Displaced Loyalties: The Effects of Indiscriminate Violence on Attitudes Among Syrian Refugees in Turkey

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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 quasi-experiment owing to the inaccuracy of barrel bombs to examine the effect of having one's home destroyed on political loyalties. We find that refugees who lose a home to regime-inflicted barrel bombs are *less* supportive of the opposition and more likely to say no armed group in the conflict represents them. Suggestive evidence supports two explanations for this: individuals may blame the opposition for provoking regime violence, and they may feel more generally "pro-peace", withdrawing support from any group that employs violence. These findings diverge from the expectations of existing theories, which assume that civilians are captive in the conflict zone and must choose sides for protection.

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 differ when 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 (Kalyvas, 2006). 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. 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 who have fled such violence view the conflict and the parties fighting in it.[1]

In the well developed and studied "captive civilian" model of civil wars, it is theorized that armed groups compete for the cooperation of a fixed population, and civilians must choose sides to achieve their own security and personal aims (Kalyvas, 2006). The ways in which civilians react to such violence when they can leave the conflict zone, however, may differ. On the one hand, it is reasonable to expect that refugees that experience higher levels of incumbent indiscriminate violence will show support for insurgents. Alternatively, refugees who have faced such violence could turn away from all armed groups, incumbent and insurgent alike. Our principal contribution is an empirical test of these two alternative reactions to indiscriminate violence in the case of Syrian refugees in Turkey.

Specifically, using an original survey of 1,384 Syrian refugees living outside of camps in Turkey, we analyze a tragic quasi-experiment that arises due to the technical limitations and tactical use

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

of barrel bombs. This approach allows us to estimate which response—turning towards insurgents, or withdrawing support from both sides—dominates in this particular case and population from which we sample. Barrel bombs are terrifying weapons that lack precision, and thus 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 one piece of the total harm and suffering refugees have endured. We describe the other types of violence our respondents lived through to better characterize these Syrian refugees' overall wartime experience, however we do not seek to make causal claims about the effects of those experiences. Rather, as is often the case in empirical work ranging from randomized trials to quasi-experiments, our focus on one factor (losing one's home to barrel bombing) does not require or imply that this is the only or most important driver of the outcomes.

Our main findings are as follows: As expected, 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 more likely to find the Assad regime a personal threat to their own security and more likely to see Assad as a threat to a future Syria. More surprisingly, experiencing these additional losses does *not* lead to a detectable increase in affinity towards the armed opposition or any other insurgent group. Rather, those who lose homes are less likely to report support for the opposition, and are commensurately 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. These patterns—especially the reduction in support for the opposition and approval of neutrality among the harmed—defy the dynamics expected by most models of civil conflict, which assume that civilians remain in the war zone and are forced to choose sides in response to violence (e.g. Lyall et al., 2013; Zhukov, 2017; Christia, 2012). We also consider the evidence for two mechanisms, "blame-attribution" and "war-weariness", potentially driving the pattern we find.

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 both below and in the Appendix.

2 Studying Indiscriminate Violence

Scholars of civil conflict routinely draw a conceptual distinction between "selective violence"—instances when combatants and/or the civilians suspected of supporting combatants are targeted based on personalized information about their 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) at the individual level 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 targeted but violence is effectively indiscriminate among those in the targeted groups or areas.

2.1 The Logic of Indiscriminate Violence: Civilian Attitudes and Open Borders

According to Kalyvas (2006), violence that is indiscriminate at the individual level is typically of little or no strategic value, since it not useful in providing a selective incentive for individuals to take sides. Indiscriminate violence may be logical, however, 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 number of related studies have attempted to estimate the impact of indiscriminate tactics to ascertain whether or not they achieve their desired objective (Dell and Querubin, 2017; Kocher et al., 2011; Lyall, 2009). For example, Dell and Querubin (2017) find that U.S. aerial bombardment in the Vietnam war

was counterproductive in that it increased the military and political activities of the insurgency. Similarly, Kocher et al. (2011) find that aerial bombardment increased Viet-Cong insurgent activities in bombed areas. In a study of the war in Chechnya, however, Lyall (2009) finds opposite: villages subjected to indiscriminate Russian artillery fire had a reduced likelihood of subsequent insurgent attacks.

As a whole, this research on the logic and efficacy of indiscriminate violence has overwhelmingly focused on collective behavioral outcomes. The emphasis is on 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 aggregate data, such as territorial control and death counts. We know much less about how civilians view and respond to indiscriminate attacks. Put simply, there are still very few rigorous empirical studies *of civilian attitudes toward combatants* during wartime, and very little attention has been given to how direct exposure to indiscriminate violence shapes individual perceptions about incumbent and insurgent groups.

As Bormann et al. (2017) find in their recent review of civil war studies, "the literature has produced relatively little systematic knowledge about individuals' post conflict grievances, attitudes toward reconciliation, and, in particular, how such individual-level consequences aggregate up to collective outcomes"(14). Undoubtedly, the paucity of studies examining actual effects of individual-level exposure to violence *during* a conflict is partly 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.

Furthermore, a central scope condition of all of the research and theoretical frameworks cited above is a *captive civilian population.* Civilians 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. The incentives for armed groups to use indiscriminate violence can change drastically, however, when civilians are not captive in the war theater. Open borders allowing civilian exit give the regime reason to use indiscriminate violence to wage counter-insurgency by "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. The same applies to insurgents, as they too may wish to use indiscriminate violence

to drive out incumbent supporters from areas they seek to control.[2] It follows that the ability to exit the war theater also potentially shapes civilian reactions to indiscriminate violence, as we examine here. Theories that assume a captive civilian population understand civilian reactions to violence through individual and group efforts to improve security. These theories may not apply when civilians can flee and avoid being forced to adjust their loyalties to maximize security.

An open question therefore remains: How do civilians who were able to exit the conflict theater react to indiscriminate violence? On the one hand, we might expect that even where populations can escape the conflict theater, exposure to additional violence by one side increases support for the opposing side. 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. Further, support for the opposition could even be reduced, owing to the opposition's failure to protect civilians, their perceived role in provoking incumbent violence, or simply because of conflict fatigue. Our primary aim in this paper is to offer quasi-experimental evidence to shed empirical light on the question of which of these reactions dominates in our case.

Beyond the theoretical motivations for our research, understanding displaced civilians' attitudes is important on a practical level 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, ultimately 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 (e.g. Lindley, 2010).

3 The Syrian 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

[2]That said, in asymmetric conflicts, the insurgents are often less capable of destruction on the scale that would cause mass displacement. Although we focus on indiscriminate violence by the incumbent regime because this is what our sample has experienced, 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).

other cities and were met with a harsh response from 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 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).

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]

At the time of our survey, there were 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

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

[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 the Turkish Disaster and Emergency Management Authority (AFAD, 2013) based on a survey conducted in 2013, long before barrel bombings were being used heavily in the Syrian civil war.

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. These improvised explosive devices (IEDs) are typically made from oil barrels, fuel tanks or gas cylinders packed with explosives and metal fragments like nails and machine parts to increase their lethality, then dropped from helicopters and planes. 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. Thus, while they may be targeted to the area of a block or so, targeting beyond this is impossible. Moreover, even once they hit a particular spot, the radius of damage from the point of impact can reportedly reach 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 desire to use a weapon with such poor targeting. 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]

[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".

[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.

4.1.1 Barrel Bomb-related Harms

We measured the following barrel bomb-related harms. Unless otherwise noted, the response options are *yes, no, don't know/refuse/no response*:

- Was the neighborhood you come from barrel bombed at some point? (Yes or No): *Barrel bombed neighborhood*. For those who say yes,
  - Were you present in that neighborhood during the time of any barrel bombing? *Present during barrel bomb*
  - Were you yourself physically injured by the barrel bombings? *Injured due to barrel bomb*
  - Thinking about your spouse, siblings, children and parents only, how many of these family members were injured due to barrel bombing? (Numerical count). *Family members injured due to barrel bomb*
  - Thinking about your spouse, siblings, children and parents only, how many of these family members were killed due to barrel bombing? (Numerical count). *Family members killed due to barrel bomb*
  - Was your home at that time destroyed or damaged so badly as to make it unlivable? *House destroyed due to barrel bomb*
  - Was your place of business destroyed by barrel bombing, while you were still living in that neighborhood? *Business destroyed due to barrel bomb*
  - Can you please tell me what other assets belonging to you or your family were destroyed by barrel bombing? (Open-ended). *Assets destroyed by barrel bomb*
- Thinking about your spouse, siblings, children and parents only, were your family members injured due to indiscriminate violence (barrel bombs, shelling or rocket attacks)? *Family injured (indiscriminate violence)*
- Thinking about your spouse, siblings, children and parents only, were your family members killed due to indiscriminate violence (barrel bombs, shelling or rocket attacks)? *Family killed (indiscriminate violence)*

We list all these harm-related questions for completeness and provide related descriptive statistics below, but the main analysis in this paper focuses on *House destroyed due to barrel bomb* 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 (as *Present during barrel bomb* could), 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 *House destroyed due to barrel bomb*) is given in Section A.5 in the Appendix. Although 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.

Regarding respondents' ability to identify the status of their homes and the source of damage, refugees that we interviewed indicated that people largely know the nature of the attacks their neighborhood experienced – they know whether their neighborhood was damaged or not, near the front line or not, and indeed whether or not barrel bombs were used. If one witnessed their home's destruction, they will know for certain the cause. If any neighbors, or neighbors of neighbors witnessed the destruction, again refugees will often find out about the extent of the damage and the cause of the destruction, as, understandably, they are highly motivated to seek out this information through their social networks and any other resources available to them. To quote one of our interviewees, "The type of damage can reveal the source of the damage and since the explosive barrels tend to have a similar effect range it can be determined through simple observation at the location."[8] Since the purpose of barrel bombing is thought to be to drive people out, it largely occurred in neighborhoods that were not yet emptied, and thus where witnesses were present.[9]

4.2 Survey Sampling

The aim of our sampling strategy was 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.[10] Syrians in these provinces comprise

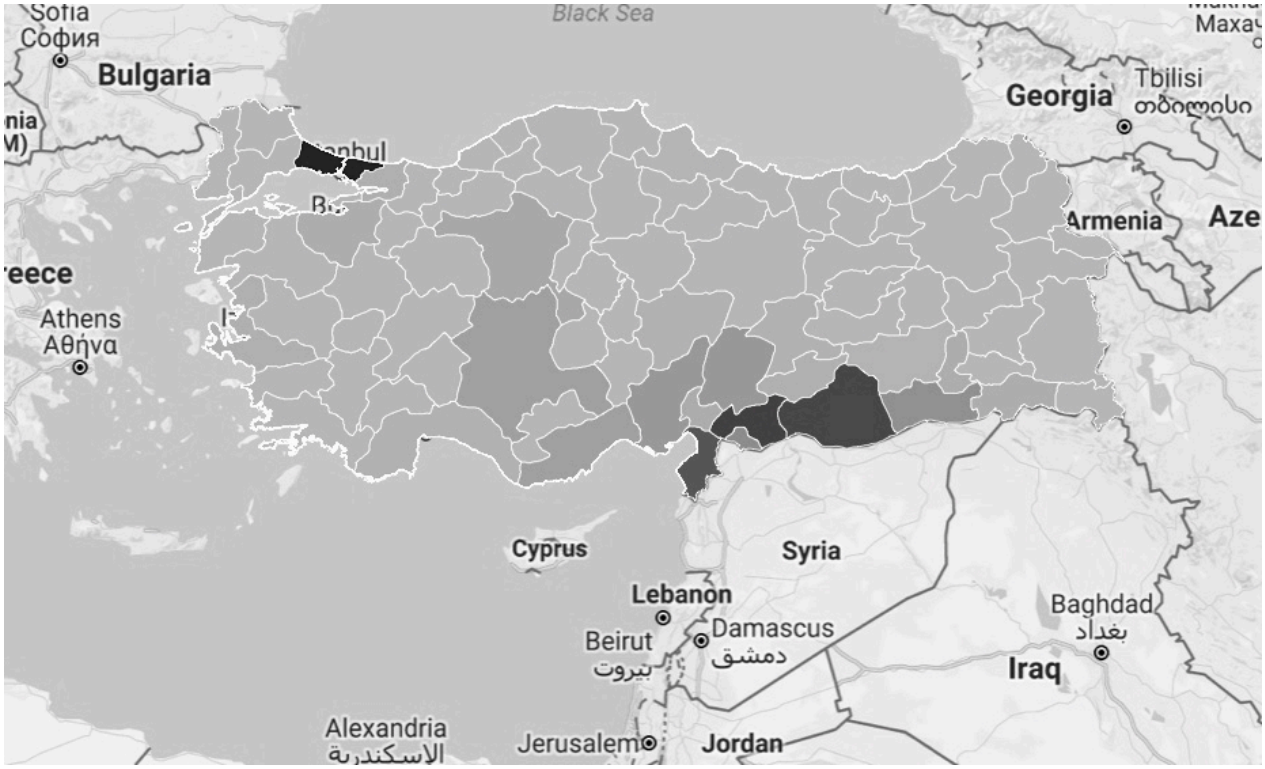
[8]Interview conducted on July 2, 2018.

[9]In our sample, only 9 percent of our respondents from barrel bombed neighborhoods chose "don't know" or no response to the question about the destruction of their house due to barrel bombs. In another recent survey conducted by the Turkish Disaster and Emergency Management Authority (AFAD) among 2,461 Syrian refugees in Turkey, only about 16 percent of respondents did not know the status of their house (AFAD, 2017). We do note that to the degree there may be motivated measurement error – with some individuals more likely to "over-report" their home being destroyed than others, we would expect that it is the more pro-opposition/anti-regime refugees who are more likely to report their home being destroyed by barrel bombs. If true, this would suggest a bias towards higher opposition support among those who lose their home, but our finding is in the opposite direction.

[10]According to the figures provided by Directorate General of Migration Management (DGMM), at the time of our

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.

Figure 1: Distribution of Syrian Refugees in Turkey



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 Turkish, they randomly selected another household on the street. If Syrian, the male head of the household was asked to participate in the survey if he was at home.[11] 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.

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.

[11]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.

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?: ISIS, Assad regime, Opposition groups, Other armed groups, International powers. *Top threat to Syria: Assad*, *Top threat to Syria: International powers*, *Top threat to Syria: ISIS*

- In your opinion, which of these do you consider the biggest security threat to you personally in a future Syria?: ISIS, Assad regime, Opposition groups, Other armed groups, International powers. *Top threat to you: Assad*

- Which party to the conflict do you think most closely represents your interests? (Open-ended). *Support no party*, *Support opposition*

- If a member of your community refused to take a position in support of any side to the conflict, would you approve? (Yes or No). *Neutrality acceptable*

- If you encountered a fellow Syrian that you knew had fought with the regime and that person needed immediate life-saving assistance, would you help the person? (Yes or No). *Would help regime member*

In order to explore possible mechanisms, we further employ the following variables:

- Do you do any volunteer work for services that help Syrian refugees? *Volunteer for refugees*

- How closely do you follow the news from Syria? *Follow Syria news*

- In a post-war Syria, what do you think are appropriate punishments for the following groups (those who fought with the regime, members of the regime, members of the opposition that killed civilians)?: (they should not be punished; they will get their own punishment; they should have to admit their actions and seek forgiveness from those they harmed; they should serve time in jail; they should be tried by an international court; they should be executed; they should be executed; they should be tried by Syrian National Courts; the should be tried

by Syrian Local Administration Councils) *Execute regime members*, *Execute regime fighters*, *Execute opposition*

- If a family member of yours spoke out publicly calling for an end to fighting, to what extent would you agree? *Support peace if family does*

- What kind of political settlement do you think the leadership that most closely represents you should accept in order to put an end to the violence?: (Accept a peace settlement that ends the fighting, regardless of who maintains control; Accept a peace settlement that ends the fighting, but only if Syria becomes a federal country with some areas outside of the Assad regime's control; Accept a peace settlement that ends the fighting, but only if the regime is removed entirely from Syria; No political settlement is acceptable, fighting should continue until a outright military victory) *Compromise for peace*

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).[12] 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 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

[12]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`

is 2.25 and 1.0 square kilometers. This is a reasonable size for our conditioning strategy.[13]

4.3 Estimation Procedures

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 find 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. However, the possibility of different forces driving men and women to become refugees in Turkey conditional on having their home destroyed motivates a useful robustness test for selection-into-sample effects in Figure A-2 of the Appendix. As shown there, the results within each gender tend to be extremely similar to each other.[14] To guard against this imbalance on gender driving differences between the groups, we match on gender in all of our analyses.

The necessary conditioning 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 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.6 in the Appendix.

[13] 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.

[14] 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 Disaster and Emergency Management Authority in 2013 (AFAD, 2013).

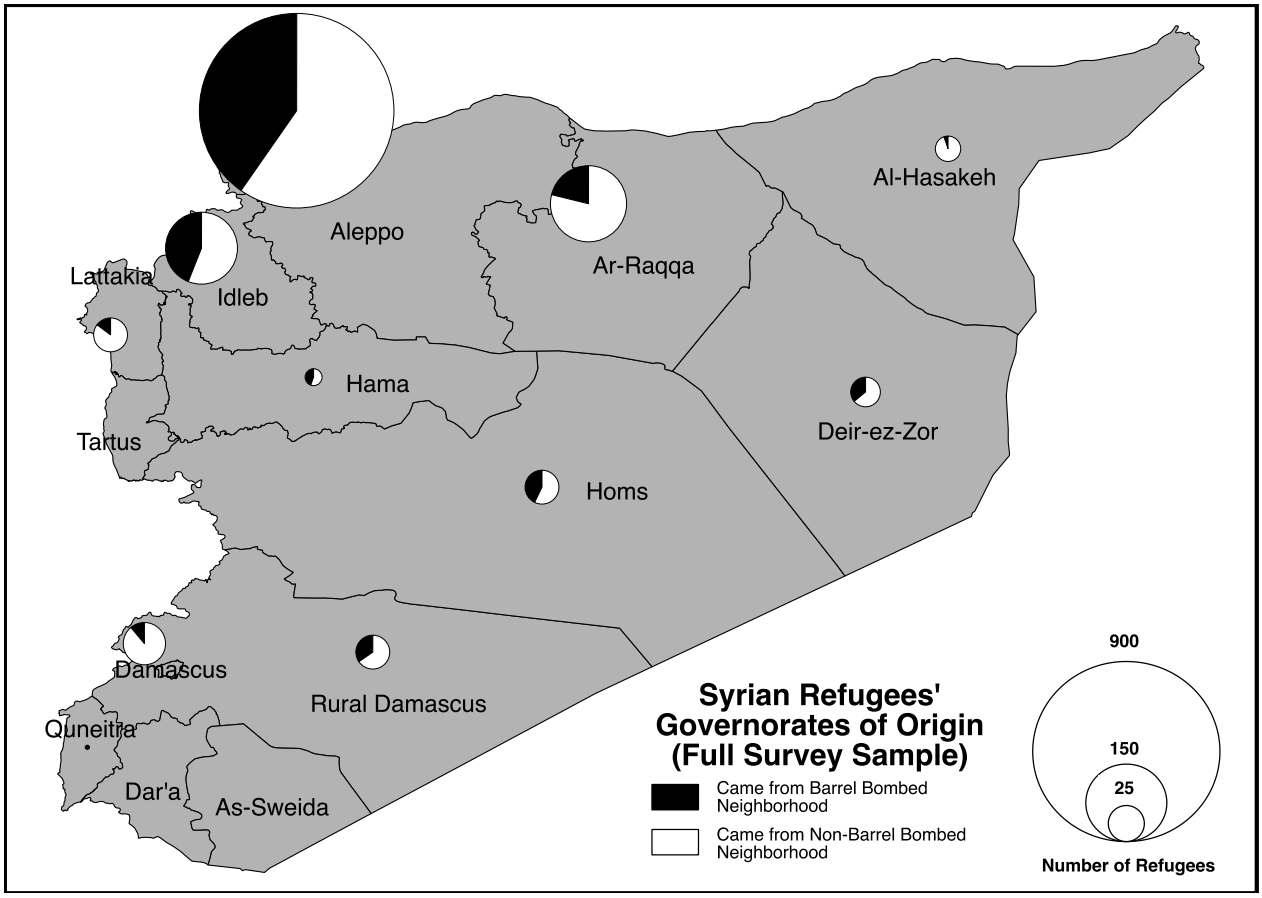
5 Results

5.1 Descriptives: Distribution of Relevant Characteristics

Full Sample

The diverse geographic distribution of our sample of 1,384 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.

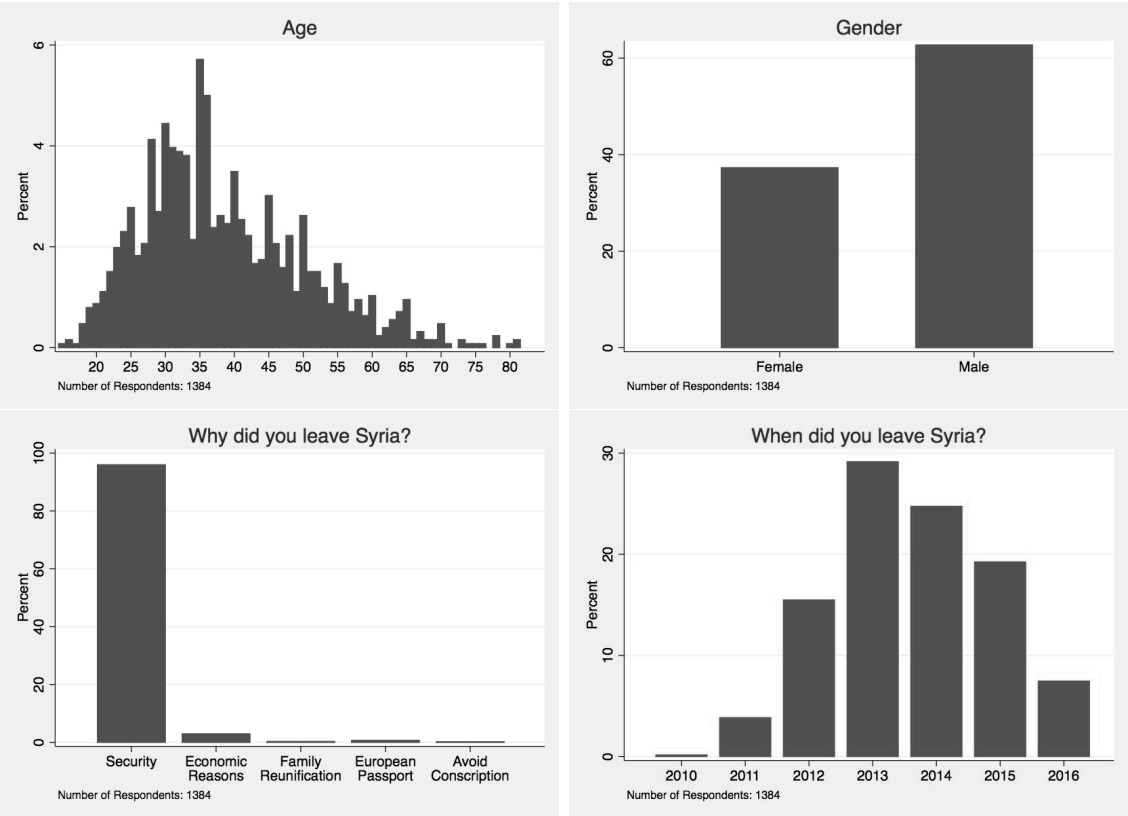
Figure 2: Full Survey Sample



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.

Figure 3: Key Demographic Descriptives



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

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.[15] 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).

[15]A recent survey of in- and out-of-camp refugees conducted 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).

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
Employed before attack	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
Children	0.92	0.27	1384	0.92	0.28	452
Rooms in house	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
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 members injured due to barrel bomb	0.10	0.30	482	0.09	0.29	412
Family members 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
Assets destroyed by barrel bomb	0.88	0.33	356	0.88	0.32	304
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						
Top threat to Syria: Assad	0.35	0.48	1267	0.47	0.50	405
Top threat to you: Assad	0.48	0.50	1258	0.54	0.50	400
Top threat to Syria: International powers	0.04	0.19	1267	0.06	0.25	405
Top threat to Syria: ISIS	0.60	0.49	1267	0.44	0.50	405
Neutrality acceptable	3.00	1.47	1290	3.48	1.69	413
Would help regime member	0.54	0.50	1224	0.55	0.50	408
Support no party	0.49	0.50	1384	0.70	0.46	452
Support opposition	0.50	0.50	1384	0.30	0.46	452
Mechanism Variables						
Follow Syria news	0.50	0.50	1384	0.61	0.49	452
Volunteer for refugees	0.64	0.48	1225	0.84	0.36	406
Execute regime members	0.09	0.28	1224	0.08	0.27	375
Execute regime fighters	0.20	0.40	1221	0.11	0.31	376
Execute opposition	0.37	0.48	1210	0.42	0.49	375
Compromise for peace	2.24	1.08	1072	1.79	0.97	290
Support peace if family does	1.75	1.06	1328	1.42	0.98	439

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. We inquired about various forms violence, including injury or death due to insurgent violence, either in their family or neighborhood. We also asked about torture and injury or death due to sniper fire. Rates on all of these forms of violence were extremely low, and thus they are not reported in the table, but we do discuss them below. Finally, we queried respondents 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 killed or injured. 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.

Finally, an important aspect of our data, theory, and analysis is that the violence experienced by those in our sample is almost entirely due to the regime, and not the opposition or other insurgent groups. Fewer than 0.4% of respondents reported having a family member or even a neighborhood member injured by insurgent violence. Likewise, the predominant form of violence people would be likely to experience due to insurgents would typically be sniper fire, and again only 0.4% of respondents report having a family member injured or killed in sniper fire. This is consistent with our understanding of the violence faced in the areas our respondents came from: massive violence due to the regime, mostly through aerial forms of bombardment.

5.2 Analysis under Identification Strategy

Restricted Sample: Respondents From Barrel Bombed Neighborhoods

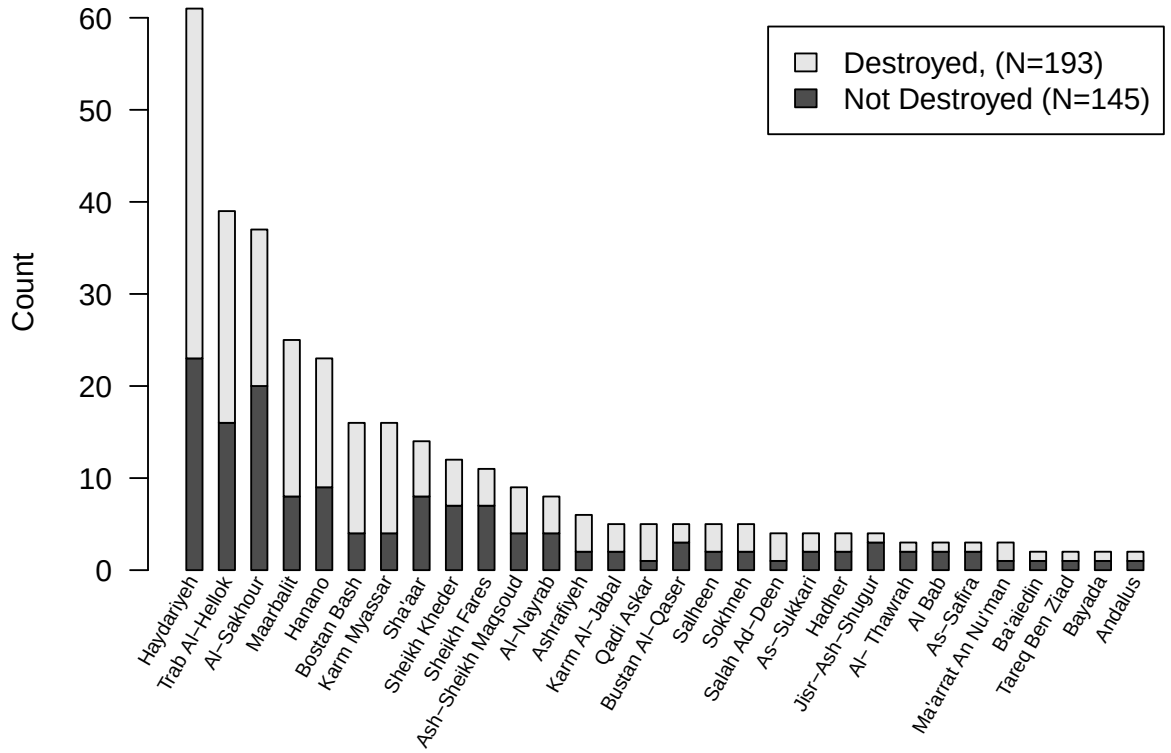
Our identification approach requires restricting the sample to those whose neighborhoods were reportedly barrel bombed.[16] This reduces our sample size to 493. It also requires dropping those for whom we were unable to get sufficiently good information on the location of their neighborhood,

[16]In section A.4 of the Appendix we perform a naive analysis using the full sample for the sake of comparison.

trimming an additional 41 observations. Table 1 provides an overview of descriptive statistics computed instead on this "restricted sample".

Moreover, as is commonly the case, the matching procedure changes the sample about whom an inference is made beyond these general restrictions.[17] The resulting "post-matching sample" from which the results below are reported has 338 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 4).

Figure 4: Post-Matching Sample by Geographic Unit



Note: Barplot showing each neighborhood and the number who lost homes (grey) or did not (black) due to barrel bombing.

Urban neighborhoods make up 23 of these units, mostly in Aleppo, while 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.[18] The share of refugees from Aleppo city is larger because the share of refugees from Aleppo in

[17]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. Second, the effect estimate that is made is not an average effect for everybody, but rather the average among those who lost their homes (i.e. an average treatment effect among the treated). For both reasons, the post-matching sample is not meant to be representative of the original sample or its target population.

[18]See Figure 5 for the neighborhoods of Aleppo city that are included in our sample.

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. 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.[19] 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).[20]

Figure 5: City of Aleppo

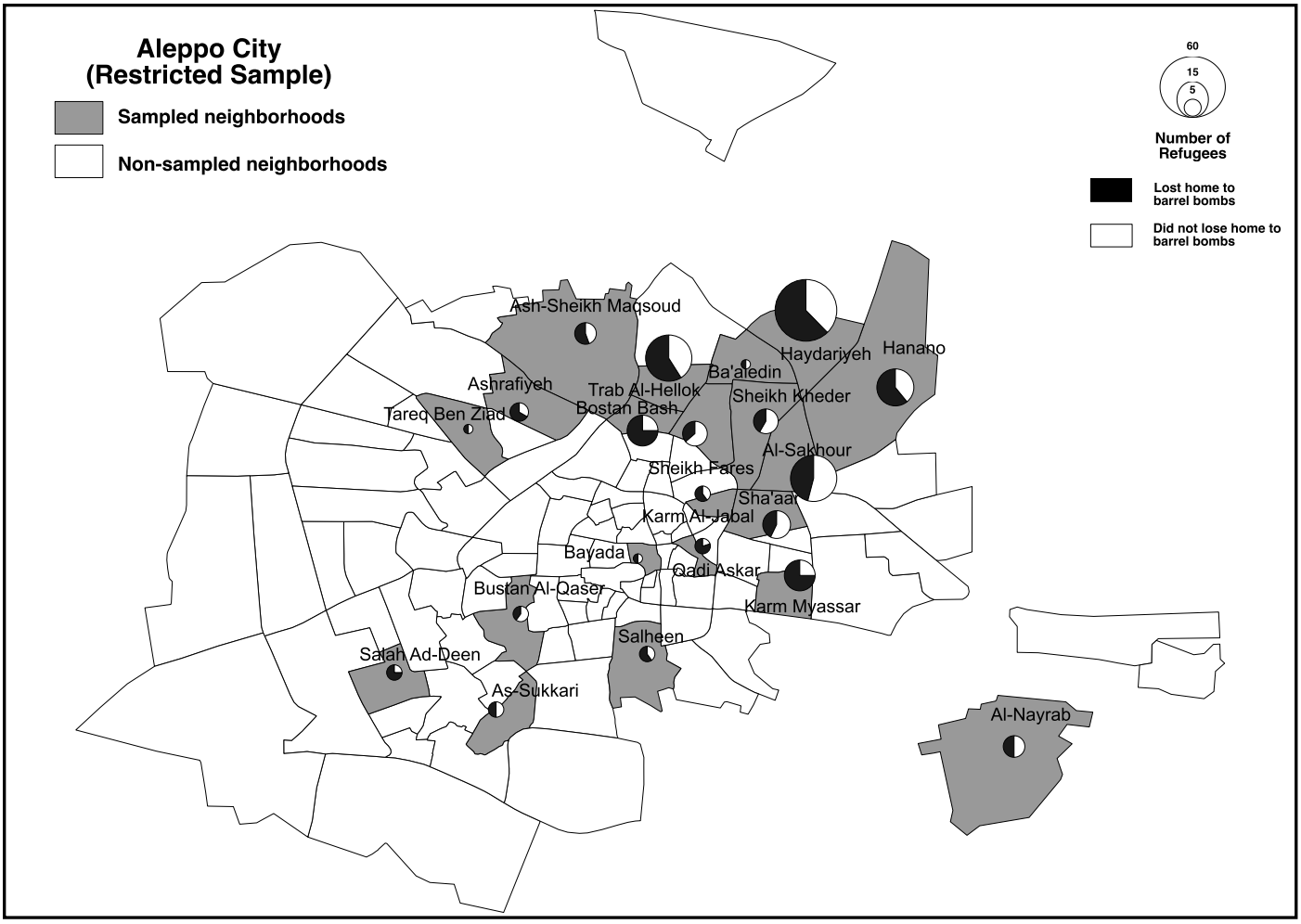
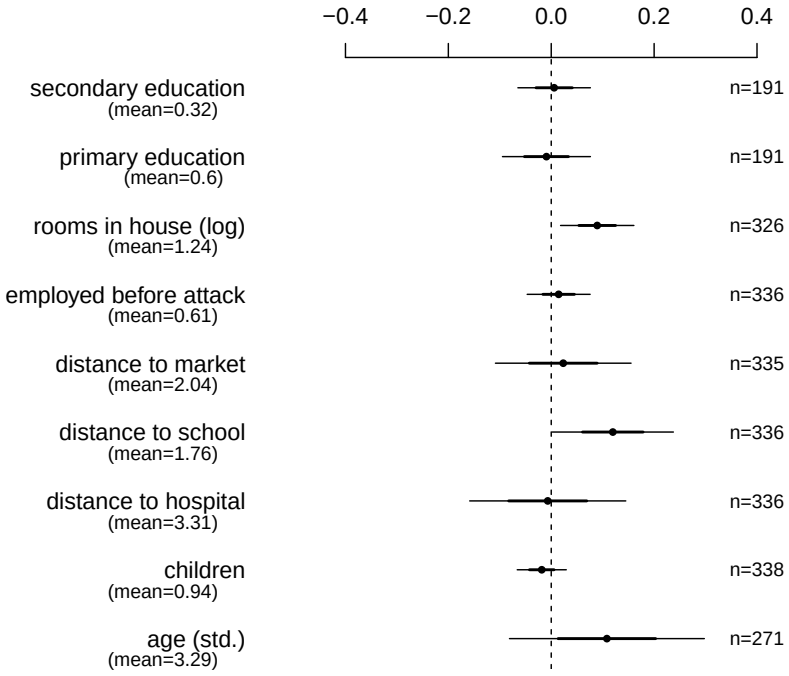


Figure 6: Conditional Balance/ Placebo Test



Note: 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.

5.3 Effects of Losing Home to Barrel Bombing

Conditional Balance

Turning now to our quasi-experimental strategy, we first examine the (conditional) balance: conditionally 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, 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 available to us 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 in house (log)*), whether the person worked prior to leaving (*employed before attack*), ordinal measures of how long it takes to walk to the

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

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

nearest market, school, or hospital (*distance to market*, *distance to school*, *distance to hospital*)[21], number of children prior to the crisis (*children*), and age (*age (std.)*) in years, which we standardize to improve visualization. We note that such a conditional balance test is also effectively a placebo test, where pre-treatment covariates 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 (*secondary education* and *primary education*). 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 6 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.[22]

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 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.3 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.

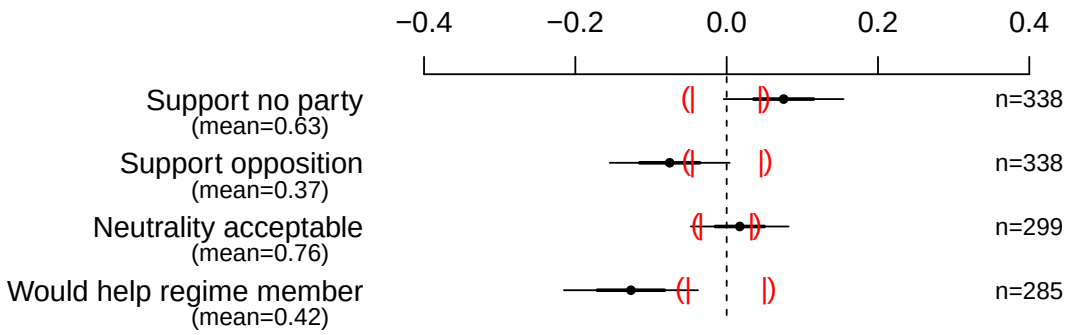
[21] Each of these variables is coded as 1 (less than 5 minutes walk); 2 (5-15 minutes walk); 3 (15-30 minutes walk); and 4 (more than 30 minutes walk).

[22] 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.

5.4 Effect Estimates

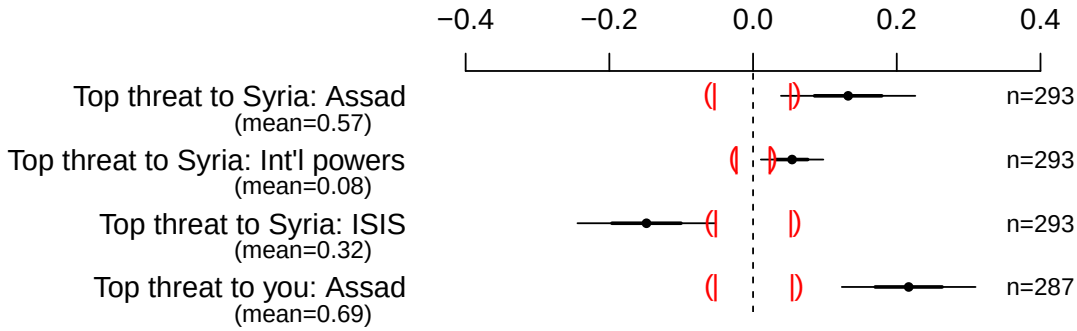
Our main effect estimates are shown in Figure 7, with additional estimates regarding the consequence of violence on threat perception shown in Figure 8 (numerical results are available in Section A.1 in the Appendix)

Figure 7: Main Estimates: Effects of Home Destroyed by Barrel Bombing



Note: ATT estimates for the effect of 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 ")".)

Figure 8: Additional Estimates: Effects of Home Destroyed by Barrel Bombing on Threat Perception



Note: ATT estimates for the effect of having house destroyed on attitudes related to threat perception. Confidence intervals for conventional and permutation-based uncertainty estimates as described in Figure 7.

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, Abadie and Imbens, 2006) 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 primary 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 ("(" 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 ones to avoid asymptotic assumptions given that in some comparisons we have relatively few units. However, for the most part the methods agree, and we report both p-values below to avoid selective reporting.

We note several findings in Figure 8 as essentially "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). Those who lose homes to barrel bombs also prove far less willing to provide life saving support to a regime member, by 12.7 percentage points ($p < 0.0001$ by permutation; $p = 0.003$ by conventional. We consider these results 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). The increases come out of the proportion who see ISIS as the greatest threat, which drops by a commensurate 15 percentage points ($p = 0.002$ by conventional).[23]

Turning to our key variables of theoretical interest regarding civilians' attitudinal responses vis a vis armed groups (shown in Figure 7): support for the opposition 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 on whether or not respondents believed that neutrality is acceptable lies near zero, indicating little or no change in the permissibility of remaining neutral in the conflict.

Finally, we note two additional analyses that have been reserved for the Appendix. First, while the above analyses have conditioned at the finest geographic level available, the size of these units can vary considerably. Most notably, the majority of neighborhoods in our matched

[23]While it would also be interesting to see the effect on naming the opposition as the primary threat to Syria, fewer than 1% do so in this sample.

sample are regarded as "urban", and have on average only one quarter the square area as the neighborhoods regarded as non-urban. Limiting our analysis only to the urban areas therefore allows finer conditioning and strengthens the credibility of the identifying assumptions. As shown in Appendix A.2, the results are very similar when we do so, supporting the same substantive conclusions. Second, in the Appendix, we discuss possible concerns of selection-into-the-sample. While we discuss the possible sources and directions of bias extensively there, one important fact is that the forces driving individuals to leave Syria are largely gendered, and selection concerns about whether people stay (to support the insurgency) or leave are almost certainly much stronger among men. We thus examine results separately for men and women. Finding nearly identical results in the two sub-groups, it becomes more difficult to argue that our findings are generated by selection into the sample.

6 Discussion

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, consistent with displaced civilians responding to such losses by instead withdrawing their support from the opposition. A concomitant increase is found in the proportion of people finding that *no group* represents their interests.

Perhaps the most likely bias given our design, we argue, would 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 the degree of confounding that would be required to alter our conclusions in Section A.2 of the Appendix.

These results are consistent with a withdrawal of support from armed groups in response to violence – something civilians can do once 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 whether or not neutrality is considered acceptable is fairly precisely estimated to be near zero. This suggests that those who

lost homes are not detectably more or less adamant that other members of their community take a side and declare allegiances.

6.1 Possible Mechanisms

Why might individuals react to regime-caused destruction in this way, not only failing to show increased support for the opposition, but further showing *reduced* support for them? In this particular case, we can rule out that individuals were directly attacked by opposition. However, several other possible mechanisms could generate this effect. Here we briefly consider what evidence we can bring to bear that is either consistent or inconsistent with two possible mechanisms. Future research would have to be conducted and designed specifically to further test these mechanisms, and we hope scholars will do so.

One possible explanation can be found in "blame attribution", by which civilians may blame the opposition for regime violence, either because they believe the opposition provoked it, or because they failed to protect against it. Although we have emphasized that the captive civilian model differs from the situation here, Kalyvas (2006) endeavors to explain the use of apparently irrational *indiscriminate* violence in the captive civilian model by suggesting that an armed group without any control of a given territory could attempt to use it in hopes that civilians will blame their incumbent for failing to protect them. Though indiscriminate violence will not make those who perpetrate it popular with targeted civilians, it could expose the weakness of armed actors who fail to protect civilians or who are thought to have provoked that violence. Likewise, again in a "captive civilian" framework, Lyall (2009) and Bausch et al. (2016) describe contexts in which indiscriminate violence by the state is likely to be viewed as a response to insurgent provocation, leading civilians to actually blame the insurgents for bringing violence down on them.

Working instead with refugees who have fled, we find evidence consistent with this argument, in that indiscriminate violence caused by one side (the regime) erodes support for its enemies (the opposition). In our case, because the aerial bombardments cannot easily be stopped by any technology under the opposition's control, it is more likely that civilians blame the opposition for attracting violence from the regime than failing to stop the bombs as such. We find suggestive evidence for this: As also shown in Figure 9, the proportion of respondents saying opposition fighters culpable for civilian deaths should be executed rises by almost 7 percentage points among those who lose their homes to barrel bombs, though it is only marginally significant ($p=0.06$ by permutation; $p=0.11$ by conventional test). Losing one's home to barrel bombing had no detectable

effect on the proportion of civilians saying that execution would be the appropriate punishment for regime members and regime fighters. While only suggestive, this is consistent with respondents feeling more negatively towards the opposition after losing their homes. Whether this is due specifically to *blaming* the opposition for bringing government violence down on them cannot be determined with our data and design.

A second possibility we consider is that violence has a "weary" or "pro-peace" effect on individuals (see e.g. Tellez, 2018 for recent evidence at the individual level, and works such as Levy and Morgan, 1986 on earlier country-level work). This in turn could cause individuals to feel any armed group that uses violence to no longer represent their interests, whatever their greater goals. We find strong evidence that such a pro-peace or weary shift in attitudes occurs. In one question we ask participants to choose what type of peace deal they would accept. Two of these options involve compromises that would end the war even if the regime retains control over some or all of the country. The other two demand complete victory, with the regime either ceding all governance and leaving the country as the result of the deal, or a "no deal" outcome in which fighting continues until regime is defeated militarily.

The first two options signal a willingness to compromise in the name of peace, whereas the latter two involve no effective compromise, at least on the issue of who governs. We find that those who lose homes to barrel bombing are a surprising 14 percentage points more likely to support plans that call for compromise in order to achieve peace, suggesting a greater desire for peace ($p = 0.008$ by conventional, $p = 0.001$ by permutation). Similarly, another question asks if participants would approve of a family member who calls for peace, with options ranging from "strongly disapprove" (1) to "strongly approve" (5). Standardizing the numerical response to create a simple score, individuals who lost homes to barrel bombs are 0.24 standard deviations higher in their approval than those who did not ($p = 0.02$ conventional, $p < 0.001$ by permutation). We thus see strong evidence of a pro-peace or "weary" effect of losing one's home to barrel bombing, which again runs contrary to the biases that would be expected if our identification approach failed.

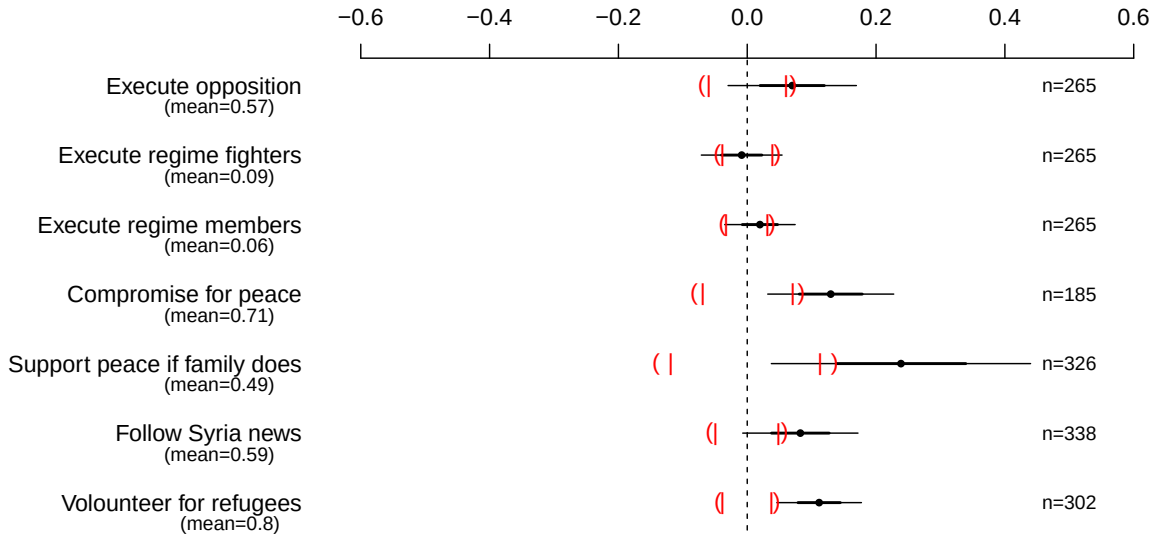
It is important to clarify that we do not find evidence consistent with the idea that this "pro-peace" or "weary" effect is the product of depression, political apathy and/or social withdrawal.[24] For one, we find that those who lost homes to barrel bombs are more likely to follow what is happening in 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), which suggest at least a marginal level of continued political engagement. Furthermore, those losing homes to barrel bombings are

[24]For work on the link between violence, depression and resultant symptoms including social withdrawal, see Pham et al. (2009).

much more likely to report supporting their fellow refugee community through volunteer work, by 11 percentage points ($p < 0.001$ by permutation and conventional tests). This suggests that exposure to violence does not lead to social withdrawal in our case, and instead generates a pro-social, altruistic response towards other fellow refugees, though not towards regime members (note our findings above about decreased willingness to help a regime member in need of life saving assistance). Our findings are consistent with a burgeoning literature on the potentially pro-social consequences of exposure to violence, at least parochially, as reviewed in Bauer et al. (2016).

We tentatively conclude that both the "blame attribution" and a general "weary effect of violence"—to be distinguished from social withdrawal or apathy—are credible mechanistic explanations of our findings worthy of further examination in future research.

Figure 9: Effects of barrel bombing on additional variables: evidence for possible mechanisms



Note: ATT estimates for variables used to examine evidence of three mechanisms. All procedures are identical to those used to estimate the main effect estimates above; only the outcome variables differ.

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. 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 and not negatively judge those who remain neutral.

We emphasize that this is a single study of a particular sub-group of refugees from Syria, and while we argue that the results are internally valid for this group, they do not speak to how other civilian groups, such as those who have remained in Syria, might react. More broadly, our findings are intended as a building block in our knowledge, and establishing their generalizability will require follow-on studies. Of particular interest would be the study of other circumstances sharing the key central elements that shaped our theoretical question and expectations in this case. The first of these is that violence is targeted to groups (such as communities thought to support the opposition) but is indiscriminate within some social or geographic unit. Such a condition also aids in allowing credible inferences by providing a degree of conditionally random variation in exposure to violence. The second key condition in our case is that the civilians in question were able to and did flee the conflict zone: we theorize that whether civilians are captive or have fled could determine whether we expect them to show support for whomever controls their territory, or to withdraw support for "both" armed groups, as found here. Finally, this study was also made simpler by the fact that the refugees we worked with faced violence almost entirely committed by the regime. Cases where individuals face violence from multiple sides (as may be found even in other areas of the Syrian conflict), may generate a different type of response, though be equally worth examining.

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. 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 in which civilians flee. We hope this work stimulates 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 Main Results Table

	$\widehat{ATT}$	$\widehat{SE}(AI)$	$p(perm.)$	$p(AI)$	N
Support no party	0.08	0.04	0.01	0.06	338
Support opposition	-0.08	0.04	0.01	0.06	338
Neutrality acceptable	0.02	0.03	0.40	0.59	299
Would help regime member	-0.13	0.04	0.00	0.00	285
Top threat to Syria: Assad	0.13	0.05	0.00	0.00	293
Top threat to Syria: Int'l powers	0.05	0.02	0.00	0.01	293
Top threat to Syria: ISIS	-0.15	0.05	0.00	0.00	293
Top threat to you: Assad	0.22	0.05	0.00	0.00	287

Table A-1: Numerical results corresponding to coefficient plots in Figure 7 and 8. $\widehat{ATT}$ is the estimated average treatment effect on the treated. $\widehat{SE}(AI)$ give the Abadie-Imbens standard errors, while $p(perm)$ and $p(AI)$ give the p-values using either permutation inference or the Abadie-Imbens standard errors.

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A.2 Robustness and Threats to Validity

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

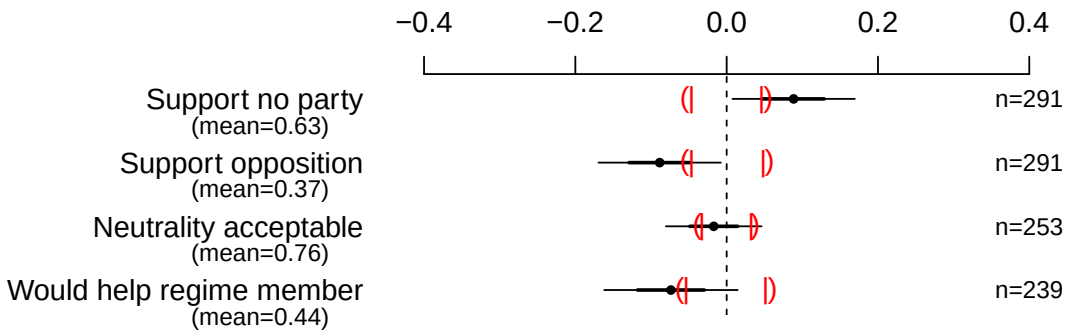
Results for Urban Areas

As noted in the text, our identification strategy is most credible in smaller neighborhoods, because conditioning on the smaller areas makes the indiscriminacy of barrel bombing damage within that area more defensible. Because some of the neighborhoods in our sample are designated as "rural" and have four times the area of those designated as "urban", we conduct an additional analysis restricting our sample to the urban areas. Appendix A.2 shows the resulting estimates, which are substantively identical to those in the full analysis in the main text.

Imbalanced Covariates as Placebo Treatments

Two covariates showed statistically significant imbalances in conditional balance tests in Section 5.3 by conventional tests (though see Hartman and Hidalgo (ress) regarding the use of equivalence tests as the appropriate tool for testing imbalance.) As suggested by a reviewer, we repeat our estimation procedure but employing these two covariates as false "treatments", to determine if

Figure A-1: Effect Estimates: Home Destroyed by Barrel Bombing, Urban Areas Only



Note: ATT estimates for the effect of having house destroyed on attitudes related to security, restricting to the set of urban neighborhoods. 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 ")".)

they also relate to the outcome and thus could act as confounders.

We first make both covariates binary (to allow the same matching procedure to be used) with a median split. Repeating the estimation approach above, we fortunately see no false "effect" of these covariates on the main outcomes of interest, *Support opposition*, *Support no party*), with all p-values at or above 0.60.

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 section A.3 of this 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.

We note that these scenarios are consistent with an assumption that it was regime-led violence that individuals in our sample have suffered and that drove them out of Syria, and not violence attributed to the opposition. As described in the main text, this is our understanding both qualitatively and in our data, as only 0.4% report any insurgent-caused harms at the family or neighborhood level.

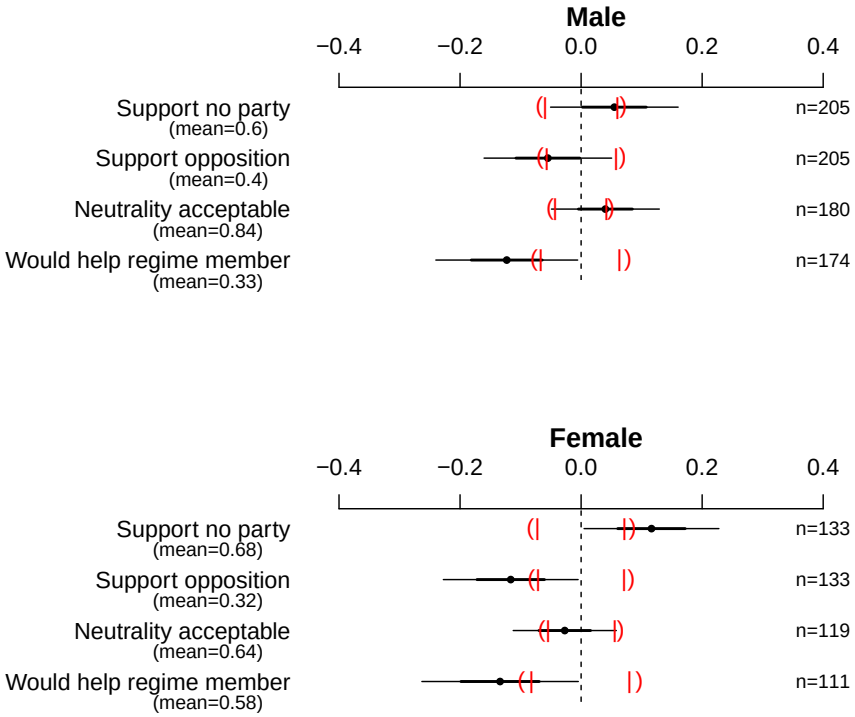
How would each of these affect our estimates? In scenario (1), if it is the pro-opposition individuals who are reluctant to leave Syria unless prompted by losing their home, this is unproblematic: the bias generated would be opposite to our estimate. However, if it is instead the relatively pro-regime individuals who are reluctant to leave, this would pose a problem, as it could be an alternative explanation for our findings on *Support no party* and *Support opposition*. 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.[25]

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 *Neutrality acceptable*

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

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 A-2 shows all the effect estimates separately for male and female respondents. We note that men and women likely face very different motives and conditions for leaving Syria. Most importantly, men almost certainly face more severe problems of selection into our sample as the option of staying behind to fight is both more common and perhaps culturally demanded among men. If our results are strongly affected by selection into the sample, they would then be likely to vary by gender. However, we find strikingly similar results among men and women (Figure A-2). Some variation and loss of statistical power are expected. This finding makes it much more difficult to sustain any argument that we can think of in which selection into the sample accounts for our results.

Figure A-2: Effect Estimates by Gender



Note: ATT estimates for the effect of 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 ")".)

A.3 Formal Sensitivity Analysis

In addition to such qualitative reasoning about potential confounders, a formal sensitivity analysis can help to quantitatively characterize the types of confounders that would alter our conclusions. Employing the method of Cinelli and Hazlett (2018), we examine the sensitivity of a regression version of our estimate. We first apply a simple linear model with location and gender fixed effects, together with the set of pre-treatment covariates used above for balance testing. Such a regression estimates that having one's home barrel bombed is associated with 7.8 percentage points lower support for the opposition, very similar to the matching estimate (7.5 percentage points), though the regression result is not statistically significant ($t = -1.02$).

Table A-2 provides a regression table, augmented by several sensitivity statistics as recommended in Cinelli and Hazlett (2018). The "robustness value" (RV) of 7.1% means that a confounder explaining 7.1% of *both* the residual variation of "treatment" (*Home destroyed due to barrel bomb*) and of the outcome (*Support opposition*) would be sufficient to reduce the true adjusted point estimate to zero. A confounder explaining less of both would not be sufficient to eliminate 100% of effect. The $RV_{\alpha_0.05}$ is a version of the RV describing the confounder required to bring the effect estimate "below significance" – which requires no confounding in this case.[26]

Outcome: *Support for Opposition*

Treatment:	Est.	SE	t-value	$R^2_{Y\sim D\|\mathbf{X}}$	RV	$RV_{\alpha=0.05}$
Home destroyed due to barrel bomb	-0.078	0.076	-1.02	0.5%	7.1%	0%
df = 193						

Table A-2: Regression result with Sensitivity Information

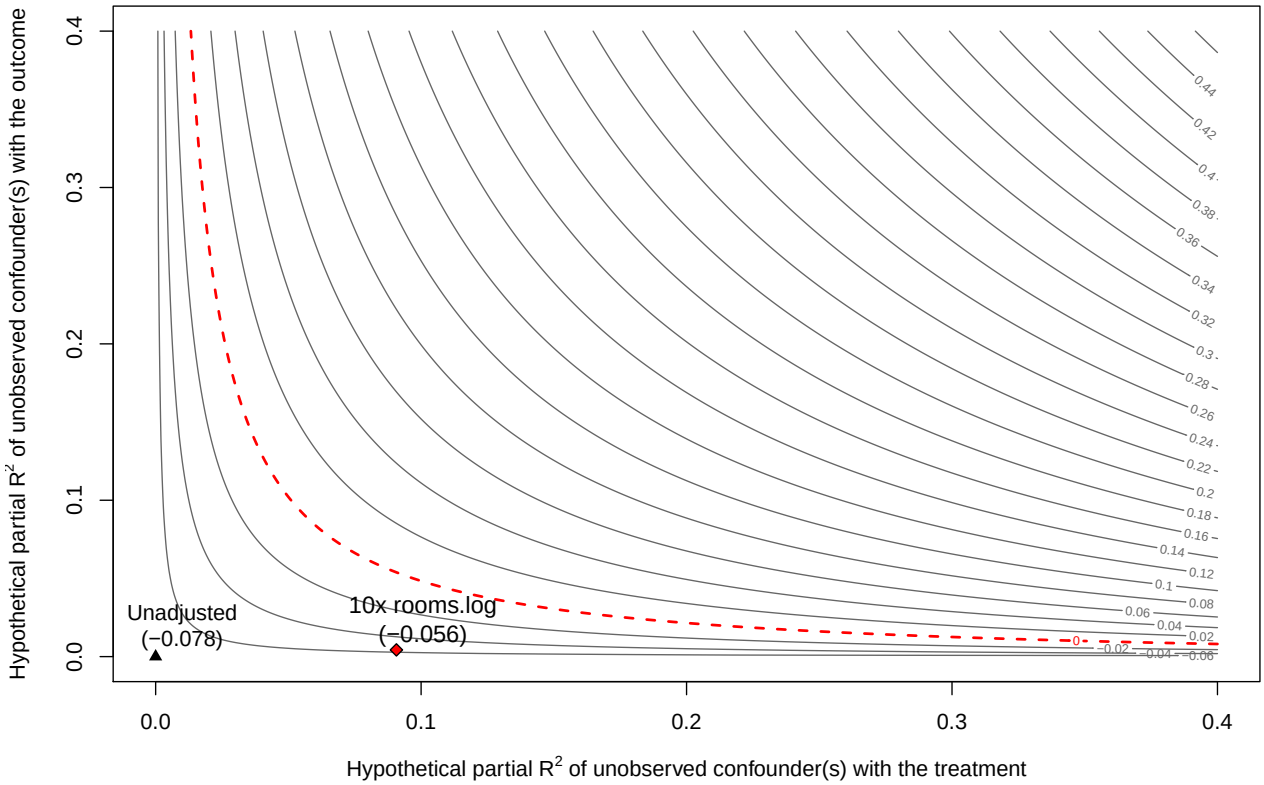
Figure A-3 further visualizes the sensitivity. The coordinates given by the horizontal and vertical axes indicate how strongly a hypothesized confounder is related to the treatment (horizontal) and the outcome (vertical) in terms of its partial R^2. The contour lines are labeled to show the treatment effect estimate after adjusting for the bias that would be due to such a confounder.

To employ our contextual knowledge in an effort to rule out larger confounders, we consider a bounding exercise. Given that there is very little scope for targeting of barrel bombs within the neighborhood, perhaps the size of a home is among the most important remaining confounders. Recalling our conditional balance table (Figure 6), the number of rooms in the house was among the most imbalanced variable, indicating that it could explain variation in which homes are destroyed conditionally on neighborhood. Let us make the assumption that, however much of the residual

[26]The $R^2_{Y\sim D|\mathbf{X}}$ value of 0.5% indicates that under the "extreme scenario" in which 100% of the residual outcome variation is explained by a confounder, that confounder would need to explain only 0.5% of the residual variation of the treatment in order to "reduce" the adjusted effect estimate to zero.

variation in treatment assignment (barrel bombing) is explained by house size (*rooms in house (log)*), confounding explains up to 10 times that amount. Likewise let us assume that confounding explains up to 10 times the residual variance of the outcome (support for opposition) compared to what house size can explain. Under such assumptions, we can bound how bad confounding can be, and this bound is shown at the point marked *10x rooms.log* on Figure A-3.

Figure A-3: Sensitivity Analysis for Effect of Harm on Supporting "No Party"



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 point labeled "10x rooms.log" shows the bound on confounding under the assumption that "confounding is not more than 10 times stronger than house size (*rooms.log*) in explaining residual variation in treatment and outcome". Note that if such a confounder existed, it would imply only that the point estimate changes from 7.8% to 5.6%. Thus the result – at least as a point estimate – is robust to confounders much stronger than house size.

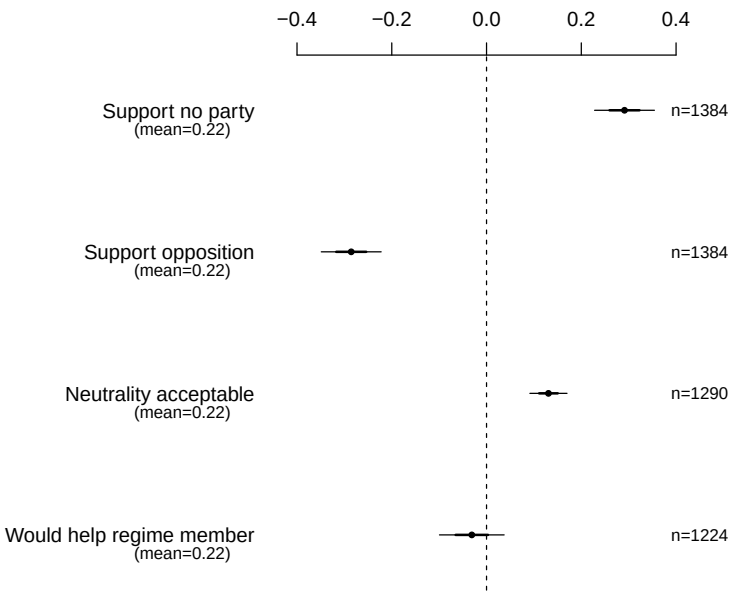
We regard these results as relatively strong given our contextual knowledge: little can be done to target some homes rather than others within a given neighborhood, and further, a home's size seems to be likely among the strongest remaining characteristics that could explain why some homes are more likely to be hit than others within a neighborhood. That said, sensitivity analysis does not rule out potential confounding, it only clarifies what confounding would be necessary to alter our conclusion. Confounders explaining far more of which homes are destroyed, and/or of the outcome, could be possible – we just cannot imagine what they might be. Critiques of this paper can usefully suggest confounders to consider that fit these criteria. Caution is required however, as the above analysis pertains to point estimates, but the sample sizes are insufficient to produce

a significant regression result even under the assumption of zero confounding.

A.4 Naive Comparison

Whereas in the body of the paper we apply an estimation procedure consistent with our identification assumptions and strategy, Figure A-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 A-4: Naive Comparison



Note: 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 no party*), and are less likely to support the opposition (*Support opposition*) 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 (*Neutrality acceptable*), while we see no difference on their willingness to provide life-saving aid to a regime member in need (*Would 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. For this reason we have sought to rule out alternative explanations based on confounding by employing the identification described in the Methods Section.

A.5 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 *Business destroyed due to barrel bomb* 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, *Barrel bombed 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 during barrel bomb* 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.[27] We also do not use *Present during barrel bomb* 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 *Assets destroyed by barrel bomb* is complicated by the fact that those with more

[27] Conditioning on *Present during barrel bomb* 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.

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, making this simply a proxy for having one's home destroyed.

Fifth and finally, asking whether participants were injured by barrel bombing (*Injured due to barrel bomb*) 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.

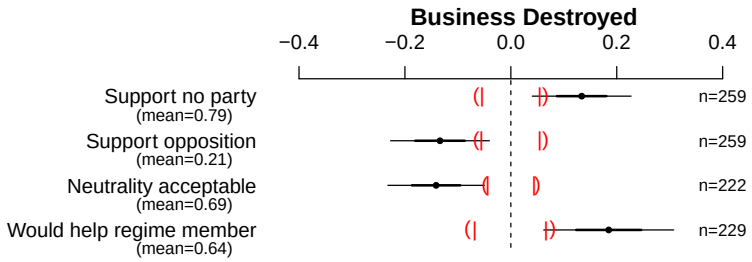
The *House destroyed due to barrel bomb* variable sidesteps this problem, because a person's house stays whether the person leaves or not. Within neighborhoods that are attacked, we argue, which houses are destroyed is effectively random. This holds whether the person is more risk-avoidant (and left already) or more risk-tolerant (and stayed longer). [28] Given the various identification and practical concerns with each other variable discussed above, we are left with *House destroyed due to barrel bomb* as the only wise option.

An added challenge with these other harms is the low amount of variation: there are only 12, 25, and 27 "treated" individuals left in the analysis after matching, respectively for whether the respondent was injured in barrel bombing, whether they had family members injured, and whether they had family members killed respectively. Regarding having one's business destroyed, there are 69 remaining treated units after matching. This is still too few for a reliable estimate. Nevertheless, Figure A-5 shows results, using the same matching procedure described in the text. These are largely consistent with our main findings: higher support for "nobody" and lower support for the opposition. They disagree on other outcomes, particularly *Would help regime member*, and *Neutrality acceptable*. Given the small remaining sample size and that we argue this variable should not be used as a treatment as it does not suit our identification strategy, we do not make much of these differences. We also note that, if we were to speculate as to the direction of the bias for this outcome, the main concern here is that only those who had businesses to begin with can have

[28] 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, we find this assumption plausible.

them destroyed, so these estimates may simply reflect ways in which business owners differ from non-business owners, without implying any effect of violence whatsoever.

Figure A-5: Descriptive Result: Business Destroyed



Note: Descriptive examination of relationship between having a business destroyed and each outcome. As noted in the text, we do not expect this to reveal a causal relationship owing to differences between those with and without businesses.

A.6 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 *House destroyed due to barrel bomb*. 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).