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Abstract

How does violence during conflict affect the political attitudes of civilians who leave the conflict zone? Using a survey of 1,384 Syrian refugees in Turkey, we employ a natural experiment owing to the inaccuracy of barrel bombs to examine the effect of having one's home destroyed on political and community loyalties. We find that refugees who lose a home to barrel bombing, while more likely to feel threatened by the Assad regime, are less supportive of the opposition, and instead more likely to say no armed group in the conflict represents them – opposite to what is expected when civilians are captive in the conflict zone and must choose sides for their protection. Respondents also show heightened volunteership towards fellow refugees. Altogether, this suggests that when civilians flee the conflict zone, they withdraw support from all armed groups rather than choosing sides, instead showing solidarity with their civilian community.

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1 Introduction

How does indiscriminate violence shape civilian attitudes during civil conflict? How do the strategic logic and impact of indiscriminate violence in civil wars change when a large portion of civilians flee the conflict zone? In the last decade, efforts to understand how civil war violence shapes civilian attitudes and behaviors have produced a wide and nuanced literature. Recent work distinguishes between violence based on selective targeting versus that based on indiscriminate practices of civilian victimization. Either by assumption or through case selection, however, this same literature has not considered how the logic (for perpetrators) and effects (for civilians) of indiscriminate violence might radically differ when the war theater is not closed, but instead characterized by mass exit and forced displacement. Given that many conflicts produce enormous refugee flows, this oversight is due to be addressed. In this paper we examine a case where both indiscriminate violence and civilian exit are sadly plentiful: the Syrian civil war.

As our research and that of others illustrates, a substantial number of Syrians have been exposed to indiscriminate violence, much of it in the form of barrel bombings. For example, the Syrian Network for Human Rights documented 12,194 deaths from barrel bombing in Syria between January 2012 and 2015. Of these, 96% have been civilian deaths. Still, these horrific barrel bombings have not been well studied. With the notable exception of Tyner (2016), almost no published academic work has shed light on the impact of such indiscriminate violence in Syria. What is more, little is known about how the millions of civilian refugees that have fled such violence view the conflict and the parties fighting in it.[1]

Using original survey data from 1,384 Syrian refugees living outside of camps in Turkey collected in four provinces in the fall of 2016, we aim to better understand Syrian refugees' reactions to violence, particularly in the form of barrel bombing, by focusing on attitudes related to their political positions and loyalties. To do so we employ a tragic quasi-experimental approach that arises due to the technical nature and tactical use of barrel bombs. Because these terrifying weapons lack precision, the damage they cause is effectively random within a sufficiently small area targeted by the regime. This makes barrel bomb-related harms well-suited to quasi-experimental research in which we condition on small geographic areas.

For inferential reasons described below, our primary results focus on the effects of losing one's home to barrel bombing. Of course, having one's home destroyed by barrel bombs represents only a minor fraction of the total harm and suffering refugees have endured. We do describe the other types of violence our respondents lived through to better characterize Syrian refugees' overall wartime experience. However, we do not seek to make credible causal claims about the effects of those cumulative wartime experiences. Rather, our research design relies on a premise borrowed from randomized trials in medicine: A "treatment" can have an identifiable and detectable causal effect on a health outcome even if the subjects differ with regard to other factors affecting the overall outcome. Using this premise, we focus on destruction of homes by barrel bombs, which uniquely allows us to study how this particular form of indiscriminate violence changes civilian attitudes towards armed groups in a context where civilians have experienced various types of violence and have left the conflict zone.

When borders are open and civilians can flee, we argue both that the incentives for armed groups to use indiscriminate violence, and the ways in which civilians react to that violence, can change drastically and remain under examined. On the one hand, among refugees, it is reasonable to expect that experiencing higher levels of incumbent indiscriminate violence drives up support for the insurgents. Alternatively, it is also reasonable to expect that refugees instead turn away from all armed groups – both those who attacked them, and those would oppose their attackers, but have manifestly failed to protect them. Our principal contribution is an empirical test of these two alternatives, using a quasi-experimental approach to determine which response dominates in the particular case and population from which we sample. We know of no prior work emphasizing civilian flight from the war theater and what such an exodus means for how displaced civilians

[1] A recent exception is the Arab Barometer Survey of Syrian refugees in Jordan and Lebanon (Ceyhun, Huseyin Emre, 2017).

react to indiscriminate violence.

Our main findings are as follows. Experiencing additional violence (in the form of losing one's home to barrel bombing) has a strong effect on threat perceptions, with those affected being 20 percentage points more likely to find the Assad regime a personal threat to their own security and nearly 10 percentage points more likely to see Assad as a threat to a future Syria. This is expected, yet surprisingly this does *not* lead to a detectable increase in affinity towards the armed opposition – or any other armed group. In fact, those who lose homes are less likely to report support for the opposition and more likely to report that no party to the conflict represents their interests. Nor does such harm make individuals significantly more disapproving of those who wish to remain neutral in the conflict. This indifference towards others' neutrality cannot be attributed to political apathy amongst the harmed; in fact, we find evidence that the harmed are more politically engaged, and more likely to follow events in Syria closely. Instead, losing a home to barrel bombing increases volunteership within the refugee community, which we regard as an effect on in-group altruism or social cohesion. Consistent with emerging evidence that the apparently pro-social effects of violence may be parochial in nature (Rohner et al., 2013; Choi, 2007; Bauer et al., 2016a), those harmed are also less likely to report willingness to render life-saving assistance to a regime member in need. Collectively, we take these findings to imply that for the displaced, the experience of indiscriminate violence – in the limited sense we can study it here – has led to heightened security concerns and solidarity with the community of other civilian refugees, without driving victims further towards the opposition or heightening their disapproval towards those who opt to remain neutral. These patterns defy the dynamics that have been theorized and found to operate in other civil wars (e.g. Lyall et al., 2013; Zhukov, 2017; Christia, 2012), where escape may not be an option, and civilians are thereby forced to choose sides.

Although relying on the limited targetability of barrel bombs allows us to address the concern that some individuals are at higher risk of harms from this type of violence than others (i.e. "selection into treatment"), another major concern – and one common to much of the work on refugees and migrants – is selection into the sample. Specifically, if the chances of ending up in the population of refugees we sample in Turkey depends upon *both* exposure to barrel bombing and the attitudes we study as outcomes, it could cause bias. Perhaps the most obvious concern of this type would be that the most fervently pro-opposition individuals would be reluctant to leave Syria, preferring to stay and support the opposition, but that losing their homes makes these individuals more likely to emigrate. Such a case would, however, generate a bias opposite to what we find. Nevertheless, not all concerns of this type can confidently be ruled out. We discuss various selection into sample concerns, the evidence for or against them, and their potential impact on our results at length below.

2 Background

2.1 Theory: Logic of Indiscriminate Violence

Scholars of civil conflict routinely draw a conceptual distinction between "selective violence" – instances when combatants and/or the civilians that have materially supported combatants are targeted based on personalized information about their individual actions (Kalyvas, 2006)– and "indiscriminate violence". Indiscriminate violence is variably defined as: targeting everyone in a particular area with no effort to determine guilt or innocence (Downes, 2007); targeting violence without making credible efforts to distinguish between combatants and civilians (Lyall, 2009) and/or; targeting violence based on guilt-by association or concepts of collective guilt (Kalyvas and Kocher, 2007). Straus (2015) usefully introduces the term "group-selective violence" for violence not unlike that studied here, where one can say *groups* are selectively targeted, even though violence is effectively indiscriminate among those in the targeted groups or areas.

In the Syrian case, it is unlikely that barrel bombing is meant to kill insurgents directly. This is evidenced by the fact that barrel bombing is most intense in the communities away from the

front lines on which combat is occurring. If the point is not to kill insurgents, what then is the logic of such indiscriminate violence? In the rich theoretical framework articulated by Kalyvas (2006), indiscriminate violence is of limited strategic value, since it is not useful in providing a selective incentive for individuals to take sides. The one place where indiscriminate violence may be logical, in this framework, is in territories where one side (the incumbent or insurgent) has almost no control or information. As examined empirically in Kalyvas and Kocher (2007), these are areas where selective violence is not possible and where an attacker may hope to turn civilians against the party holding the territory by showing they are incapable of providing protection or perhaps are responsible for provoking violence.

A central scope condition for the logic articulated above is its focus on "coercive" rather than "eliminative" violence. In Kalyvas (2006), the defining distinction between coercive and eliminative violence is given as "whether at least one political actor intends to govern the population it targets for violence"(26). Thus, mass deportation and ethnic cleansing signal an *eliminative* logic in which the perpetrators have no interest in ruling over these populations.

While we focus mainly on indiscriminate violence by the incumbent regime, scholars have also proposed explanations for why and when insurgent groups victimize civilians, given these groups' need to extract resources from communities, and despite the cost of alienating civilian communities (see e.g. Weinstein, 2006; Humphreys and Weinstein, 2006).

2.1.1 Open Borders and Mass Displacement

Returning to the Kalyvas (2006) framework, civilian populations are largely assumed to be static and violence is said to be "coercive", because each side uses it in an effort to coerce support from a fixed population they seek to rule. By contrast, Kalyvas (2006) labels violence as "eliminative" when its purpose is to remove or kill a group of people rather than rule them. Although it may at first seem that mass displacements, such as in Syria, should be treated eliminative violence – driving people out to remove them rather than killing them – this is not a straightforward distinction. Perpetrators perusing mass displacement also use violence coercively, first hoping to cutoff civilian support for the rebels, then to drive out civilians – even if temporarily – from the battle zone. The *goal* need not be to eliminate these populations as such, and the incumbent may even hope that displaced civilians return once the area is cleared of insurgents. Rather than attempt to categorize mass displacement and violence in the Syrian case as coercive or eliminative, we argue the regime's use of violence is better understood in terms of a strategy of "draining the sea" (Valentino et al., 2004): removing populations from large areas serves to flush out insurgents and displace those who would otherwise support them with aid, information, or concealment.

Accordingly, a key factor in determining how armed groups might use indiscriminate violence is the ability of individuals to leave the conflict zone. Open borders allowing civilian exit give the regime reason to use indiscriminate violence to wage counter-insurgency by "draining the sea". The same applies to insurgents: they too may wish to use indiscriminate violence to drive out incumbent supporters from areas they seek to control.[2] Finally, the ability to exit also shapes civilian reactions to indiscriminate violence. The coercive or "captive civilian" model understands civilian reactions to violence through their efforts to improve their own security. The model does not necessarily apply when civilians can leave the conflict zone rather than adjust their loyalties to maximize security. An open question therefore remains: how do civilians who were able to flee the conflict theater react to indiscriminate violence? We turn to this below, reviewing relevant literature and the remaining gap in the literature around this question.

[2]That said, in asymmetric conflicts the insurgents are often less capable of destruction on the scale that would cause mass displacement.

2.2 Relevant Empirical Work

Wartime Efficacy

Beyond work that seeks to identify the logic and strategic reasoning behind civilian victimization, a number of other studies estimate the impact of indiscriminate tactics to ascertain whether or not they achieve their desired objective. For example, Dell and Querubin (2016) find that U.S. aerial bombardment in the Vietnam war was counter-productive: it increased the military and political activities of the insurgency, weakened local governance, and reduced non-communist civic engagement. In a closely related study, Kocher et al. (2011) find that aerial bombardment increased Viet-Cong insurgent activities in bombed areas. In contrast, Lyall (2009) shows that Chechen villages indiscriminately shelled by Russians experienced a substantial reduction in subsequent insurgent attacks.

Behavioral and Attitudinal Responses

Other relevant studies are interested in how exposure to violence shapes various types of "civic" behaviors and attitudes including participation in civil society, political engagement and altruistic actions and beliefs. For example, Gilligan et al. (2016) show that communities affected by violence in Nepal's civil war exhibit higher levels of voting, altruistic giving, public good contributions, and investment in trust-based interactions. Most importantly, one mechanism discussed is a purge mechanism by which those with less pro-social attitudes flee violence-affected communities while those with more pro-social attitudes remain. In contrast, our work considers the impact of indiscriminate violence on those who have left the conflict-affected areas in Syria and finds altruistic effects of violence within the group of those who left. Finally, in her work on the Spanish civil war, Balcells (2012) finds that victimization matters for political attitudes, identities and behaviors. Those victimized by violence reject the perpetrators' identities, and this rejection can be passed through to subsequent generations who do not themselves experience the trauma directly. Focusing on political engagement, Blattman (2009)'s study of ex-combatants in Northern Uganda finds that those conscripted through abduction exhibit a substantial increase in voting and community leadership, but not nonpolitical forms of social engagement. We refer the reader to Bauer et al. (2016b) for an extensive review of works, some of it quasi-experimental, on cooperation and other potentially pro-social reactions to violent exposure.

Despite this plethora of work on how violence effects attitudes and behaviors, there are still very few rigorous empirical studies *of civilian attitudes toward combatants* during wartime. Undoubtedly, few studies examine actual individual-level exposure to violence during a conflict due to the logistical and security challenges associated with such research. To circumvent these challenges, Fair et al. (2016) use a survey prime to manipulate perceptions of violence and thereby study support for militant policies. In another pioneering attempt to study effects of violence on attitudes toward the warring parties themselves, Lyall et al. (2013) reveals that civilians suffering violence under the International Security Assistance Force in Afghanistan shift attitudinal support towards the Taliban, but the opposite does not hold. This asymmetry in responses suggests the need to "integrate perceptions of harm and other individual level characteristics into our models...to understand how violence is understood by civilians and how it affects both attitudes and subsequent behavior"(19). Our work takes up that call, while also employing individual level exposure to violence as a key explanatory variable.

2.3 Addressing the Gap in Understanding Civilian Responses to Indiscriminate Violence

The work surveyed above leaves important gaps in our understanding of how civilians might respond to indiscriminate violence under a "draining the sea" rather than "captive civilian" scenario,

thereby setting up our empirical question.

First, and most broadly, what are the implications of taking seriously the possibility of civilian exit from the conflict zone in thinking about civilian reactions? On the one hand, we might very reasonably expect that displaced individuals who suffer higher levels of indiscriminate harm increase their support for the opposition even if they leave the conflict zone. This could involve behavioral support, but also the expression of political support, alignment of policy preferences with the opposition, or feeling that the opposition represents them. Alternatively, incumbent indiscriminate violence may not drive displaced individuals into the arms of the opposition, as there is no security rationale for such a response. Support for the opposition therefore could remain unchanged and/or the opposition's failure to protect – and perhaps their perceived role in provoking incumbent violence – could lead individuals away from the opposition as well. Our aim in this paper is to offer quasi-experimental evidence to shed empirical light on the question of which of these reactions dominates, at least in the Syrian case.

Second and relatedly, the studies cited above overwhelmingly focus either on collective behavioral outcomes, or on individual outcomes that neglect attitudes toward the warring parties themselves. Many focus on outcomes such as shifting zones of control, up-ticks in insurgent activity, and the overall efficacy of indiscriminate bombing strategies on motivating civilians to switch sides. There is also a heavy reliance on cross national data and aggregate data, like death counts. With a few exceptions such as Lyall et al. (2013) and Fair et al. (2016), little attention has been given to how direct exposure to indiscriminate violence shapes *individual* perceptions about incumbent and insurgent groups and/or the acceptability of remaining neutral.

Third, we seek to make the most credible causal claim possible in the Syrian case regarding the *effect* of indiscriminate violence on attitudes. Recent qualitative and descriptive work has carefully examined how regime violence has led certain individuals to develop distinct political attitudes – for instance, when it comes to tolerance for ISIS (Gerges, 2016). However, it has not previously been possible to say these differences are due to harm from violence rather than other factors, such as group membership or pre-existing political ideology. Our research design, while imperfect, seeks to provide the most credible estimates possible absent the ability to experimentally randomize actual exposure to violence. We follow this with balance and placebo tests, discussions of the potential remaining biases and their direction, and a sensitivity analysis to formalize the remaining threats to our conclusions due to the risk of unobserved confounding.

Beyond these contributions, understanding displaced civilians' attitudes is important for several additional reasons. The refugees we sampled remain involved in Syrian affairs – e.g. 90% report that they expect to return to Syria, 91% have family members there, and at least 11% have returned to Syria to visit their homes at least once already. Refugees are thus likely to be part of a post-conflict Syria, and their attitudes are important as they could act as spoilers if ignored. Although elites may appear to control decisions about peace and conflict, they must appeal to what they believe civilians want (Hoddie and Hartzel, 2010). Furthermore, the remittances civilian refugees provide can serve as a continuing form of material support for political and armed groups (Lindley, 2010).

3 The Syrian Civil War

3.1 Background on the Civil War and Displacement

The Syrian civil war was sparked by protests that began in March 2011, when children aged between 9 and 15 were detained and reportedly tortured for writing graffiti denouncing the Assad regime on the walls of their school in Der'a(McHugo, 2014). Soon the protests started to spread to other cities and were met with a harsh response form the regime (Hokayem, 2013). By July 2012, the initial protests that were largely semi-urban and peaceful, had turned into a brutal civil war, fought between the Syrian government forces and multiple rebel factions, including both secular

and Islamist groups (McHugo, 2014; De Juan and Bank, 2015).

The war entered a new phase when the Islamic State of Iraq and Syria (ISIS) captured the Iraqi city of Mosul in June 2014, and declared an Islamic caliphate with the Syrian city of Raqqa as its de-facto capital. As the territorial losses of the regime mounted, in the fall of 2015, Russia started its first air strikes to bolster regime control. By early 2016, Russian intervention had reset the military balance in Syria (Casagrande and Cafarella, 2016). Around this time, the war also had clearly become a proxy conflict between actors supporting the regime (Russia, Iran, and Hezbollah) and those who support the opposition (US, Turkey, Saudi Arabia and Qatar). At the end of 2016, the regime recaptured the entire city of Aleppo (BBC, 2016).

The human costs of the war have been devastating: an estimated 475,000 people have died, and close to 14 million Syrians have been wounded or displaced (SOHR, 2017). Among those displaced, more than 5 million had to leave Syria and have become refugees (UNHCR, 2017).

Syrian Refugees in Turkey

Turkey hosts the largest number of Syrian refugees (UNHCR, 2015) and is the primary destination for Syrians exposed to indiscriminate violence in the civil war, and especially to barrel bombs. According to the statistics provided by the Turkish Directorate General of Migration Management (DGMM), as of November 9, 2017, there are 3,303,113 registered Syrian refugees in Turkey. About 7% of them are settled in the 24 camps run by the Turkish government, while the vast majority reside among the Turkish population in urban areas. About 57 % of non-camp refugees are living in 4 provinces of Turkey: İstanbul, Gaziantep, Hatay, and Şanlıurfa.[3]

There are no officially released data on the origin areas or settlement patterns of refugees entering Turkey.[4] The absence of official data on refugee movements required us to obtain much of our preliminary information through key informant interviews at civil society organizations serving Syrian refugee communities in Turkey. The data we collected subsequently and present here is, to our knowledge the largest and most comprehensive survey of the political attitudes of Syrian refugees in Turkey.

4 Methods

4.1 Identification Strategy

As our identification strategy shapes much of our research design, it is helpful to describe it at the outset. A careful identification strategy is needed to differentiate claims of the type that "violence causes a higher proportion of people to have a given attitude" from findings that "those people most subject to violence are also those more likely to have a given attitude." That said, such an approach admittedly limits the types of questions we can hope to credibly answer. In our case, it restricts us to examining the impacts of barrel bomb-related harms, and specifically to *losing one's home to barrel bombing.* To explain our identification approach and why it focuses on barrel bombs, we next describe the nature of violence due to barrel bombing.

[3]The province-level numbers of registered Syrian refugees in Turkey are available at `http://www.goc.gov.tr/icerik6/gecici-koruma_363_378_4713_icerik`.

[4]The one exception is the report published by AFAD (2013) based on a survey conducted in 2013, long before barrel bombings were being used heavily in the Syrian civil war.

Indiscriminate Barrel Bombing and Conditional Randomness

Our identification strategy rests on the claim that *while barrel bombs can be targeted to one neighborhood versus another, their effects are indiscriminate within that neighborhood.* In other words, conditional on the neighborhood, we expect barrel bomb-related harms to be distributed effectively at random. Our principal argument for this claim is that the military targets neighborhoods for barrel bombing, but the targeting of specific individuals or even buildings within these neighborhoods is limited for two main reasons: (a) *technical limitations* and (b) *tactical purpose.*

First, regarding *technical limitations*, barrel bombs are imprecise weapons. Barrel bombs are improvised explosive devices (IEDs) dropped from helicopters and planes. They are typically oil barrels, fuel tanks or gas cylinders packed with explosives and metal fragments like nails and machine parts to increase their lethality. Barrel bombs are "inherently indiscriminate due to a lack of even the most rudimentary guidance system: they are rolled out the door of a moving aircraft or dropped from ropes slung below a helicopter, thereby removing any possibility of aiming them."[5] As these bombs fall, their trajectory is unpredictable. In other words, while barrel bombs may be targeted to the area of a block or so, precision targeting beyond this is impossible. Moreover, even once they hit a particular spot, the radius of damage from a barrel bomb's site of impact can reach about 500 meters, with some people surviving unscathed, some injured to various degrees and others killed.[6]

A second qualitative argument for this "indiscriminate-within-neighborhood" claim derives from what we understand to be the tactical purpose of using barrel bombs, which also explains why the regime would use such a poorly targetable weapon. We argue that the principal aim of barrel bombing has been to make the area inhospitable to civilians so they either withdraw support for the opposition or leave the city. Killing rebels themselves was not the main goal of these bombings; in fact, barrel bombings did not actually focus on the front lines where active fighting was occurring and where rebels were known to be operating. Rather, areas away from the front line were most heavily targeted, in an effort to clear them of civilians. Even if the regime did intend to target rebels or suspected sympathizers in these areas, the limited accuracy of the weapon results in placing the civilians within a neighborhood at similar risk to each other. Indeed, the inability to know where a barrel bomb will drop (when seeing and hearing it from the ground) prevents people from effectively avoiding harm – and makes them all the more terrifying. As the director of a Syrian NGO described: "Barrel bombs are weapons that do not accurately target. They are relatively cheap but random weapons that indiscriminately kill civilians and cause massive destruction to civilian infrastructure, especially in residential areas."[7]

4.1.1 Barrel Bomb-related Harms

The barrel bomb-related harms we measured were:

- Was the neighborhood you come from barrel bombed at some point? (Y or N): *bbomb_neighborhood*
- Were you present in that neighborhood during the time of any barrel bombing? (Y or N): *present_bbomb*
- Were you yourself physically injured by the barrel bombings? (Y or N): *injured_bbomb*
- Thinking about your spouse, siblings, children and parents only, how many of these family members were injured due to barrel bombing? *bbomb_fammember_injured*

[5] Assessment by Marc Garlasco, in a 2016 report by Handicap International entitled "Qasef: Escaping the Bombing. The use of explosive weapons in populated areas and forced displacement: perspectives from Syrian refugees" See: `https://d3n8a8pro7vhmx.cloudfront.net/handicapinternational/pages/2303/attachments/original/1475080189/Study_EWIPA-Syria-2016_Web_final.pdf?1475080189`

[6] Information gathered from discussions with medical responders in preliminary fieldwork in Turkey in October 2015.

[7] Interview conducted on October 4, 2015 in Istanbul, Turkey.

- Thinking about your spouse, siblings, children and parents only, how many of these family members were killed due to barrel bombing? *bbomb_fammember_killed*
- Was your home at that time destroyed or damaged so badly [by barrel bombs] as to make it unlivable? (Y or N): *bbomb_house_destroyed*
- Was your place of business destroyed by barrel bombing? (Y or N): *bbomb_business_destroyed*
- Can you please tell me what other assets belonging to you or your family were destroyed by barrel bombing? *bbomb_assets_destroyed*: (open-ended)
- Thinking about your spouse, siblings, children and parents only, were your family members injured due to indiscriminate violence (barrel bombs, shelling or rocket attacks)? (Y or N): *familyinjured_indisviolence*
- Thinking about your spouse, siblings, children and parents only, were your family members killed due to indiscriminate violence (barrel bombs, shelling or rocket attacks)? (Y or N): *familykilled_indisviolence*

We list all these harm-related questions for completeness, but our analysis in this paper focuses on *bbomb_house_destroyed* as the harm uniquely suited to credible causal inference. This is because a person's house is destroyed (or not) based on its location relative to where barrel bombs happen to strike. This does not depend upon how risk tolerant a person is (the way *present_bbomb* could be), or the behavior of one's family members (the way the family-related harms are). A complete description of the concerns that prevent us from using each of the measures above (besides *bbomb_house_destroyed*) is given in Section A.1 in the Appendix.

Accordingly, our primary results and our conclusions are driven by our analysis of the effects of *bbomb_house_destroyed*. While we will limit our analysis in this way to maximize the credibility of our identifying assumptions, it admittedly narrows what we can study. We emphasize that harms beyond having one's home destroyed clearly matter, both in human terms and in terms of their effects on civilian attitudes.

4.2 Survey Sampling

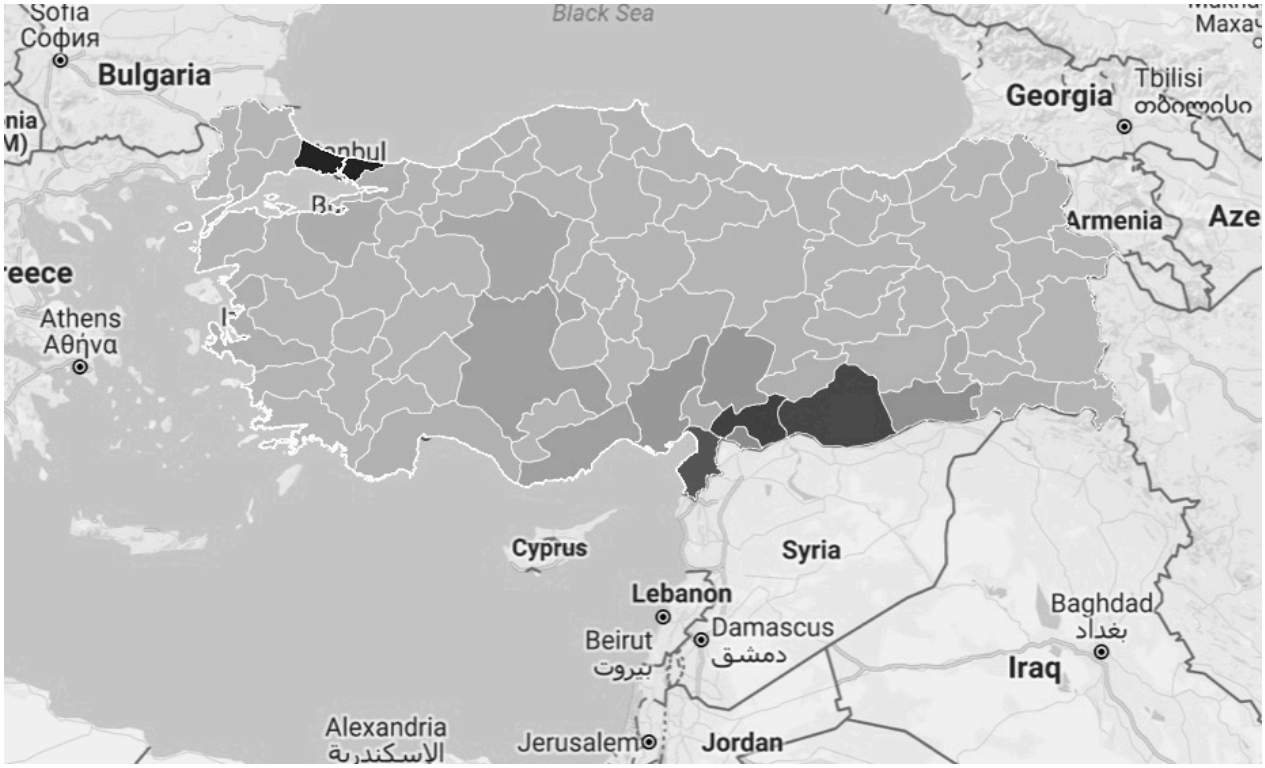
Our aim is not to be representative of Syrian civilians or Syrian refugees, but rather to sample individuals from the population of out-of-camp Syrian refugees in the four locations where we worked.

This involved three stages: First, we sampled Turkish provinces with the highest number of Syrians present: İstanbul, Hatay, Şanlıurfa and Gaziantep.[8] Syrians in these provinces comprise about 57% of all out-of-camp Syrians living in Turkey. Figure 1 shows all provinces of Turkey, with the number of Syrian refugees in each indicated by color. The four darkest areas are those in which we sampled. In earlier fieldwork in these provinces, we found that while Syrians residing in Gaziantep are mostly Sunni Arabs, Syrian Kurds live in Şanlıurfa, and Alawite Syrians are more likely to reside in Hatay.

The second stage of our sampling involved choosing districts within these provinces that have the highest concentration of Syrians, according to information we collected in 2015 during our interviews with NGOs that assist Syrian refugees in these provinces. In the third stage, within each neighborhood with a heavy concentration of Syrians, our enumerators randomly chose a street and then randomly selected households on that street. The enumerators asked if the household is Turkish or Syrian. If it is Turkish, they randomly selected another household on the street. If the household is Syrian, the male head of the household was asked to participate in the survey if he

[8] According to the figures provided by Directorate General of Migration Management (DGMM), at the time of our survey, the number of Syrians living in each of these provinces were as follows: İstanbul: 397,456; Hatay: 377,053, Şanlıurfa: 395,823, Gaziantep: 318,290.

Figure 1: Distribution of Syrian Refugees in Turkey



was at home.[9] If the male head of the household was not at home or (in a rare number of cases) if the female head of household opened the door, she was asked to participate. If the respondent agreed to participate, the enumerators proceeded to the survey. Having completed at most 10 interviews per street, our enumerators randomly picked another street and made the next round of interviews using the same sampling procedure.

The overall response rate (completed interviews divided by attempts) was approximately 34%, but this rate varied across different provinces: respondents agreed to participate per 5-6 attempts in İstanbul, per 2 attempts in Gaziantep and Şanlıurfa, and per 4-5 attempts in Hatay.

Outcome Measures

Here we briefly describe the most important questions for our outcomes of interest.

- In your opinion, which of the following groups I will read is the biggest security threat to the country of Syria?: *syria_threat1_ISIS*, *syria_threat1_Assad*, *syria_threat1_opposition*, *syria_threat1_international*, *syria_threat1_otherarmed*.
- In your opinion, which of the following groups I will read is the biggest security threat to you personally in a future Syria?: *personal_threat1_ISIS*, *personal_threat1_Assad*, *personal_threat1_opposition*, *personal_threat1_international*, *personal_threat1_otherarmed*.
- How closely do you follow the news about Syria?: *follow_syria*.
- Do you do any volunteer work for services that help Syrian refugees?: *volunteer1_bin*
- Which party to the conflict do you think most closely represents your interests?: *support_party_opp, support_party_none*
- If a member of your community refused to take a position in support of any side to the

[9]We would have of course preferred to use a Kish grid or other randomization procedure to choose who to survey within each household, however, this was determined to be too inconsistent with cultural demands.

conflict, would you approve?: *refuse_position*

- Would you be willing to give life saving assistance to a regime member?: *help_regime_member*

Geographic Variables Used for Identification

Obtaining the location of respondents' homes in Syria presented logistical challenges as only 18% were able to locate their homes using Google Maps on our enumerators' smart phone to obtain GPS coordinates. Instead, the most accurate method we were able to employ began by asking participants to identify governorate, city and neighborhood in which they used to live. If they are from a rural area, we then asked which governorate and village they are from. Using this information about respondents' original homes, we matched each respondent to one of the administrative units in Syria, relying on the list provided by United Nations Cartographic Section (UNCS) and United Nations Office for Coordination of Humanitarian Affairs (OCHA)[10]. For our respondents from the capital cities of each governorate such as Aleppo or Ar-Raqqa, these units are neighborhoods in these cities. For our respondents from outside the cities in each governorate, these units are either small provincial towns or villages. Overall, we were not able to match 135 respondents to a unit because either the respondent failed to provide any information or we were not able to match respondent's answer to the list of administrative units available to us.

Finally, the effectiveness of conditioning on neighborhood location to ensure barrel bomb harms are effectively random within these units depends on neighborhoods being relatively small. In the final matched sample utilized for analysis (see below), the mean and the median area of the 23 urban neighborhoods included are 1.25 and 0.94 square kilometers, respectively. The mean and the median area for the total 30 units including the provincial towns is 2.25 and 1.0 square kilometers. This is a reasonable size for our conditioning strategy.[11]

4.3 Estimation Procedures

To establish the quasi-experimental estimates of the effects of harm, our identification strategy requires (a) restricting our sample to those whose neighborhoods were barrel bombed, and (b) conditioning on location. We also condition on gender, because we have found a surprising imbalance on gender in the data, with more men than women reporting harms such as having their home destroyed. We remain uncertain as to why this is the case.[12] However, because this imbalance on gender could drive various differences between the groups, we match on gender in all of our analyses. As shown below, the results within each gender tend to be very similar to each other.

The necessary conditioning or covariate adjustment is done straightforwardly by matching on location indicators and gender, using the *Match* package (Sekhon, 2011). Because location and gender are discrete, matching is exact and there is no need for a bias correction due to inexact matches. This produces an average treatment effect on the treated (ATT) estimate, as each treated unit is matched to control units, or otherwise dropped if no control is available. The Abadie-Imbens standard errors for matching (Abadie and Imbens, 2006) are used to construct 95% confidence intervals, which we label as the "conventional" confidence intervals. However, because our estimator leaves relatively few matched units with which to construct each difference-in-means estimator, we also sought to employ a more robust inferential approach for purposes of inference. To this end, we also show the 90% and 95% boundaries of a null distribution generated

[10]The list of administrative units in Syria and their maps are available at `https://data.humdata.org/dataset/syrian-arab-republic-administrative-boundaries-populated-places`

[11]A circle with an area of 1 square kilometer has a radius of 564 meters. Given the inability to accurately target where a barrel bomb will land, plus the 500 meter range of destruction even around the point where it lands, conditioning on neighborhoods of this size helps considerably in ensuring individuals within these units have similar risks of having their homes destroyed.

[12]We note also that a larger percentage of men report large scale damage to their homes than women also in the survey conducted by the Prime Ministry Disaster and Emergency Management Authority in 2013 (AFAD, 2013).

by permutation inference. We take the unconventional approach of plotting results that show both the conventional confidence intervals (centered on the effect estimate), together with markers indicating the 90% and 95% boundaries of this permutation (sharp) null distribution, which is naturally centered around zero. We describe the permutation inference procedure in more detail in Section A.2 in the Appendix.

5 Results

We begin our analysis by presenting the distribution of relevant characteristics in our full sample as well as the the distribution of violence. Next we take a brief descriptive look at the relationship between barrel bomb-related harms and attitudes. Finally, we proceed to apply the best identification strategy we can in an effort to rule out alternative explanations.

5.1 Descriptives I: Distribution of Relevant Characteristics

We first describe the full sample, and then the restricted sample that will be required by our identification strategy.

Full Sample

The diverse geographic distribution of our sample of 1384 respondents is given in Figure 2, by Syrian governorate. While 67 percent of the respondents are from Aleppo, 10 percent are from Ar-Raqqa, and 9 percent are from Idleb. Among those from Aleppo, 40 percent came from barrel bombed neighborhoods. In Ar-Raqqa and Idleb, 21 and 44 percent of the respondents come from barrel bombed neighborhoods, respectively.

Our sample is relatively well balanced on gender, with 37 percent of our sample being female, despite the difficulty of interviewing Syrian refugee women that has affected other surveys of Syrians, in which the gender ratio is highly imbalanced (Giebler, 2015). Figure 3 shows the distribution of age in our sample together with several other demographics. The descriptive statistics for additional variables are shown for the full sample in Table 1 under the "full sample" columns. Most of our respondents are young or middle-aged Syrians. It is also important to note that when asked for the main reason why they left Syria, an overwhelming majority of our respondents specified security concerns, as opposed to other reasons, such as economic considerations, family reunification, escaping to Europe or avoiding conscription. 80 percent of respondents left Syria in 2013 or later, when the fighting became especially severe, and barrel bombs became a widely used form of attack by the Assad regime especially in Aleppo (Amnesty International, 2015). 13 percent of our full sample consist of Kurdish speakers.

One question that arises when limiting attention to out-of-camp Syrian refugees is to what extent they are representative of the whole Syrian refugee population in Turkey. First, it is worth emphasizing that only 7 percent of the refugees live in camps in Turkey, and we sampled from urban neighborhoods that host the majority of Syrians. Second, in a 2013 survey that allows a comparison of in and out-of-camp refugees, the two groups of refugees were similar in terms of gender distribution, while in camp refugees are slightly more educated, younger, and poorer.[13] In-camp refugees also report slightly more damage to their houses because of the war, and report slightly higher numbers of family members injured or killed because of the war (AFAD, 2013).

[13] A recent survey of in- and out-of-camp refugees conducted in 2017 by Erdogan (2017) partially replicate these findings: surveyed in-camp-refugees are younger and poorer but slightly more educated than out-of-camp refugees (109-122).

Figure 2: Full Survey Sample

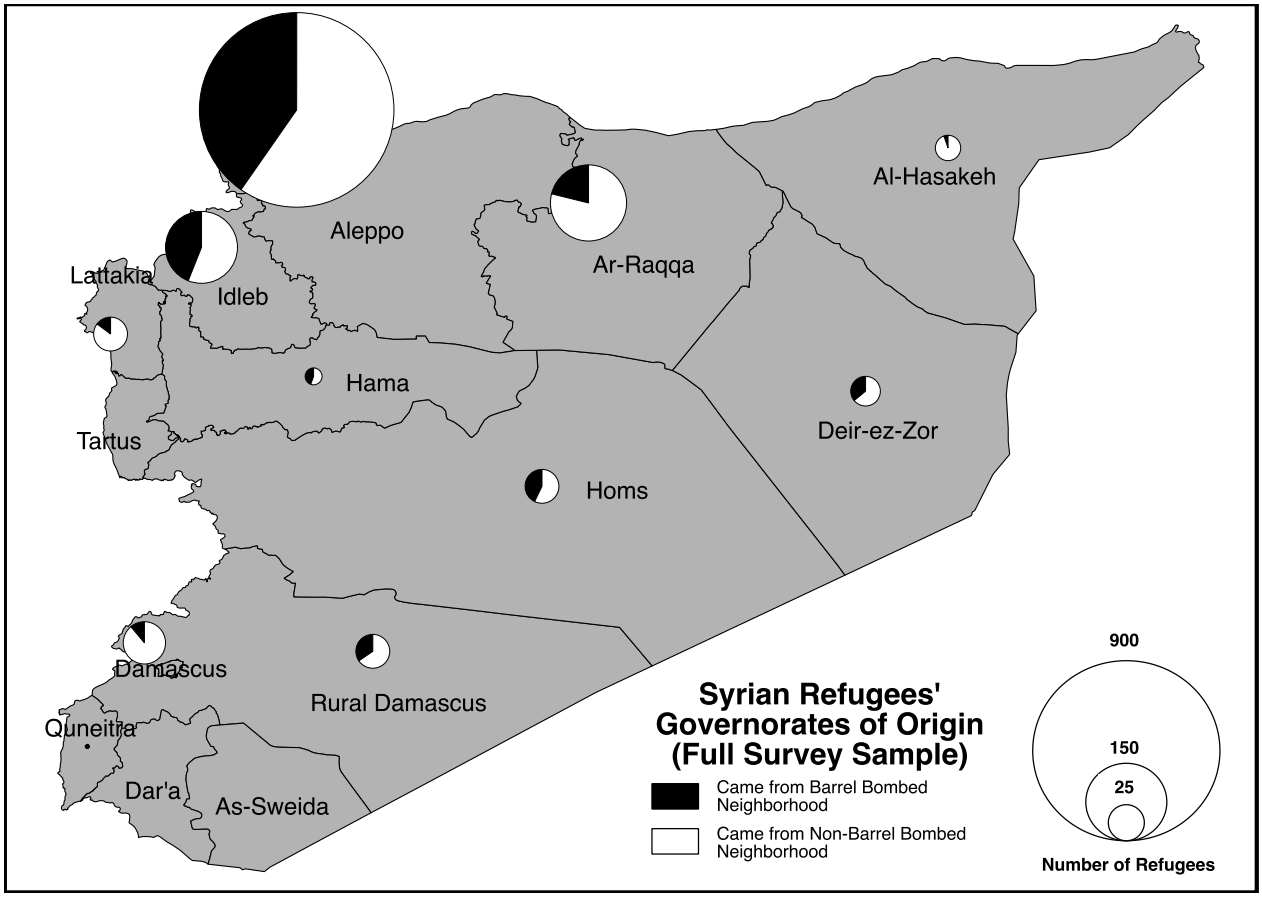
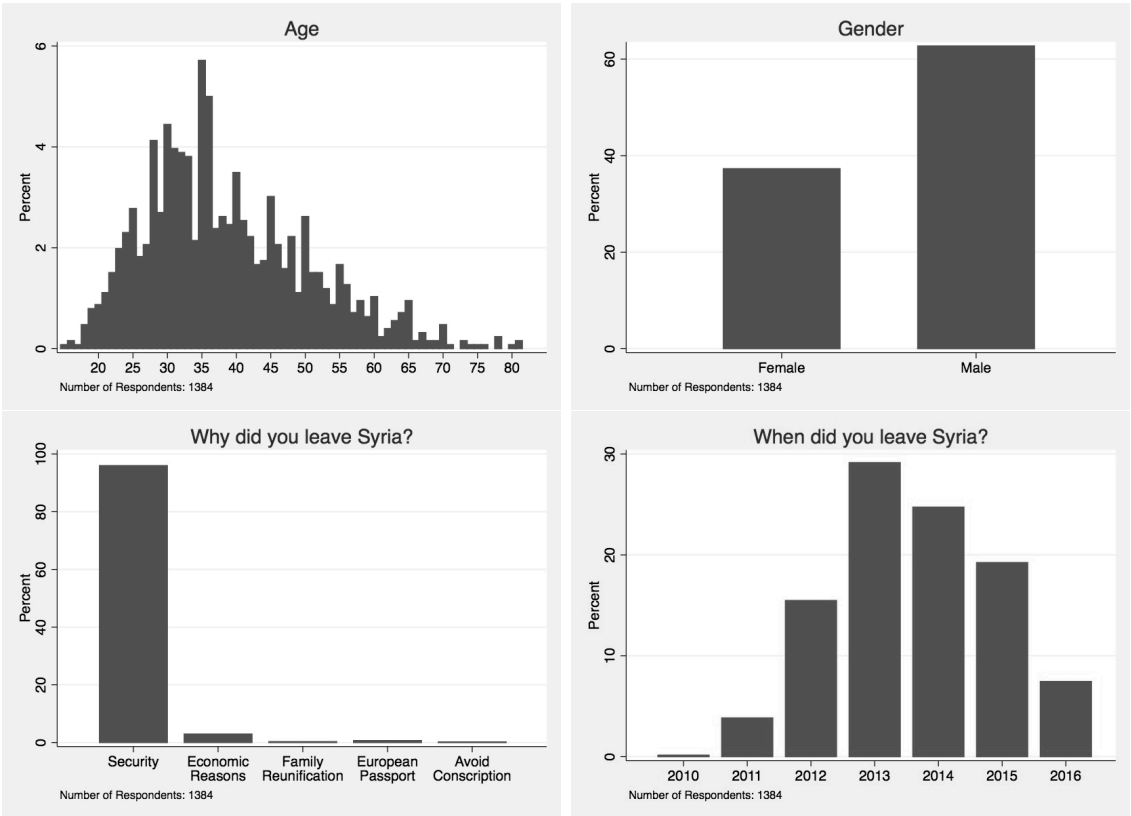


Figure 3: Key Demographic Descriptives



(a) Key demographic statistics: Age distribution (*top left*), gender distribution (*top right*), stated reason for leaving Syria (*bottom left*), and year departing Syria (*bottom right*).

Table 1: Descriptive Statistics in Full and Restricted Sample

	Full Sample			*Restricted Sample*		
	Mean	Std.Dev.	N	Mean	Std.Dev.	N
Demographics						
Male	0.63	0.48	1384	0.61	0.49	452
Age	38.61	11.86	1260	38.75	11.44	377
Worked in Syria	0.54	0.50	1366	0.57	0.50	449
Kurdish	0.13	0.33	1384	0.06	0.24	452
Education	1.59	1.03	1102	1.58	1.00	298
Has children	0.92	0.27	1384	0.92	0.28	452
Rooms	3.10	1.61	1334	3.27	1.74	439
Lived in an urban area in Syria	0.67	0.47	1353	0.75	0.43	444
Has family members in Syria	0.90	0.30	1384	0.85	0.36	452
Follows news	0.50	0.50	1384	0.61	0.49	452
Distance to market	2.11	0.95	1376	2.04	0.85	448
Distance to school	1.80	0.76	1379	1.82	0.67	450
Distance to hospital	2.77	1.02	1375	3.05	0.96	449
Year left Syria	2013.62	1.27	1383	2013.69	1.21	452
Exposure to Violence						
Barrel bombed neighborhood	0.37	0.48	1343	1.00	0.00	422
Present during barrel bomb	0.60	0.49	489	0.59	0.49	418
Injured due to barrel bomb	0.04	0.21	476	0.04	0.20	408
Family injured due to barrel bomb	0.10	0.30	482	0.09	0.29	412
Family killed due to barrel bomb	0.07	0.26	481	0.08	0.26	411
House destroyed due to barrel bomb	0.67	0.47	449	0.68	0.47	387
Business destroyed due to barrel bomb	0.35	0.48	299	0.33	0.47	250
Family injured (indiscriminate violence)	0.11	0.31	1055	0.19	0.40	270
Family killed (indiscriminate violence)	0.09	0.28	1052	0.18	0.38	268
Outcome Variables						
Assad top threat to Syria	0.35	0.48	1267	0.47	0.50	405
Assad top personal threat	0.48	0.50	1258	0.54	0.50	400
International powers top threat to Syria	0.04	0.19	1267	0.06	0.25	405
Volunteer work for Syrians	0.64	0.48	1225	0.84	0.36	406
Refuse to take a position	3.00	1.47	1290	3.48	1.69	413
Help regime member	0.54	0.50	1224	0.55	0.50	408
No group represents me	0.49	0.50	1384	0.70	0.46	452
Opposition represents me	0.50	0.50	1384	0.30	0.46	452

(a) Descriptive statistics for *full sample* and for the *restricted sample*. The former gives a sense of the group from which we were sampling as a whole. The latter, described below, restricts attention to those individuals who report their neighborhood was barrel bombed, and whose neighborhood identifier is non-missing, as these restrictions are required for the better identified comparison we make below.

Distribution of Violence

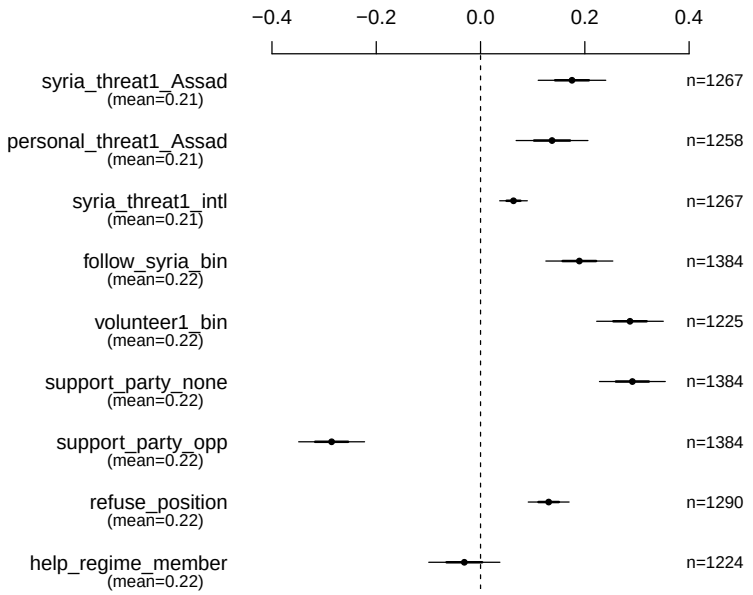
While our identification strategy requires focusing narrowly on one type of violence, for descriptive purposes we report the levels of violence experienced by participants in Table 1. Some forms of violence we asked about were extremely low and so not reported here: fewer than one third of one percent reported an injury or death due to insurgent violence, either in their family or neighborhood. Rates were similarly low for questions about torture and injury or death due to sniper fire. We also inquired about family members injured or killed by barrel bombs, shelling, and rockets. Since the rates were low for all three, we join them together on Table 1 as "Family injured (indiscriminate violence)" and "Family killed (indiscriminate violence)".

About 37 percent of our sample came from barrel bombed neighborhoods and 60 percent of those who are from barrel bombed neighborhoods were personally present during an attack. 67 percent of these people also report to have lost their homes to barrel bomb attacks. 7 and 10 percent of our respondents from barrel bombed neighborhoods report family members being injured or killed. There are small differences between these figures and the percentages for respondents with injured or killed family members due to indiscriminate violence, which includes shelling and rocket attacks in addition to barrel bomb attacks.

5.2 Descriptives II: Naive Comparison

Before applying an estimation procedure consistent with our identification assumptions and strategy, Figure 4 presents a naive comparison, simply showing the observed relationship between losing one's home to barrel bombing and the outcomes of interest in the full sample.

Figure 4: Naive Comparison



(a) Descriptive examination of relationship between losing home to barrel bombing and the outcomes of interest.

In this completely uncontrolled comparison, the results most central to our question of interest show that those who lose their homes to barrel bombing are far more likely to say they support no political actors (*support_party_none*), and are less likely to support the opposition (*support_party_opp*) than those who did not lose their homes to barrel bombing. They are also more likely to accept a community member who refuses to take a position on the conflict (*refuse_position*), while we see no difference on their willingness to provide life-saving aid to a regime member in need (*help_regime_member*).

Before making too much of these results, we note that nearly everything is highly significant, which is concerning. More importantly, we have not yet invoked the identification strategy discussed above. These results may therefore be due to confounding and must be understood entirely as descriptive. We next seek to rule out alternative explanations based on confounding by employing the identification described in the Methods Section.

5.3 Analysis under Identification Strategy

Restricted Sample: Respondents From Barrel Bombed Neighborhoods

Our identification approach requires restricting the sample to those whose neighborhoods were reportedly barrel bombed. This reduces our sample size to 493. It also requires dropping those for whom we were unable to get sufficiently good information on the location of their neighborhood, trimming an additional 51 observations. Table 1 provides an overview of descriptive statistics computed instead on this "restricted sample".

Moreover, the matching procedure changes the sample about whom an inference is made beyond these general restrictions.[14] The resulting "post-matching sample" from which the results below are reported has 341 respondents (when there is no missingness on the outcome), spread out across 30 geographic units with similar proportions of treated and control in each (see Figure 5).

23 of these units are urban neighborhoods in cities, mostly in Aleppo, and the remaining 7 are small provincial towns. The matched sample is 37.5 percent women, with a mean age of 39. About 95 percent of respondents left Syria in 2012 or later. The majority (76 percent) of these individuals are from the governorate of Aleppo, and out of these, 88 percent are from the city of Aleppo.[15] The share of refugees from Aleppo city is larger because the share of refugees from Aleppo in our full sample is high (67 percent) to begin with, and because among all governorates in Syria, Aleppo has suffered the most concentrated number of barrel bombings and civilian deaths. According to an interview with a representative of the Violation Documentation Center, Aleppo suffered 3,124 barrel bomb related deaths between January 2014 and March 2015, with the most intense campaigns occurring in the fall of 2014. Barrel bombing in Aleppo subsided in February 2015.[16] 12 percent of our post-matching sample comes from Idleb, and 4.5 percent are from Ar-Raqqa. Note that, due to sensitivities we were prevented from asking about religious affiliation or sectarian identity of the respondents. However, the neighborhoods of Aleppo included in our sample are known to be mostly Sunni, while none of them are Alawite, the sect of the president Bashar Assad (CAERUS, 2014, 91-93).[17]

5.4 Effects of Losing Home to Barrel Bombing

We now turn to our quasi-experimental strategy for estimating the effect of having one's house destroyed by barrel bombing.

Conditional Balance

As a first step, we assess "conditional" balance. If we believed that having your home destroyed by barrel bombs was entirely random, we could check balance in the usual (unconditional) way – i.e. check that the distribution of (pre-bombing) covariates among those whose homes were destroyed looks similar to the distribution among those whose homes were not destroyed. However, here our assumption is not that barrel bombing is unconditionally random, but rather that those within a

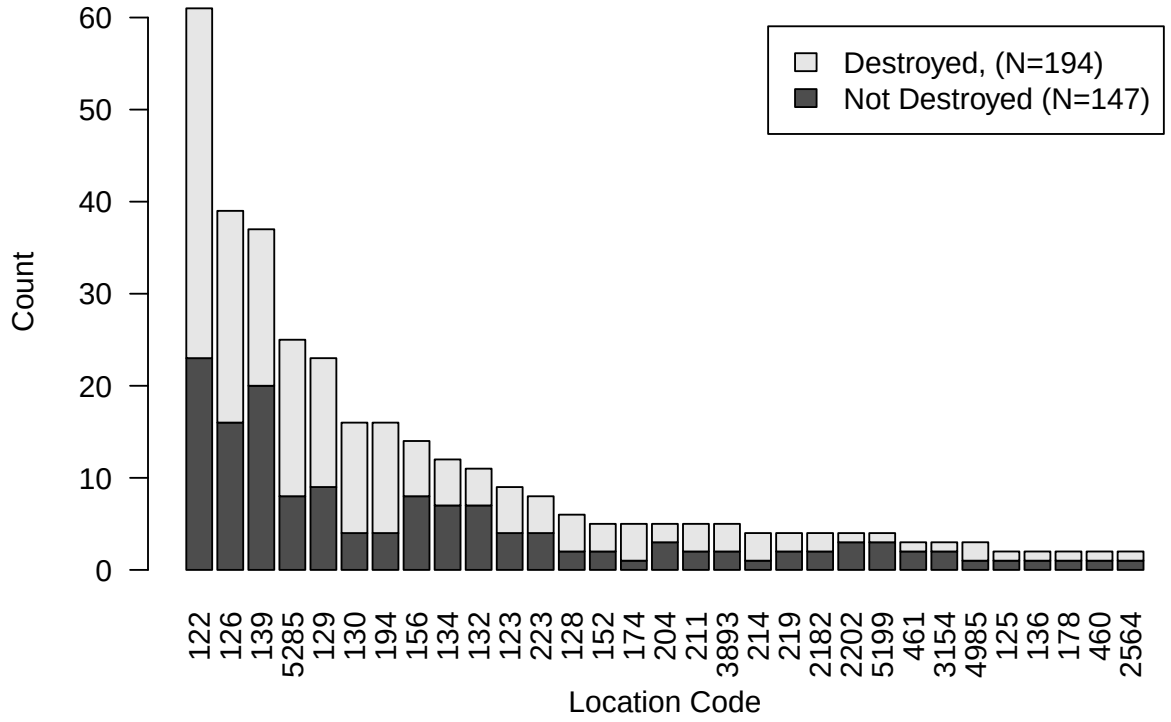
[14]First, the process of conditioning on location and gender by matching effectively limits the sample about which we are making an inference to those unit-gender strata in which there is at least one individual whose house was destroyed and one whose was not remain in the matched sample. Second, for those individuals that remain, for each individual who lost their home to barrel bombing, a comparable individual who did not lose their home is found. The aim in doing this is to make the distribution of characteristics among the "untreated" like that of the "treated". This leads to estimating an average treatment effect *among the treated* – and to a post-matching sample that is not meant to be representative of the original sample or its target population.

[15]See Figure 6 for the neighborhoods of Aleppo city that are included in our sample.

[16]Interview conducted on October 1, 2015 in Istanbul, Turkey.

[17]The only neighborhoods in our sample that are mixed with Muslim and Christian or Yazidi residents are Ash-Sheikh Maqsoud and Ashrafiyeh.

Figure 5: Post-Matching Sample by Geographic Unit



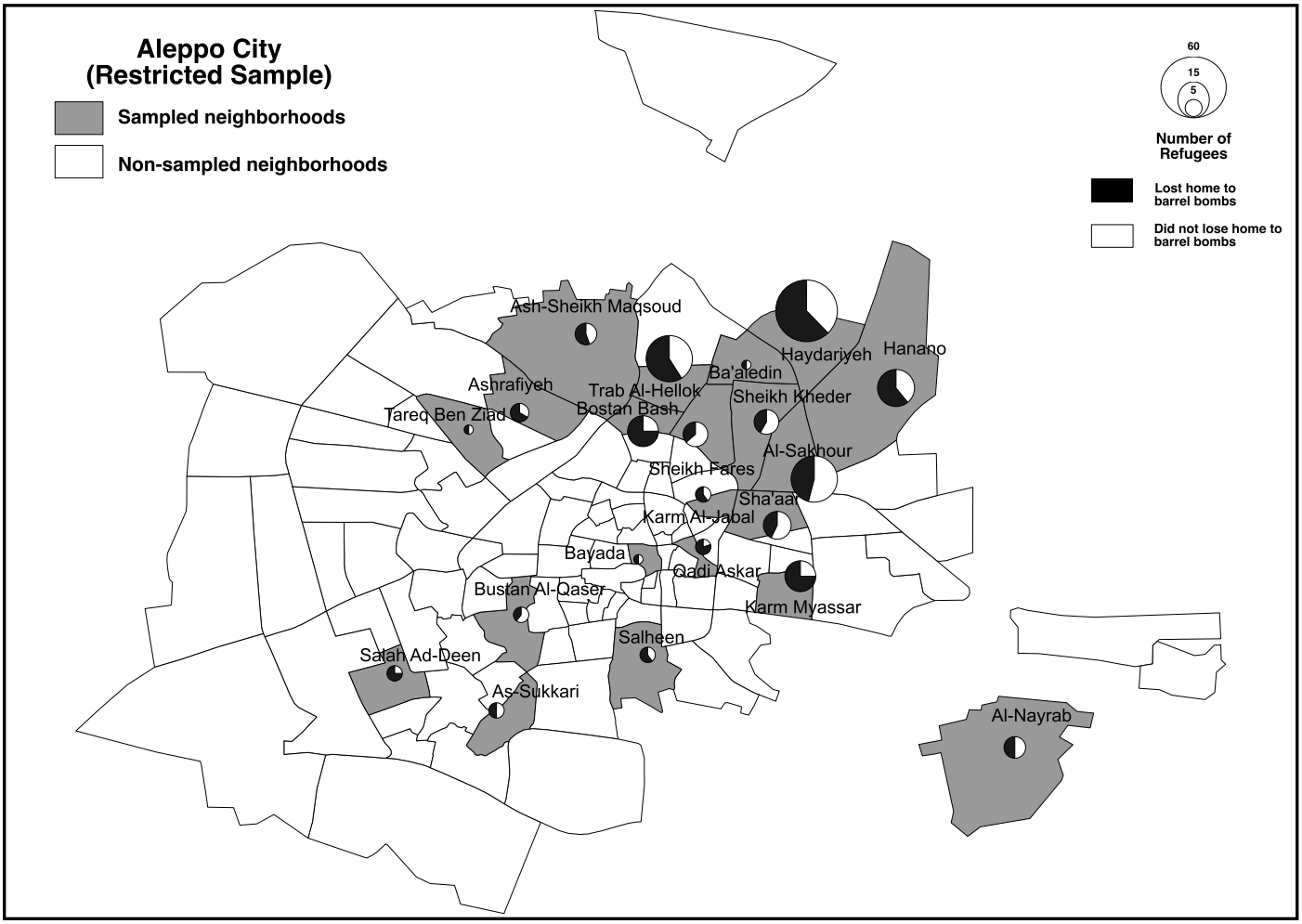
(a) Barplot showing each neighborhood (labeled numerically). The top (*light gray*) bar shows the number whose homes were destroyed in that area; the bottom (*dark gray*) shows the number whose homes were not destroyed in that area. *Aleppo city*: Haydariyeh (122), Ash-Sheikh Maqsoud (123), Ba'aiedin (125), Trab Al-Hellok (126), Ashrafiyeh (128), Hanano (129), Bostan Bash (130), Sheikh Fares (132), Sheikh Kheder (134), Tareq Ben Ziad (136), Al-Sakhour (139), Karm-al Jabal (152), Sha'aar (156), Qadi Askar (174), Bayada (178), Karm Myassar (194), Bustan Al-Qaser (204), Salheen (211), Salah-Ad-Deen (214), As-Sukkari (219), Al-Nayrab (223). *Aleppo governorate*: Hadher (2182), Al Bab (2202), As-Safira (3154). *Ar-Raqqa city*: Andalus (460), Al-Thawrah (461). *Idleb governorate*: Ma'arrat An Nu'man (4985), Jisr-Ash-Shugur (5199), Maarbalit (5285). *Homs governorate*: Sokhneh (3893).

given neighborhood (and gender) have equal chances of having their home destroyed. As noted, the neighborhood units we condition on are relatively small, with median areas of approximately a kilometer in both urban and rural areas. Thus, the appropriate balance test is a conditional one: conditional on neighborhood and gender, do those whose homes were destroyed look similar to those whose homes were not? To test this, we condition on neighborhood and gender by exact matching (see Effect Estimates subsection), and consider each pre-treatment covariate as if it were an outcome. There should be no "effect" of having one's house destroyed on these covariates, if the identification assumption holds.

We use this procedure to check conditional balance on all the covariates we have that (a) show variation (at least 5 percent in the minority category for binary variables), and (b) are convincingly "pre-treatment", i.e. we are sure they are unaffected by barrel-bombing. These include the (log) number of rooms in the house (*rooms.log*), whether the person worked prior to leaving (*work_before*), ordinal measures of how long it takes to walk to the nearest market, school, or hospital (*walk_market*, *walk_school*, *walk_hospital*)[18], number of children prior to the crisis (*children*), and age_std (*age* in years, standardized to improve visualization). We note that such a conditional balance test is also effectively a placebo test, where pre-treatment covariates

[18]Each of these *walk_x_ordinal* variables is coded as 1 (less than 5 minutes walk); 2 (5-15 minutes walk); 3 (15-30 minutes walk); and 4 (more than 30 minutes walk).

Figure 6: City of Aleppo



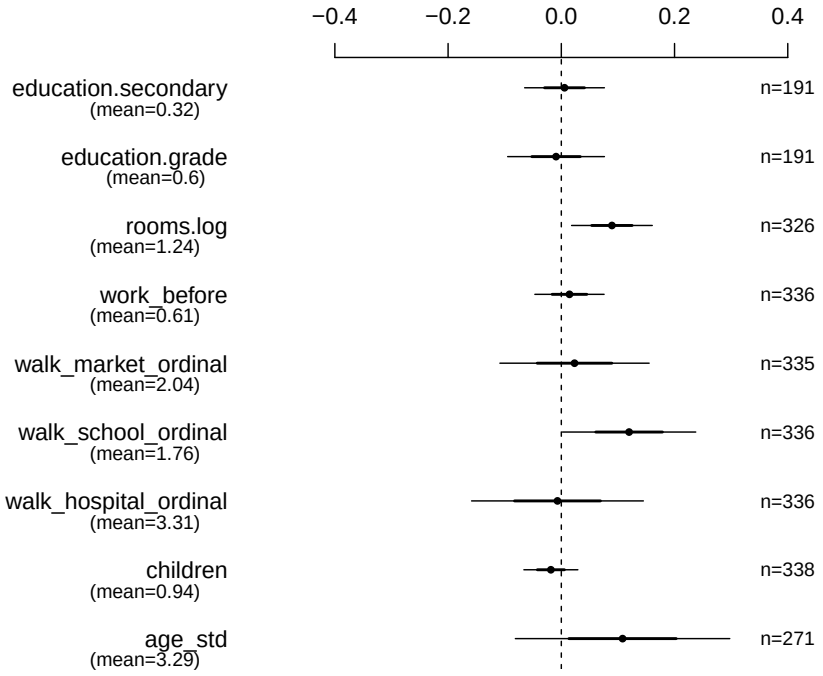
are used as if they were outcome variables that we know should not be affected by the treatment. We also include grade school and secondary education (*education.secondary* and *education.grade*). We exclude higher levels of education because they show little variation, and also risk being post-treatment, whereas primary and secondary education are most likely unaffected by barrel bombing in our sample since all participants are at least 18 years of age.

Our conditional balance/ placebo tests referenced in (Figure 7) are largely consistent with a case in which home destruction by barrel bombing is random conditional on neighborhood. Of course, they cannot show if unobservables are also balanced, and hence are not dispositive. We note potentially concerning imbalances on two variables. First, homes with more rooms were more likely to be destroyed. This is sensible, simply because homes with more rooms are larger and thus more likely to be hit. It could of course generate an imbalance (and bias) with those who have their homes destroyed more often being among the better off socio-economically. However, we note that education is well balanced, making socio-economic status likely to be balanced as well.[19]

The other variable not well balanced is the distance to the nearest school. Those exposed to barrel bombing tend to have lived slightly farther from the nearest schools. We are not entirely sure how to interpret this result. One plausible explanation relates to wartime education programming. In the phase of the war when these bombings occurred, many schools were run by the community and sometimes moved from their original location. As schools were scattered and moved to avoid violence, efforts may have been made to place them in areas thought to be relatively safer, even

[19]We did check balance on self-reported socio-economic status in Syria (low, low-middle, middle, upper-middle, or upper) as well. While it proved to be well balanced, we came to realize that socio-economic status is potentially post-treatment, since losing one's home could affect one's answer on this question. Thus we rely on education instead as reported.

Figure 7: Conditional Balance/ Placebo Test



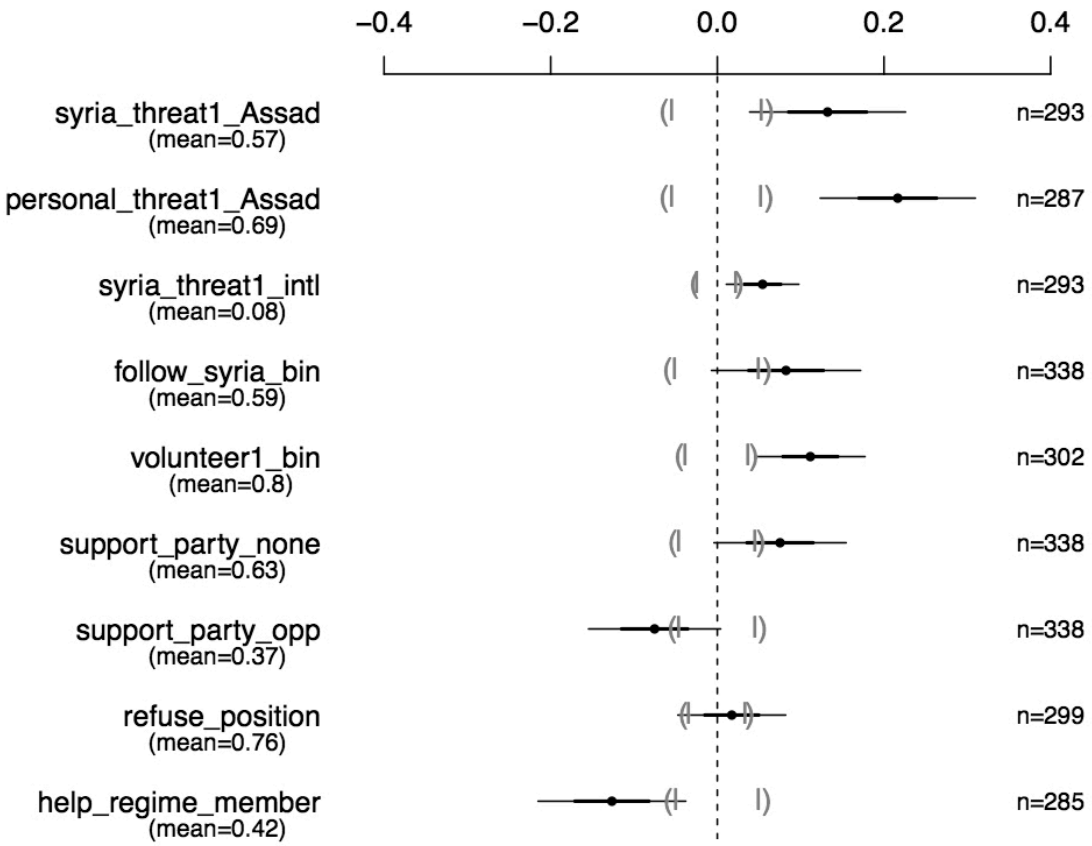
(a) Plots showing balance, after conditioning on neighborhood and gender as per our main identification and estimation strategy. Each pre-treatment covariate is treated as if it were an outcome variable, and the "effect" (imbalance) of the treatment is estimated on each via exact matching on neighborhood and gender.

within a given neighborhood, such as buildings with underground space. Such wartime activity may create the observed imbalance. However, we have no expectation that distance to these school locations would be associated with the outcome so as to make it a confounder. Furthermore, in the sensitivity analysis presented in Section A.4 in the Appendix we show that the confounding that would be due to such a covariate – or to an unobservable similarly related to both bombing and outcomes – would have very little impact on our estimates.

5.5 Effect Estimates

Our main effect estimates are shown in Figure 8 (numerical results are available in Section A.3 in the Appendix). These plots allow for inference both by conventional tests and permutation inference at the same time. We show ATT estimates with "conventional" 95% confidence intervals (using the Abadie-Imbens standard errors for matching) so if readers would like a traditional test of the p-value at the 5% two-sided level, they can perform the usual visual test, seeing if these intervals include zero or not. However our intended analyses rely on permutation inference. For this, we indicate the 90% and 95% percentiles of the permutation null (the "sharp null") around 0 using "|" and ")" symbols respectively. Point estimates (solid dots) that fall with the 90% ("|") indicators are points for which we cannot reject the null hypothesis of no effect with p-values of 0.10. Point estimates outside the 95% interval (red "(" and ")" indicators) are those for which we can reject the null at the conventional p-values of 0.05 or less. And those point estimates falling between the "|" and ")" markers are those where the p-value would fall in the marginal range between 0.05 and 0.10. We make our inferences based on the permutation test rather than the classical one, as it is assumption free. However, for the most part the methods agree, and we report both p-values below to avoid selective reporting.

Figure 8: Effect Estimates: Home Destroyed by Barrel Bombing



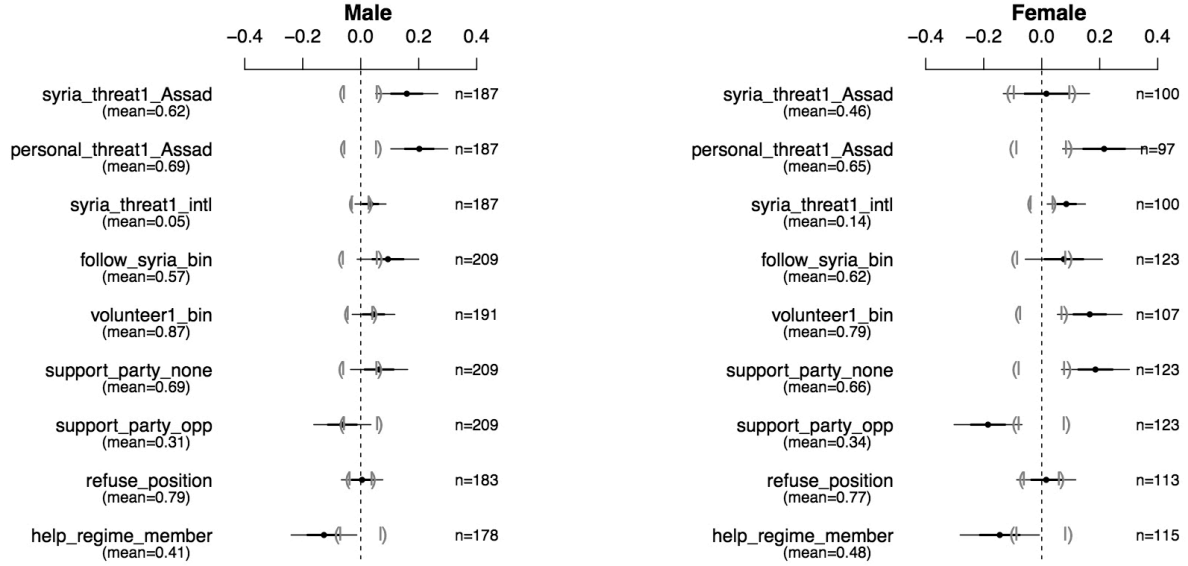
(a) ATT estimates for the effect having house destroyed on attitudes related to security. Conditioning on neighborhood and gender is done by exact matching. Confidence intervals by classical inference (whiskers) and boundaries of the sharp-null distribution (90% given by "—"; 95% given by ")".)

We note first several basic findings and "sanity checks". Those who lost homes to barrel bombing are 13 percentage points more likely to find Assad as the number one threat to the country, and 22 percentage points more likely to say Assad is the number one threat to them personally, both of which are highly significant by either the conventional test or permutation inference ($p < 0.005$ for all tests). We consider this result unsurprising, and largely a sanity check. Those who lost homes to barrel bombing were also 5 percentage points more likely to say that international powers are the primary threat to Syria ($p = 0.023$ by permutation; $p = 0.027$ by conventional). Next, we find that those who lost homes to barrel bombs are more likely to follow Syria "very closely" or "somewhat closely" in the news by 8.25 percentage points ($p = 0.006$ by permutation test; $p = 0.065$ by conventional).

Turning to our key variables of theoretical interest regarding civilians' attitudinal responses vis a vis armed groups: support for the opposition (*support_party_opp*) is 7.5 percentage points *lower* among those who lost homes to barrel bombs ($p = 0.006$ by permutation, $p = 0.057$ conventional). This runs contrary to what is expected in a "captive civilian" scenario. Likewise, those who lost homes to barrel bombs are 7.5 percentage points *more* likely to say that "no party" represents their views ($p = 0.007$ by permutation, $p = 0.057$ by the conventional test). Losing one's home to barrel bombing thus does not apparently push people to take sides – nor does it make them think that others should choose sides: the estimated effect of *refuse_position* lies near zero, indicating little or no change in the permissibility of remaining neutral in the conflict.

Coming to attitudes towards others in the community, those losing homes to barrel bombings are much more likely to report supporting their community through volunteer work, by 11 percentage points ($p < 0.001$ by permutation and conventional tests). This evident effect on social cohesion is apparently parochial, rather than reflecting a general increase in altruism towards others: those who lose homes to barrel bombs also prove far less willing to provide lifesaving support to a regime member, by 12.7 percentage points ($p < 0.0001$ by permutation; $p = 0.003$ by conventional).

Figure 9: Effect Estimates by Gender



(a) ATT estimates for the effect having house destroyed on attitudes related to security, by gender. Conditioning on neighborhood and gender is done by exact matching. Confidence intervals by classical inference (whiskers) and boundaries of the sharp-null distribution (90% given by "—"; 95% given by ")".

5.6 Results by Gender

We further split the sample by gender and conduct the same analysis in both subsets. We had not planned hypothesis tests regarding gender and make no claims regarding gender. Rather, the purpose is to (a) see if the results would "replicate" well in these two independent samples, and (b) as discussed below, men likely face more severe problems of selection into our sample, which makes this comparison useful to rule out such selection as the source of our effects. Results are shown in Figure 9. Some variation and loss of statistical power are expected. Regardless, the overall pattern of results is very similar across genders. While women do not show the same increase in reporting Assad to be the greatest threat to Syria, they do show the increased personal threat from Assad, and very similar increased international threat. They have similar effects as men on other variables – but show markedly stronger effects on they key variables of support for the opposition and support for no party. Women also show a more pronounced effect on volunteership.

6 Discussion of Findings

Given our the theoretical considerations related to displacement, we find that losing one's home to barrel bombing has effects on attitudes toward allegiances, side-taking and community solidarity. In a theory that treats civilians as captive actors who must choose sides, one might expect that

having your home destroyed increases the probability of seeing the opposition as the political group most closely representing you. Importantly, our effect estimate lies widely in the opposite direction. This is more consistent with the idea that displaced civilians respond to such losses not by flocking to the opposition, but by either remaining unchanged in their support or withdrawing it. Further, we find a significant rise in the proportion of people finding that *no group* represents their interest. Again this is consistent with withdrawal of support in response to violence, which civilians can get away with when they exit the war theater, but which they may not be able to afford if captive in the conflict zone. Similarly, while captive civilians are expected to choose sides – and would expect others to choose sides – our estimate for *refuse_position* is fairly precisely estimated to be near zero.

Perhaps the most likely bias given our design would, we argue, have the opposite effect: if some individuals are more targeted by the regime, these individuals are apt to be the more pro-opposition. Hence, if some characteristic makes some individuals (within a given neighborhood) more likely to have their homes destroyed, we would expect these individuals to be more pro-opposition. Such a confounder would produce an apparently positive relationship between home destruction and pro-opposition attitudes, while we observe the opposite. We further examine such potential confounders and robustness to them in the next section.

Collectively, we interpret these findings as evidence that in instances of mass displacement, civil war dynamics are altered in important ways. First, with respect to debates on the effectiveness of indiscriminate violence during counterinsurgency campaigns, we find evidence that indiscriminate violence indeed pushes civilians away from the party who perpetrated it. More importantly, however, because these civilians were able to flee and hence to protect themselves from future predation by all sides, refugees subjected to government barrel bombing can withdraw their support from *all* armed groups rather than turn to the enemy of the perpetrator. Second, a dominant theme in civil war research sees macro-level dynamics emerging from a patchwork of shifting local bargains and alliances (e.g.Kalyvas, 2006; Christia, 2012). These alliances, while fluid, are theorized to result from groups and individuals deciding who to side with, in part out of security considerations. While this may be an apt description when civilians are captive and must make such choices, this logic need not operate among civilians who have fled the war zone. Third, once out of the war theater, we find that refugees facing additional losses from violence are more likely to show heightened community solidarity *within* the refugee community as evidenced by their reported increase in volunteer-ship. To this end, our paper also contributes to a growing literature on the effects of exposure to violence on civic and social behavior. As Bauer et al. (2016a) reviews numerous findings that violence increases in-group social cohesion, we also find increased volunteerism. This apparent increase in volunteerism, however, does not extend to regime-members – this may be a bridge too far for those who have suffered at the hands of the regime, or it may reflect that the apparent increase in community solidarity after violence is of a parochial nature.

6.1 Robustness and Threats to Validity

We consider here two main threats to validity: remaining confounders (or selection into "treatment"), and selection into our sample.

Remaining Confounders

We emphasize that while our approach is more credible than observational studies that fail to articulate an identification strategy rooted in plausible assumptions, it too depends on unprovable assumptions. Here we discuss threats due to possible remaining confounders by qualitatively considering what types of confounders could explain away our results. In the Appendix, we provide a formal sensitivity analysis.

If the neighborhood units we used are too large and targeting is possible within these areas, it is easy to think of confounders such that individuals whose homes are more likely to be destroyed

are *more* likely to be pro-opposition. For example, being pro-opposition itself could lead one's building (or other sub-neighborhood area) to be known as sympathetic to the opposition, and thus subject to greater targeting. This would drive a positive relationship between having one's home destroyed and being pro-opposition. We observe just the opposite. It is more difficult to think of confounders that would make those who are more likely to lose their homes also be *less* supportive of the opposition. Similarly, it is difficult to think of confounders that would make those who are more likely to lose their homes also be *more* supportive of "no" party. Thus, while we cannot rule out all possible unobserved confounders, we struggle to identify a likely candidate that could produce the results we observe.

Risk of Differential Selection into Sample

Our identification strategy would best complement a sampling strategy of randomly sampling households that had lived in each neighborhood where bombing occurred. Unfortunately, this was not feasible, and we are instead restricted to working with refugee populations. This introduces additional concerns, as individuals select into the populations from which we can sample them.

Concerns of this type plague research on refugees in particular, as some process determines whether and where they move to begin with. In general, if the process that determines where people move to is a function of peoples' attitudes (or other characteristics) but is not affected by or related to whether they lost their homes, this does not undermine our results: it affects the population we make inferences about, but not the validity of the causal claim within that population. However, if the process that determines someone's chances of entering our population (moving to Turkey) is a function of *both* their attitudes (or other characteristics) and whether they lost their home, this problematically generates a difference between the type of people in our sample who did and did not lose their homes.

We consider here three examples of possible scenarios regarding these selection pressures and how they may relate to home destructions:

(1) Suppose that hardcore opposition supporters are most reluctant to leave Syria. Yet, if their home is destroyed, it may push them to leave, possibly coming to Turkey. Alternatively, suppose the opposite: that those more supportive of the opposition generally come to Turkey, while the less supportive (or pro-regime) are reluctant to leave – unless their home is destroyed.

(2) Assume that barrel bombing monotonically makes everyone more pro-opposition, and those who are very highly pro-opposition are less likely to become refugees. Further assume that there is variation in initial levels of opposition support.

(3) Another, more complicated class of cases are those in which barrel bombing *heterogeneously* or even non-monotonically affects attitudes. Specifically, the effects of losing one's home on attitudes would have to depend upon individuals' prior attitudes toward the opposition. Among the simplest example we can construct is a scenario that involves three assumptions: (i) First, suppose there are two types of people, defined by their reactions to losing their homes: those who will become more pro-opposition, and those who will not change. (ii) Second, suppose that individuals whose (prior) level of pro-opposition support is high, also have a higher probability of being the first type, i.e. they react to losing their homes by becoming more pro-opposition. (iii) Third, assume that becoming even more pro-opposition increases one's chances of joining opposition insurgents rather than becoming a refugee. The consequence of these three assumptions would be that those who had their homes destroyed *and* who were already opposition supporters (and became even more pro-opposition once bombed) will be under-represented in the sample, driving down the apparent opposition support among those *in the refugee sample* whose homes have been destroyed.

How would each of these affect our estimates? In scenario (1), if it is the pro-opposition individuals who are reluctant to leave Syria unless prompted by losing their home, this is unproblematic: the bias generated would be opposite to our estimate. However, if it is instead the relatively

pro-regime individuals who are reluctant to leave, this would pose a problem, as it could be an alternative explanation for our findings on *support_party_none* and *support_party_opp*. We cannot entirely rule this out, though this scenario seems unlikely to us – relatively pro-regime individuals would not be apt to stay in the highly opposition-dominated areas our participants come from. They may leave to other areas rather than Turkey, but then their chances of coming to Turkey at some later point would not likely depend upon their house being barrel bombed.[20]

Regarding scenarios (2) and (3) above, we also cannot entirely rule these out, but we argue they are unlikely to account for our results for several reasons. First, in both of these scenarios, the effect of the missing-opposition-supporters on the estimate would be mitigated or reversed by the effect of increased opposition support among those who were not yet at the threshold to become fighters. Second, if the most pro-opposition individuals who also had their home destroyed are simply missing from the sample, one would also expect a (false) result on *refuse_position* because many of those who would say that it is unacceptable to refuse taking a position would be missing from the treated sample. But this effect was close to zero and among our most precisely estimated. Third, our strongest argument against selection processes widely affecting our estimates lies in the similarity of results by gender. Figure 8 referenced above shows all the effect estimates separately for male and female respondents. The striking similarity of results for men and women is reassuring from a statistical perspective. Moreover, we conducted this analysis because if there are selection processes that drive some individuals to stay behind in Syria in support of an armed group, they surely would have a greater impact on males in our sample, as they make up a strong majority of those fighting. Nevertheless, we see very similar results for men and women, including on *support_party_none* and *support_party_opp*.

7 Conclusion

The effects of indiscriminate violence on civilians – especially when those civilians can and do leave the conflict zone – are not well understood. We have studied the effects of indiscriminate violence among displaced civilians by conducting a survey of Syrian refugees in Turkey. Using a quasi-experimental research design, based on the indiscriminate use of barrel bombs, our goal is to understand how losses from violence shape attitudes.

Among a captive population, when faced with violence by one party (the regime, here), casting one's support towards the opposing party sounds plausible. One might suspect that even where populations can escape instead of take sides, a reactionary and/or anger-driven side-taking may occur nevertheless. However, instead, we find that among civilians who have escaped, they no longer react as if they are forced to make such a choice: anti-regime need not be pro-opposition, pro-ISIS, or anything else. Nor do we find evidence that this form of violence causes victims to view those who do opt to remain neutral less favorably. While those whose homes are destroyed in regime-inflicted barrel bombing are more likely to see the regime as the greatest threat, both to the country and to their own security, we argue that for civilians who can remove themselves from the conflict zone, the usual logic that forces communities to politically align with one armed group or another is not in operation. Rather, it is entirely possible for civilians harmed by one side to withdraw support from both sides, not negatively judge those who remain neutral, and instead engage in civic activities that benefit the civilian refugee community.

Understanding how civilians respond to experiences of violence in terms of their support for armed groups is critical for illuminating the dynamics of conflict, and so has understandably spawned considerable theoretical and empirical work. Yet, work on both the logic of violence and civilian responses to it has focused principally on cases where civilians are assumed to be captive in the conflict zone. By contrast, the enormous numbers of refugees around the world – many forced out of their homes by civil conflict – demand that we also begin to understand conflicts

[20]Furthermore, the negative relationship we see between losing one's home and *help_regime_member* would be surprising if this scenario is driving our effect estimate. At a minimum, the effect estimate on *help_regime_member* we have would be a conservative one if this selection process is occurring to any degree.

in which civilians flee. We hope that our work here provides an initial finding to be tested by other investigators operating in this and other conflict areas, regarding how the choices made by civilians who leave conflict zones differ from those who remain. More broadly, we hope to stimulate further theoretical and empirical work on questions such as how porous borders or other conditions favoring mass displacement alter the strategic logic of violence during conflict, and how refugees, many of whom seek to return home eventually, respond to such atrocities.

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A Appendix

A.1 Barrel Bombing Harms other than House Destroyed

First, a number of these measures relate to the harm experienced by individuals other than the respondent, particularly the variables measuring barrel bombing and other indiscriminate harms (rockets, shelling) that injured or killed family members. This poses a problem because it involves the behavior and choices of a person not in our sample. We cannot know, for example, where those family members were when they were injured (or where family members who were not injured were). Thus our strategy of conditioning on neighborhood because the probability of barrel bombing related harms is equal across that neighborhood fails when we think of the harms faced by family members, who may have been in other neighborhoods. To be clear, those individuals who have family members who may be more apt to be injured may themselves be more likely to hold certain attitudes. In retrospect, we should have assessed whether family members who were living together with the respondent were injured or not, so that the neighborhood conditioning would remain correct.

Second, using *bbomb_business_destroyed* poses several problems. First, not all people had businesses, and those that do not may simply answer "no" on this question. Second and relatedly, mostly men answered "yes" on this question, suggesting that when women were interviewed, even if the family's business was destroyed they may not regard it as "their" business.

Third, *bbomb_neighborhood* cannot be used as a "treatment" in our main analysis, as we work only with neighborhoods that have been barrel bombed. Relatedly, we collected the variable *present_bbomb* for purposes of potentially identifying the individuals who were present during barrel bombing and thus stood equal risk of harms such as injury. We later came to realize this is an unwise strategy.[21] We also do not use *present_bbomb* as a "treatment" harm because it allows self-selection, with some taking greater risks and choosing to remain present while others do not.

Fourth, using bbomb_assets_destroyed is complicated by the fact that those with more assets can have more destroyed, creating a confound. Moreover, the vast majority of people with an answer to this question simply said their home was destroyed.

Fifth and finally, asking whether participants were injured by barrel bombing (*bbomb_injured*) at first seems to comport with our identification strategy. However, beyond having low variation (with few injured respondents), this variable poses an identification concern: It is possible that certain types of people prefer to stay longer into the barrel bombing than others, and those people are also at greater risk of being injured. This would create a confounding opportunity that is very difficult to solve: we cannot find an effective control group for those who are injured during barrel bombing, if some people experienced a smaller amount of barrel bombing and left before being injured. In other words, we cannot ask people if they were present during *the particular* barrel bombing incident in which they would have been injured, had they opted to stay longer and then been injured.

The *bbomb_house_destroyed* variable sidesteps this problem, because a person's house stays whether the person leaves or not. Within neighborhoods that are attacked, we argue, which houses are destroyed is effectively random. This holds whether the person is more risk-avoidant (and left already) or more risk-tolerant (and stayed longer). [22]. Given the various identification and

[21] Conditioning on *present_bbomb* would be problematic for two reasons. First, our question asked people whether they were present during *any* barrel bombing in their neighborhood, not necessarily the barrel bombing that destroyed their house. For those who did not lose their house, there is no way of asking whether they were present during the barrel bombing that "would have" destroyed their house. Second, it is potentially post-treatment. For example, many people moved multiple times during the conflict. At some point, those who did not lose their home may move back, and be present for barrel bombing. Others may have lost their home while away, discouraging them from ever moving back and thus being present during barrel bombing there.

[22] This does imply that we assume people know whether their house was destroyed or not, through their contacts, even if it was destroyed after leaving. Because there is generally considerable communication with those who stayed behind,

practical concerns with each other variable discussed above, we are left with *bbomb_house_destroyed* as the best option.

A.2 Permutation Inference

To better understand the logic behind our permutation inference procedures, consider first a dataset consisting of the matched pairs found by matching (with the weights implied by the matching procedure, for multiply-matched units). In each pair (i.e. one row of the matched dataset), one unit was originally harmed ("treated") and the other was not, and the standard ATT estimate is constructed by taking the difference in outcomes within each pair, then averaging these differences together across all pairs (weighting each pair using the given weights). To construct a distribution of the effect estimates one would see had there been no effect within any pair (known as the "sharp" null of no treatment effect within pair), we randomly re-assign harm within each matched pair. Each time we randomly reassign which of the units is considered the harmed one for all matched pairs, we can recompute an effect estimate one would expect to see under this null. We do this 10,000 times, producing a distribution of outcomes under this null. The 95% two-sided null interval is then constructed using the 2.5^{th} and 97.5^{th} percentiles of this distribution. The same process can be used by showing the 5^{th} and 95^{th} percentiles to construct the 90% two-sided null interval.

We take the unconventional approach of plotting results that show both the conventional confidence intervals (centered on the effect estimate), together with markers indicating the 90% and 95% boundaries of this permutation (sharp) null distribution, which is naturally centered around zero. Thus, in addition to examining whether the conventional confidence interval includes zero or not, when any point estimate falls outside these regions, it allows us to reject the sharp null at the 5% and 10% two-sided levels. When the point estimate falls between the 90% and 95% boundaries of the sharp-null, one can say the p-value is between 0.05 and 0.10 by permutation inference.

We add this permutation inference as it avoids making assumptions that the sample is large enough for convergence to the theoretical null (z or t distributions). In this case, the power to reject the sharp-null tends to be slightly greater – the width of the 95% null-distribution is very similar to, but on average slightly smaller than, the width of the 95% confidence intervals. We note that matching has the effect of dropping observations that come from location-gender strata that have no variation on *bbomb_house_destroyed*. This reduces sample size, making permutation inference more reliable. Moreover, given these dropped units – and the original restriction to those who report their neighborhood was barrel bombed to begin with – this ATT estimate is best understood as a "feasible sample average treatment effect on the treated" (FSATT) (King et al., 2014).

we find this assumption plausible

A.3 Main Results Table

Table A-1: Table of Main Results

	$\widehat{ATT}$	$\widehat{SE}$ (AI)	p (perm.)	p (AI)	N
help_regime_member	-0.13	0.04	0.00	0.00	285
refuse_position	0.02	0.03	0.40	0.59	299
support_party_opp	-0.08	0.04	0.01	0.06	338
support_party_none	0.08	0.04	0.01	0.06	338
volunteer1_bin	0.11	0.03	0.00	0.00	302
follow_syria_bin	0.08	0.04	0.00	0.07	338
syria_threat1_intl	0.05	0.02	0.00	0.01	293
personal_threat1_Assad	0.22	0.05	0.00	0.00	287
syria_threat1_Assad	0.13	0.05	0.00	0.00	293

Table of main results corresponding to Figure 8. $\widehat{ATT}$ gives the estimated average treatment among the harmed and $\widehat{SE}$ *(AI)* gives the Abadie-Imbens standard error estimate for matching. The p-value by permutation inference is given by *p (perm)* and the p-value using the Abadie-Imbens standard error (and the normal approximation) is given by *p(AI)*. The number of units in the final matched sample responsible for each estimate is given by N.

A.4 Formal Sensitivity Analysis

In addition to this qualitative reasoning about potential remaining confounders, a formal sensitivity analysis can help to characterize the types of confounders that would be problematic. The analysis we use here follows Cinneli and Hazlett (2017) and analyzes the sensitivity of coefficients from a linear regression. We specify a model with location fixed effects and gender fixed effects. As covariates and for benchmarking purposes, we include all of the pre-treatment covariates used above for balance testing. Such a model, while different from the matching estimators used above, estimates that having one's home barrel bombed is associated with 7.7 percentage points lower support for the opposition, very similar to the matching estimate (7.5). Figure A-1 shows a sensitivity plot for the opposition support outcome. In brief, the coordinates given by the horizontal and vertical axes indicate how strongly a hypothesized confounder is related to the treatment (horizontal) and the outcome (vertical). The contour lines are labeled to show the treatment effect estimate after adjusting for the bias that would be due to such a confounder. The points shown on the plot are benchmarks, which indicate how an unobserved confounder would alter our estimate if it has been as strongly related to the treatment (horizontal position) or outcome (vertical position) as the covariate named next to each point. For readability, we show only the several benchmarks that are most troubling or otherwise of interest, as they are all clustered in the bottom left.

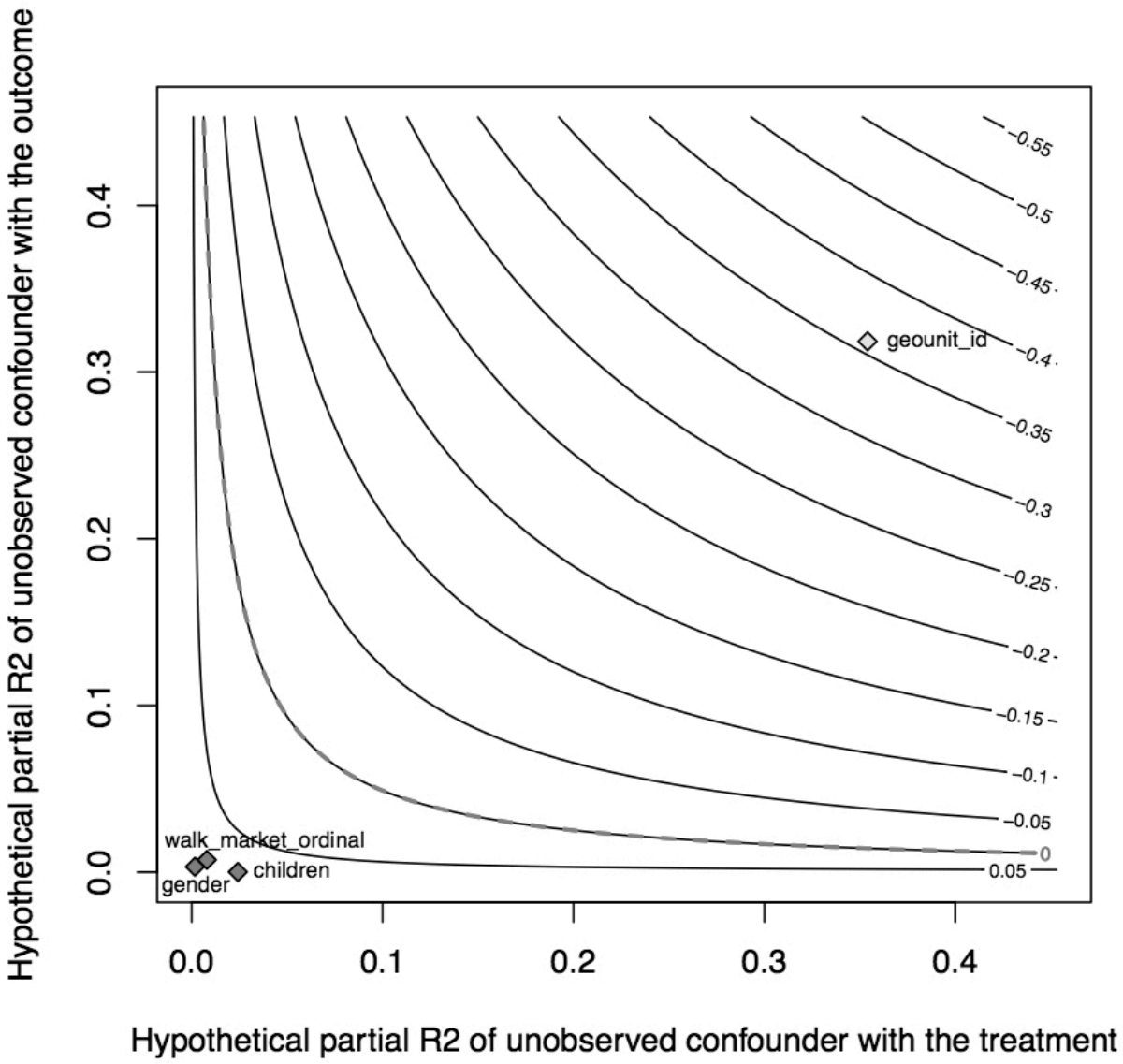
Note that the point marked "geounit_id" is on the opposite side of the lines crossing through zero. This implies that a confounder as "powerful" as the neighborhood fixed effects (jointly) would change our answer. This is to be expected. Among each of the remaining covariates, we find that an unobserved confounder that is as strongly associated with the treatment and as strongly associated with the outcome as that covariate would not be in any way problematic. Specifically, looking to the covariates most strongly associated with either the outcome or losing one's home, two illustrative examples of what we can conclude from the analysis are:

- an unobserved confounder that is "as strongly associated with the treatment"as *children* ($R^2_d = 0.024$)[23] would have to be at least 1672 times more strongly associated with the outcome (reaching $R^2_y = 0.208$)[24] in order to reduce the estimated effect of *bbomb_house_destroyed*

[23]This R^2_d is the additional proportion of variation in the treatment explained by *children* in a model that also includes the other covariates.

[24]This R^2_y is the additional proportion of variation in the outcome explained by *children* in a model that also includes the treatment and the other covariates.

Figure A-1: Sensitivity Analysis for Effect of Harm on Supporting "No Party"



Sensitivity analysis following Cinneli and Hazlett (2017). Contour lines show "adjusted effects" corresponding to a hypothetical confounder with strength of relationship to the treatment as indicated by the horizontal axis and strength of relationship to the outcome as indicated by the vertical axis. The points on the plot show how an unobserved confounder as related to the treatment and outcome as the corresponding covariate would influence the result. Failing to adjust for the neighborhood (*geounit_id*) would entirely change the sign of the effect, as expected. For an unobserved covariate to substantively change out conclusion, it would have to be much more strongly related to the treatment and outcome than any of the covariates shown (bottom left), which are those used in balance testing.

to 0

- an unobserved confounder as strongly associated with the outcome as "walk_market_ordinal" ($R^2_y = 0$) would have to be at least 40 times as strongly associated with the treatment (reaching $R^2_d = 0.976$) to reduce the estimated effect of *bbomb_house_destroyed* to 0

We regard these results as relatively strong in a context where we know the fundamental inaccuracy of barrel bombs makes it very unlikely that any unobservable could be very strongly related to the treatment. This does not mean our result is impervious to unobserved confounding.

If some confounder akin to neighborhood exists and remains unaccounted for, it would have the power to change our answer. However, it is difficult to think of such a confounder, particularly one whose direction would produce the effect we observe rather than its opposite.