The Paradox of Confrontation: Experimental Evidence on the Audience Effects of Protests*

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Do protests increase political engagement among the general public? It is often necessary for social movements to induce widespread political engagement in order to gain leverage over elected officials, but this consequence of protest activity has never been tested or verified. Indeed, empirical research on the public effects of protests has largely been handicapped by methodological limitations. I designed a two-pronged experimental design that causally identifies the effects of protest exposure. The first stage uses a vignette experiment in Mexico to capture indirect exposure, and the second stage uses a field experiment to directly expose the same respondents to real street protests. All of the treatments for the vignette and field experiments piggyback off of the 2014-2015 protests against organized crime in Mexico. Through this two-pronged experiment, I find that the form of exposure is critical in identifying the engaging effects of protests. While the general public might become enthusiastic and engaged upon hearing news of mass mobilization, the same people tend to disengage when faced with an actual protest.

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1. Introduction

For many, political activism can be a frustrating enterprise. A movement may be dedicated and rich in resources, but it is effectively toothless if it fails to move an ambivalent and fickle general public. What leverage does a protest have against elected officials if it fails to trigger a reaction from outside its own group? Indeed, many protest movements fizzle away without ever engaging the wider public, and it is not surprising when these movements do not receive concessions from political leaders. For better or worse, protesters often rely on the public audience, whose support can apply formidable pressure on elected officials and whose debates can shift the political agenda.

The public can be an ally or an obstacle for protesters, yet its role as an audience has largely been ignored in academic research (Giugni 1998). This may be due, in part, to the difficulty of causally identifying these effects. Most protest data suffer from selection bias, including only the protests that successfully capture the public's attention. There are also concerns of endogeneity; it is not clear if public engagement is caused by the protests or the political environment that generated the protest. Finally, by relying on news reports of protests for data, extant research fails to capture the confrontation and drama that is so inherent to real protests.

I have developed two-pronged experimental design to overcome these limitations and causally identify the audience effects of protests. This design differentiates between the two main ways that audiences are exposed to protest: indirect exposure through the news and direct exposure in the streets. First, a vignette experiment conducted in Mexico City treats individuals with news stories about protests with randomized characteristics. Second, a field experiment examines the same respondents as they are personally confronted with real protests. All of the treatments for the vignette and field experiments piggyback off of the 2014-2015 protests against organized crime in Mexico.

The two experiments yield starkly different results. I find that protests *can* engage the general public, but only when they are exposed indirectly. Exposure to a news story about protests increases the individual's willingness to talk about a political issue, sign a petition, and vote. But protest is a double-edged sword. Exposure to a real

street demonstration *decreases* the individual's willingness to talk about a political issue, protest, and vote. Ultimately, while the general public might become enthusiastic and engaged upon hearing news of mass mobilization, the same people tend to disengage when faced with an actual protest.

This paper contributes to the literature on protests in a number of ways. Substantively, these findings represent a trade-off for protesters, who must confront a wide audience to attract attention at the expense of alienating that very audience. In the discussion section, I suggest a number of practical ways that activists could balance the techniques and geography of protest to minimize the negative effects of confrontation. Methodologically, these findings serve as a cautionary tale for political science research. A great deal of recent work in political science attempts to reduce complex political phenomena to clean and easily manipulated experimental treatments, often at the expense of causal complexity (Barabas and Jerit 2010, Franco et al. 2015). However, the contrasting findings for the two experiments demonstrate the importance of a diverse set of treatments and methods that seek to replicate real-life stimuli and make generalizable claims.

2. Protests and the Public

Thus, at face value, the necessarily public nature of protest activity suggests that it would lend itself easily to public response, but literature rarely seeks to examine the effects that protests can have on the public. In the first study to empirically assess the capacity of protest to shape public opinion, Berkowitz (1973) collects data on large, anti-war protests and compares trends of protest activity with public opinion between 1965 and 1971. While most of the public's attitudes are not significantly influenced by protests, he finds that the presence of anti-war protests *increased* the president's approval ratings and popularity. These results are troubling for Berkowitz, who concludes his article by asking, "What will we do if we face up to the possibility that nothing works, or that nothing works well, that our society is too atomized, too well-insulated for any social movement or any scientifically imposed principle to turn it around?" (Berkowitz 1973, p.13). He then begs for additional research to resist the acceptance of null results

and to isolate when and how social movements can influence public opinion.

For the most part, Berkowitz's plea for further research on protests and public opinion went unanswered. Four decades later, recent research highlights the role that protests can play in shaping public opinion. Much of this scholarship capitalizes on a wave of immigrants' rights protests that overlapped with the data collection of the Latino National Survey in the United States. This work demonstrates that spatial proximity to protests shaped Latino Americans' feelings of empowerment and alienation (Wallace et al. 2014), issue saliency (Carey et al. 2014), group identity (Mohamed 2012), and policy preferences (Branton et al. 2015). Evidence beyond the immigration-based protests leads to similarly promising findings. Andrews et al. (2015) merge protest data with a representative survey conducted in 1961 to explain why a small subset of white Southerners supported integration, and they find that protest activity in a respondent's county helped to garner sympathy for the civil rights movement.

But what is the relationship between protests and political engagement? A small but productive literature examines the consequences that protests have on individual protesters, concluding that protests directly and indirectly mobilize individuals to participate in subsequent political activity. Evidence from panel studies in East and West Germany suggests that protest activity increases protests' political efficacy in addition to creating a robust direct effect on participation that persists for all model specifications (Finkel 1987). Sociological literature contributes to these findings by analyzing the duration of these consequences. Scholars follow the New Left activists for several decades, and they find that former activists continue to promote leftist attitudes (Marwell et al. 1987), self-identify as liberals (Fendrich and Tarleau 1973), and remain mobilized in social movements at consistently higher levels than non-activists (Fendrich and Lovoy 1988, McAdam 1989). In addition, scholars of protest diffusion have found substantial evidence for "cycles of contention," or periods in which contention of one movement in one location sparks chain reactions in which new social movements and protest groups emerge at the peak of a protest wave (Klandermans 1990, Tarrow and Tollefson 1994).

To be sure, activists have an incentive to spread political engagement outside of the

set of protest participants. The ability of protests to induce policy change often depends on public support. Many scholars have demonstrated that, without favorable public opinion, the civil right protests of the 1960s and 1970s would have had very little influence over policy (Burstein 1998, Santoro 2002, Soule and Olzak 2004). While numerous causal mechanisms might link protests to policy outcomes (Burstein and Linton 2002), most evidence supports the expectation that protests have a stronger effect when they operate in conjunction with public opinion (Agnone 2007).

Not unlike protesters, who experience psychological and emotional benefits from the protest experience (Finkel and Muller 1998, Jasper 1998, Yang 2000), those who are outside the group may also experience some participatory externalities from a protest. For example, social capital theorists contend that individuals learn how to engage in politics from a broader culture of participation (Putnam et al. 1994, Putnam 2001). Similarly, casual observation of political participation has been found to be a significant driver of individual participation (Cho and Rudolph 2008). Cho and Rudolph (2008) find that when individuals are casually exposed to their neighbor's campaign paraphernalia, discussions, or other overt political activity, they glean information about participatory norms in their community. Thus, individuals who are exposed to protests receive important information about the norms of political engagement in their environment. Even if the exposed individuals are largely ignorant or unaware of the protesters' claims, casual observation of the protest indicates that a political issue has attracted crowds of citizens to the streets.

3. Exposure as Confrontation

Demonstrating a causal link between protests and public engagement faces a significant challenge i.e., identifying exposure to protests. Of course, much of the public is not an audience at all. As non-participants of the protest, much of the general public may be unaware of - or deliberately avoid - protests. Even among the audience that is aware of the protest, there is a great deal of heterogeneity in how they experience the event. Some

of the audience may have encountered the protest through the media, others through word-of-mouth, and others by physical confrontation.

The notion that protest audiences differ by the form of exposure is non-trivial. It is well-documented in the literature that the medium by which individuals are exposed to something may be as important as the thing itself. A classic example is public's response to crime. Scholars have identified a steady increase in the public's fear of crime, despite the fact that crime rates have remained relatively constant. It is often argued that the underlying root of fear is the shift in media portrayals of crime, rather than crime itself (Gordon and Heath 1991, Jaehnig et al. 1981, Chiricos et al. 1997, Lowry et al. 2003). Studies that attempt to compare the effects of real-life crime and media portrayals of crime consistently find divergent effects (Weitzer and Kubrin 2004, Custers and Van den Bulck 2011). Because an individual's perception of crime varies so much between personal experience and the media, it is unsurprising that different forms of exposure lead to distinct outcomes.

I argue that reactions to protests also vary as a function of exposure. Specifically, I argue that personal exposure to protest events differs from exposure through the media. Charles Tilly and Sidney Tarrow put it aptly when they describe protests as "performances" (2015). They constitute a form of political participation that wants to be seen and experienced by an audience (Benford and Hunt 1992, McAdam 1996). The theatricality at the heart of protest activity arises from the necessity of attracting attention. Through traffic jams, loud noises, and congregations in important public spaces, they seek to create their own publicity and attract the most attention as wide an audience as possible. Media reports may be able to describe the protest, but the drama and disruption of the full protest experience is necessarily absent from such reports.

To date, scholars have made few efforts to disentangle real protest events from media reports. *The Collective Action Observation Primer* (McPhail et al. 1997) is a notable exception. This handbook describes a methodology to send trained observers to record what actually happens during various protest events and then compare those notes with subsequent media coverage. The principal finding from their recorded observations is

that collective action only very seldom appears uniform or organized (McPhail and Schweingruber 1998). The protests they attended looked more like loosely connected congregations of small groups than like homogenous demand-making bodies. While this disarray might be all-too apparent to an onlooker, it is generally absent from protest accounts in the media. The authors found that media reports of the same events largely failed to convey any of these complexities, focusing instead on the small fraction of behaviors that were most organized and collective. Although the protest event is the same and the message is the same, the audience of the news report will come away with a very different experience than that of the audience in the street.

With this in mind, how might the audience's reaction to a real protest differ from that of a protest reported in the media? In the only chapter to date on protest bystanders, Gamson offers a possible answer. He supposes it unlikely that the public will make an effort to understand and appreciate the unruly and disruptive protest. "For [bystanders of the protest]," he intimates that "the issues being debated by the contestants in the arena are unimportant relative to the collateral damage and inconvenience they produce" (2004, p. 244). This suggestion challenges the vast majority of social movement literature, which is highly aware of the protesters' need to occupy and interrupt public life in order to capture an audience (Benford and Hunt 1992, McAdam 1996, McCarthy and McPhail 2006), but widely assumes that the public does not mind the interruption. It is possible that the public begrudges its role as a captive audience and focuses more on the inconvenience of the event than the message.

This hypothesis has never been empirically tested, but pieces of evidence appear between-the-lines of journalists' protest reports. Although interviews with protest bystanders are fairly rare, examples from the US (Newcomb 2011), Egypt (Afify and Audiaug 2011), Russia (Mackey and Roth 2013), Hong Kong (Branigan 2014), and Mexico City (Proal 2015) tend to describe the non-participating public as confused, annoyed, frustrated. Certainly, these bystanders do not react as if they desperately crave to join the picket-line or research the protesters' claims. On the contrary, when they are confronted with the protests' disruptive or confusing tactics, these bystander audiences fail

to notice the protest message at all. Rather than observing a broad political struggle, they appear only see a personal inconvenience, a traffic jam, or a scattered crowd. Although anecdotal, these reactions are indicative of an important, though mostly unspoken, public reaction to protest.

In this context, it is likely that the public's response to protest depends on how they are exposed to the event. The media can separate the message of the protest from the experience of the event. The audience is pointed directly to the protesters' goals and targets without experiencing the crowd and the distractions. Meanwhile, for those who are exposed to the protest in the streets, delivery of the protest message depends on the protest experience, which often does not reduce to the well-defined objectives and clear narratives identified in news reports.

4. An Experimental Design for Protest Exposure

The empirical designs traditionally used to examine public consequences of protest have struggled to identify and verify protest exposure. From Berkowitz in the 1970s to the Latino public opinion research of 2014, the methodological crux of all these designs depend on the merging of media-based protest even data into large, scientifically sampled surveys. This approach represents a creative and innovative way to study the effects of protest on public opinion, but it does come with its own shortcomings. First, individual exposure to the protest is not verified. Exposure to protest is operationalized as living with a specifically-assigned radius of a reported protest. In a traditional experiment, one might say that only the *Intent-to-Treat* - rather than the actual treatment - can be verified. In a fully experimental setting, analysis of the ITT would be justified by random assignment, but in a quasi-experimental setting such as this, the assumption of random assignment does not hold. Indeed, the cities in which protests take place are almost certainly different from those without protests, and those individuals who are exposed to the protest are almost certainly different from those who are not exposed. In this case, the ITT is heavily confounded by variables at the individual-level and the

higher geographic level.

Additional shortcomings stem from the media-based data on protests. The assumption that protest report data creates a comprehensive or representative sample of all protests has been taken to task on multiple occasions, and many critics argue that these data suffer from selection bias (Franzosi 1987, Rucht and Neidhardt 1999, Oliver and Maney 2000, Earl et al. 2004). More pertinent to my research question, these data fail to distinguish between different types and degrees of exposure. The scholars who depend on media reports of protest have no idea whether or not, to what degree, and by what medium individuals may have been treated. Indeed, the heterogeneity of exposure may potentially determine its effects, so it is important to incorporate them into the research design.

In order to explain the effects of protests on public opinion while addressing the methodological challenges of previous research, I have designed a two-stage experiment. In the first stage, individuals in five centrally-located *colonias* in Mexico City [1] are sampled and contacted for a phone survey, which includes an embedded vignette experiment on protests. For the second stage, all respondents for the phone survey are invited to attend a follow-up face-to-face interview. During that interview, respondents are exposed (or, in the case of the control group, not exposed) to a real protest as they answer a battery of items on political engagement. The different stages of the experiment expose the same respondents to two types of protest treatments - a vignette treatment that represents media exposure to protests and a field treatment that represents a real protest. The experimental design aims to measure both types of exposure media and confrontational with the expectation that the effects would diverge.

The vignette and field treatments were based on real anti-violence protests in that took place in Mexico City in 2014-2015. In September 2014, these protests began in response to the disappearance and alleged mass murder of 43 leftist student teachers from the Ayotzinapa teachers' college in the southern town, Iguala, in the state of Guerrero. Although there was no central social movement to organize events after the attack,

[1] Azcapotzalco, Gustavo Madero, Benito Juarez, Cuauhtemoc, Miguel Hidalgo, and Venustiano Carranza

a series of protests took the nation by storm as hundreds of thousands of participants mobilized to demand government accountability, improved democratic processes, justice for the victims' families, and a general break from the corrupt status quo in Mexican politics. Although there are countless protests to choose from in Mexico City, these protests are ideal for this study because they were 1) expected to last for the duration of the study, 2) highly diverse in terms of protest techniques and strategies, 3) not overtly partisan, and 4) substantively important for Mexico and other countries suffering from organized violence.[2]

4.1. Political Engagement

Political engagement, as a multifaceted concept, is operationalized through six items that range from deliberative engagement to active participation, and they include institutionalized forms of action such as voting alongside contentious forms of action like protest. I operationalize post-treatment engagement through a battery where the respondent is asked, "On a scale of 1 to 4 (where 1 means very likely and 4 means not at all likely), how likely are you to do the following things in the next two weeks?"The activities are: 1) *Talk* with friends and/or family about organized crime, 2) *Read* about organized crime in the news, 3) sign a *Petition* demanding that the government take action against organized crime, 4) *Contact* a politician to address the topic of organized crime, 5) participate in a *Protest* against organized crime, and 6) *Vote* for a candidate in the June 7th elections because of his position on organized crime.

The field experiment also provided an opportunity to evaluate observable behavior, rather than self-reported willingness to engage in politics. At the end of the face-to-face interview, the enumerators asked respondents if they would like to sign a petition against

[2]More information on case selection can be found in Appendix A.

organized crime.[3] and/or take home a pamphlet of information about the protests.[4] With the addition of these items, I removed the *Petition* item from the self-reported engagement indicators in the field instrument to avoid repetition.

4.2. Stage 1: The Vignette Experiment

Through a Random-Digit-Dialing process, respondents in Mexico City were contacted to participate in a phone survey on political participation in Mexico.[5] The phone survey was conducted in two separate samples; the first sample was collected in late April (n=606) and the second in early May (n=600).[6] The entire phone survey was timed to last for 15 minutes, and it included items to measure basic demographics, political engagement and preferences, exposure to protests, a randomized vignette item, post-treatment engagement indicators, and contact information for the follow-up component.

Near the end of the survey instrument, the respondents received a vignette of a fictional, though plausible, news story.[7] Four out of every five respondents heard a story about a protest against organized violence in Mexico. The vignette followed a fully factorial design with two factors (size and level of violence) with two levels for each ([thousands of participants / a small group] and [march with little candles, ended peacefully / angry march with torches, ended in a violent confrontation with police], and resulted in four possible combinations.[8] Given the multitude of protests that occur in Mexico City on a regular basis, it would not have been surprising that the protest may have

[3]The team from BGC Beltrán helped to write the petition to elicit signatures from a wide-range of respondents. In order to avoid pushing away respondents, the petition described very moderate demands such as justice and accountability, and it deliberately avoided the inflammatory language of some protest groups and politically divisive demands (such as the presidents resignation from office).

[4]Respondents who responded affirmatively received pamphlets given to us by the Plantón por Ayotzinapa

[5]All data were collected by the Mexico City-based survey firm, BGC Beltrán y Asocs.

[6]Appendix B presents descriptive statistics on each of the samples and shows that they are not significantly or substantively different from one another.

[7]It was a relatively long vignette for a phone survey. To help respondents keep their attention, we timed it to last no longer than one minute, and we pre-tested the vignette several times until respondents confirmed that it did not feel long. We also introduced the vignette by telling respondents that they would have to answer some questions on what they heard so they might pay closer attention to contact.

[8]See Appendix C for the English versions of the vignettes.

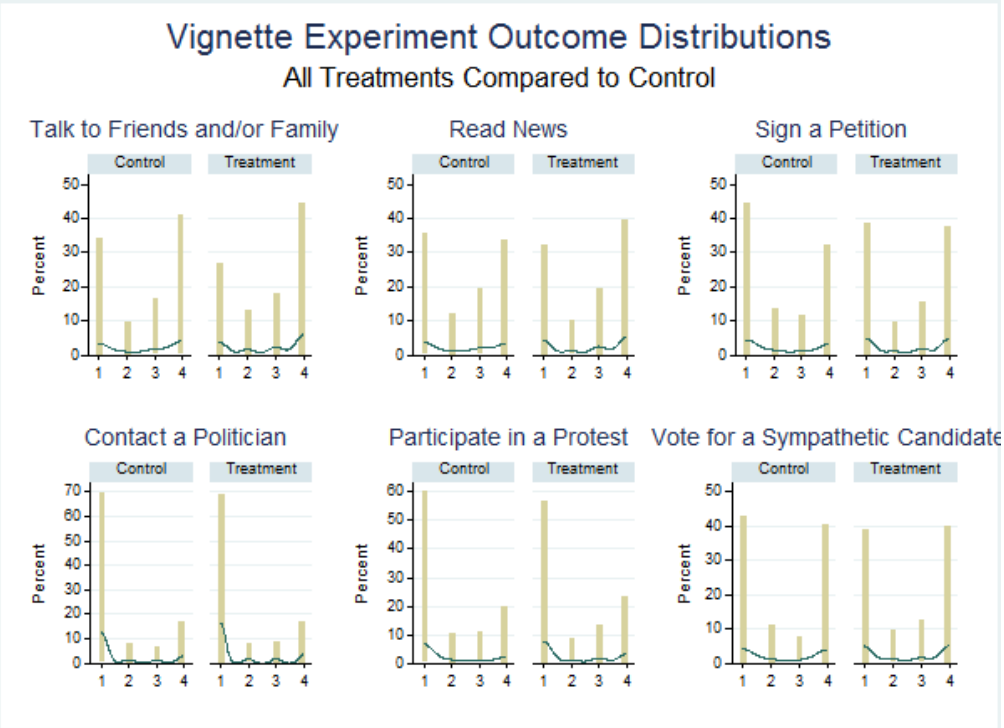
actually occurred. Furthermore, the enormous diversity of marches following the events Ayotzinapa made any of the four versions of the protest (small/violent, small/peaceful/ large/violent, and large/peaceful) very realistic and credible.

One in every five respondents received a control version of the vignette. The control version was designed to contain the same basic information without including any mention of a protest. To make this possible, the control vignette discussed the results of a recently published survey that contained the general messages as the protest. The survey mentions that a majority of citizens are tired with organized crime in Mexico since the events in Ayotzinapa, but there is no consensus regarding the culpability of the crimes or what should be done about it. This vignette controls for the possibility that the respondent changes their opinions by mechanisms that are not explicitly related to the protest - such as following the crowd or observing political discord.

Figure 1 shows the distributions of the engagement indicators for the control group and treatment groups in the post-treatment outcomes. For parsimony, I have combined all of the treatment groups so that the difference between the treatment and control represents exposure to any vignette with a protest. The distributions reveal interesting information about the participatory patterns of the respondents. Clearly, of all the indicators, respondents were least likely to contact a politician. Respondents were nearly as unlikely to participate in a protest. For all other engagement indicators, the responses appear more or less bimodal, with clear stacking at both extremes.

For many of the engagement indicators, there are observable differences between the control and treatment groups. Relative to the control group, the treatment groups demonstrate greater willingness to participate in nearly all of the participatory acts, with the least noticeable differences present for contacting a politician. For nearly all acts, there is a decline in the number of respondents who report being very unlikely to participate (indicated by a score of 1) when respondents are primed with a protest vignette. The protest-treated respondents have greater representation on the higher end of the scale, with higher frequencies at scores of 3 and 4.

Figure 1:



4.3. Stage 2: The Field Experiment

At the end of the phone survey, respondents were notified that they could receive compensation for completing a follow-up, face-to-face interview at a specific time, date, and location.[9] Reminders of the times and locations of the follow-up interviews were sent to each respondent who provided contact details. The date and time block designated to each respondent determined whether or not they would be exposed to a protest during their follow-up interview.[10] Respondents were invited to one of three follow-up interview dates: a small protest (estimated 300 participants), a large protest (estimated 5,000 participants), and a control with no protest.[11] All respondents contacted in April for the phone survey were invited to the small protest, and all respondents in May were invited

[9]Respondents received a gift card to *Liverpool*, an upscale department store, valued at $500 MX (approximately $40 USD).

[10]Respondents were not told that there would be a protest upon their arrival. All respondents in the treatment and control groups were told that they should leave plenty of time for traffic, given that there are often protests and events downtown.

[11]See Appendix D for more information about the protests used as treatments and the control condition.

to the large protest. For the control event, respondents who did not attend either event were re-contacted and re-invited. After respondents were exposed (or, in the case of the control group, not exposed) to the treatment, they responded to an abridged version of the phone survey instrument.

Protests are always, to some extent, unpredictable, and running an experiment under necessarily unpredictable circumstances made pure randomization logistically impossible. In large part, I designed the experiment to accommodate the imperfect randomization. All comparisons were drawn among *compliers* in the control and treatment groups. The pre-treatment questionnaires asked that all respondents to go through the same process of being interviewed and re-interviewed. The control and treatment groups are pooled from exactly the same set of respondents who express interest in participating in a follow-up study, and they are asked to go to a directly comparable location as the treatment group. The only difference is the day in which they are asked to arrive, such that one group encounters a protest and the other does not.

This does not guarantee that respondents in all groups are equivalent. Furthermore, the type of person who complied with the invitation would almost certainly differ from those who did not comply. I accounted for imbalances and self-selection in the analysis stage. Table 1 presents a clear summary of the analytical techniques used to account for non-randomness.

Table 1: Quasi-Experimental Design and Analysis

Stage	**Groups**	**Non-Randomness**	**Analysis Stage**
Treatment Assignment	Small Protest Large Protest Control	Random assignment between small and large protests; control respondents re-invited	Inverse probability weighting balancing treatment and control groups
Treatment Compliance	Treatment Compliers Treatment Non-Compliers Control Compliers Control Non-Compliers	All respondents were invited, only few self-select into the sample	Calculate treatment effects with selection model, correcting for the differences between attendees and non-attendees

Out of all 1,200 invited subjects, 89 attended a follow-up interview event.[12] The small

[12]For more information on these compliers - how many respondents provided contact information and what percentage of those actually attended - please see Appendix E.

protest event resulted in 16 respondents, 54 in the large protest event, and 19 respondents in the control event. Of all of these attendees, only one respondent was unable to be identified with a match in the phone survey. This respondent, who attended the large protest, was removed from the field data, resulting in a total of 88 compliers. It is hard to know exactly why the large protest event resulted in much higher turnout than the other events, but the likely explanation is the timing. The first protest took place on a weekend, and the second took place on a weekday, when many respondents would have already been downtown.

Figure 2 presents the distributions of the engagement indicators for the control group and treatment groups in the post-treatment outcomes. As with the vignette experiment, I have combined all of the treatment groups so that the difference between the treatment and control is exposure to either of the protest treatments. Relative to the pre-treatment engagement measures and the phone survey control group, very few respondents in the face-to-face control group are willing to tell the enumerator that they are not at all likely to participate in the political activities. It is likely that these differences resulted from social desirability bias, which would likely be higher in the face-to-face interviews than over the phone. Thus, mode differences make it imprudent to compare the control group of the vignette experiment to the control of the field experiment.

Direct comparisons between the face-to-face treatment and control groups are very revealing. Relative to the subtle differences in the vignette experiment, the differences between the response distributions in the field experiment are visually striking. On every engagement indicator, the treatment group responses are stacked farther to the extremes than the control group. Values of 2 and 3 are quite rare in the treatment groups. The highest proportion for a value of 2 or 3 appears in the *Vote* indicator, where 16% of the treatment group stated a value of 2. On the other hand, very high proportions are stacked at values of 1 and 4. Approximately 50% of the treatment groups selected a value of 4 for *Read* and a value of 1 for *Protest*. In similar fashion as the phone survey, contacting a politician is a very unpopular activity, where 68% of the control group and 77% of the treatment group selected values of 1.

Figure 2:

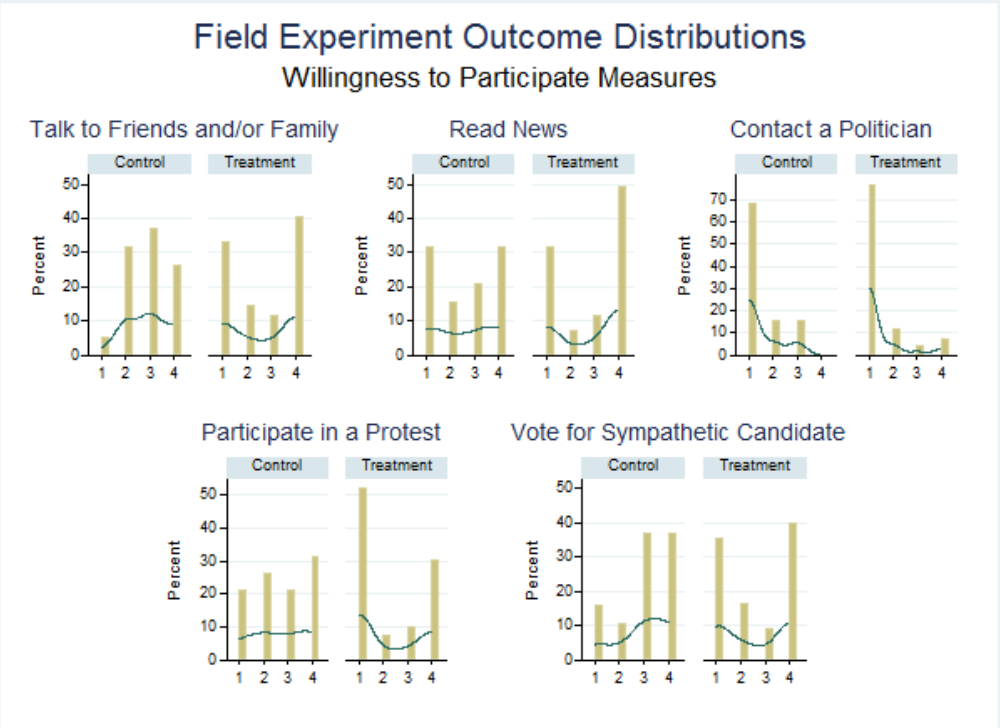
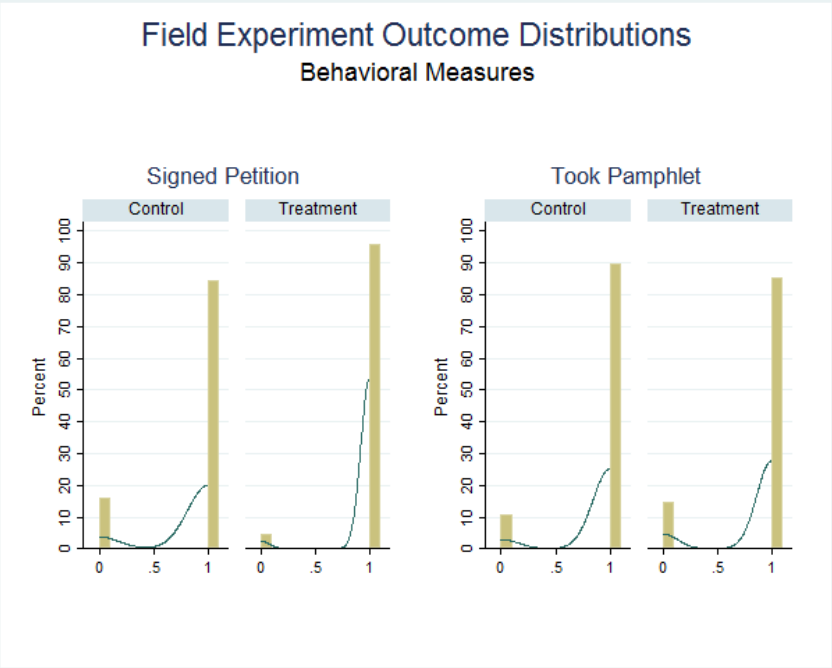


Figure 3 shows the results for the two behavioral indicators: signing the petition against organized violence and taking a pamphlet of information about the issue. For both the treatment and control groups, very high proportions signed the petition and took the pamphlets. For the pamphlet indicator, the distributions are nearly identical with approximately 90% of each group accepting the offered informational pamphlet. The petition indicator shows some differences between groups; 10% more respondents in the treatment group signed the petition than the control group.

Figure 3:



5. Analysis and Results

5.1. Vignette Experiment: Estimation

First, I will discuss the estimation and results of the vignette experiment. Although the dependent variables were measured using Likert response scales, I modeled them as binary outcomes, where 0 represents a score of 1 or 2 for willingness to participate in each activity and, and 1 indicates score of 3 or 4. These transformations are more consistent with the response distributions, which tended to stack heavily on the lowest and highest extremes of the scale. Logit models also ease the interpretation of the results relative to ordered models. All results are displayed in terms of marginal effects so that the magnitude of the effect can be interpreted directly. The statistical models include indicators for each version of the treatment while assigning the control version as the reference group. Thus, the marginal effect of each treatment represents the effect of receiving a specific protest condition relative to the control vignette.

These models include a number of control variables. In addition to standard demographic items, the models include political and protest-related variables measured in the phone survey instrument. More information on these variables (including question-wording, response options, and summary statistics) can be found in Appendix B.

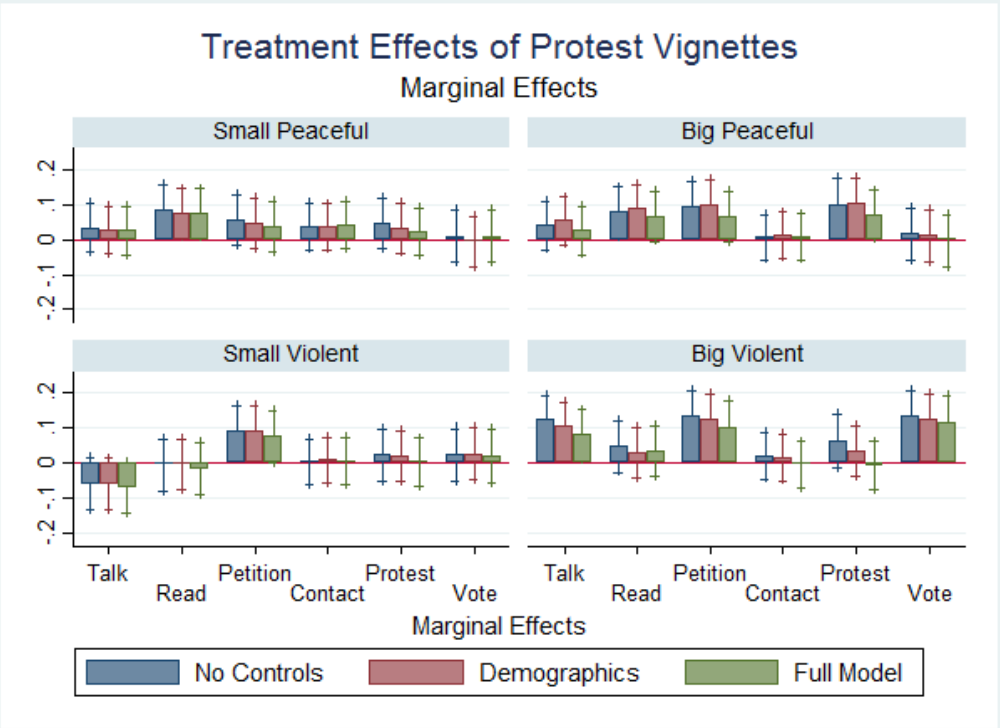
5.2. Vignette Experiment: Results

Figure 1 illustrates the treatment effects of each vignette for different model specifications. The blue bars represent the model with no controls, the pink bars include only demographic variables (sex, age, SES, education, and traditional partisanship), and the green bars include the entire list of covariates. Full output can be found in Appendix F. A comparison of all models in Figure 1 suggests that the results are fairly robust to the inclusion of covariates, although the marginal effects of the treatments tend to decrease as more controls are added. Statistical significance does not vary for most models, with the clear exception of the *Big/Peaceful* treatment. This vignette results in positive increases in *Read*, *Petition*, and *Protest* for the reduced model and the demographic model, but these effects are insignificant when all of the covariates are included.

The fully-specified models show that hearing about a protest - as one would on the radio can make individuals more likely to engage in political issues. Upon hearing about a recent protest against organized crime, respondents were significantly more likely to consider talking about, signing a petition against, or voting base on organized crime. Many effects are only significant for the large protest treatments, suggesting that respondents who hear news of small protests are less likely to engage. The effects are strongest and most consistent for the *Big/Violent* protest treatment. When briefly primed with a news story about such a protest, respondents are 8.7% more likely to talk about the issue with their friends or family, 10% more likely to sign a petition about organized crime, and 12% more likely to vote for a sympathetic candidate.

There are also significant effects for the *Small/Violent* vignette, though these effects

Figure 4:



are only significant at the 90% level. As is shown in Figure 1, this treatment has a negative effect on the respondent's willingness to talk about organized crime and a positive effect on petition signing. The magnitude of these effects are relatively strong; receiving this treatment makes the respondent 8% less likely to talk and 8% more likely to sign a petition.

The differences between the null models and the fully controlled models suggest that some treatment effects may be conditional on pre-treatment covariates. In particular, it is plausible that the effect of the vignette treatments would diverge between those who support or oppose the protest. A full discussion and analysis of heterogeneous effects can be found in Appendix G, but the analysis generally indicated that protest support can be a significant moderator for the vignette experiment.

5.3. Field Experiment: Estimation

The causal effect of the field experiment is not so straight-forward to identify. Although I restrict the analysis to only compare compliers across groups, there is still a possibility that the respondents who attend the control group event are different from those who attend a treatment event. I use inverse probability weighting (IPW) to account for residual differences between the treatment-group compliers and control-group compliers. IPW is a two-step estimation procedure that corrects for differences between treatment and control groups. First, it computes the inverse probability weights as the inverse of the probability of receiving treatment, given X covariates, or *1/Pr(T=1—X)*. Next, it calculates the differences between the newly weighted means. Thus, this method corrects for the observable differences between treatment and control groups before assessing the differences between those groups on the outcomes.

To generate the weights, I modeled treatment assignment as a function of the following covariates: political interest, partisanship, willingness to debate, past protest behavior, and knowing a victim of organized crime. Theoretically these variables might influence an individual's choice to be re-interviewed on a day when there is an Ayotzinapa protest relative to a day without a protest.[13] I did not include demographic variables in the weights because they imposed high restrictions on the overlap assumption. This assumption states that $0 < Pr(t_i)|X < 1$ for all treatment levels. That is, every respondent must have some chance of being assigned to the treatment group or the control group. IPW is highly sensitive to overlap; if the treatment model is too restrictive, and if the data show that there are covariate patterns for which there are no respondents in either the treatment or control groups, the weights will be too large and the estimates unstable. [14] Not only were demographic variables too highly correlated with one value of treatment assignment, but they do not explain treatment assignment in a way that is conceptually distinct from the selected variables.

[13] I did attempt to work with protest routes that were not advertised in advance in order to keep this from significantly driving results.

[14] Appendix H discusses the overlap of the IPW treatment and control groups and shows plots of the final models.

5.4. Field Experiment: Results

Figures 4 and 5 show the differences in means between the treatment and control groups. Using the inverse-probability weights, selection into the treatment and control groups is considered to be as-if-random, and differences between groups reflect the presence of a protest at the time of interview. Consistent with the vignette experiment, the dependent variables are transformed as a dummy variable in which 0 stands for scores of 1 and 2 while 1 stands for scores of 3 and 4. The observational outcomes in Figure 5 are dichotomous in nature, where individuals either take the pamphlet/sign the petition or not. For both figures, the first panel compares the small protest to the control, the second panel compares the large protest to the control, and the last panel compares the presence of either protest to the control.

Figure 5:

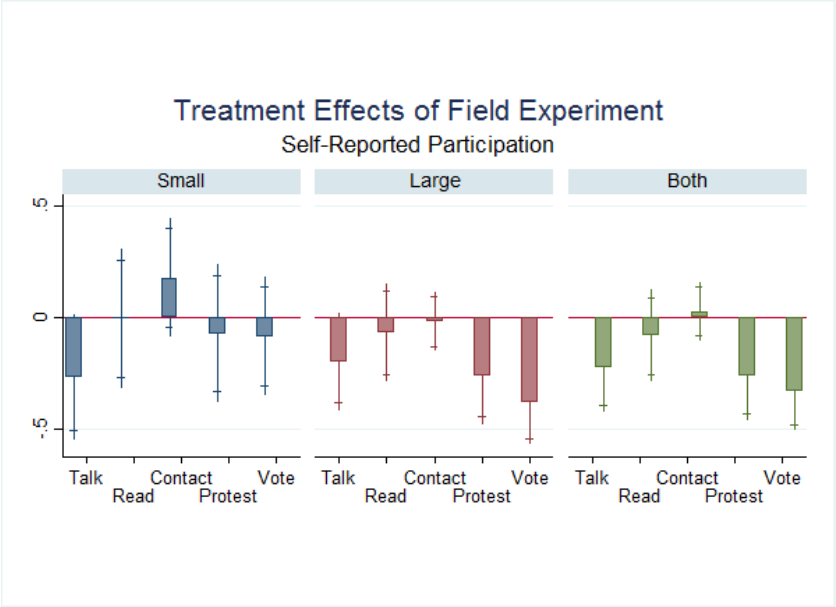
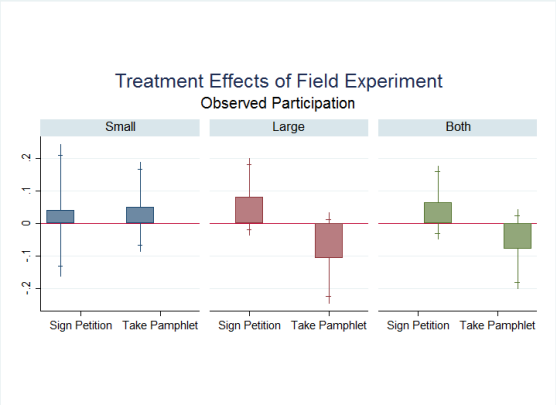


Figure 4 depicts fairly consistent, primarily negative, effects of the protest treatments. On average, exposure to protests in the field makes individuals less likely to talk about

Figure 6:



political issues, to join a protest, and vote for a sympathetic candidate. The impact on willingness to read about the issue is quite small and not significantly different from zero, and the effects on the willingness to contact a politician are ambiguous.

These effects are relatively consistent across treatment groups. The small protest has less clear effects with much wider confidence intervals. Indeed, the only indicator that achieves statistical significance (at the 90% level) is *Talk*. The most obvious explanation for the wide confidence intervals is the smaller sample that arrived at the small protest follow-up. Nevertheless, it is noteworthy that the point estimates for both protest treatments have the same sign for every indicator except *Contact*. Of course, it is difficult to draw conclusions about effects that do not achieve significance, but it is possible that smaller protests do not draw the same antagonism against elected politicians as larger protests.

The results from the large protest and the combination indicator are much easier to interpret. In the presence of a large protest, the average respondent feels less likely to engage in politics than he would in the absence of a protest. More specifically, the presence of a large protest made individuals 20% less likely to talk about the organized crime, 26% less likely to join a protest against organized crime, and 38% less likely to vote for a candidate because of his stance on violence.

As is clear in Figure 5, the effects of protests on the behavioral measures of engagement were far less dramatic. There were no significant differences between treatment and

control groups for either receiving the pamphlet or signing the petition. Recalling Figure 3, between 80% and 90% of respondents in all three groups took the pamphlet, and even more signed the petition.With such low variation, there is little room to estimate a treatment effect.

Do the starkly different findings in the two experiments necessarily indicate disparate responses to the protest treatments? Or might the divergent results be driven by the characteristics of the distinct samples? The vignette experiment benefits from a large random sample, and outcomes are observed for every respondent. The field experiment, on the other hand, represents a truncation of that sample in which outcomes are only observed among those who select into that sample. Truncation alone is not necessarily problematic when the selection process is "ignorable," or missing at random. That is not likely to be the case in this design. The type of respondent who is likely to attend a follow-up interview is expected to be systematically different from the type of respondent in the full sample who chooses not to attend.

To test if the divergent effects of the field and vignette experiments are driven by selection bias, I analyze the field experiment using selection models. These models incorporate information from the full sample to parametrically detect and correct for the selection bias of the field experiment (Heckman 1976; 1979, Greene 1981) The selection model follows a two-step procedure. The first step estimates the effect of the treatment on the outcome variables, and the second estimates selection into the sample using the full list of pre-treatment covariates measured during the phone survey.

Table 2: Selection Equation

	(1) Attendance b/se
Male	-0.095
	(0.23)
Age	-0.001
	(0.01)
Education	0.039
	(0.07)
SES	0.027
	(0.05)
EPN Approval	-0.096
	(0.14)
Political Interest	0.329**
	(0.12)
Traditional Partisan	0.492+
	(0.25)
Debater	0.549*
	(0.25)
Protester	-0.280
	(0.29)
Number of Protests	0.088
	(0.10)
Victim	-0.091
	(0.24)
N	1113

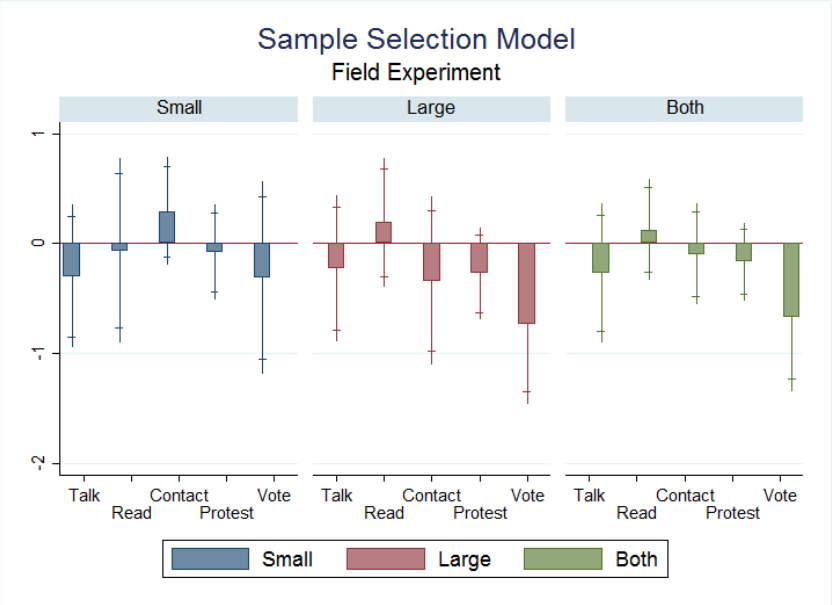
In Table 2, I demonstrate that this selection equation captures important differences between compliers and non-compliers.[15] The model shows that two variables positively predict attendance at a follow-up interview: interest in politics and willingness to debate contentious issues. This finding has face validity. It seems clear that attendance for a follow-up interview about political issues would be related to interest in politics and willingness to discuss politics. Intuitively, these individuals would be least likely to respond negatively to the protest experience. Should the entire sample from the phone survey have complied with the field experiment, it seems unlikely that the treatment effects would be positive, as they were in the vignette experiment.

Next, I use the selection equation from Table 2 to correct for sample selection bias in the treatment effects of the field experiment. The results of these models are depicted in Figure 6. There are few statistically significant findings emerging from these

[15]This model predicts correct outcome 71.90% of the time.

models; only willingness to vote passes the threshold to significance. The wide confidence intervals are not surprising, given the small sample size for the outcome equation.[16]

Figure 7:



Despite the lack of significance, there are meaningful implications to draw from Figure 6. While the wide confidence intervals make it impractical to draw conclusions about the effects of the field experiment, the selection models help to rule out some of the more problematic alternative hypotheses. Specifically, it appears unlikely that the negative direction of the field experiment's treatment effects was simply a function of the truncated sample. Controlling for the differences between compliers and non-compliers, the treatment effects in the selection models still imitate the average treatment effects of the field experiment quite closely. In fact, the coefficient for *Vote* is much greater in magnitude than for the complier-only model. This weakly suggesting that the full sample (consisting of compliers and non-compliers) might have actually responded more strongly to the treatment than did the compliers only. Considering the differences be-

[16]It is worth noting that the estimates for *Contact* are potentially unstable. For these models, the repeated rho ranges from -1 to 1.

tween compliers and non-compliers, this finding would not seem out of place. If the sample were not truncated to the most political interested and contentious respondents, protests may have even stronger negative effects on political engagement. The evidence, while still very inconclusive, suggests that the positive treatment effects in the vignette experiment and negative effects in the field experiments are not simply a function of the non-random compliance.

6. Discussion

It is hardly disputable that many protests in democratic countries seek to ignite widespread political engagement. After all, as general engagement in politics increases, so too does the pressure on political decision-makers. This implicit goal was made explicit by civil rights activist, Martin Luther King, who maintained that protest is a powerful weapon because it forces the public to discuss, challenge, and fight for political issues. Protest creates "a tension in the mind" and "a situation so crisis-packed" among the masses until negotiation becomes the only way out (King Jr 1963, p.4-5). Protests thrust the broader public - even those who never join the movement - into the political debate until elected officials have no choice but to respond.

Despite the fact that social movements so often aspire to mold, engage, and interrupt civil society, these consequences of protest activity are largely neglected in the literature. This is the first empirical research to address how protests can engage or disengage the public audience. I find that news of protest activity can engage people in political issues and even inspire them to take action, but those same people can disengage when confronted with real protests. Encountering a protest indirectly through the media can have a contagious effect on political engagement. However, the real-life disruption that protests cause can have a counter-productive outcome. Taken at face value, these findings represent a trade-off for protesters. On the one hand, a wide audience is necessary to spread a message and pose a credible threat to the status quo. On the other hand, the strategies available to protesters - disruption, confrontation, civil disobedience, or

even violence - may drive away that audience.

Evidence for these findings is based on a two-pronged experimental design, with a vignette and a field component. Both experimental designs demonstrated that protest exposure can influence political engagement, but the direction of the effects differs for the two experiments. On the one hand, the vignette treatments generally resulted in increased engagement. The significant effects are largely limited to the vignettes representing large protests, but the positive direction of the effect is fairly consistent throughout the treatments.[17] On the other hand, the field treatments resulted in political disengagement over a wide range of indicators. Again, this effect was more pronounced for the large protest treatment than the small protest, but the direction of the effect is consistent. Moreover, the direction of the effect does not appear to change when adjusting the selection into the field experiment.

The apparent divergence in treatment effects has substantive and methodological implications. Substantively, this is preliminary evidence that 1) protests do influence public engagement, 2) the effect can be positive or negative, and 3) the way in which respondents encounter a protest is more determinant of their reaction than the actual characteristics of the protest they observe. Of course, further analysis is necessary to assess the mechanisms and validity behind these effects, but the basic findings represent a substantial contribution to the literature on protest consequences.

Of course, further research will be necessary to draw generalizations from this case. There are some important scope conditions for the geographic and thematic cases selected. Regarding the geographic selection, Mexico City is an especially easy case to test in some respects. Citizen participation habits are quite flexible, and there is a constant stream of protest activity. Under these circumstances, it is imprudent to make generalizations about the effects of protests where participation is highly institutionalized and ritual or where protests occur infrequently. Also, the topic of the protests political violence or organized crime is not as controversial or contentious as many topics of protest. Again, it is plausible that the experiment would yield different outcomes

[17] Appendix G shows that these effects are strongest and most consistent among those respondents who disapprove of the sitting president.

for a different set of protests. For these reasons, further research is necessary to determine if these findings extend to other cities and other protests. But what does this mean for protesters? My findings suggest that protesters can strategize their techniques and protest geography to minimize the number of directly-exposed bystanders while maximizing indirectly-exposed audience. Protests that expand spatially without gaining significant media coverage appear counter-productive. This would include the Occupy Wall Street camps that spread to medium-sized and small cities throughout the United States even though most media attention focused on the New York site. On the other hand, techniques that focus primarily on attracting the media, techniques such as *Hacktivism*, might benefit from this trade-off.

This brings me to the methodological contribution. Given the complex and highly confounded nature of protest emergence, there is ample potential for experimental designs to answer challenging questions on protest contexts and political behavior (for an example, see McClendon (2014)). However, the key take-away point is that neither experiment would have captured these effects in isolation. The causal relationship takes form through the combination of the field and survey experiments. This suggests that experimental designs may need to conjoin multiple forms of treatments in order to fully explain complex social processes. While this paper illustrates the use for multiple treatments, there are certainly other examples where personal encounters with political events invoke emotional, visceral, or confused reactions that can only be captured via direct-exposure treatments. Examples of such events might include repression, group discrimination, or campaign events. Clean, hypothetical, and easily manipulated treatments may tell us *something* about how individuals respond to such events, but we are likely missing a large part of the story.

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Appendices

A. Case Selection

I have chosen to conduct this project in Mexico City. As a result of the country's gradual process of democratization, Mexican citizens are still questioning and developing their patterns of participation (Almond and Verba 1965, Lawson and McCann 2003). Weak ideological and partisan attachments make Mexican voters particularly malleable and uncommitted (Lawson 2015, Nichter and Palmer-Rubin 2015). This type of flexible citizen is ideal for testing hypotheses on the increases and decreases in engagement. Furthermore, Mexico City contains a rich diversity and heavy level of protest activity. Indeed, the city government counted approximately 3,000 protests of all shapes and sizes in 2012 (Sheridan 2013). Unlike other countries, in which protests are dominated by a particular sector, Mexico's revolutionary past and relatively closed political system spark protests among nearly all sectors of society, including the agricultural sector, intellectuals, students, working poor, and the informal sector (Gutmann 2002, Hodges et al. 2002). Significant events, such as the student massacre in 1968 and the 1985 earthquake in Mexico City, have justified protest activity as a crucial and respected means of political participation (Wood 2001, Johnston and Almeida 2006, Mattiace 2011). Finally, the immense size of the city also makes it ideal to examine the self-selection of protest exposure. Because of the structure of neighborhoods, or *colonias*, it is very possible for individuals to avoid protest-ridden areas if they please. As a result, although all respondents will be from the same city, the respondents' local contexts will vary a great deal in terms of past exposure to protest activity.

All treatments were based on real anti-violence protests in that took place in Mexico City in 2014-2015. In September 2014, these protests began in response to the disappearance and alleged mass murder of 43 leftist student teachers from the Ayotzinapa teachers' college in the southern town, Iguala, in the state of Guerrero. Although there was no central social movement to organize events after the attack, a series of protests took the nation by storm as hundreds of thousands of participants mobilized to demand

government accountability, improved democratic processes, justice for the victims' families, and a general break from the corrupt status quo in Mexican politics. These protests are ideal for this project for a number of reasons. Although there are countless protests to choose from in Mexico City, these protests were 1) expected to last for the duration of the study, 2) highly diverse, 3) non-partisan, and 4) substantively important.

The demands of the protests were so unattainable that there was no obvious end to the mobilizations. The primary rallying-cry of the protest is, "They brought [the 43 students] alive, we want them brought back alive" ("Vivos los llevaron, vivos los queremos!"), even though there is certainly no doubt that the students were killed shortly after being taken. If there is any unattainable goal, it is the reappearance of the 43 students. Furthermore, the protests deeper goals of political accountability, the eradication of corruption, and a permanent solution to drug violence appear to be nearly as unattainable as the students' miraculous reappearances. The apparently insatiable demands of the protests were very important to this project because the movement did not depend on one specific issue with a finite horizon, and they could mobilize throughout the year of fieldwork.

Also, these protests were highly diverse and did not confine themselves to any specific repertoire. This is largely possible because no single group dominated in the organization of protests. In an early protest, a subgroup of a larger march burned a massive effigy of President Peña Nieto in the middle of the Zócalo and set fire to the door of the National Palace (Sim 2014). In other marches, participants held little candles to represent the demand for peace (Olivares 2014). There were sit-ins in front of the attorney general's office (Méndez 2014), caravans that traveled from all over the country to march on the nation's capital (Saldaña 2014), and highly creative artistic exhibitions (Marrón and Jiménez Jaramillo 2015). Some protests were very small with a few hundred participants, and others contained thousands of people. This diversity is critical for validity of the treatments. The treatments randomize characteristics of the protests in order to measure the relative impact of different protests, but few movements could contain such a wide set of protest events to make this randomization plausible.

Third, relative to most protest organizations in Mexico, these events appeared to be

distinctly non-partisan. While some of the protests were organized by groups with clear partisan affiliations, there were so many groups mobilizing for the same cause that the partisan message was not immediately clear or consistent. Indeed, the demands of the group (accountability, justice, and an end to violence) are largely non-controversial, and not a single political party stood up against the protesters' claims. This is important because there would be few respondents who would oppose these protests on behalf of a deeply-set political affiliation, and theoretically, any respondent of any party would have the potential to be swayed to action as a result of protest exposure.

Finally, these protests are substantively important for Mexico and other countries that struggle with corruption and violence. Over the past decade, many thousands of Mexicans have died as a result of cartel violence, but the gruesome massacre of 43 student teachers hit a nerve that reverberated throughout the world (Kennis 2014). The impact of this event was so strong that protests continued for approximately a year after the original killing took place. Did these protests bring the general public to become political engaged and involved in these political issues? If protests can generate public engagement, these effects would be most important for an issue with such high stakes and wide-reaching significance as political/drug violence.

Of course, single case studies never allow for unlimited generalization, and this dissertation is no exception. There are some important scope conditions for the geographic and thematic cases selected. Regarding the geographic selection, Mexico City is an especially easy case to test in some respects. Citizen participation habits are quite flexible, and there is a constant stream of protest activity. Under these circumstances, it is imprudent to make generalizations about the effects of protests where participation is highly institutionalized and ritual or where protests occur infrequently. Also, the topic of the protests - political violence or organized crime - is not as controversial or contentious as many topics of protest. Again, it is plausible that the experiment would yield different outcomes for a different set of protests. For these reasons, it is important to test the external validity of these findings in future research.

B. Descriptions of covariates

Table B.1: Indicators

Indicator	Survey Item	Response Options
Male	Respondent's gender	1=Male ; 0=Female
Age	How old are you?	Open ended
Education	Now can you tell me the highest level of education you have achieved?	1= No schooling; 2= Primary School; 3=Middle School; 4=High School; 5=University+
SES	I'm going to read you a list of things that people often have in their homes. Please tell me if you have the following items in your home: (read options)	Items: refrigerator, computer, washing machine, sound system, cable, television, radio, cell phone, iPad
Traditional Partisan	Regardless of which party you vote for, do you normally consider yourself a PAN-ista, PRI-ista, PRD-ista, or do you identify with some other party? (IF OTHER: Which party?)	Party options: PAN, PRI, PRD, Partido Verde Ecologista, Partido del Trabajo, Convergencia, MORENA, Partido Nueva Alianza, Partido Encuentro Social, Partido Humanista
Political Interest	How interested are you in politics?	1=Not at all; 2=A little; 3=Quite; 4=Very
EPN Approval	In general, how would you rate the performance of President Enrique Peña Nieto?	1=Very poor; 2=Poor; 3=Good; 4=Very good
Debater	Do you like to debate political issues with people who do not necessarily agree with you?	1=Yes; 0=No
Number of Protests	Approximately, how many protests have you personally seen since the beginning of the year? (Read all options)	1=0-1 proests; 2=2-5 protests; 3=6-10 protests; 4=+10 protests
Protester	Have you participated in a protest in the last five years?	1=Yes; 0=No
Victim	Do you personally know anyone who has been a victim of organized crime?	1=Yes; 0=No

Table B.2: Gender

	Part I	Part II	Total
Male	308	310	610
%	50.83	51.67	51.24
Female	298	290	588
%	49.17	48.33	48.76
Total	**606**	**600**	**1,206**
%	**100**	**100**	**100**
Pearson chi2(1)=0.0855	*Pr = 0.770*		

Table B.3: Age

	Part I	Part II	Total
18-25	71	66	137
%	11.72	11	11.36
26-40	109	122	231
%	17.99	20.33	19.15
41-60	244	253	497
%	40.26	42.17	41.21
61+	181	159	340
%	29.87	26.5	28.19
Total	**605**	**600**	**1,205**
%	**100**	**100**	**100**

Pearson chi2(3)= 2.4799 Pr = 0.479

Table B.4: Education

	Part I	Part II	Total
No Schooling	4	3	7
%	0.66	0.5	0.58
Primary	61	73	134
%	10.1	12.21	11.15
Middle School	102	19997	199
%	16.89	16.22	16.56
High School	140	154	294
%	23.18	25.75	24.46
University	297	271	568
%	49.17	45.32	47.25
Total	**604**	**598**	**1,202**
%	**100**	**100**	**100**

Pearson chi2(4)= 3.1700 Pr = 0.530

Table B.5: SES

	Part I	Part II	Total
0-1 Items	32	15	47
%	5.28	2.5	3.9
2-4 Items	88	89	177
%	14.52	14.83	14.68
5-7 Items	184	207	391
%	30.36	34.5	32.42
8-10 Items	302	289	591
%	49.83	48.17	49
Total	**606**	**600**	**1,206**
%	**100**	**100**	**100**
Pearson chi2(3)= 7.7638	*Pr = 0.051*		

Table B.6: Party Identification

	Part I	Part I	Total
PAN	49	61	110
%	8.09	10.17	9.12
PRI	62	56	118
%	10.23	9.33	9.78
PRD	44	38	82
%	7.26	6.33	6.8
Other	88	46	134
%	14.52	7.67	11.11
No Party	343	376	719
%	56.6	62.67	59.62
NR	20	23	43
%	3.3	3.83	3.57
Total	**606**	**600**	**1,206**
%	**100**	**100**	**100**
Pearson chi2(5)= 16.9119	*Pr = 0.005*		

According to the chi-squared test, there was a significant difference in party support between the two samples. The descriptive statistics in Table B.6 suggest that most of this discrepancy can be explained by "other" supporters and non-partisans. It is possible that Mexicans who reject the three main parties are somewhat indifferent between the two responses. They may be somewhat sympathetic with a minor party one week and then be frustrated with the whole political system the next week. Because most of

the difference occurs in these two categories, it seems unlikely that the chi-squared test indicates an ideological or deeply rooted difference between the samples.

Table B.7: Traditional Partisanship

	Part I	Part II	Total
No/Other Party	451	445	896
%	74.42	74.17	74.3
Traditional Partisan	155	155	310
%	25.58	25.83	25.7
Total	**606**	**600**	**1,206**
%	**100**	**100**	**100**
Pearson chi2(1)=0.0103	*Pr = 0.919*		

Table B.8: Political Variables

Variable	**Range**	**N**	**Mean**	**Std. Dev.**	χ^2
Interest	1-4	1201	2.48	1.08	0.839
EPN Approval	1-4	1146	1.88	0.88	0.463
Protest Exposure	1-4	1180	2.31	1.23	0.149
Have Protested	0-1	1204	0.19	0.40	0.913
Debate	0-1	1206	0.41	0.49	0.974
Victim	0-1	1202	0.45	0.50	0.928

C. Vignettes (English Versions)

C.1. Treatment Vignettes

"Recently in Mexico City, [thousands of / a small group of] students and people from different civil society organizations marched with [torches / small candles] from the Paseo de la Reforma to the Zócalo to demand that the federal government put an end to the violence and corruption related to drug trafficking. In memory of the normalistas in Ayotzinapa, protesters stated that they would continue to march until there was peace and justice in Mexico. Some claimed that the government of Mexico was responsible for the crimes that took place; others marched simply because they were tired of the violence in Mexico. [The protest, a multitude of candles lit in the hope of ending violence, interrupted traffic routes. The march caused traffic jams for several hours in the city center and ultimately ended peacefully. / The march interrupted traffic routes, causing traffic jams for several hours in the city center. The protest ended in clashes with the police, who used tear gas and rubber bullets to disperse the protesters. According to initial reports, several participants were arrested and others were brought to local hospitals.]"

C.2. Control Vignette

"A recently published survey shows that a majority of citizens are tired with the level of violence in the country, but there is some disagreement about who is responsible for the violence and what should be done to establish peace. The survey follows the unfortunate occurrences that took place in Iguala (Guerrero) and seeks to understand what Mexican citizens think about the violence from organized crime and what should be done about it."

D. Information about the field treatments

The small protest was organized by the Plantón por Ayotzinapa, a group demanding justice for the Ayotzinapa victims. The group maintained a small campsite - including food, tents, thematically relevant art, and information about the movement - with twenty-four hour presence in front of the attorney general's office on the Paseo de la Reforma in the city center. They began camping in December 2014 in order to apply constant pressure on the Mexican government for the events of September. They organize cultural events and demonstrations on a regular basis, on some occasions organizing more than once a day. Most events are very small, involving a few hundred participants, and depend on a network of highly dedicated activists and their presence on social media.

The small protest took place on April 26th between 4-6 pm. It began at the Ángel de la Independencia on the Paseo de la Reforma, where earlier that day the plantón had erected a monument to the 43 victims, and culminated at the Zócalo. It is highly unlikely that survey respondents would have anticipated the march because details of the event were not publicly dispersed in the media. Indeed, the march was a relatively low priority event for the day. The morning began with a large cultural event with 43 speakers and an opportunity for artists to create sculptures of turtles to represent the slow progress towards justice. The few flyers that were distributed among activist organizations provided details of these earlier events, but not for the march that would follow hours later. Approximately 300 people attended the late afternoon march, many of them wearing the turtle sculptures on their backs as they marched down Reforma. A photo of this event, posted on the twitter account for @plantonporayotzi, appears in Figure D1. For this first event, a team from BGC Beltrán waited in a Starbucks caf located at the Ángel de la Independencia between 4-6 pm. The caf has large windows facing the Ángel to maximize visualization of the protest.

Figure D.1: Small Protest

The large protest took place on May 15th and was organized by the National Union for Teachers (CNTE). In Mexico, May 15th is a National Day for Teachers, and in 2015 the unions decided to celebrate their day with a massive protest. As is the case with most large protests, there were a diverse set of interests represented at the march. There was a call for education reform, but a significant proportion of the attendants demanded justice for the 43 normalista students. Since the events of September 26th, the Ayotzinapa protests were heavily supported by teachers' unions, who felt solidarity for the victimized teachers and students.

The protest was massive. Estimates suggest that there were 5,000 participants, with representatives from sections 7, 9, 14, 18, 22, and 23 of CNTE in addition to representatives from the families of the Ayotzinapa victims. The original route went from San Cosme, passed through the city center, and then ended at the Zócalo. The day of the march, to accommodate the size of the protest, they changed the route to end at Bellas Artes, where the team from BGC Beltrán was waiting for respondents in a caf called Cielito. Despite the fact that there were policemen throughout the route, directing traffic and restraining the protesters, the march ended peacefully. A photo of the march from @Coordinadora1DM appears in Figure D2:

Figure D.2: Small Protest

Upon arrival at both protest events, survey enumerators were instructed to administer a manipulation check. Before asking substantive questions, they asked each respondent, "Did you see the protest going on outside?" The purpose of this item was to call attention to the protest treatment even if respondents had not noticed it independently.

CNTE and the teachers continued to protest in the city center for several weeks following the second protest. In order to conduct the control event without protest interference, we had to wait until mid-June for the mobilizations to die out. On June 23rd, the BGC Beltrán team waited between 3-7 in a Starbucks on Reforma (close to the Parque Chapultepec and far from the Zócalo, to minimize the possibility of protest exposure).

E. Compliance

The full phone sample included 1200 individuals in the five selected *colonias*. The response rates follow in Table E1:

Table E.1: Phone Survey Responses

AAPOR Response Rates	
Response Rate	6.4%
Cooperation Rate	35%
Refusal Rate	10.6%
Contact Rate	18.2%

Of course, most of those 1200 respondents were not compliers for the field experiment. For both treatment and control events, the compliers are a subsample of the respondents of the phone survey who expressed interest in attending the follow-up interview. Of the entire phone sample, 40% of respondents (n=478 of 1,198) indicated an interest in the face-to-face follow-up. The 60% (n=720) who refused to participate in the study gave a number of reasons. Many were too busy, were occupied on the days offered, or simply saw no reason to go. Many others admitted that the face-to-face follow-up seemed suspicious, and they were worried about falling victim to some sort of trap.

Table E.2: Contacting Potential Compliers

	Provided Cellphone		Provided Email	
	N	%	N	%
No	250	52.3	298	62.34
Yes	228	47.7	180	37.66
Total	**478**	**100**	**478**	**100**

Not all of the 478 respondents who were open to the idea of attending the follow-up interview were equally likely to comply. Of these respondents, only 37% (n=180) provided an email address, 48% (n=228) provided a cellphone number, and 23% (n=108) provided both. This information was used to send reminders closer to the date and give confirmatory details about the timing and location of the interview. The 37% of respondents who did not provide this information - and the greater (unknown) number

who provided misleading information - were not likely to remember to attend or to know when and where to arrive. Ultimately, a total of 300 respondents - minus those respondents who provided disingenuous contact information - were considered to be potential compliers.

Table E.3: Compliance Rates

	N attended	% with contact	% phone sample
Small Protest	16	5%	1%
Large Protest	54	18%	5%
Control	19	6%	2%
Total	**89**	**30%**	**7%**

F. Vignette Experiment, Full Output

Table F.1:
Vignette,Experiment, Marginal Effects
DV:,1=likely, very likely to engage

VARIABLES	(1) Talk	(2) Read	(3) Petition	(4) Contact	(5) Protest	(6) Vote
Small/Peaceful	0.022	0.073+	0.037	0.041	0.022	0.009
	(0.042)	(0.043)	(0.045)	(0.041)	(0.042)	(0.045)
Big/Peaceful	0.018	0.062	0.063	0.006	0.069	-0.001
	(0.043)	(0.043)	(0.044)	(0.040)	(0.042)	(0.045)
Small/Violent	-0.074+	-0.020	0.075+	0.004	-0.000	0.020
	(0.045)	(0.045)	(0.045)	(0.041)	(0.043)	(0.046)
Big/Violent	0.088*	0.039	0.099*	-0.006	-0.003	0.117*
	(0.043)	(0.045)	(0.046)	(0.041)	(0.042)	(0.046)
Male	0.036	0.035	0.071*	0.021	0.055*	0.071*
	(0.029)	(0.029)	(0.030)	(0.026)	(0.027)	(0.030)
Education	0.006	0.009	-0.018*	-0.002	-0.002	0.001
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)
SES	0.005	0.010	0.019**	-0.001	0.003	0.015*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Traditional Partisan	0.014	0.042	-0.002	0.034	-0.069*	0.090**
	(0.033)	(0.034)	(0.035)	(0.031)	(0.031)	(0.035)
Political Interest	0.061***	0.050***	0.038*	0.047***	0.055***	0.057***
	(0.014)	(0.015)	(0.015)	(0.013)	(0.013)	(0.015)
EPN Approval	-0.036*	-0.040*	-0.048**	0.015	-0.060***	0.034+
	(0.017)	(0.017)	(0.018)	(0.015)	(0.016)	(0.018)
Debater	0.103**	0.093**	-0.001	0.026	-0.012	0.060+
	(0.031)	(0.032)	(0.032)	(0.028)	(0.028)	(0.033)
Number of Protests	0.028*	0.015	0.023+	0.022*	0.050***	-0.038**
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.012)
Protester	0.030	0.062	0.150***	0.019	0.251***	0.100*
	(0.039)	(0.039)	(0.040)	(0.034)	(0.039)	(0.040)
Victim	-0.004	0.031	0.025	-0.015	0.038	0.037
	(0.030)	(0.031)	(0.031)	(0.027)	(0.028)	(0.031)
Observations	1,113	1,105	1,109	1,107	1,108	1,074

Standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

G. Heterogeneous Effects

Will protests have uniform effects across the population? Or might protest exposure increase political engagement among some people more than others? In this section, I will examine the heterogeneous effects of the protest treatments, focusing particularly on the vignette experiment. I focus on the vignette experiment for practical and theoretical reasons. In practical terms, it would be methodologically infeasible to estimate heterogeneous effects for the field experiment, which is limited to 88 respondents and requires statistical weighting to balance treatment and control groups.

In addition to this methodological caveat, there are more theoretical reasons to emphasize the vignette experiment. According to well-established research in political communication, the media focus of the vignette experiment is a highly appropriate context to examine heterogeneous audience effects. This literature has long found that the effects of media messages depend on the characteristics of the viewer (Klapper 1960). Klapper's classic meta-analysis concludes that the media might appear to have a minimally persuasive impact, in large part, because it operates more frequently as an agent of reinforcement than as an agent of change (p. 15). Viewers who already agree the message are more likely to be impacted by the media content, whereas viewers who do not already agree are less impressionable (Holbert, Garrett and Gleason 2010). These effects are perpetuated because individuals actively choose to follow media that conforms to their opinions (Prior 2007; Arceneaux and Johnson 2013) and because they dismiss opposing viewpoints when forced to encounter them (Taber and Lodge 2006). Given this extant research, it is fitting to ask whether protests reported in the media face similar constraints as other political information.

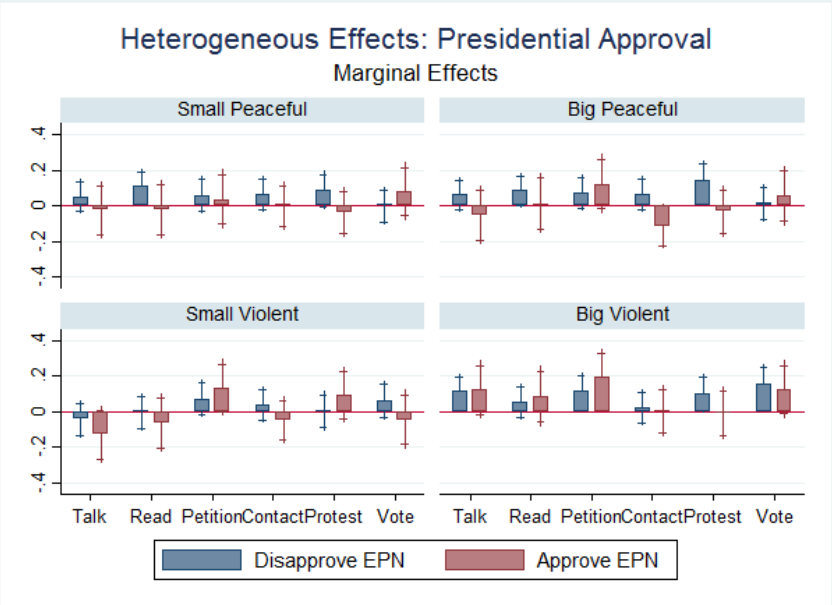
The key characteristic that conditions the effect of media messages is the viewer's prior support of the message (Klapper 1960). Individuals who support the protest are likely to respond positively to the message, and individuals who do not support the protest are either less likely to exhibit any effects, or they may even respond negatively. For the protests selected in this dissertation, it is not reasonable to expect respondents to explicitly state opposition the protest message; few respondents would condemn protests

that demand peace from organized crime. However, many respondents may condemn the implicit message of the protests, which suggested that Enrique Peña Nieto and the political establishment were responsible for the students' disappearances. Thus, I operationalize protest support through variables that capture support for President Peña Nieto and the traditional political parties. Additionally, though I expect few respondents would oppose the message of the protest, some observers may be more receptive than others. Specifically, I expect that respondents who have personally suffered from political violence, or who know someone else who has suffered, may be more significantly impacted by the protest than their counterparts.

First, I will discuss the conditional effects of presidential approval. For these models, presidential approval was transformed as a dummy variable, where 0 represents those who disapprove and strongly disapprove of Peña Nieto's work in office and 1 represents those who approve and strongly approve. Figure G1 reports the heterogeneous effects of this variable on the engagement indicators. I ran separate models for each dependent variable and at each level of the moderating variable. To maintain consistency between all of the heterogeneous effect models, these models do not control for other covariates beyond the moderators and each of the four treatments.

Figure G1 provides some evidence for the reinforcing effects of the protests. As the figure indicates, many of the models that showed significant, positive effects of protests are only significant for those who disapprove of Peña Nieto. That is, the individuals who demonstrated positive treatment effects in the previous models are the individuals who are most receptive to protests critical of the president. Relative to the control group, the Peña Nieto disapprovers who are treated with the small, peaceful protest and the big, peaceful protest vignettes more likely to read about and join a protest about political violence, but these effects do not carry over to respondents who approve of the president. There are also some significant differences for the large, violent protest vignette. For this treatment, the positive effects on *Talk* and *Protest* (at the 90% level) are only significant among those who disapprove of Peña Nieto. The large, violent protest treatment has a stronger significant effect on *Vote* for those who disapprove of the president.

Figure G.1:

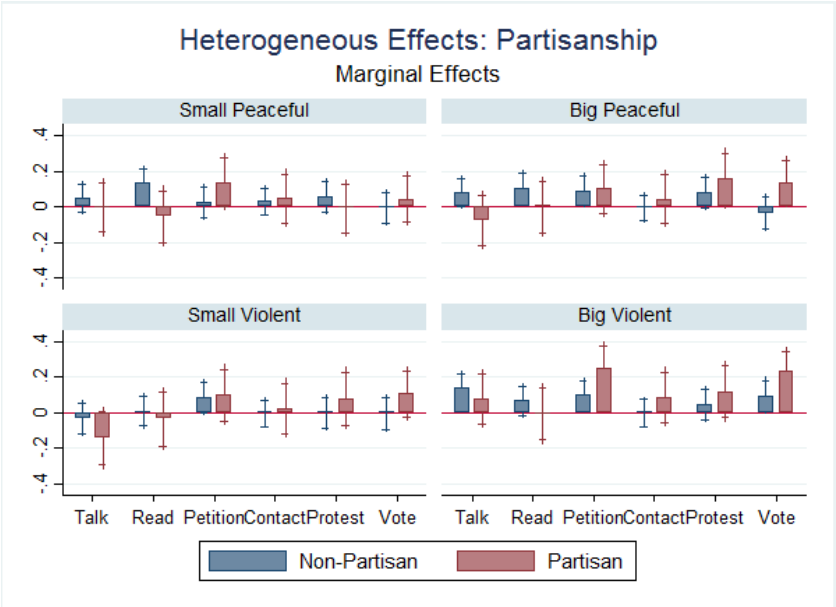


At the same time, those who express approval of Peña Nieto experience some negative effects of the protest treatments. These individuals are less likely to contact politicians in response to the large, peaceful protest and less likely (at the 90% level) to talk about political violence in response to the small, violent protest vignette. Indeed, the only effect that runs counter to the media reinforcement hypothesis is the effect of the small, violent protest on *Petition*. At the 90% level, this positive effect is only significant among Peña Nieto supporters.

Figure G2 shows that the conditional effects of partisanship are less clear than those of presidential approval. Because the protests generally cast blame upon all of the Mexican political establishment, I expected that the positive effects of protests would be strongest respondents who reject the traditional political parties (PRI, PAN, and PRD). This is true for a number of models, but there are a number of cases in which the positive effects of the protest treatments are stronger or are only significant among traditional partisans. For example, the positive effect of the big, peaceful protest on *Vote* is only significant for partisans, as is the effect of the small, peaceful protest vignette on *Petition* (at the 90% level). Similarly, the *Big/Violent* has significantly greater marginal effects among

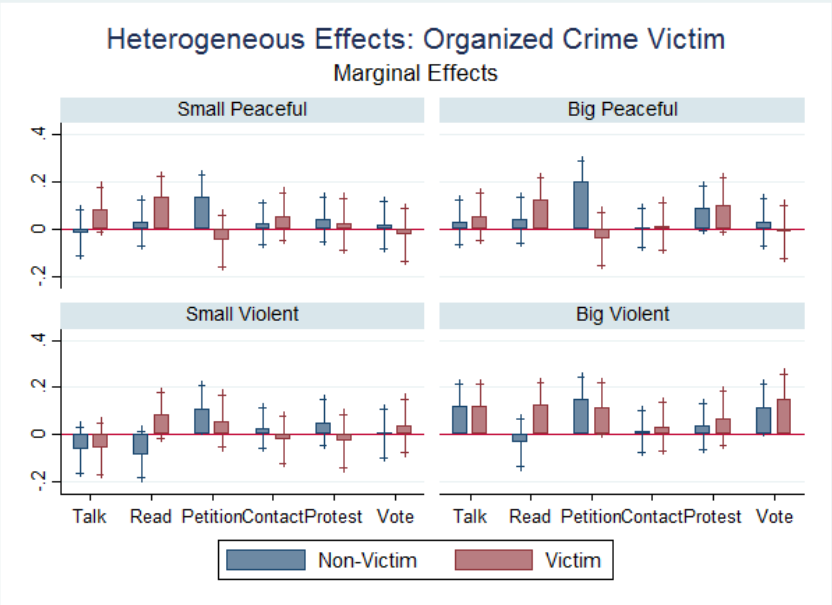
partisans on the willingness to petition and vote. It is possible that the heterogeneous effects would be more consistent if they were conditioned on PRI support, rather than support for any of the three traditional parties. However, the low proportion of PRI-istas in the sample (around 10%) prevents me from estimating these effects with any precision.

Figure G.2:



The conditional effects by crime victimization are even less clear. As I show in Figure G3, many effects are only significant for organized crime victims, but at the same time, several effects are restricted to non-victims. For example, the effect on *Petition* is only significant among non-victims. This effect is relatively consistent across the protest treatments. The effects of both peaceful protest treatments and the small, violent treatment are only statistically significant among non-victims, and the effect of the large, violent protest is only significant at the 90% level among victims.

Figure G.3:



In summary, there is some evidence for the heterogeneity of protest effects. Research in political communication finds heterogeneous constraints on the media's ability to influence viewers. Scholars do not find that any individual can be persuaded by the messages they encounter in the media. But rather, the persuasive influence of the media is generally limited to those individuals who were already supportive of the message. When I examined the conditional effects of the vignette experiment, I found some evidence that protest reports face similar constraints as other persuasive messages in the media. This is particularly true when support for the protest is operationalized as disapproval for the sitting president. Those who disapprove of President Peña Nieto are consistently more likely to increase in their willingness to engage as a response to the protest treatments, and those who approve of the president sometimes react negatively to the treatments. Meanwhile, other operationalizations of protest support - such as rejection of the traditional parties or organized crime victimization - do not result in such predictable outcomes. In spite of the theoretical expectations, many of the positive effects of the protest treatments are targeted among partisans and non-victims.

H. Overlap

Figure H1 shows the overlap plots of the treatment assignment model. The top figure combines both treatment groups, and the bottom figures address each separately. Given the covariates listed above, there are no large masses at either 0 or 1. That is, the treatment model is not so restrictive that it perfectly predicts assignment to the control group or either of the treatment groups. In fact, the treatment model shows very clear overlap of density masses, particularly for the large protest treatment and the combined treatments. As an additional check, I ran a test on each model to identify any observations that violate the overlap assumptions and found zero violations. These tests provide sufficient evidence that the predicted inverse-probability weights should not be too large to compute stable estimates.

Figure H.1: