MB processors) when data are merged. For example, the merged data in Figures 1 and 2 show the Pro frames increase support for the casino and Patriot Act, respectively, by .40 (i.e., 4.32–3.93) and .23 (i.e., 3.77–3.53). Yet among the MB processors the corresponding effects are .55 (4.56–4.01) and .33 (3.88–3.55; see Figures 3 and 4).

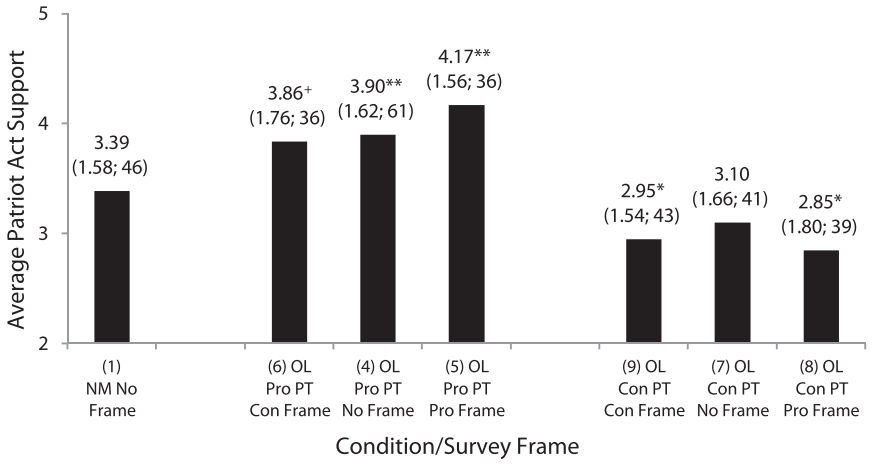
It turns out, though, that the null effect for OL processors is only part of the picture. Figures 5 and 6 display results *only for OL processors*, by pretreatment condition. Asterisks here denote statistical significance relative to our basic control group baseline (no manipulation, no survey frame, which is the first bar in Figures 5 and 6). For both issues, we see there were in fact framing effects—the pretreatment environment significantly pushed opinions

FIGURE 5 Casino Pretreatment Effects among OL Processors



(***p < .01; **p < .05; *p < .1; +p < .13; for one-tailed tests, versus "NM No Frame")

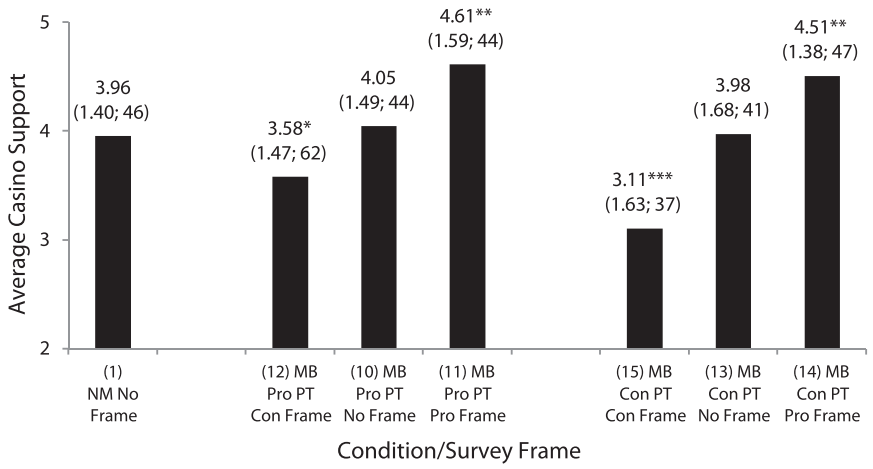
FIGURE 6 Patriot Act Pretreatment Effects among OL Processors



(***p < .01; **p < .05; *p < .1; +p < .13; for one-tailed tests, versus "NM No Frame")

in the direction of the given pretreatment frame. For example, on the casino issue, the Pro pretreatment effect led to significant increases in support regardless of which survey frame respondents later received. Those exposed to the Pro pretreatment environment (emphasizing the economic frame) but the Con survey frame (emphasizing social costs) still registered an average opinion of 4.47, which significantly exceeds the 3.96 control ($t_{80} = 1.76$; $p \leq .05$). We see the same dynamic for the other two Pro pretreatment conditions. In contrast, all of the Con pretreatment conditions where respondents received the social-costs frame prior to the survey experiment showed significant decreases in support. Even those who later received the Pro economic survey frame registered an opinion of only 3.15, significantly lower than 3.96 ($t_{83} = 2.62$; $p \leq .01$). We see virtually the same dynamic, albeit to a slightly less extent, for the Patriot Act issue (see Figure 6). Clearly, a framing effect occurred among OL processors—it just did so prior to the survey experiment. Once in the survey experiment, these individuals then ignored or rejected the frames. In short, OL processors exhibited pretreatment effects but not survey framing

FIGURE 7 Casino Pretreatment Effects Do Not Occur among MB Processors



(***p < .01; **p < .05; *p < .1 for one-tailed tests, versus "NM No Frame")

effects. Indeed, as was evident in Figures 3 and 4, there are no survey framing effects among OL processors, even when broken down by specific conditions.

In Figures 7 and 8, we offer the same results but for MB processors. The point here is that pretreatment effects are not evident for either issue; instead we see significant survey framing effects regardless of the pretreatment environment. In every case, the survey frame pushed opinions in the expected directions regardless of the pretreatment environment. MB processors dramatically differ from OL processors, with the former being susceptible to the latest frame as offered in the survey experiment and the latter ignoring or rejecting that later frame while clinging to the information provided in the pretreatment environment.[24,25]

Discussion

Our results, across both issues, show that pretreatment effects occur among exposed individuals who were motivated to form strong attitudes. Such individuals do not react to experimental frames that match those in the pretreatment environment, and they reject contrary experimental frames (e.g., motivated reasoning). The overall experimental effect discovered thus stems from a subgroup of respondents who are less motivated (e.g., MB respondents)—and ironically, these respondents were actually more affected than the aggregate results suggest. In the end, the picture of citizens is not particularly salubrious as it is some mix of malleably reactive individuals and dogmatic individuals who display a tendency to dismiss contrary evidence (although these individuals were influenced by pretreatment communications). Methodologically, the results make clear that what happens prior to the experiment can matter and, once accounted for, may reveal a more dynamic, heterogeneous group of individuals (e.g., understate the effects on some groups and overstate them on others). We recognize that our treatments were strong. We partially address these concerns in our next study, to which we now turn.

Study 2

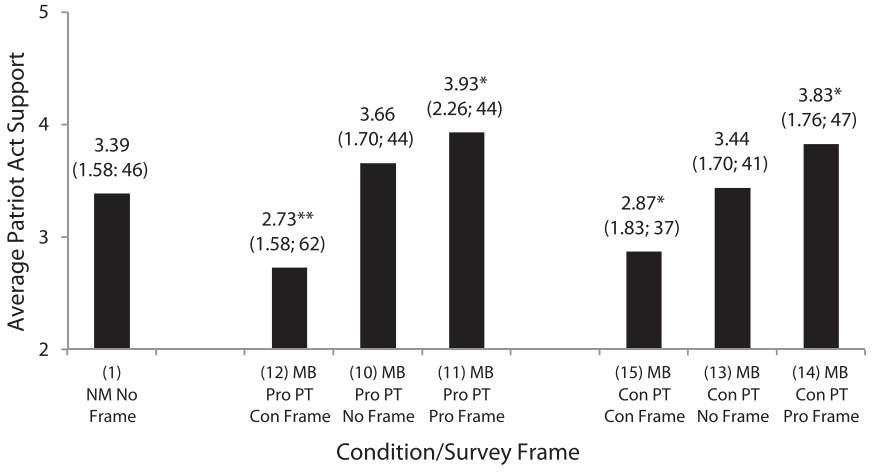
We present results from another study which complements our first study in at least three important ways.[26] First, our Study 2 takes place outside of the laboratory and reveals pretreatment effects happen even when beyond the experimenter's control. As such, the study provides a blueprint of how scholars might go about identifying pretreatment effects and their consequences when the prior

[24]The differences between MB and OL processes are significant; we do not present these analyses here as it should be clear that random assignment to processing mode means these differences are likely significant.

[25]In results available from the authors, we offer suggestive evidence that OL processors engage in motivated reasoning when they receive a survey frame that contrasts with their pretreatment environment.

[26]Parts of the study description come from Druckman (2010), which reports distinct data from the same survey.

FIGURE 8 Patriot Act Pretreatment Effects Do Not Occur among MB Processors



(***p < .01; **p < .05; *p < .1 for one-tailed tests, versus "NM No Frame")

environment is beyond their control Second, we employ a distinct measure of attitude strength—one that is more accessible in nonexperimental work (given it is a measure and not a manipulation). Third, we study a longer time lag between pretreatment and the survey experiment, thus providing insight into the longevity of pretreatment.

This study again focuses on opposition to or support for a state-funded gambling casino. This time, though, we study these attitudes in the context of an actual proposal during the 2006 Illinois gubernatorial election. We used an Election Day exit poll that (randomly) offered respondents different frames (e.g., social costs, economic) regarding the casino. For reasons we now discuss, we expected the pretreatment context to influence responses in the Election Day survey experiment.

Pretreatment Context

The 2006 Illinois gubernatorial election pitted Democratic incumbent Rod Blagojevich against Republican Judy Topinka. The campaign's initial focus concerned the declining state economy and the candidates' plans to raise revenue (for discussion, see Druckman 2010). Topinka's economic plan—as enunciated on August 23—revolved around a proposal to create a land-based, state-owned Chicago casino that would fund education and property tax relief. Blagojevich instead proposed leasing the state lottery to generate revenue. Topinka's casino idea split the public—a mid-September *Chicago Tribune* poll showed 54% in opposition to the plan—and cut across partisan lines.[27] While Blagojevich opposed the proposal, he had a year earlier proposed doubling gambling positions, and Chicago Democratic Mayor Daley was open to the plan.[28]

A content analysis of *Chicago Tribune* coverage of the campaign (from the date of the casino proposal until Election Day)[29] showed that the casino proposal initially received substantial coverage—15% of all issue coverage focused on the casino for the two weeks following Topinka's proposal. Virtually all the coverage focused on the potential economic revenue that would be generated (see Druckman 2010). This cohered with the most covered issue during this time period: the dire economic situation (35% of all issue coverage).

Just as it appeared that the economy and the candidates' revenue proposals would dominate discourse, the campaign took an unexpected turn (on September 9). With little forewarning, a rash of corruption allegations was launched, including accusations that Blagojevich traded state jobs for personal payoffs and improperly spent state money. Topinka also received scrutiny for

[27]The casino plan was not an issue on which voters would directly vote (e.g., an initiative), but it initially appeared to be a critical campaign issue (see Druckman 2010).

[28]Interestingly, after being reelected, Blagojevich expressed support for a casino plan.

[29]This covers August 24 through November 6. Details on the content analysis are available from the authors.

her role in the administration of previous Governor George Ryan, who was on trial for charges of corruption. Corruption came to dominate coverage, with a full 50% of all issue coverage devoted to it during the last month of the campaign. Coverage of the economy dropped precipitously to 5% during the last month, and the casino proposal virtually disappeared, receiving just 2% of coverage. For us, this course of events means that the pretreatment environment regarding the casino was discrete and asymmetric, with a clear focus on the positive economic benefits.

Election Day Exit Poll

We explored the impact of the pretreatment context by implementing an Election Day exit poll, which contained an embedded experiment. The exit poll makes for a relatively realistic context in which to assess communication effects since the respondents had just voted in an election where the issue at hand (i.e., the casino proposal) had relevance. We implemented the exit-poll survey experiment by assembling 24 teams of two-student pollsters. We then randomly selected polling locations throughout the northern part of Cook County, Illinois. Each polling team spent a randomly determined two-to-three-hour daytime period at their polling place. A pollster asked every third voter to complete a self-administered, anonymous questionnaire in exchange for $5. This approach enabled us to obtain a fairly heterogeneous group of 338 participants (for details on the sample, see Druckman 2010). We also used a fairly short survey to ensure a representative sample beyond just those who have the time and interest to complete a lengthy survey.

The dependent variable gauged support for the state-owned gambling casino on a 7-point scale, with higher scores indicating increased support. Respondents were randomly assigned to one of four frame conditions, which resembled those in Study 1 (see Appendix C for some discussion on the design). The *control group* received the dependent variable question and no other information (N = 117); it asked, "A proposal is being considered for the Illinois state government to operate a land-based gambling casino. What do you think—do you oppose or support the proposal for a state run gambling casino? Circle one number on the following 7-point scale." The *social-costs frame* group received the casino support question that also stated: ". . . Some say that a state run casino will have severe social costs, such as addiction and debt . . ." (N = 57). Those assigned to the *economic frame* condition read: ". . . Some say that the revenue from the casino would provide tax relief and help to fund education . . ." (N = 109).[30] Finally, those in the *dual frame* condition read: "Some say that a state run casino will have severe social costs, such as addiction and debt. Others say that the revenue from the casino would provide tax relief and help to fund education . . ." (N = 55). (See note 33 for an explanation of why the Ns vary across conditions.)

Absent any pretreatment effects, we expect, as in Study 1, that the social-costs frame would decrease support for the casino, the economic frame would increase support, and the dual frame would have no effect relative to the control group. The dual frame prediction builds on prior work that shows simultaneous exposure to competing strong frames results in no influence (e.g., Chong and Druckman 2007; Druckman et al. 2010; Sniderman and Theriault 2004). Evidence of a pretreatment effect would manifest, given the early campaign focus on the casino's positive budgetary implications, by vitiating the impact of the economic frame (since yet another exposure would have minimal additional effect). It also would cause individuals who formed strong supportive attitudes due to that early exposure to *reject* the social-costs frame. We expect these noneffects to occur only among those who were exposed and attentive to the campaign *and* engaged in processing that increased attitude strength (which leads to attitude stability). Only these voters would have received the early coverage and formed initial opinions that maintained until Election Day. They also might, on average, be more supportive of the casino due to the early focus on the economy.

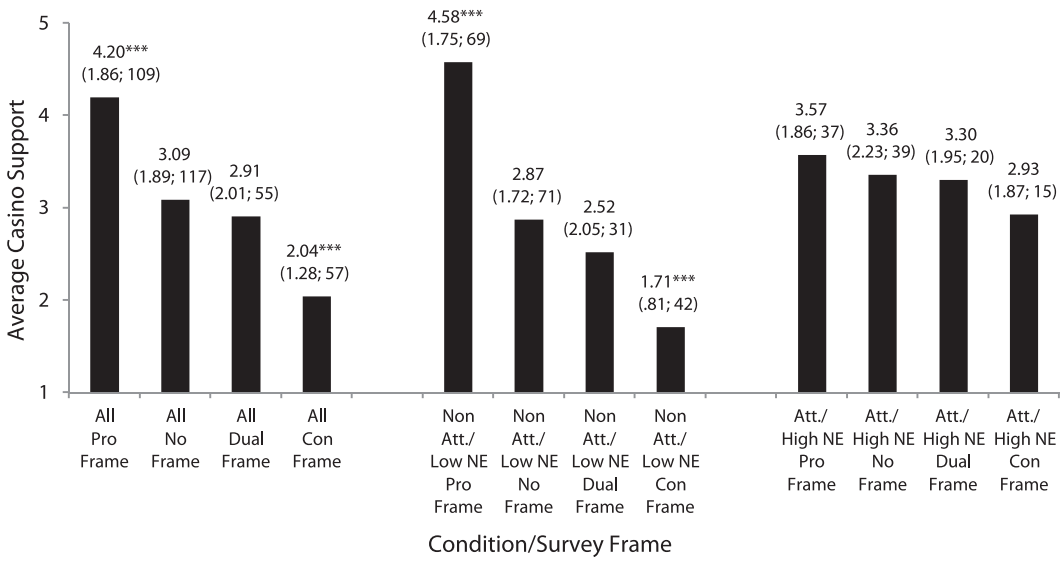
To measure campaign exposure and attention, we asked respondents if they subscribed to either of the two local newspapers and how many days, on average, they read the front-page and/or metro sections of the paper (Druckman 2004).[31] We captured the processing variable with the aforementioned need-to-evaluate (i.e., NE) construct, which asked respondents: "Compared to the average person, do you have far fewer opinions about whether things are good or bad, somewhat fewer opinions, about the same number of opinions, somewhat more opinions, or far more opinions?"[32] As explained, pretreatment effects likely occur only among voters who are *both* attentive *and* high on the need-to-evaluate variable. We identified such voters as those who were above the median score for

[30] As in Study 1, we pretested the frames and found both were strong and directionally distinct. Details are provided in Druckman (2010).

[31] We recognize self-report recall measures are debated, and we discuss the advantages of our measure in Appendix C.

[32] We used only one item due to the requirement that the exit poll be short.

FIGURE 9 Exit Poll Casino Support by Condition and Attention/NE



(***p < .01; **p < .05; *p < .1 for one-tailed tests, versus "No Frame" within each group)

both the newspaper variable and the NE variable; we refer to these voters as "Attentive/High NEs" (N = 111), and others as "Nonattentive/Low NEs" (N = 213).[33]

Results

We present our results by exploring mean support scores across conditions for (1) everyone ("All"), (2) Nonattentive/Low NEs, and (3) Attentive/High NEs. In Appendix C, we present multivariate analyses—which is necessary since we measured and did not manipulate (as in Study 1) exposure/attention and attitude strength. These analyses replicate what we present here, and thus we focus in the text on simple means. Figure 9 presents the mean support scores broken down by the three groups, and then within each, by survey experimental condition (i.e., frame).

The first four bars, which report results for all respondents, show clear survey framing effects. Relative to the No frame control group, the Pro frame caused a significant increase in support ($t_{224} = 4.47$; $p \leq .01$), the Con frame caused a significant decrease ($t_{172} = 3.79$; $p \leq .01$), and the dual frames canceled out ($t_{170} = .56$; $p \leq .30$). The next set of bars reveals even more dramatic experimental framing effects among Nonattentive/Low-NE voters, with the Pro and Con frames having substantial impacts (respectively, $t_{138} = 5.81$; $p \leq .01$, $t_{111} = 4.10$; $p \leq .01$) and the dual frame having no effect ($t_{100} = .91$; $p \leq .20$). As in Study 1, these respondents, who presumably formed weaker attitudes, exhibit larger experimental framing effects than do all respondents merged together. The results for "all" thus understate the effect size among the very subset of respondents who were significantly influenced. Indeed, the other subset of respondents—the Attentive/High-NE individuals—displays no susceptibility to experimental framing effects (none of the differences are significant, even at the .20 level).

The Attentive/High-NE results reflect a pretreatment effect such that exposure to another economy frame does little beyond the effects of prior exposure, and the social-costs frame was rejected. This latter dynamic occurred because of the strong opinions formed in the initial response to the economy pretreatment. As predicted, the economic pretreatment environment also increased support among these respondents; putting aside respondents who received the economic survey experiment frame (since this had such a notable effect on Nonattentive/Low-NE respondents), the Attentive/

[33]Some respondents did not answer the need-to-evaluate or newspaper reading question and thus the N shrunk a bit. Also, it turns out those above the median read a paper at least five days a week. We do not expect a monotonic relationship with a combined version of the NE and newspaper variable since high levels of both variables are expected to be necessary. Our use of a median split follows many others who employ analogous measures (e.g., Ansolabehere, Rodden, and Snyder 2008, 224–25; Druckman and Nelson 2003; McGraw and Dolan 2007, 311–12; Miller and Krosnick 2000, 305).

High-NE respondents registered a significantly higher support score across conditions ($t_{216} = 3.08$; $p \leq .01$).[34]

Overall, our results echo those found in the laboratory experiment. Individuals who are attentive to earlier information and motivated to form strong attitudes exhibit pretreatment effects. This is again suggestive of two groups of voters—one that is reactive to experiential stimuli and another that, while affected by earlier communications, rejected the messages in the experiment.[35]

The Longevity of Pretreatment

In Study 1, we separated the pretreatment communications from the survey experiment by only five days. However, roughly two months separated pretreatment and treatment in Study 2, thus suggesting that pretreatment effects can endure. Further evidence along these lines comes from follow-up e-mail surveys we conducted with both studies. The surveys took place approximately 30 days and 10 days, respectively, after the final experimental treatments in Studies 1 and 2. For each, we found that the OL group (Study 1) and the Attentive/High NEs (Study 2) demonstrated considerable attitude stability (reflective of pretreatment). Other respondents' opinions moved toward the control group mean.[36] The effects are real and can endure. That said, pretreatment effects do not last indefinitely—even strongly formed attitudes decay (e.g., Conner and Armitage 2008, 271; Krosnick 1988; Zanna et al. 1994). Chong and Druckman (2011b) test the longevity of framing effects, among OL processors, by comparing two distinct lag periods. They find over the short lag, attitudes maintained and OL processors resisted counter frames, but over the longer lag, attitudes decayed and the counter frame prevailed.[37]

Longevity of pretreatment effects undoubtedly depends on a number of factors. First, counter frames may matter (e.g., a social-costs frame launched in a pretreatment environment dominated by economic frames), but in complex ways. In some conditions, they may vitiate the impact of pretreatment, but in other conditions, they may generate counterarguments and inadvertently strengthen pretreatment effects (see Chong and Druckman 2011b). Future work is clearly needed to study how more competitive pretreatment environments influence subsequent response. Second, the amount of pretreatment communications (e.g., does the pretreatment communication appear a few times or many times?) varies, and it is not clear what is sufficient. Chong and Druckman (2010) report that a single communication inoculates OL processors from later counter frames. Yet, it is not clear how often, if ever, this occurs; moreover, repetition does help. Druckman, Fein, and Leeper (n.d.) report that repeating a frame, even when not moving opinions, enhances attitude strength and, consequently, stability.

Third, issues differ widely in their salience. Most past work on over-time political communication focuses on low-salience issues (e.g., de Vreese 2004, 202; Druckman and Nelson 2003; Druckman et al. 2010; Tewksbury et al. 2000). While this minimizes the threat of pretreatment, it also leads to less generalizable results. Our approach provides leverage into how one can examine more timely issues. Fourth, individuals vary when it comes to attitude strength. The 2004 American National Election Study survey reveals that there was substantial variance in NE (using two items, on a 0–1 scale, the mean score is .58, the standard deviation is .21, and the respective quartiles are .42, .58, and .71). Individual differences also may depend on age (e.g., attitude strength tends to be highest in middle age; Visser and Krosnick 1998) and on the likelihood of seeking out new information. Druckman, Fein, and Leeper (n.d.) show that initial frames often prompt individuals to seek out reinforcing information that strengthens pretreatment effects.

We view identifying the conditions and extent of pretreatment effects as one of the most pressing questions

[34]The Attentive/High NEs registered an average of 3.26 (2.07; 74) compared to 2.46 (1.66; 1.44) for Nonattentive/Low NEs. As in Study 1, we also found suggestive evidence that Attentive/High NEs engaged in motivated reasoning when exposed to a contrary survey frame (results are available from the authors).

[35]It is possible that our results do not reflect pretreatment effects per se and rather stem from Attentive/High NEs being less vulnerable, in general, to framing effects (see, e.g., Druckman and Nelson 2003). We explored this possibility with a pretest conducted prior to the exit poll, with individuals not residing in Illinois (and hence not exposed to the pretreatment communications; N = 174). We found in these pretests (the results of which are available from the authors; a distinct part of the pretest is described in Druckman 2010) that Attentive/High NEs exhibited susceptibility to both the social-costs and economic frames. This suggests that the pretreatment environment itself is critical and Attentive/High NEs are not inherently unaffected by frames. We also checked to see if NE is a proxy for general sophistication, but our results (i.e., the moderating effect of Attentive/High NEs) do not hold when we use education or political knowledge as a substitute for NE.

[36]We found no evidence that follow-up attribution was contingent on processing mode, and thus the results do not reflect differential rates of attrition.

[37]Interestingly, Chong and Druckman's (2011b) "long lag" was just short of a month, which is shorter than the exit poll time lag. Clearly, the exact durability of pretreatment effects varies across issues, times, and contexts. One possibility is that in our exit poll, the Attentive/High NEs sought out further casino information consistent with the initial campaign, thereby solidifying the pretreatment effect (see Druckman, Fein, and Leeper n.d.).

in need of future research. Ultimately, the importance of pretreatment can only be learned if studies explicitly attempt to account for the possibility. Even if pretreatment effects are not so pervasive, it would suggest people form weak attitudes or are extremely inattentive.

Conclusion

Experiments have emerged as a central methodology in political science (e.g., Druckman et al. 2011). Perhaps no other field has benefited as much as work in political communication. A nagging, long-standing concern about some of this work involves the implications of relying largely on experiments (e.g., Hovland 1959). As Kinder explains, "Taken all around, we now seem quite a ways further along toward the 'science of communication'. . . Of course, there is still quite a bit left to do . . . experimental research [needs to] be balanced off with more studies of framing au naturel" (2007, 159–60). He continues, "Enough already with the experiments . . ." (157). We took Kinder's advice (partially) to heart by exploring how events outside and previous to the prototypical experimental setting affect participants and, consequently, the inferences that can be drawn from common experiments.

Our findings suggest that accounting for the pretreatment context leads to a number of novel insights, that if nothing else demand further exploration. These insights are as follows.

- Average experimental treatment effects may miss important variations among subgroups (also see Kent and Hayward 2007):
  - averages may reflect an effect present only among a subgroup that formed weak attitudes (e.g., MB processors) on the issue (Barabas and Jerit 2010), and
  - averages may understate the size of this effect among those individuals.
- The nonexistence of an experimental effect may stem from a large number of individuals forming strong attitudes (e.g., OL processors) in response to earlier communications, prior to the experiment.[38]

[38]In our studies, such individuals were limited; however, it is perfectly conceivable that on a particular issue in certain populations, it could be a more naturally occurring phenomenon. For example, Hillygus and Jackman (2003) report presidential conventions exerted a larger impact than subsequent presidential debates, which could reflect pretreatment effects (in terms of the debate's noneffect).

Consequently, when we find experimental effects, it will be in populations and on issues where people, on average, are not forming strong opinions. These opinions, in turn, will be relatively fleeting. In our experiments, pretreatment communications likely did affect MB processors and nonmanipulated individuals, but these effects failed to endure (also see Chong and Druckman 2010). Let us be clear, however, that short-term effects should not be equated with unimportant effects. Aside from the normative implications, short-term effects can matter if properly timed, such as just prior to an important public opinion poll or an election. Moreover, communications that appear to have fleeting effects sometimes are later culled from memory in surprising and influential ways (e.g., Druckman et al. 2010; Priester et al. 1999).

Another insight concerns the inferences based on the larger political communication literature. In light of well-documented publication biases toward significant results (e.g., Gerber et al. 2010), it may be that the picture emerging from published survey framing effect studies overstates the existence of effects (see Barabas and Jerit 2010 for similar evidence). Either scholars seek out issues and/or populations where weak attitudes are likely or studies focused on stronger attitudes that find noneffects do not survive the publication process. Consequently:

- The mass public is less malleable and holds more stable opinions than would be suggested by the aggregation of experimental results.

Those who do form initially strong opinions—perhaps from early exposure to communications (e.g., pretreatment)—appear then to dogmatically reject subsequent contrary arguments (e.g., they engage in motivated reasoning). This is nearly the opposite reaction to those malleable individuals affected by the treatment. As a result:

- The mass public shows signs of being heterogeneous, with some being malleably reactive and others being dogmatically invulnerable to communication effects.

We suggest this dualistic possibility cautiously. Participants who resisted influence in the experiment were affected by pretreatment communications and thus were not so dogmatic as to reject all communications. Had they rejected all communications, our results in some sense would be much less interesting—they would merely reveal that longer-term predispositions overwhelm short-term communication effects (see Malhotra and Margalit 2010). In our case, it is not deeply held prior opinions or values but a small number of brief messages that conditioned subsequent response. This resistance to later

messages is more troubling because the dogmatism stems not from long-standing predispositions, but from whatever messages happened to come first. On the flip side, reactive respondents exhibited great malleability to the latest message (in the experiment). Either way, the fairly arbitrary sequence of messages drives opinions.[39]

We conclude by emphasizing that our findings should not be taken as an indictment of the experimental approach.[40] Rather, the central point is that opinions are not fixed in time and time dynamics need greater attention. The influence of timing will, in turn, depend on attitude strength. We recommend that public opinion researchers define the time period of their study, just as they identify other units (e.g., the individual respondents). If one's goal is to evaluate the impact of a communication, then pretreatment-effect possibilities can be explored by accounting for the prior rhetorical context, or even better, by conducting replications with distinct populations or at different times.[41] Time dynamics have nontrivial implications, and accounting for them will lead to a more accurate understanding of how political communications shape opinions.

Appendix A

Casino Economic Frame Using the Online Manipulation

. . . we are testing materials for use in a study that is related to the kinds of *opinions* people form about public policies. Along these lines, we would like you to read a series of paragraphs, taken from recent news editorials, on . . . a proposed state-run casino in Illinois . . .

Please read the following paragraphs and, for each, rate the extent to which it decreases or increases your support for the state-run casino. In subsequent surveys, *we will ask you for your overall opinion about the state-run casino* (i.e., the extent to which you oppose or support the state-run casino). There are no right or wrong opinions and your responses to all questions are completely confidential.

Please read the paragraphs carefully and, after each one, rate the extent to which it decreases or increases your support for the state-run casino.

Paragraph 1: You don't have to live near Las Vegas or Atlantic City to encounter a casino these days. Gambling establishments are an increasingly common landmark in cities and towns across the US. Moreover, many state legislatures, including Illinois', are currently debating whether casinos should be legalized and the extent to which public funds should be entangled with such enterprises.

To what extent does this statement decrease or increase your support for the state-run casino?

1	2	3	4	5	6	7
decreases a lot		*neither decreases nor increases*				*increases a lot*

Paragraph 2: In 2006, destination casinos Foxwoods and Mohegan Sun in Connecticut generated $3.15 billion in gross revenue, and tax revenue to the state of Connecticut approached $500 million. The two destination casinos in Connecticut have directly employed over 24,000 individuals since they were created in the 1990s.

To what extent does this statement decrease or increase your support for the state-run casino?

1	2	3	4	5	6	7
decreases a lot		*neither decreases nor increases*				*increases a lot*

Paragraph 3: There is little doubt that destination casinos in Illinois could draw customers from every gaming market in the nation except Las Vegas. A Federal Reserve Bank study in 2006 stated, "new resort casinos attract a significant number of patrons from neighboring states, even if they are in competition with existing casinos. Out-of-state customers amount to as much as twenty percent of all casino visitors in some cases." Many other Illinois residents echo a similar sentiment. "The region would get an economic shot in the arm," said John Rusinowski, a resident of Joliet who was thrilled to hear

[39] Our findings also have implications for the incongruent findings between micro and macro studies. The modal micro study that explores the stability of communication effects suggests the effects are fleeting (consistent with our argument that effects occur among those with weak attitudes; see, e.g., Chong and Druckman 2010; Druckman and Nelson 2003; Tewksbury et al. 2000). Yet, most macro studies suggest stability (e.g.,Wood and Vedlitz 2007, 553). We suspect these contrasting findings stem from a focus in macro studies on long-standing salient issues compared to the micro focus on attitudes toward relatively novel and specific issues that enjoy heightened but short-term salience (for further discussion, see Chong and Druckman 2010).

[40] We also want to make clear that while we focused on the effects on a conventional survey experiment, the role of pretreatment effects applies to experiments carried out in any setting.

[41] Studies across populations are particularly intriguing. For example, Rohrschneider and Loveless (2010) find that variance in a country's context (i.e., affluence and governance quality) influences the basis on which people base their opinions about the European Union. This type of context is analogous to a pretreatment effect.

that developers were eyeing his hometown as a prime location for a casino. "A lot of industries have gone, and it would bring in a lot of jobs and taxes."

To what extent does this statement decrease or increase your support for the state-run casino?

1	2	3	4	5	6	7
decreases a lot		*neither decreases nor increases*				*increases a lot*

How effective are the paragraphs you just read in terms of providing information and/or presenting an argument about the state-run casino?

1	2	3	4	5	6	7
definitely NOT effective			*not sure*			*definitely effective*

Remember that we will recontact you in subsequent surveys when we will again ask you some questions about the state-run casino.

Patriot Act Civil Liberties Frame Using a Memory-Based Manipulation

. . . we are testing materials for use in a study of the *structure of sentences* people use when writing news editorials. Along these lines, we would like you to read a series of paragraphs, taken from recent major newspaper editorials . . .

Please read the following paragraphs and, for each, rate how dynamic you think it is. *A paragraph is more "dynamic" when it uses more vivid action words.* For example, a statement like, "He **sped** up and **raced** through the light before **crashing** into the swerving truck," seems more dynamic than, "He **went** faster to **get through** the light before **having** an accident." The action words in the first sentence (which we have highlighted in **bold**) seem more dynamic or vivid than those contained in the second sentence. There are no right or wrong opinions and your responses to all questions are completely confidential.

Please read the paragraphs carefully and, after each one, rate the extent to which you think it is dynamic.

Paragraph 1: With the passage of the Patriot Act in 2001, the FBI can now enter your home, search around, and doesn't ever have to tell you it was there. You could be perfectly innocent, yet federal agents can go through your most personal effects. When considering new laws, a test of the impact on liberty should be required. On that test, the Patriot Act fails. At a massive 342 pages, it potentially violates at least six of the ten original amendments known as the Bill of Rights—the First, Fourth, Fifth, Sixth, Seventh and Eighth Amendments—and possibly the Thirteenth and Fourteenth as well.

How dynamic would you say this paragraph is? (Remember that a paragraph is more dynamic when it uses more vivid action words.)

1	2	3	4	5	6	7
not at all dynamic			*moderately dynamic*			*very dynamic*

Paragraph 2: Without oversight, there is nothing to stop the government from engaging in broad fishing expeditions, or targeting people for the wrong reasons, and then preventing Americans from ever speaking out against potential abuses of intrusive surveillance power. With the passage of the Patriot Act we are edging ever closer to losing our basic civil liberties.

How dynamic would you say this paragraph is? (Remember that a paragraph is more dynamic when it uses more vivid action words.)

1	2	3	4	5	6	7
not at all dynamic			*moderately dynamic*			*very dynamic*

Paragraph 3: Of all the protections found in the Constitution, the Fourth Amendment stands as the final barrier between the privacy rights of Americans and the potential for government abuse of power. But if law enforcement officials can search citizen homes and records without having to go through a judge, then the principle of the Fourth Amendment has been rendered essentially meaningless.

How dynamic would you say this paragraph is? (Remember that a paragraph is more dynamic when it uses more vivid action words.)

1	2	3	4	5	6	7
not at all dynamic			*moderately dynamic*			*very dynamic*

Appendix B

No Frame Patriot Act Survey Question

The Patriot Act was enacted in the weeks after September 11, 2001, to strengthen law enforcement powers and

technology. What do you think—do you oppose or support the Patriot Act? Choose one number on the following 7-point scale.

1	2	3	4	5	6	7
oppose strongly			*not sure*			*support strongly*

Con Frame Patriot Act Survey Question

The Patriot Act was enacted in the weeks after September 11, 2001, to strengthen law enforcement powers and technology. Under the Patriot Act, the government has access to citizens' confidential information from telephone and e-mail communications. As a result, it has sparked numerous controversies and been criticized for weakening the protection of citizens' civil liberties. What do you think—do you oppose or support the Patriot Act? Choose one number on the following 7-point scale.

1	2	3	4	5	6	7
oppose strongly			*not sure*			*support strongly*

Pro Frame Patriot Act Survey Question

The Patriot Act was enacted in the weeks after September 11, 2001, to strengthen law enforcement powers and technology. Under the Patriot Act, the government has more resources for counterterrorism, surveillance, border protection, and other security policies. As a result, it enables security to identify terrorist plots on American soil and to prevent attacks before they occur. What do you think—do you oppose or support the Patriot Act? Choose one number on the following 7-point scale.

1	2	3	4	5	6	7
oppose strongly			*not sure*			*support strongly*

Patriot Act Survey Importance Ratings (Casino Issue Is Analogous)

We are now going to list a few ideas that individuals have expressed when describing their opinions about the Patriot Act. Some of these ideas may seem important to you as you think about the Patriot Act, while others may seem less important. Please tell us how important each of these ideas is to you when thinking about your overall evaluation of the Patriot Act.

Idea**:** *"Protecting Civil Liberties."*

Is this idea unimportant or important in your overall evaluation of the Patriot Act?

1	2	3	4	5	6	7
very unimportant						*very important*

Idea**:** *"Preventing Terrorist Attacks."*

Is this idea unimportant or important in your overall evaluation of the Patriot Act?

1	2	3	4	5	6	7
very unimportant						*very important*

Idea**:** *"Ensuring an Appropriate Amount of Government Power."*

Is this idea unimportant or important in your overall evaluation of the Patriot Act?

1	2	3	4	5	6	7
very unimportant						*very important*

Idea**:** *"Ensuring Sound Implementation of Public Policy."*

Is this idea unimportant or important in your overall evaluation of the Patriot Act?

1	2	3	4	5	6	7
very unimportant						*very important*

Appendix C

In this appendix, we discuss three aspects of our Study 2 exit poll: (1) we provide additional details on the design, (2) we explain the merits of our Study 2 exit poll media

exposure and attention measure, and (3) we present Study 2 results with control variables added.

In terms of design, the experiment contained various other randomly assigned conditions that are not relevant to our focus. This included various mixes of "weak" or nonpersuasive frames, including a morality, an entertainment, and a corruption frame. There was clear evidence that the corruption frame registered no effect on opinion (i.e., respondents ignored it; see Druckman 2010). Thus, in the results we present here we merged our corruption-only condition with the control group (as the two conditions produced nearly identical effects) and a condition that provided the economic and corruption frames with the economic frame condition (as the two conditions also produced nearly identical effects). This increases our N, allowing us to explore the need-to-evaluate moderator, and seems quite feasible given the clear noneffect of the corruption frame. We do not combine conditions with the other weak frames (i.e., morality, entertainment) because the evidence of their noneffects is less clear. However, the results are robust (and in fact nearly identical) if we do merge conditions with these other weak frames. Details are provided in Druckman (2010).

In terms of our exposure and attention measure—we believe it deals relatively well with the three common problems inherent in measuring exposure and attention (Southwell et al. 2002). First, it does not ask for a self-assessment of a subjective state (such as interest in the campaign; see Zaller 1992, 6), or for recall of a unique event such as remembering a campaign ad—indeed, people presumably know if they subscribe to a local newspaper (as they pay the bill and receive it daily) and have some reliable sense of how often they read the paper (as it typically reflects a habitual behavior). Second, while those exposed may differ systematically from those not exposed, these differences stem largely from sociodemographic variables (e.g., education, age) for which we can control, and not political variables (see Bizer et al. 2004). Third, the measure matches our campaign content measure (in the aforementioned content analyses), and thus there is no concern of the medium not capturing the campaign content (Price and Zaller 1993, 136). The measure also accounts for both exposure and attention, and, as others have shown, people receive substantial campaign information from local newspapers (e.g., Mondak 1995). Finally, as we would expect, increased readership significantly correlates with increased discussion of the campaign, interest in the campaign, following of the campaign, and more accurate knowledge regarding the candidates' issue positions (details available from the authors).

In terms of control variables, the exit poll survey included items that we expect to correlate with casino support. One question measured a respondent's values toward government regulation of business. The precise question asked, "In general, do you feel that government regulation of business: usually does more harm than good; or is necessary to keep businesses from engaging in practices that are harmful to the public?" with higher scores indicating increased support for regulation. Another measure asked respondents**:** "How many times have you ever been to a casino?" Response options included "Never," "1–2 times," "3–5 times," "6–10 times," and ">10 times." Given the prominence of the corruption theme in the gubernatorial campaign (as explained in the text), we also asked respondents: "In your opinion, to what extent, if any, has the Blagojevich administration engaged in corrupt practices?" with higher scores indicating increased perceptions of corruption. Along similar lines, we included a standard trust in government item, asking, "How much of the time do you think you can trust the government in Washington to do what is right?" (with choices being "just about always," "most of the time," or "some of the time"). We asked respondents to name the gubernatorial candidate for whom they voted (recall Topinka proposed the casino). Finally, the survey included standard demographic measures that asked for respondents' party identification (on a 7-point scale, with higher values indicating more Republican), gender (0 = male, 1 = female), minority status, education (on a 5-point scale, with higher values indicating more education), political knowledge (measured with five political fact questions), and age.[42] The main results concerning the impact of the survey frames cohere with the findings described in the text.

The central finding is that, controlling for various other determinants of casino opinions, the survey experimental frames are significant for the Nonattentive/Low NEs but are not significant for the Attentive/High NEs. In results available from the authors, we also find that the survey experimental frame coefficients across the two groups (e.g., for the Social Costs Survey Frame, −.76 versus −.07) do differ significantly from one another in each case. Discussion of the control variables results can be found in Druckman (2010).

[42] Some respondents did not answer all of the control variables and thus the N is smaller in these analyses. Also note that other than education and political knowledge, the variables are standardized to 0–1 scales.

Study 1: Support for Casino Proposal

TABLE A1 Dependent Variable: Support for Casino Proposal (1 to 7).

	All	*Nonatt./ Low NE*	*Att./ High NE*
Social Costs Survey Frame	−.54***	−.76***	−.09
	(.19)	(.23)	(.36)
Economics Survey Frame	.76***	1.09***	.28
	(.15)	(.20)	(.27)
Social Costs-Economics Survey Frame	−.17	−.41*	−.02
	(.19)	(.25)	(.32)
Administration Corruption	−.38	−.01	−.99**
	(.33)	(.44)	(.57)
Regulation Value	.09	−.12	.12
	(.28)	(.38)	(.48)
Casino Visits	.40**	.32	.79**
	(.21)	(.26)	(.39)
Distrust Government	−.45*	−.44	−.77
	(.32)	(.38)	(.75)
Vote for Topinka	.32*	.19	.78**
	(.21)	(.27)	(.39)
Partisanship (Republican)	−.19	.03	−1.12**
	(.32)	(.39)	(.67)
Age	−.21	−.69**	−.10
	(.25)	(.33)	(.43)
Minority	.29**	.20	.53
	(.17)	(.20)	(.40)
Female	−.12	−.01	−.49**
	(.13)	(.18)	(.24)
Education	−.11*	−.07	−.03
	(.07)	(.09)	(.13)
Political Knowledge	−.02	−.01	−.04
	(.04)	(.05)	(.09)
τ_1 through τ_6	See below	See below	See below
Log likelihood	−524.35	−313.88	−176.21
Number of Observations	301	192	100

Note: Entries are ordered probit coefficients with standard errors in parentheses. ***p < .01; **p < .05; *p < .1 for one-tailed tests. The coefficient and standard errors for τ_1 through τ_6 are as follows, for All: −1.70(.49), −1.13(.49), −.70(.49), −.29(.49), .11(.49), .72(.49); for Nonatt./Low NE: −1.53(.66), −.91(.65), −.34(.65), .01(.65), .40(.65), 1.10(.65); for Att./High NE: −2.41(.95), −1.89(.94), −1.57(.94), −1.05(.94), −.64(.94), −.03(.94).

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