



Bias in Political Communication Experiments

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Abstract

Research on political communication effects has enjoyed great progress over the past 20 years. A key ingredient underlying these advances is the increased usage of experiments that demonstrate how communications influence opinions and behaviors. A potential problem with nearly all of these studies, however, concerns the lack of attention to events that occur prior to the experiment—that is, “pretreatment events.” In this paper, the authors explore how and when the pretreatment environment affects experimental outcomes. They argue that ignoring pretreatment has led extant work to overstate the malleability of the mass public, miss the identification of potentially two groups of voters—what they call *malleability reactive* and *dogmatic*—and contradict macrolevel work on aggregate public opinion trends.

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Over the last twenty 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 et al. n.d.).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 and one more exposure in the experiment does little. If, on the other hand, the experiment focuses on a communication that had not been previously experienced in "reality," then one could question the relevance of the experiment. Gaines et al. (2007: 12) state that "Put simply, either there is a likelihood of contamination from real-world experience of the... [or the] experiment explores a nonexistent or politically irrelevant phenomenon."

Despite the potentially grave consequences of pretreatment effects – as raising serious questions about experimentally based inferences – there has been virtually no work on the topic (for a partial exception, see Slothuus 2009). In this paper, we provide what we believe is the first conclusive evidence

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 et al. (n.d.: 3) state that experimentation and communication effects "seem made for each other."

of a pretreatment dynamic. More importantly, we identify the conditions under which pretreatment effects occur. We test our expectations with two studies, one in a laboratory environment and the other in the context of an Election Day exit poll. Our findings suggest a bias in experiments such that they over-report effects on attitudes that tend to be malleable and fleeting (also see Barabas and Jerit 2010). Our results also reveal the possibility of two types of voters – what we call malleably reactive and dogmatic – and may explain the inconsistent results between micro studies of over-time opinions and macro trends in public opinion.

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: 226-231). The aforementioned hate group rally experiment is a classic example of a framing effect experiment (Nelson et al. 1997).² The pretreatment environment refers to the context present prior to exposing experimental participants to the frame. A pretreatment effect occurs when a prior mass communication shapes attitudes about the rally that persist at the time of the experiment and condition responses to the experimental stimuli.³ Individuals who were recently exposed previously 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 mistaken conclusion that it has no (or a smaller) effect, when in fact, it has a large effect outside of the experimental context.⁴ This

² See Chong and Druckman (2007c: 115) and Druckman et al. (2009) for discussion of how framing effects are indistinguishable from what many scholars call priming.

³ We focus on “mass” communications since that is typically what is being emulated in these experiments (e.g., as opposed to inter-personal communications).

⁴ Gaines et al. (2007: 15-16) explain that while the pretreatment effect may often cause a “downward bias,” it is also possible in some circumstances that it will lead to an upward bias depending on the type of prior exposure.

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 et al. 2007: 17, 13).⁵ Let us be clear that by pretreatment effects, 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 effects that refer to stimuli in the environment prior to treatment (e.g., Brewer 2003, Malhotra and Margalit 2010).

We posit the presence of two 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 *and* that effect must sustain until the time of the experiment.

The key element in these conditions concerns pinpointing when a pretreatment effect endures so as to influence 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).⁶ By definition, a strong attitude persists and resists change (Krosnick and Smith 1994, Krosnick and Petty 1995, Miller and Peterson 2004, Visser et al. 2006). If individuals process pretreatment communications in a manner producing stronger attitudes, this will increase the persistence of those attitudes.⁷ Consequently, receiving a similar communication in the later experiment then may register no effect on individuals with strong attitudes.

⁵ While randomization of subjects should, on average, evenly distribute those with prior message exposure between experimental groups, even with randomization estimated treatment effects may be under- or over-estimates of message exposure effects.

⁶ Attitude strength is a distinct from Chong and Druckman’s (2007) concept of “frame strength.”

⁷ 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 Petty 1995). We focus on the processes by which the attitude is formed and updated as that is most relevant to exploring over-time dynamics.

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 reject, ignore, or avoid information that is inconsistent with their prior opinions (e.g., Lodge and Taber 2000, Kunda 2001, Redlawsk 2002, Rudolph 2006, Druckman et al. 2010, Druckman and Bolsen n.d.). 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, prior to exposure to an experimental stimulus, may subsequently be immune to the stimulus either because yet another consistent exposure does little or because they reject a contrary communication.

In contrast, for individuals who process earlier communications in ways that generate weak attitudes, the effect of that communication 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 vary in their tendency to form spontaneous evaluations when processing information. An individual's "need-to-evaluate" or 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 et al. (2001: 159) explain, individuals with a high NE "possess stronger object-evaluation associations due to their chronic evaluative responding" (also see Tormala and Petty 2001, Bizer et al. 2006: 1000, McGraw and Dolan 2007: 311-312).⁸

⁸ 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., Bizer et al. 2004). It also is distinct from political knowledge, although highly sophisticated individuals are more likely to engage in on-line processing (e.g., McGraw et al. 1990). (The results we present here regarding NE are robust to controlling for knowledge.) High NE scores also correlate with other strength-related features including accessibility (Petty and Jarvis 1996) and extremity (Federico 2004). High NE individuals tend to develop more organized attitudes (e.g., more ideologically constrained attitudes) that structure opinions across issues (see Jarvis and Petty

Second, individuals may form and update their attitudes using varying degrees of either on-line or memory-based processing of information. When individuals process a message about an issue on-line, 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 on-line tally rather than reconstruct and evaluate the specific pieces of information that comprise this summary (see, e.g., Hastie and Park 1986, Lodge et al. 1995, Druckman and Lupia 2000).⁹ Importantly, these attitudes – which reflect the earlier pretreatment message – will sustain (i.e., they are strong) and thus condition response in the experiment (they maintain inertia). In contrast, individuals who use memory-based information processing 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 re-construct their attitudes later, and thus, they are less likely to experience pretreatment effects (see Tormala and Petty 2001: 1600-160, Briñol and Petty 2005: 583).

*In sum, we hypothesize that pretreatment effects (e.g., leading to no experimental stimulus effect) will be more likely to occur when individuals are: (a) exposed and attentive to earlier communications similar to the experimental stimuli; and (b) form/update their attitudes in ways that promote strength. This occurs among high NE processors and on-line processors.*¹⁰

Study 1

Our first study employs an experimental approach, which allows for exogenous control over the two conditions posited to affect the size of pretreatment effect: pretreatment exposure/attention and modes of attitude formation. We recruited 650 participants to take part in a four-part study, in the political

1996: 1006, Druckman and Nelson 2003, Federico and Schneider 2007).

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

science laboratory at Northwestern University, conducted in two waves during Spring, 2010. Most 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 based sample, an increasing amount of evidence suggests results from such samples widely generalize (e.g., Miller and Krosnick 2000, Druckman 2004, Chong and Druckman 2007b; for more general discussion, see Druckman and Kam n.d.).

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 terrorist attacks of 9/11/2001 to increase the powers of law enforcement agencies to monitor communications, records, and financial transactions in an effort to identify terror threats.¹¹ We believe these issues are representative in the sense of being periodically salient but not intensely debated (e.g., fortunately from a design perspective, neither issue received sustained media coverage during our experiment, although both have received attention at different points in time). 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, which allows us to test hypotheses about the moderating effects of attitude strength before people have developed crystallized opinions.¹² 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.

¹¹ The Act contains a number of other elements such as re-defining 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.”

¹² 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.

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.¹³ 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.¹⁴ We presented these frames in mock news editorials.¹⁵ 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, in this study, our attitude formation process involves induced memory-based (MB) versus on-line (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 exogenously inducing either MB or OL processing of messages (e.g., Hastie and Park 1986, Mackie and Asuncion 1990, Bizer et al. 2006). 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.¹⁶ Respondents in the OL condition were also told they would be

¹³ Chong and Druckman (2007b) show that, when all frames are received concurrently, stronger frames influence opinions more than weaker frames, even when the weaker frame is repeated. We follow prior work by identifying strong frames via pre-tests that ask respondents to rate the frame’s “effectiveness.” For example, strong frames for and against the hate group rally might invoke considerations of free speech and public safety while a weak opposition frame might be an argument that the rally will temporarily disrupt traffic. All frames were judged as “strong” or effective in pre-tests. Pre-tests also showed that all of our Pro frames were perceived to be significantly more supportive of the policy than the Con frames; also, the two Pro frames did not differ from one another in perceived direction nor did the two Con frames. Details are available from the authors.

¹⁴ In their content analysis of *New York Times* coverage of the Act, Chong and Druckman (n.d.) report these are among the most frequently appearing frames on each issue.

¹⁵ 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 (n.d.) 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 (also see Hänggli 2010).

¹⁶ 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).

asked to report their attitudes at later points in time (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.¹⁷ In the first wave, respondents completed a brief background questionnaire of demographics. They then were assigned into one of fifteen 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 wave 1, 2, and 3.¹⁸ While the articles at each wave used the same frame, they were written so as to be distinctive from one another.¹⁹ This approach ensures that respondents received a directionally distinct pretreatment environment. We manipulated processing mode at each wave as noted above; for example, those in the OL conditions rated the extent to which paragraph affected their opinions, at waves 1, 2, and 3.

Finally, at wave 4, respondents were randomly assigned to receive a survey question employing 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

¹⁷ 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. We also asked participants the extent to which they obtained relevant information outside of the study, on the issues. Inclusion of this variable in analyses does not affect our results. Finally, we asked factual questions about the articles respondents read to ensure attention, and evidence suggests participants were in fact attentive (which is a condition for a pretreatment effect).

¹⁸ Those assigned to the no pretreatment conditions read articles irrelevant to the issues. Also, respondents were assigned uniformly to the directional conditions for each issue (e.g., those who received Pro casino articles also received Pro Patriot Act articles).

¹⁹ Articles were pre-tested for readability and to ensure they employed the frames we believed they did.

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.

[Insert Table 1 About Here]

The full set of conditions, along with the Ns, appears in Table 1.²⁰ The first set of conditions (1-3) resembles the typical experiment where the pretreatment environment is ignored and processing mode is not manipulated. Condition 1 serves as the overall control group given there is no survey frame in that condition. The other conditions involve manipulating processing mode as well as introducing a pretreatment environment.²¹ 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. 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 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

²⁰ 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.

²¹ 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 non-manipulated, 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.

We present the results by comparing mean support scores across relevant conditions. We confirmed the success of random assignment across various demographic and political variables.²² The casino and Patriot Act results mimic one another and thus we report the findings in tandem for the issues.

[Insert Figures 1 and 2 About Here]

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), No frame, or the Pro frame (economic, terrorism). We merge conditions regardless of processing mode and pretreatment exposure. Asterisks refer to statistical significance compared to the no survey frame conditions; we use one-tailed tests throughout given our directional predictions (see Blalock 1979: 163, Nelson et al. 1997, Druckman and Nelson 2002). 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 3.51 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 when it comes support for the Patriot Act. In short, if we treated these data as is typical in the experimental literature – ignoring pretreatment exposure and processing mode – we would find fairly notable framing effects on both issues.

[Insert Figures 3 and 4 About Here]

We next delve deeper by separating out the results by processing mode; Figures 3 and 4 display mean support values, again by survey frame, but 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 framing effects for the non-manipulated group and the memory-based (MB) processors. We do *not* see framing effects, however, for the on-line (OL)

²² Details are available from the authors. Multivariate analyses, consistent with what we report below, also are available from the authors.

processors. For example, for the casino issue, non-manipulated processors exposed to No frame report an average score of 3.96, which significantly differs from non-manipulated individuals exposed to Con social costs frame (average score of 3.06) or the Pro economic frame (average score of 4.51) (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 non-manipulated and MB processors. There are three implications. First, on these issues, non-manipulated individuals appear to process in an MB fashion, given the similarities between the two groups (also see Chong and Druckman 2010). Second, the non-result for the OL processors shows that extant survey results may reflect effects present only among subgroups of the population. When individuals form strong opinions about a particular issue, no experimental effects will appear. Third, effect sizes may be under-stated for influenced sub-groups (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., 4.32-3.92) and .23 (i.e., 3.76-3.53). Yet among the MB processes the corresponding effects are .55 (4.56-4.01) and .33 (3.88-3.55) (see Figures 3 and 4).

[Insert Figures 5 and 6 About Here]

It turns out though that the apparent null effect for OL processors is illusionary. Figures 5 and 6 display results *only for OL processors*, by pretreatment condition. Asterisks this time 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 in the direction of the given pretreatment frame. For example, on the casino issue the pro-pretreatment effect lead 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.

[Insert Figures 7 and 8 About Here]

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. (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.)

Psychology of On-Line Processing

We suggested that respondents who form initially strong attitudes due to the pretreatment environment may engage in motivated reasoning when they receive the survey frame. Consequently, they would reject the survey frame, viewing it as unconvincing. Our data allow us to speculate on this dynamic. We asked respondents to rate the importance of various considerations when constructing their opinions about the casino and Patriot Act (on 7-point scales with higher scores indicating increased importance; question wording for the Patriot Act items are in Appendix B). For both issues, the rated

considerations included the relevant frames: social costs and budgetary considerations for the casino, and preventing terrorism and civil liberties for the Patriot Act. It would be consistent with a motivated reasoning account if OL processors who received a survey frame that countered their pretreatment exposure rated the survey frame's consideration as significantly less important. For example, pretreated terrorism individuals who were exposed then to the civil liberties survey frame would rate civil liberties as significantly less relevant – in other words, they would strongly react against a counter argument (see Arceneaux and Johnson 2010).²³

[Insert Table 2 About Here]

In Table 2, we report the mean rating scores for the respective considerations (for both issues) for (1) OL processors who had been in the contrary pretreatment environment to the survey frame (e.g., OL processors who received the civil liberties pretreatment and the terrorism survey frame), (2) all other respondents, (3) the subset of respondents who were OL processors and received the contrary pretreatment environment but not the incongruent survey frame (e.g., OL processors who received the civil liberties pretreatment and either no survey frame or the civil liberties survey frame).

The table reveals that in every case OL processors who received a survey frame that contradicted their pretreatment environment rated the frame's consideration as significantly less important than did all other respondents or even other OL respondents from that same pretreatment environment but who had not received the contrary survey frame.²⁴ These individuals presumably received a frame that contradicted what had become their strongly held attitude (driven by the pretreatment environment) and downgraded the relevance of that frame.

Discussion

²³ We recognize there is causal ambiguity in using belief importance scores as an indication of evaluations subsequent to the formation of one's overall opinion. A more typical usage is as a mediational variable between frame exposure and overall opinion (e.g., Nelson et al. 1997, Druckman and Nelson 2003; however, see Bullock and Ha n.d.).

²⁴ In every case, the OL processors in the contrary pretreatment environment registered the lowest importance score relative to each other condition.

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 frames). Methodologically, the results make clear that what happens prior to the experiment can lead to misinterpretations of experimental results (e.g., understate the effects on some groups and over-state them on others, or in other circumstances, conclude there are no effects when in fact there were earlier effects).

We recognize that our treatments were strong. We partially address these concerns in our next study where we attempt to measure, rather than manipulate, the pretreatment environment and processing modes. The goal is to provide a blueprint of how scholars might go about identifying pretreatment effects and their consequences when the prior environment is beyond their control.

Study 2²⁵

We focus again on opposition or support for a state funded gambling casino. This time, however, we explore attitudes towards an actual proposal in the context of 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 next discuss, we expected the pretreatment context to influence responses in the Election Day survey experiment.

Pre-Treatment Context

The 2006 Illinois Gubernatorial election pitted Democrat Incumbent Rod Blagojevich against Republican Judy Topinka. The campaign's initial focus concerned the declining state economy, and the

²⁵ Parts of the study description come from Druckman (2010), which reports distinct data from the same survey.

candidates' plans to raise revenue (e.g., Reutter 2006, Rasmussen Report 2006). Topinka's economic plan – as enunciated on August 23rd – revolved around a proposal to create a land-based state owned Chicago casino with profit to be used for state spending on education and property tax relief. In contrast, Blagojevich proposed leasing the state lottery to generate revenue. Topinka's casino idea split the public – a mid-September *Chicago Tribune* polled 54% in opposition to the casino plan – and cut across partisan lines.²⁶ Indeed, while Blagojevich opposed the proposal, he had just a year earlier proposed to double gambling positions, and Chicago Democratic Mayor Daley was open to the plan.²⁷

A content analysis of *Chicago Tribune* coverage of the campaign (from the date of the casino proposal until Election Day)²⁸ 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 also 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 9th). With little forewarning, a rash of corruption allegations were launched including accusations that Blagojevich traded state jobs for personal payoffs and improperly spent state money. Topinka also received scrutiny for 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.

²⁶ 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 (e.g., Pearson 2006).

²⁷ Interestingly, after being re-elected, Blagojevich expressed support for a casino plan (Meitrodt and Garcia 2007).

²⁸ This covers August 24th through November 6. Details on the content analysis are available from the authors.

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 twenty-four 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 and not just those who have the time and interest to complete a lengthy survey (see Traugott and Lavrakas 2008).

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. 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).²⁹ 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

²⁹ As in study 1, we pre-tested the frames, and found both were strong and directionally distinct. Details are provided in Druckman (2010).

casino would provide tax relief and help to fund education...” (N = 55). (See note 30 for an explanation of why the Ns vary across conditions.)³⁰

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., Sniderman and Theriault 2004, Chong and Druckman 2007b, Hansen 2007, Druckman et al. 2010). Evidence of a pretreatment effect would manifest, given the early campaign focus 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 non-effects to occur only among those who were exposed and attentive to the campaign and engaged in processing that increased attitude strength.* 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

³⁰ The experiment contained various other randomly assigned conditions that are not relevant to our focus. This included various mixes of “weak” or non-persuasive frames including a morality, 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 non-effect of the corruption frame. We do not combine conditions with the other weak frames (i.e., morality, entertainment) because the evidence of their non-effects 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).

³¹ This measure 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

need-to-evaluate 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 *both* the newspaper variable and the need-to-evaluate (NE) variable; we refer to these voters as “Attentive / High NEs” (N = 111), and others as “Non-Attentive / Low NEs” (N = 213).³³

Results

We present our results by exploring mean support scores across conditions for (1) everyone (“All”), (2) Non-Attentive / 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 frame exposure.

[Figure 9 About Here]

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 ($t_{224} = 4.47$;

reliable sense of how often they read the paper (as it typically reflects an habitual behavior). Second, while those exposed may differ systematically from those not exposed, these differences stem largely from socio-demographic 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).

³² We used only one item due to the requirement that the exit poll be short. The item we used has appeared on the National Election Study (NES) since 1998 (see Bizer et al. 2004).

³³ Some respondents did not answer the need-to-evaluate or newspaper reading question and thus the N is a bit smaller than that reported for “All” respondents. 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., Kinder and Sanders 1990, Krosnick and Brannon 1993, Nelson et al. 1997, Miller and Krosnick 2000: 305, Druckman and Nelson 2003, McGraw and Dolan 2007: 311-312, Ansolabehere et al. 2008: 224-225).

$p \leq .01$), 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 reveal even more dramatic experimental framing effects among Non-Attentive / 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 – display 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. 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 Non-attentive / Low NE respondents), the Attentive / High NE respondents registered a significantly higher support score across conditions ($t_{216} = 3.08$; $p \leq .01$).³⁴ As in study 1, we also found suggestive evidence of motivated reasoning – we again asked respondents to rate the importance of distinct considerations including social costs when constructing their casino opinions. We find that the Attentive / High NE respondents who received the experimental social costs frame reported a significantly lower score for the importance of social costs (2.53; 1.45, 15), than did the Non-Attentive / Low NE respondents (3.47; 1.41; 38; $t_{51} = 2.17$; $p \leq .05$) or the Attentive / High NE individuals who received No frame or the economics frame (3.09; 1.15, 74 $t_{87} = 1.65$; $p \leq .05$).

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

³⁴ The Attentive / High NEs registered an average of 3.26 (2.07; 74) compared to 2.46 (1.66; 1.44) for Non-Attentive / Low NEs.

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.

Conclusion

Experiments have emerged as a central methodology in political science (e.g., Druckman et al. 2006). Perhaps no other field has benefited as much as work in political communication. A nagging, long-standing concern about some of this work concerns the implications of relying largely on experiments (e.g., Hovland 1959). As Kinder (2007: 159-160) 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.” He (2007: 157) continues “Enough already with the experiments...” We took Kinder’s advice to heart by exploring how events outside and previous to the prototypical experimental setting affects participants and, consequently, inferences that can be drawn from common experiments.

Our findings suggest that by ignoring the pretreatment context, extant research has missed a number of intriguing dynamics, that if nothing else demand further exploration. These dynamics are as follows.

- The existence of an experimental effect can be misleading as it:
 - may reflect an effect present only among a subgroup that formed weak attitudes (e.g., MB processors) on the issue (see Gaines et al. 2007: 16, Barabas and Jerit 2010), and
 - may understate the size of this effect among those individuals.
- The non-existence of an experimental effect can be misleading as it may stem from a large number of individuals forming strong attitudes (e.g., OL processors) in response to earlier communications, prior to the experiment.

- 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.³⁵

Consequently, when we find 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 non-manipulated individuals, but these effects failed to endure (also see Chong and Druckman 2010). In light of well documented publication biases towards significant results (e.g., Gerber and Malhotra 2008, Gerber et al. 2010), it follows that the picture emerging from published survey framing effect studies over-state 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 non-effects 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 survey 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. In short, 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 bi-modal with some being malleably reactive and others being dogmatically invulnerable to communication effects (see Chong and Druckman 2010).

We suggest this bi-modal possibility cautiously. Participants who resisted influence in the experiment were affected by earlier communications and thus were not so dogmatic as to reject any communication. Had they rejected all communications, our results in some sense would be much less

³⁵ For example, Hillygus and Jackman (2003) report Presidential conventions exerted a large impact than subsequent presidential debates, which could reflect pretreatment effects (in terms of the debate's non-effect).

interesting – they would merely reveal that longer term predispositions overwhelm short term communication effects (see, e.g., Malhotra and Margalit 2010). In our case, it is not deeply held prior opinions or values, but rather what amounted to brief messages that conditions subsequent response. This type of resistance to later messages is more troubling because the dogmatism stems from whatever messages happened to come first and not more considered long term influences. On the flip side, the reactive respondents exhibited great malleability to the latest message (in the experiment). Either way, the fairly arbitrary sequence of messages drives opinions (also see Chong and Druckman 2010).

Of course the presence of stability or malleability depends on the population and the issue (e.g., the strength with which individuals form attitudes on an issue). Variation across populations and issues may explain incongruent findings between micro and macro studies. The modal micro study that explores the stability of communication effects suggest the effects are fleeting (consistent with our argument that effects occur among those with weak attitudes) (see, e.g., Tewksbury et al. 2000, Druckman and Nelson 2003, Chong and Druckman 2010). These findings contrast with the general stability of aggregate public opinion on political issues. Wood and Vedlitz (2007: 553) observe that: “systemwide definitions of most issues remain relatively constant through time,” the implication being the prominence and understanding of such issues rarely changes (see Jones and Baumgartner 2005, Baumgartner et al. 2009: 175-178). This macro-micro divide reflects can be traced to systematic differences in the issues examined and the types of information processing engendered by these issues. Aggregate studies focus on public opinion on longstanding issues (e.g., Gallup’s most important problem surveys) that, by definition, have been salient for an extended duration. These issues, owing to their enduring salience, that they elicit more on-line processing of communications and thus stronger attitudes. Most of the micro-level studies that that report decay, in contrast, focus on attitudes toward relatively novel and specific issues that enjoy heightened but short term salience such as attitudes about a particular ballot proposition (Albertson and Lawrence 2009), a competitive election involving a new candidate (e.g., Gerber et al. 2007), or regulation of hog farms

(Tewksbury et al. 2000). Opinions are more likely to be weak or nonexistent on such questions and, therefore, more amenable to influence and short-lived.³⁶ In short:

- varying levels of stability in macro and micro-level studies of opinion may stem, in part, from differences in the issues explored and accompanying differences in how people process information about those issues.

We conclude by emphasizing that our findings should not be taken as an indictment of the experimental approach. 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 tests with distinct populations or at different times.³⁷ Time dynamics have non-trivial implications and accounting for them will lead to a more accurate understanding of how political communications shape opinions.

³⁶ Significant short-term effects have also been observed on more abstract and impersonal subjects, such as people's trust in institutions (e.g., de Vreese 2004, Mutz and Reeves 2005), that may be more likely to induce memory-based processing (see McGraw and Dolan 2007).

³⁷ 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.

Appendix A

Casino economic frame using the on-line 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. After you read and rate all of the paragraphs, we will ask you for your overall opinion about the casino (i.e., the extent to which you oppose or support the state-run casino). In subsequent surveys we will again ask you for your opinion about 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 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 a lot			neither decreases nor increases			increases

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 re-contact you in subsequent surveys when we will again ask you some questions about the state-run casino.

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*

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
<i>very</i>						<i>very</i>
<i>unimportant</i>						<i>important</i>

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
<i>very</i>						<i>very</i>
<i>unimportant</i>						<i>important</i>

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
<i>very</i>						<i>very</i>
<i>unimportant</i>						<i>important</i>

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
<i>very</i>						<i>very</i>
<i>unimportant</i>						<i>important</i>

Appendix C

The study 2 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" (see Donahue and Miller 2006). 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, and age.³⁸ 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 Non-Attentive / 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 significantly from one another in each case. Discussion of the control variables results can be found in Druckman (2010).

³⁸ Some respondents did not answer all of the control variables and thus the N is smaller in these analyses.

Study 1: Support For Casino Proposal

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

	<i>All</i>	<i>Non Att./ Low NE</i>	<i>Att./ High NE</i>
Social Costs Survey Frame	-.53*** (.19)	-.76*** (.23)	-.07 (.36)
Economics Survey Frame	.78*** (.15)	1.11*** (.19)	.30 (.27)
Social Costs-Economics Survey Frame	-.15 (.18)	-.40* (.25)	-.01 (.32)
Administration Corruption	-.40 (.33)	-.01 (.43)	-1.02** (.57)
Regulation Value	.05 (.28)	-.15 (.37)	.11 (.48)
Casino Visits	.37** (.21)	.30 (.26)	.80** (.39)
Distrust Government	-.58** (.31)	-.53* (.38)	-.87 (.73)
Vote for Topinka	.31* (.21)	.19 (.27)	.77** (.39)
Partisanship (Republican)	-.18 (.31)	.04 (.39)	-1.15** (.66)
Age	-.32* (.23)	-.74*** (.31)	-.10 (.40)
Minority	.37** (.17)	.26* (.19)	.57* (.39)
Female	-.14 (.13)	-.02 (.17)	-.49** (.23)
τ_1 through τ_6	See below	See below	See below
Log likelihood	-532.84	-319.54	-178.46
Number of Observations	305	195	101

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: -1.42 (.46), -.84 (.46), -.40 (.46), .02 (.46), .40 (.46), 1.02 (.46); for Non Att. / Low NE: -1.34 (.61), -.71 (.60), -.13 (.60), .23 (.60), .61 (.60), 1.31 (.61); for Att. / High NE: -2.23 (.89), -1.71 (.89), -1.37 (.89), -.85 (.89), -.44 (.89), .17 (.88).

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Table 1: Study 1 Conditions

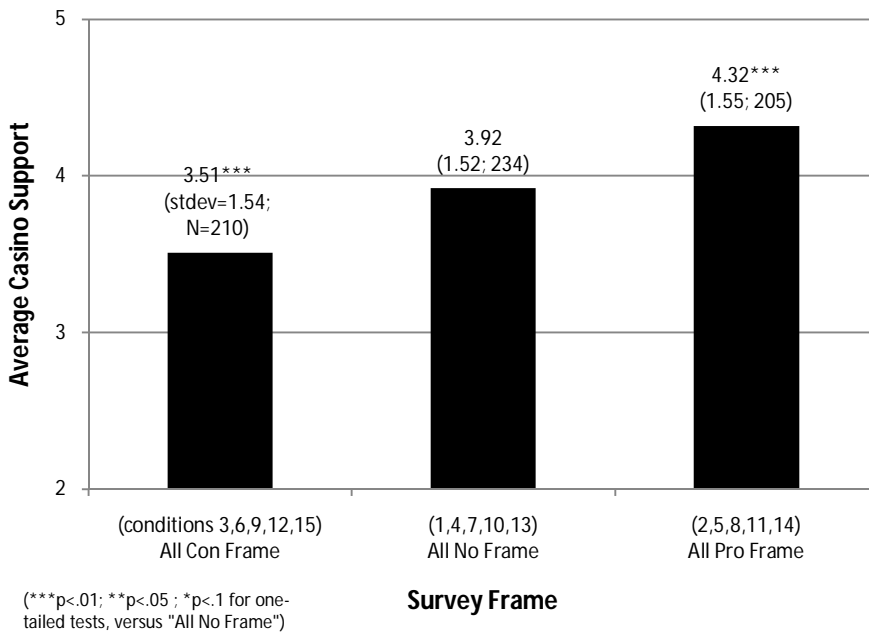
	No Survey Frame	Pro Survey Frame (Economic/Terrorism)	Con Survey Frame (Social Costs/Civil Liberties)
No Processing Manipulation			
No Pretreatment	(condition 1) N = 46	(2) N = 39	(3) N = 32
On-Line Processing Manipulation			
Pro Pretreatment (Economic/Terrorism)	(4) N = 61	(5) N = 36	(6) N = 37
Con 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

Table 2: Belief Importance

Survey Frame / Consideration	OL Processors in contrary pretreat environment and received survey frame	All Other Respondents	OL Processors in contrary pretreat environment but did not receive survey frame
Casino Con: Social Costs	3.51 (1.45; 37)	4.64*** (1.55; 615)	4.38*** (1.66; 98)
Casino Pro: Economic	3.38 (1.66; 39)	4.70*** (1.47; 612)	3.89** (1.61; 85)
Patriot Act Con: Civil Liberties	4.49 (1.71; 37)	5.55*** (1.46; 612)	4.94* (1.56; 98)
Patriot Act Con: Terrorism	3.97 (1.87; 39)	5.15*** (1.51; 609)	4.55** (1.76; 85)

*** $p \leq .01$; ** $p \leq .05$; * $p \leq .10$ for one-tailed tests, relative to OL Processors in contrary pretreat environment and received survey frame condition.

**Figure 1 : Casino Support By Survey Frame
(across pre-treatment and processing conditions)**



**Figure 2: Patriot Act Support By Survey Frame
(across pre-treatment and processing conditions)**

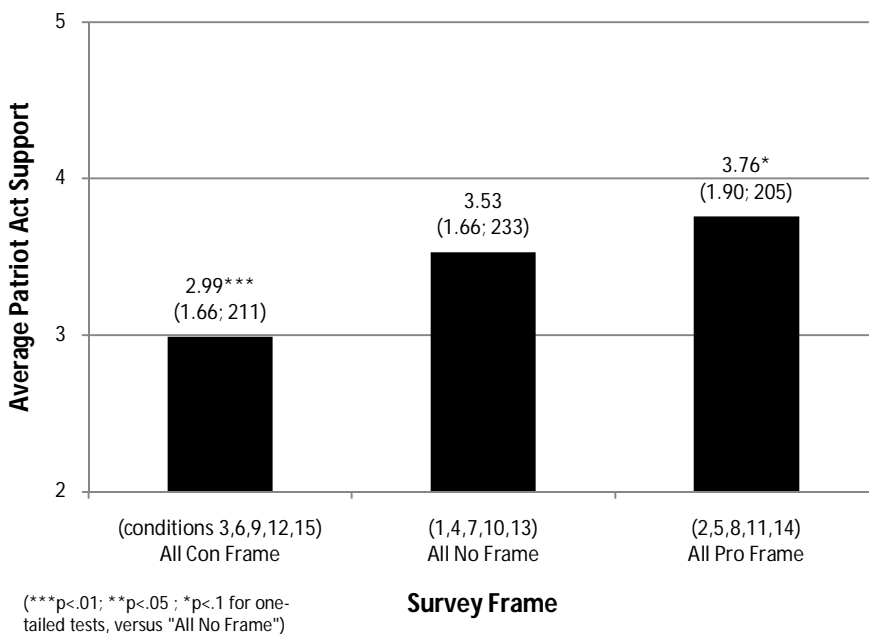
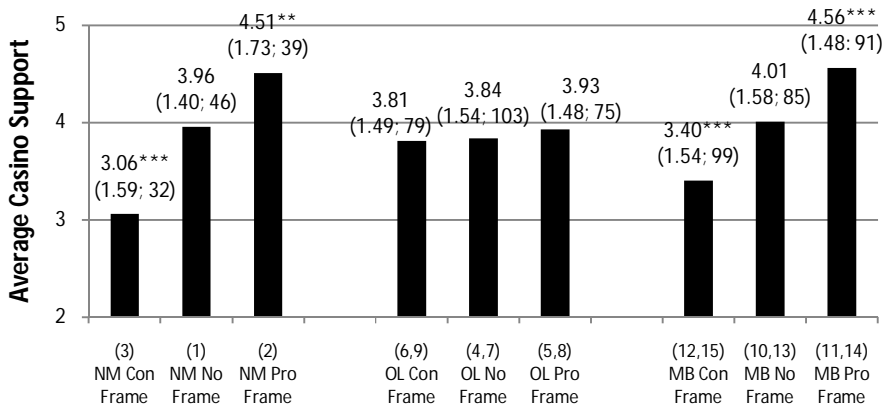
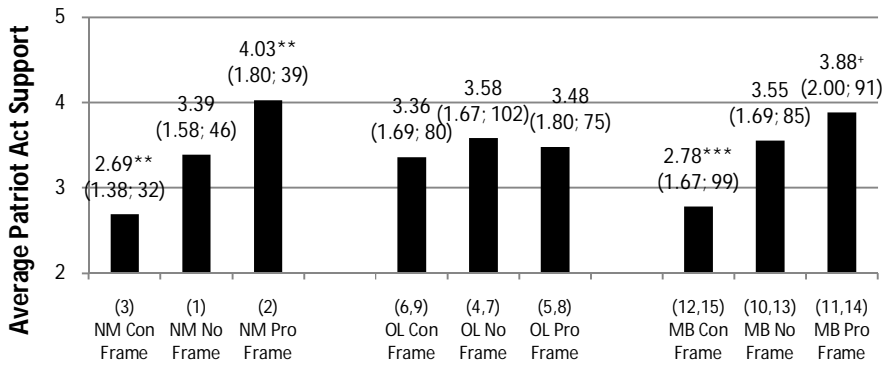


Figure 3: Casino Support By Processing Mode and Survey Frame (across pre-treatment conditions)



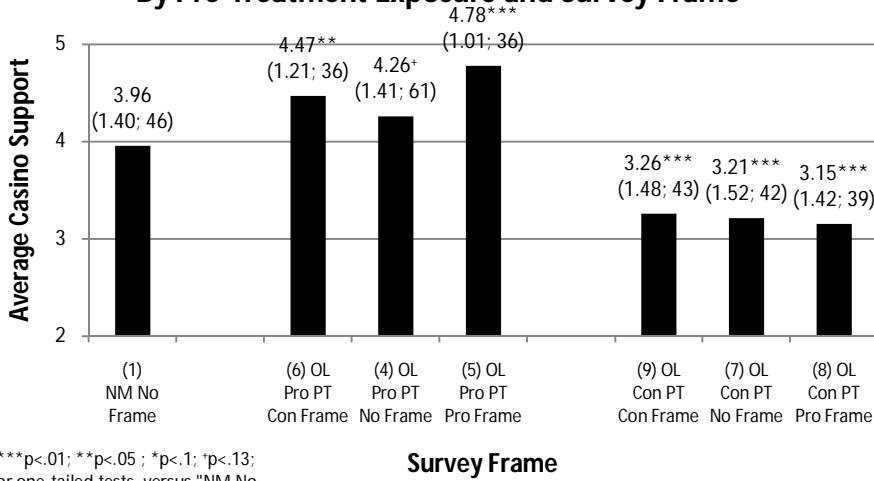
(***p<.01; **p<.05; *p<.1 for one-tailed tests, versus "No Frame" within each processing mode)

Figure 4: Patriot Act Support By Processing Mode and Survey Frame (across pre-treatment conditions)



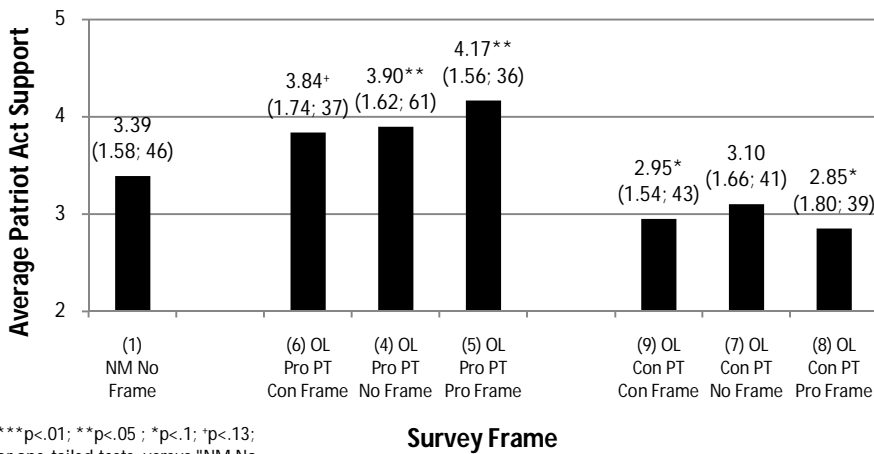
(***p<.01; **p<.05; *p<.1; +p<.13; for one-tailed tests, versus "No Frame" within each processing mode)

**Figure 5: Casino Support For OL Processors
By Pre-Treatment Exposure and Survey Frame**



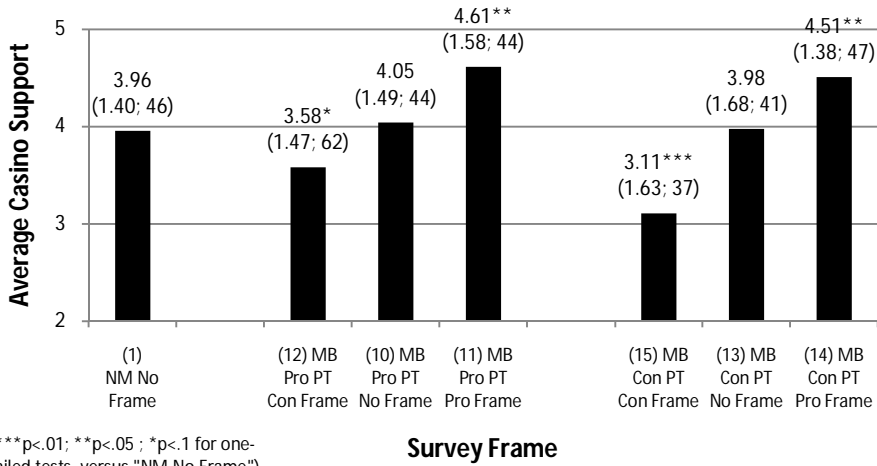
(***p<.01; **p<.05; *p<.1; +p<.13; for one-tailed tests, versus "NM No Frame")

**Figure 6: Patriot Act Support For OL Processors
By Pre-Treatment Exposure and Survey Frame**



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

**Figure 7: Casino Support For MB Processors
By Pre-Treatment Exposure and Survey Frame**



**Figure 8: Patriot Act Support For MB Processors
By Pre-Treatment Exposure and Survey Frame**

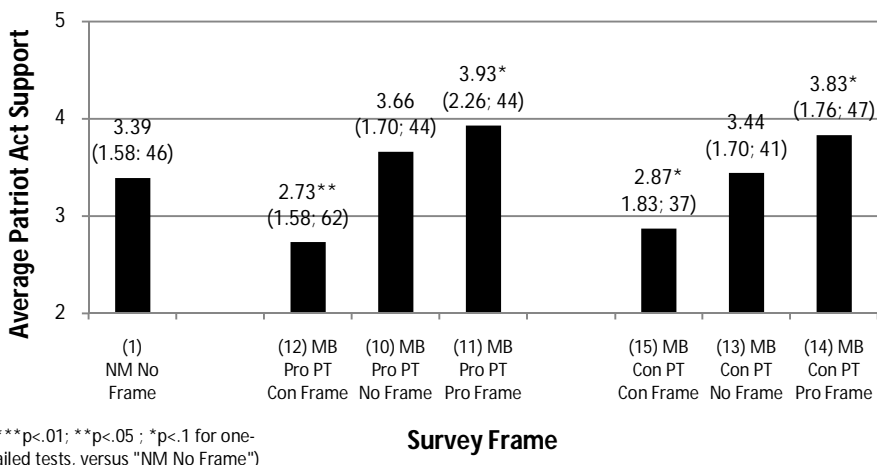
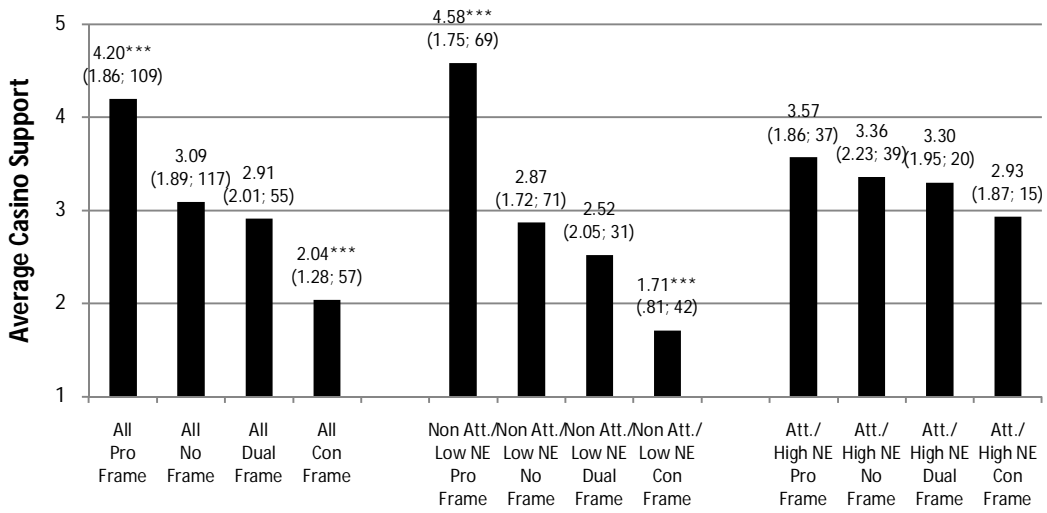


Figure 9: Exit Poll Casino Support By Conditon and Attention/NE



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

Group and Survey Frame