Threat Perceptions, Loyalties and Attitudes Towards Peace: The Effects of Civilian Victimization among Syrian Refugees in Turkey

Conflict Management and Peace Science
Revised: December 2022
Accepted: March 2023

Abstract

For refugees who have fled civil conflict, do experiences of victimization by one armed group push them to support the opposing armed groups? Or, does victimization cause refugees to revoke their support for all armed groups, whatever side they are on, and call instead for peace? This paper studies the effect of civilian victimization on threat perceptions, loyalties, and attitudes towards peace in the context of Syrian refugees in Turkey, many of whom faced regime-caused violence prior to their departure. Our research strategy leverages variation in home destruction caused by barrel bombs to examine the effect of violence on refugees' views. We find that refugees who lose their home to barrel bombs withdraw support from armed actors and are more supportive of ending the war and finding peace. Suggestive evidence shows that while victims do not disengage from issues in Syria, they do show less optimism about an opposition victory.

1 Introduction

For refugees who have fled civil conflict, experiences with violence before leaving likely intensify their fear of the armed group that perpetrated that violence. Do experiences of victimization at the hands of one armed group push civilians into the arms of the opposing armed groups? Or, does exposure to violence cause refugees to revoke their support for all armed groups, whatever side they are on, and call instead for peace? This paper studies the effect of exposure to violence on refugees' threat perceptions, loyalties, and attitudes towards peace in the context of Syrian refugees in Turkey, many of whom faced regime-caused violence prior to their departure.

These questions are of interest to political scientists, first, because they are relevant to the experiences and political activities of millions of refugees from numerous recent and ongoing conflicts. They are also of interest for scholars of civil conflict, as civilian refugees who do not return and form diaspora communities remain influential through their connections to their country of origin, by providing a flow of remittances to fuel political and armed groups (e.g. Lindley, 2010), or even directly supplying arms and giving military support (Hockenos, 2003). Further, many civilians eventually do return home—and indeed many already have. According to official statistics, between 2016 and 2021, more than 300,000 Syrians have already returned to Syria (UNHCR, 2022c). Finally, while political and armed elites play key roles in decisions about peace and conflict, ultimately their ability to maintain order rests on appealing to what civilians want (Hoddie and Hartzel, 2010), including returnees.

Our study examines the Syrian civil war, focusing exclusively on the attitudes of refugees, against whom indiscriminate violence has been used extensively by a domestic incumbent government. Building from several strands in literature on wartime experiences and civilian victimization, we are particularly interested in how exposure to violence effects refugees' threat perceptions, loyalties, and attitudes towards peace. A substantial number of Syrians have been exposed to indiscriminate violence, much of it in the form of barrel bombings. Although precise figures are not available, more than 11,000 civilians are estimated to be killed by barrel bombs, including thousands of women and children (SNHR, 2017; Amnesty International, 2020). 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, we are only beginning to learn how the millions of civilian refugees who have fled such violence view the conflict and the parties fighting in it.[1]

[1]Recent exceptions include the Arab Barometer Survey of Syrian refugees in Jordan and Lebanon (Ceyhun, Huseyin Emre, 2017) and work by Corstange (2019, 2020) on public opinion of Syrians in Lebanon regarding the conflict. See also Mironova et al. (2019a) and Mironova et al. (2019b) that study determinants of Syrians' commitment to rebellion

Employing an original survey of 1,384 Syrian refugees living outside of camps in Turkey in the summer and fall of 2016, we ask whether a particular violent harm—losing one's home to barrel bombing—turns civilians towards insurgents or away from them and how it effects their attitudes towards compromise. This harm is by no means the only or the most important harm faced, but we focus on it because it provides a rare empirical opportunity. For most harms, a comparison of outcomes among those who did and did not face that harm would be hopelessly confounded by other likely differences between these groups. However, as we describe at length below, because barrel bomb can be targeted roughly to neighborhoods but not within them, a within-neighborhood comparison of those who did and did not lose their homes to barrel bombing can avoid or, at very least, greatly mitigate such confounding. As one would expect—and as we show below—individuals who lose homes to barrel bombs are made more vulnerable to a number of other subsequent harms, such as the loss of family members and personal injury, making it important to note that the effects we attribute to house destruction by barrel bombs are inclusive of any other harms that it made more likely.

We find, first, that those who lost their home to barrel bombing perceive the regime, which perpetrated that violence, as more threatening. This supports the expectation that violence makes civilians feel more threatened by its perpetrator (Hirsch-Hoefler et al., 2016), and confirms that they know who is to blame for this violence. This could presumably motivate civilians to more strongly support the perpetrator's opponents. Yet, and to our primary question, we find that refugees who lose homes to barrel bombs do *not* show greater affinity towards the armed opposition or any other insurgent group operating in Syria. Rather, they 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. Those who lost homes are also more approving of fellow Syrians who refuse to choose sides in the conflict and are more willing to compromise for peace. Considering possible mechanisms for these results, we find that while refugees who lost homes to barrel bombing remain engaged with events in Syria and are just as likely to say they will return to their home country, they show greater pessimism about the likelihood of an opposition victory. This last result suggests that pragmatic considerations about military effectiveness could be a plausible driver of the anti-opposition and pro-peace effects that we find.

Although we argue our conditioning strategy and the low accuracy of barrel bombs greatly reduces the scope for unobserved confounding, we also conduct sensitivity analyses (reported in the Appendix) that describe what strength of residual confounding would be required to alter

and risk preferences, respectively, using data from surveys conducted among Syrians both inside and outside Syria.

our conclusion, and show that our conclusions are very unlikely to be driven entirely by residual confounding. Further, we consider the ways in which "selection into" the study population (refugees who came to Turkey and were there at the time of the survey) threatens the (internal) validity of our estimated effect within this group, and describe analyses that help to mitigate this concern.

2 Background and Related literature

2.1 Case Specific Considerations

Our work examines how refugees' attitudes toward combatants and peace are shaped by their experiences with violence during wartime. One relevant feature of our case is that we focus on a situation with ongoing violence at the time of the research. Unfortunately, there are very few empirical studies of civilian attitudes *toward combatants during wartime* at all,[2] and less attention still has been given to how direct exposure to incumbent-inflicted violence shapes individual perceptions about insurgent groups. Undoubtedly, the paucity of studies examining actual effects of individual-level exposure to indiscriminate violence during conflict is partly due to the logistical and security challenges associated with such research. As such, there is a heavy reliance on remote, aggregate measures, such as territorial control and death counts, which fail to take civilian attitudes into account.

A second relevant feature of the particular case we study is our focus on a certain type of indiscriminate violence and one that, as we illustrate below, was perpetrated solely by the incumbent regime. Categories of violence are sometimes contentious, but 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", which targets everyone in a particular area with no effort to determine guilt or innocence (Downes, 2007). Straus (2015) usefully introduces the term "group-selective violence", where one can say *groups* are targeted but violence is effectively indiscriminate within those targeted groups or areas. The terms "categorical violence" (Goodwin, 2006; Fortna et al., 2018) and "collective targeting" (Steele, 2017) have also been used to describe similar patterns of violence.[3] These terms capture well the phenomenon of barrel bomb attacks, which we study here: The Assad regime dropped barrel bombs in opposition-

[2]Some notable exceptions are Lyall et al. (2013) in Afghanistan, Fair et al. (2016) in Pakistan and Hazlett (2016) in Darfur.

[3]Kalyvas calls all such violence "indiscriminate", whereas Gutiérrez-Sanín and Wood (2017) astutely observe that "collapsing identity-based targeting and indiscriminate violence blurs variation in how armed organizations target civilians." (pg. 22)

controlled areas of Syria without any targeting *within* those areas for a number of reasons, which we discuss in detail below. Our focus on a case where the regime is the perpetrator of violence also constitutes a contribution, as most existing studies of indiscriminate violence focus on cases involving violence perpetrated by external actors. For instance, all the cases of indiscriminate artillery shelling cited in Lyall (2009) are by foreign actors.[4] Recent published work on other instances of indiscriminate violence similarly focus on cases with external counter-insurgents, such as aerial bombardment by the US in Vietnam (Kocher et al., 2011; Dell and Querubin, 2017) or collateral damage by Coalition forces in Iraq (Kondra and Shapiro, 2012).

A third and a final feature of our case is that we focus on refugees who have escaped the conflict zone. This differentiates our study from a major thread of the literature on civil conflict to date, which has theorized and studied the responses of civilians remaining in the conflict zone, with implications for the strategic choices made by armed actors (see e.g. Kalyvas, 2006; Christia, 2012). By focusing on refugees—and particularly on the effect of experiencing home-destruction for those within this group of displaced individuals—we contribute to emerging work on the consequences of civilian victimization (Balcells and Stanton, 2021). Understanding displaced civilians' views of the regime and insurgent groups is also important for several 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. These descriptive results suggest that the population of displaced individuals that we study are likely to play a significant role in the future of Syria, whether as returnees or as a diaspora group with familial–and possibly financial–connections to the homeland.

Beyond these case specific considerations, our work engages a rich and growing literature on the consequences of civilian victimization and exposure to violence, which finds that not all civilian responses to violence are uniform (Balcells and Stanton, 2021). Below we review the relevant studies that inform our expectations about how exposure to violence can shape civilians' threat perceptions (Hirsch-Hoefler et al., 2016), their political allegiances and loyalties (Wood, 2008; Balcells, 2012a; Hadzic et al., 2020; Villamil, 2021), and their attitudes towards peace and compromise (Hall et al., 2018; Hirsch-Hoefler et al., 2016).

[4]These cases are France in Algeria, United States in Vietnam and Iraq, Soviet Union in Afghanistan, Russia during the first Chechen War, Britain in Afghanistan and Israel during the Lebanon War (332).

2.2 The Consequences of Civilian Victimization: Threat Perceptions, Loyalties, and Attitudes Towards Peace

First, and perhaps unsurprisingly, the literature on exposure to violence finds that victimization leads to an increased threat perception of the perpetrators of violence (Hirsch-Hoefler et al., 2016). Perceptions of threat relate to the appraisal of danger that the perpetrator poses to the security and well-being of the individual and, in some instances, the broader group with which that individual identifies (Canetti-Nisim et al., 2009; Huddy et al., 2002). Others who have studied the consequences of indiscriminate violence during counterinsurgency campaigns—albeit violence carried out by external actors—have indeed found that incumbent-inflicted indiscriminate violence makes civilians feel more threatened and less sympathetic towards its perpetrators (Anderson, 2005; Hashim, 2006). In our case, the perpetrator of the violence is the Assad regime, and we thus expect that civilians who lost a home due to barrel bombing will find the regime more threatening, both individually and to Syria as a whole.

Second, existing research demonstrates that violence against civilians has consequences for their loyalties and allegiances, though there is still considerable debate about precisely what those consequences are (Cederman et al., 2020). According to Kalyvas (2006), a regime that perpetrates violence indiscriminately (at the individual level) may push civilians to support the insurgents, either because these civilians become more vengeful towards the perpetrators of violence (Anderson, 2005; Hashim, 2006) or because they turn to the opposition in search of protection (Goodwin, 2001). A diverse set of empirical studies using evidence from Iraq, Vietnam, Afghanistan, and Gaza have all shown that indiscriminate violence can turn civilians away from the perpetrators of violence and towards adversarial insurgent groups (Condra and Shapiro, 2012; Benmelech et al., 2015; Kocher et al., 2011; Dell and Querubin, 2017; Schutte, 2017). Critically, these studies examine the behaviors of civilians in the conflict zone, where a security-seeking logic more readily suggests this result. By contrast, where civilians have fled the conflict zone, such a logic—and pro-opposition effect—may no longer hold, and this is, in part, the subject of our study. Researchers, however, have begun to unpack intra-case variation in civilian responses to indiscriminate violence, finding, for example, that the backlash against the perpetrators can be stronger when civilians perceive them to be "outsiders"(Lyall et al., 2013) and that civilians are only driven toward the adversary when they are located at a safe enough distance to do so (Schutte, 2017). Other relevant work finds that civilian victimization can produce moral outrage and leads individuals to side with the opposition (Wood, 2003, 2008), that it can solidify ethnic polarization (Hadzic et al., 2020), that it can influence post conflict elections (Costalli and Ruggeri, 2015), and that its effects can span

entire generations (Balcells, 2012b).

Still, some scholars point out that indiscriminate violence perpetrated by an incumbent may make civilians feel that opposition groups are incapable of providing protection or are even responsible for provoking violence (Lyall, 2009; Souleimanov and Siroky, 2016). Such violence may thus cause civilians to reduce support toward the party holding or operating in the territory that was attacked Downes (2007). For example, Schubiger (2021) finds that in Peru in the 1980s civilians responded to government inflicted violence by rallying *against* insurgents in an effort to signal their opposition to them and thereby avoid future state targeting. Furthermore, as work by Pechenkina et al. (2019) demonstrates, the consequences of civilian victimization may be contingent on one's proximity to the atrocities, with those who only experience the violence indirectly (as opposed to directly) more likely to view indiscriminate attacks as a response to rebel provocation. How civilian victimization will shape the loyalties of refugees who have fled the conflict zone thus remains an open question.

Our study also enables us to learn more about the relationship between exposure to violence and attitudes towards peace. On one side of the ledger, scholars have found that experiences with violence can render civilians more hostile and war prone. For example, Hirsch-Hoefler et al. (2016) finds that experiencing violence "hardens the hearts" against peace efforts, Canetti et al. (2017) find that exposure to violence provokes an "ethos of conflict" that hinders support for compromise, and Vinck et al. (2007) trace a link between victimization, post-traumatic stress disorder and depression, and decreased support for non-violent compromise. Similarly, work in the realm of transitional justice indicates that victimized individuals can be more likely to hold retributive preferences towards the perpetrators of violence (Hall et al., 2018; Kao and Revkin, 2021), though this may not always be the case, as shown by Dyrstad and Binningsbø (2019) and Nussio et al. (2015). Indeed, other relevant literature suggests that it is equally possible that the dominant reaction to civilian victimization is either no effect at all (Liendo and Braithwaite, 2018) or even a "pro-peace" effect, by which exposure to violence may generate more pacific attitudes and increase the desire for settlement and compromise. Several recent studies provide evidence of this pro-peace response at the individual level (Bakke et al., 2009; Hazlett, 2016; Tellez, 2018) and earlier works such as Levy and Morgan, 1986 discuss country-level notions of war-weariness.[5]

[5]Here it is worth noting that there is also a related and robust literature on how exposure to violence makes people more altruistic (Voors et al., 2010) and effects various types of "civic" behaviors and attitudes, including participation in civil society, political engagement and altruistic actions and beliefs—though these apparently altruistic positions are in some cases parochial and so may not suggest improved altruism towards out-groups. We refer the reader to Bauer et al. (2016) for an extensive review of this work, some of it quasi-experimental, on cooperation and other potentially pro-social reactions to violence.

In summary, the research cited above illustrates the active debates around both (i) whether civilian victimization perpetrated by one side leads to increased support for its adversary, and (ii) whether experiences of civilian victimization lead to a pro- or anti-peace attitudinal response. Since existing theory and empirical research could reasonably point in either direction, our aim is not to pose directional hypotheses but to provide an additional data point towards our future understanding, empirically assessing the effects of civilian victimization on support for the opposition and on support for peaceful settlement in the context of Syrian refugees who have fled to Turkey.

In addition to these empirical estimates, we consider suggestive evidence for two possible mechanisms behind our results. First, given that other research has found that exposure to violence can potentially lead to certain types of disengagement and social apathy (Pham et al., 2009), we examine whether those who lose a home also report decreased levels of engagement with the homeland more generally, measured by whether they follow the news in Syria, volunteer with the Syrian community, and intend to return home to Syria (Alrababa'h et al., Forthcoming). Second, we consider if losing a home to barrel bombing makes refugees more pessimistic about the possibility of an opposition victory. Prior work has shown that indiscriminate violence by an incumbent can lead to skepticism about the opposition (Lyall, 2009; Souleimanov and Siroky, 2016). Accordingly we test whether losing one's home decreases belief in the likelihood of an opposition victory, which could trigger pragmatic calculations whereby refugees withdraw support from an opposition that they think is unlikely to win and have an increased interest in reaching a peaceful compromise.

3 The Syrian Civil War and Displacement

The Syrian civil war was sparked by protests that began in March 2011, when a group of children 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 spread to other cities throughout Syria and were promptly met with a harsh response from the regime (Hokayem, 2013). By July 2012, the initial protests, which were largely semi-urban and peaceful, had spiraled into a brutal civil war, fought between Syrian government forces on one side and multiple rebel factions, including both secular and Islamist groups, on the other (McHugo, 2014). As the conflict escalated, the Assad regime began aerial bombardments of opposition-controlled areas, and in mid-2013, regime forces in Aleppo began using barrel bombs (Amnesty International, 2015). In response, the UN Security Council adopted Resolution 2139, which explicitly demanded an end to the victimization of civilians through the use of indiscriminate weapons, including barrel bombs (UNSC, 2014). This

resolution, however, was not at all effective in curbing barrel bomb attacks. The regime continued to use barrel bomb attacks extensively throughout the war, dropping them on a range of other opposition-controlled areas of Syria, including in Idlib, Hama, Homs, and the suburbs of Damascus (SNHR, 2017).

By 2019 the war was slowly coming to an end and the Assad regime emerged victorious, thanks in large part to heavy support from Russia and Iran. The human costs of the conflict have been devastating: Close to 600,000 people are estimated to have died (SOHR, 2020), including more than 11,000 civilian deaths because of barrel bomb attacks (Amnesty International, 2020). More than 13 million Syrians have been displaced. Among those displaced, more than 6 million had to leave Syria and have become refugees in other countries (UNHCR, 2022a). Turkey hosts the largest number of Syrian refugees (UNHCR, 2022b) and is the primary destination for Syrians exposed to indiscriminate violence, and especially to barrel bombs. According to the statistics provided by the Turkish Directorate General of Migration Management (DGMM), as of December 1, 2022, 3,570,234 registered Syrian refugees resided in Turkey. Less than 2% of these refugees are currently settled in the 7 camps run by the Turkish government, and the vast majority live among the Turkish population in urban areas. About half of all refugees are living in 4 provinces of Turkey: İstanbul, Gaziantep, Hatay, and Şanlıurfa.[6]

4 Research Design and Methods

We focus on estimating the effect of a particular type of of civilian victimization—losing one's home to barrel bombing—on threat perceptions, support for the opposition, and attitudes towards compromise. The key assumption behind our research strategy is that because barrel bombs are inaccurate weapons, they may be targeted to particular geographic areas (neighborhoods, defined in Section 4.2) but within these areas they cannot be effectively targeted to a certain building rather than others. If there is exactly zero ability to preferentially hit one building rather than another within a neighborhood, then a within-neighborhood comparison of those whose homes were destroyed to those whose homes were not would allow us to estimate a causal effect with zero confounding bias. We thus adopt a research strategy whereby we rely on within neighborhood estimates, estimating our effects by comparing those who did and did not lose their homes to barrel bombs *within* each neighborhood. Below in section 4.5, we describe how such within-neighborhood (and gender) comparisons are realized by matching on neighborhood and gender. Although we have reason to believe targeting within neighborhoods is highly unreliable, one can

[6]The province-level numbers of registered Syrian refugees in Turkey are available at `https://www.goc.gov.tr/gecici-koruma5638`.

always doubt that such assumptions hold precisely or contemplate that some degree of partial targeting is possible. Thus, thinking more conservatively, we also examine how hypothetical non-zero amounts of within-neighborhood targeting (and thus confounding) would influence the results through sensitivity analyses (Section F in the Appendix).

Here we emphasize that our goal in this analysis is "internal validity", i.e. estimating the effect within the sample in hand. However, in Appendix E.2, we describe the conditions whereby individuals "select into" our sample (i.e. by choosing to come to Turkey and to stay in Turkey until the time of the survey) can bias even our estimate of the effect within this group. We also provide analyses that help mitigate this concern in 5.5.

In what follows, we describe the arguments for barrel bombs' inaccuracy and discuss other relevant features of our research design and survey.

4.1 The inaccuracy of barrel bombs

Barrel bombs are improvised explosive devices (IEDs) typically made from oil barrels, fuel tanks, gas cylinders or other large metal containers packed with explosives and metal fragments like nails and machine parts to increase their lethality. The lack of within-neighborhood targeting of barrel bombs is supported by two arguments: technical limitations and strategic purpose. Regarding technical limitations to targeting, notwithstanding efforts reportedly made by the Syrian military to predict where barrel bombs may land by considering wind speed and other factors,[7] such strategies appear to be insufficient to direct bombing below the neighborhood level. As SNHR (2017) reports at length, helicopters drop these bombs, weighing between 50 and 1000kg.s, while still moving and from an altitude of three to five kilometers in order to minimize exposure to man-portable air defense (MPAD) surface-to-air missiles. In evidence of their inaccuracy, although one helicopter frequently drops multiple bombs (generally two to four) within a few seconds, the individual bombs may land 500 meters apart from one another (SNHR, 2017, 14).

The second argument for the indiscriminate nature of barrel bomb attacks derives from the Assad regime's objective in using them: to make certain areas inhospitable to civilians so they either withdraw support for the opposition or leave the area. The goal of these instances of bombing was not to kill rebel fighters. In fact, barrel bombings did not 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 civilian communities. As one member of the Civil Defense, a voluntary group of rescue workers, in Aleppo told Amnesty International in

[7] Gibbons-Neff (2015). We thank an anonymous reviewer for noting this.

2015, "The people who have been killed are not the people who were fighting." Furthermore, we know from interviews and the reporting of humanitarian organizations that civilians on the ground can see and hear a barrel bomb fall, yet they cannot know exactly where it will ultimately strike. This inability to discern where destruction will occur prevents civilians from effectively avoiding harm. A shopkeeper in Aleppo remarked, "After you see the barrel falling you don't know where to go...Sometimes we accidentally run towards the barrel." (Amnesty International, 2015).

4.2 Survey Sampling and Location Data

In the summer and fall of 2016, we surveyed 1,384 out-of-camp Syrian refugees in the four Turkish provinces with the highest number of Syrian refugees: İstanbul, Hatay, Şanlıurfa and Gaziantep. A detailed description of our survey design, sampling strategy, and the ethical procedures we followed are available in the Appendix A.

In practice, obtaining the location of respondents' homes in Syria presented logistical challenges, as only 18% were able to locate their homes using Google Maps on their enumerator's smart device to obtain GPS coordinates. Instead, the most accurate method we were able to employ began by asking participants to identify the governorate, city and neighborhood in which they used to live. If they are from a rural area, we then asked which village they are from. Using this information about respondents' original homes, we matched each respondent to one of the administrative units in Syria, using the list provided by United Nations Cartographic Section (UNCS) and United Nations Office for Coordination of Humanitarian Affairs (OCHA).[8] For our respondents from the capital cities of governorates such as Aleppo or Ar-Raqqa, these administrative units are neighborhoods in these cities. For our respondents from outside the cities, these administrative units are either small provincial towns or villages.[9]

The mean and the median area of the 27 urban neighborhoods included in our post-matching sample (see below, Section 4.5) are 1.18 and 0.92 square kilometers, respectively. The mean and the median area of non-urban neighborhoods are 5.92 and 3.82 square kilometers, respectively. Note that a circle with an area of 1 square kilometer has a radius of 564 meters. If two bombs dropped together can land 500m apart, as reported—and there is further inaccuracy in timing and positioning of the drop itself relative to a given target—this leaves little hope for even approximate targeting of blocks or buildings within a given neighborhood, particularly in urban neighborhoods. Conditioning on neighborhoods of this size therefore helps considerably in ensuring individuals

[8] See `https://data.humdata.org/dataset/syrian-arab-republic-administrative-boundaries-populated-places`

[9] 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 available list of administrative units.

within these units have similar risks of having their homes destroyed. As we describe below, our results hold when we limit our analysis to urban neighborhoods, or to neighborhoods below the median size.

4.3 Home destruction

To study the effects of civilian victimization, our analyses focus narrowly on whether individuals' homes were destroyed by barrel bombs as the harm in question.[10] This measure is uniquely well suited to the inferential challenges at hand because whether a person's house is destroyed (or not) is based only on its location relative to where barrel bombs happen to strike. That is, the inability to target barrel bombs within a neighborhood alone implies that the probability of a home being destroyed does not vary within neighborhood—regardless of factors such as how risk tolerant a person is, their attitudes, their support for the opposition, the behavior of their family members, etc. We emphasize that suffering harms from violence beyond having one's home destroyed clearly matters, both in human terms and in terms of their effects on civilian attitudes, even if we have no means to examine their causal effects. A complete description of the concerns that prevent us from using measures other than *House destroyed due to barrel bomb* is given in Section D in the Appendix.

An important assumption we require is that our respondents know with reasonable accuracy whether their homes were destroyed by barrel bombing, despite them possibly having left before this occurred. Refugees that we interviewed prior to the survey relayed confidence that they and others 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.[11] If one witnessed their home's destruction, they will know for certain the cause. If any neighbors witnessed the destruction, again refugees will often find out about the extent of the damage and the cause of the destruction, as they are understandably highly motivated to seek out this information through their social networks and any other resources available to them.[12]

Available eyewitness accounts of attacks that involve both barrel bombs and missiles also

[10]The exact wording of the question is: "Was your home at that time destroyed or damaged so badly as to make it unlivable?", with the answer categories "yes" or "no".

[11]As one interviewee described it, "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." Interview conducted on July 2, 2018.

[12]We later came to realize that in the context of our survey, it is possible for participants to have mistakenly thought we were asking if their house was destroyed only prior to when they left. It is unlikely that this is the case for most respondents, owing to the relatively large proportion of respondents saying their house was destroyed by barrel bombs (22% in our sample), despite many leaving in the earlier part of the violence. However if this did occur, it would cause a fraction of those whose homes were destroyed to report otherwise, mitigating the effect we find towards zero.

indicate that witnesses were able to distinguish between these different types of attacks and the damage they cause. For instance, an eyewitness account of multiple airstrikes on and around a Red Crescent center in Urm al-Kubra in Western Aleppo in 2016 describes it as follows: "I stopped about 200 meters away from the Red Crescent center when I heard the sound of the helicopters attacking the location. I saw two helicopters drop four barrel bombs at approximately 7:12. A few minutes later, another strike was carried out by two Russian warplanes which targeted the same location with two thermobaric missiles that caused a huge explosion and fires. We couldn't see anything in the darkness of the night but the flames. After that, the helicopters came back and dropped another four barrel bombs." (SNHR, 2016)[13] Similarly, one Syrian interviewee working with children who had witnessed bombings told us that even they were able to discern between barrel bombings and other types of airstrikes based on the sounds that accompanied such acts of violence.[14] Consistent with the purpose of driving civilians out, barrel bombing largely occurred in neighborhoods that were not yet emptied, and thus where witnesses were present.[15]

We note that to the degree there may be non-random 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. Finally, regarding the context and who civilians hold responsible for these attacks, it is widely known that these barrel bombs have only been used by the Syrian regime, and not by Russian or international coalition forces (SNHR, 2017).

4.4 Outcome and Mechanism Measures

Here we briefly describe our outcomes and mechanisms of interest and the names of the variables (italicized) that we construct. The full wording of the questions from the survey and the description of how the variables are constructed are in the Appendix B.

[13]Several other eyewitness accounts of barrel bomb attacks in different governorates of Syria are available in SNHR (2017).

[14]Interview conducted in Kilis, Turkey on October 8, 2015.

[15]In our sample, when asked if their home was barrel bombed, only 9 percent of our respondents from barrel bombed neighborhoods chose "don't know" or "no response". In another survey conducted by the Turkish Disaster and Emergency Management Authority (AFAD) among 2,461 Syrian households in Turkey, only about 16 percent of respondents did not know the status of their house (AFAD, 2017).

Outcomes

Our first two primary outcomes are designed to measure threat perception, with the expectation that individuals who lost homes to barrel bombing will find the Assad regime to be a greater threat to themselves and to the country. Using the answers to two different questions in which individuals choose the group that they consider as the biggest threat to themselves and to Syria, we constructed two variables: *Top threat to Syria: Assad*, *Top threat to you: Assad*.

The next and arguably most important outcome measure assesses loyalties, looking at the degree of support for the warring parties, using the answers to an open-ended question in which respondents were free to name any party to the conflict they wished: *Support opposition*; *Support no party*. Our other primary outcomes assess attitudes towards compromise, neutrality and peace: *Compromise for peace; Fight until victory; Neutrality acceptable; Support peace if family does*.

Mechanisms

We consider evidence for two possible mechanisms behind our results: First, a reason victimized individuals may report being less supportive of the opposition or continued fighting might be simply that they are less socially engaged and active overall, or have little intention of ever returning to Syria, and therefore are indifferent about the actors in the conflict. To consider this general issue of engagement, we focus on respondents' intention to return to Syria (*Will likely return*), how closely they follow the news from Syria (*Follow Syria news*), and whether they do volunteer work for other refugees (*Volunteer for refugees*).

Second, we examine respondents' assessment of the opposition's ability to achieve victory (*Opposition could win*), as this could potentially alter considerations about the benefits and drawbacks of supporting the opposition and favoring peace.

4.5 Estimation: Matching and Regression with Sensitivity

As mentioned above, our research strategy requires conditioning on neighborhood, i.e. comparing those who did and did not lose their homes to barrel bombs *within* each neighborhood. Furthermore, we also make comparisons only within gender, both in the interest of conducting separate analyses by gender, and because we note a moderate (but insignificant) tendency for more men than women to report losing their homes to barrel bombs. We note that a larger percentage of men also report large scale damage to their homes than women in the survey conducted by the Turkish Disaster and Emergency Management Authority in 2013 (AFAD, 2013). Conditioning on neighborhood and gender is straightforwardly achieved by exact matching, using the `Matching`

package for R (Sekhon, 2011). As employed here, 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.

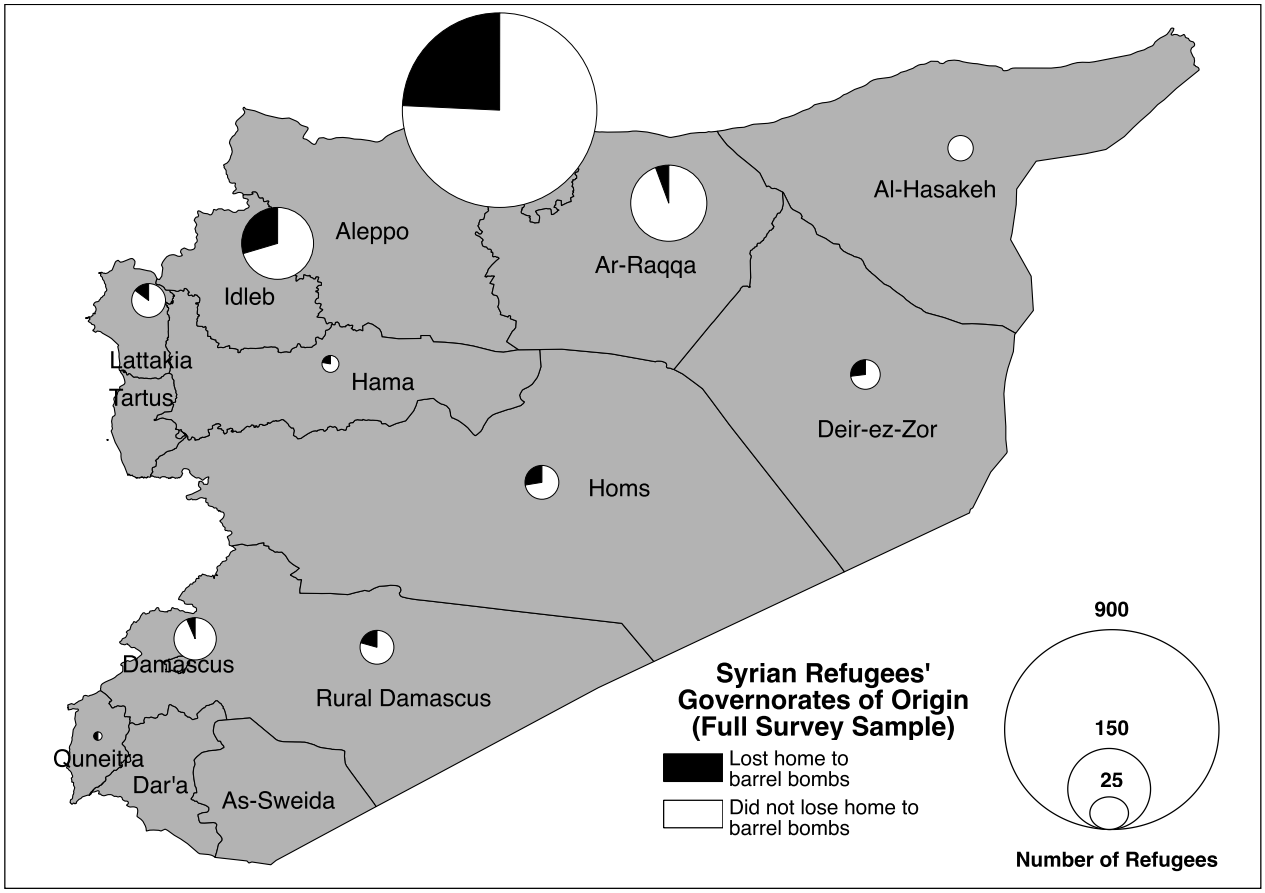
Finally, we additionally present results from a regression (OLS) model, which includes available pre-treatment covariates, with an indicator for gender and fixed effects for neighborhood. This regression model is then used in the sensitivity analyses to determine what bias would be suffered due to confounders of varying strengths, described in Section F in the Appendix.

5 Results

5.1 Descriptive Results

The diverse geographic distribution of our sample of 1,384 respondents is given in Figure 1, by Syrian governorate. While 67% of the respondents are from Aleppo, 10% are from Ar-Raqqa, and 9% are from Idleb. Among those from Aleppo, 24% lost their homes to barrel bombs. In Ar-Raqqa and Idleb, 6% and 30% of the respondents lost their homes to barrel bombs, respectively.

Figure 1: Survey sample



Our sample is relatively well-balanced on gender with 37% 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.[16] The descriptive statistics for additional variables are shown for the full sample in Table 1.

While we do not seek to make claims about the external validity of our findings, in the Appendix G, we provide detailed descriptive statistics that compare our sample to the Syrian population before the war, to another survey of Syrian refugees in Turkey, and to surveys of Syrian refugees in neighboring Lebanon and Jordan. These comparisons show that the composition of our sample is not very different than the Syrian population before the war, or the broader Syrian refugee population in Turkey and in the other neighbouring countries of Lebanon and Jordan.

Table 1: Descriptive characteristics of sample

	Mean	Std.Dev.	N
Demographics			
Male	0.63	0.48	1384
Age	38.61	11.86	1260
Employed before attack	0.54	0.50	1366
Kurdish	0.13	0.33	1384
Education	1.59	1.03	1102
Children	0.92	0.27	1384
Rooms in house	3.10	1.61	1334
Lived in an urban area in Syria	0.67	0.47	1353
Has family members in Syria	0.90	0.30	1384
km to market	0.94	0.88	1376
km to school	0.65	0.57	1379
km to hospital	1.56	1.08	1375
Year left Syria	2013.62	1.27	1383
Outcome Variables			
Top threat to Syria: Assad	0.35	0.48	1267
Top threat to you: Assad	0.48	0.50	1258
Support opposition	0.50	0.50	1384
Support no party	0.49	0.50	1384
Neutrality acceptable	0.33	0.47	1290
Compromise for peace	0.43	0.50	1072
Fight until victory	0.08	0.28	1072
Support peace if family does	0.78	0.42	1328
Mechanism Variables			
Follow Syria news	0.50	0.50	1384
Volunteer for refugees	0.64	0.48	1225
Will likely return	0.87	0.33	1335
Opposition could win	0.89	0.31	1056

Note: Descriptive statistics on key demographic statistics, on each outcome variable used, and on variables that we examine regarding potential mechanisms.

[16]See e.g. Giebler, 2015. The share of women in Corstange (2020) is similar to ours, 40 percent.

5.2 Distribution of Violence

Out of 1384 participants in our survey, 303 (22%) report losing their homes to barrel bombs.[17] In the matching analyses below, we can only use participants who provided accurate information on their neighborhood of origin, which reduces the numbers to 264 who lost their homes to barrel bombs and 832 who did not.

The violence experienced by those in our sample is almost entirely due to the regime and its supporters, 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, a predominant form of violence people would be likely to experience due to insurgents is 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 at the time we surveyed: massive violence due to the Assad regime and its supporters, mostly through aerial forms of bombardment.

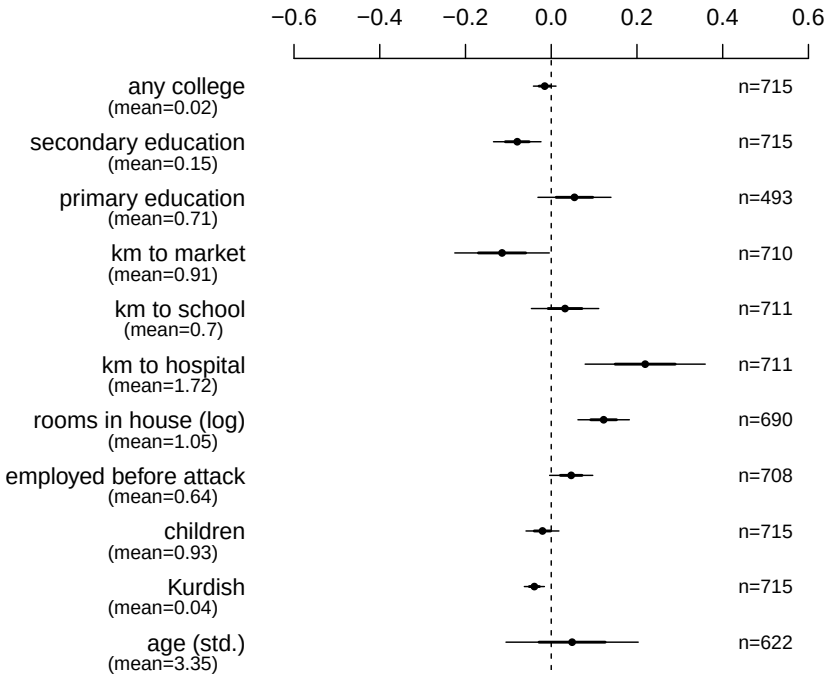
5.3 Balance

Before proceeding to effect estimates, an empirical implication of our "no within-neighborhood targeting" assumption is that within a given neighborhood, we would expect those whose homes were destroyed to be similar to those whose homes were not destroyed in terms of their pre-treatment characteristics. That is, we expect "conditional balance", though this is insufficient to ensure the identification strategy holds, as it is blind to unobserved confounders.

To test for conditional balance, we simply use the same matching approach we use for effect estimation (exact matching on neighborhood and gender), but consider each pre-treatment covariate as if it were an outcome. We hope to find no (placebo) "effect" of having one's house destroyed on these covariates if losing one's home to barrel bombing is indeed random within each neighborhood-gender stratum. We use this procedure to check conditional balance on covariates available to us in the survey that are effectively "pre-treatment", i.e. unaffected by barrel-bombing. These include the (log) number of rooms in the house (*rooms in house (log)*), whether the respondent spoke Kurdish (*Kurdish*), whether the person worked prior to leaving (*employed before attack*), number of children prior to the crisis (*children*), and age in years, which we standardize to improve visualization (*age (std.)*). To test whether houses that were destroyed tended to be closer to schools,

[17] 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 D.2 in the Appendix. We inquired about various forms of 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.

Figure 2: Conditional Balance/ Placebo Test



Note: Plots showing conditional balance, given neighborhood and gender. 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. The n shown gives the number of unique treated units plus unique control units contributing to each estimate. Whiskers show the 90% (thick) and 95% (thin) confidence intervals using Abadie-Imbens standard errors.

markets, or hospitals (which might be expected if there is effective within-neighborhood targeting that defies our no within-neighborhood targeting assumption), we check balance on the approximate distances from each participant's home to the nearest market (*km to market*), school (*km to school*), or hospital (*km to hospital*).[18] We also include education, with indicators for completing primary school (*primary education*), secondary school (*secondary education*), and any college (*any college*).[19] Note that balance is irrelevant for neighborhood-level features, such as urban/non-urban or neighborhood size, since the matching approach conditions on neighborhood. Further, some variables that may at first seem to be of interest for balance testing are post-treatment, such as self-reported socio-economic status, which would very likely be influenced by having one's home destroyed.

The conditional balance as visualized in Figure 2 is imperfect. While some variables such as age,

[18]The distance variables are approximations constructed as follows: we asked individuals about the walking time to each of these locations, giving options of zero to five minutes, between five and 15 minutes, 15 to 30 minutes, or more than 30 minutes. We then replace the answer with a numerical value using the middle of each interval (i.e. 2.5 minutes for the zero to five minute category), using 45 minutes for the "over 30" category. We then divide this by 15 to achieve approximate distances in kilometers (km).

[19]All participants in the survey were 18 or over when surveyed, but could have been 14-15 years old in 2012. Thus, for a small fraction of younger participants, it is possible that *any college* or even *education secondary* could be influenced by exposure to barrel bombs. However, since this pertains to only a small portion of the sample, we decided to consider them pre-treatment.

number of children, and employment in Syria are relatively well balanced, we also see some notable imbalances. Perhaps the most striking imbalance is on *km to hospital*. This imbalance is relatively small in real terms, at 0.22 kilometers (or a standardized imbalance of 0.20 SD), and notably, not in the anticipated direction of concern: Had it been possible to target barrel bombs, knowing that hospitals were a target in general, we would worry that those losing their homes to barrel bombs would be nearer to hospitals, but we find they are slightly farther. It is possible that hospitals were effectively targeted by other weapons such as rockets or mortars, and that barrel bombing was then less concentrated in these areas.[20] On the other hand, we see imbalance in the opposite direction on *km to market*, again with a small magnitude (0.11 km or a standardized imbalance of 0.13 SD). Additional imbalances include that those with secondary or college education are slightly less likely to have lost their home, and homes with more rooms were more likely to be destroyed. The latter imbalance is mechanically sensible, simply because homes with more rooms are larger and thus more likely to be hit. This could in principal generate a bias due to socioeconomic differences between those whose homes were and were not destroyed. These imbalances reinforce that while one would ideally estimate effects under an assurance of zero confounding bias, we must also consider how varying degrees of confounding might impact the result and particularly how much confounding would be required to alter the conclusion through sensitivity analysis (reported in Section F in the Appendix).

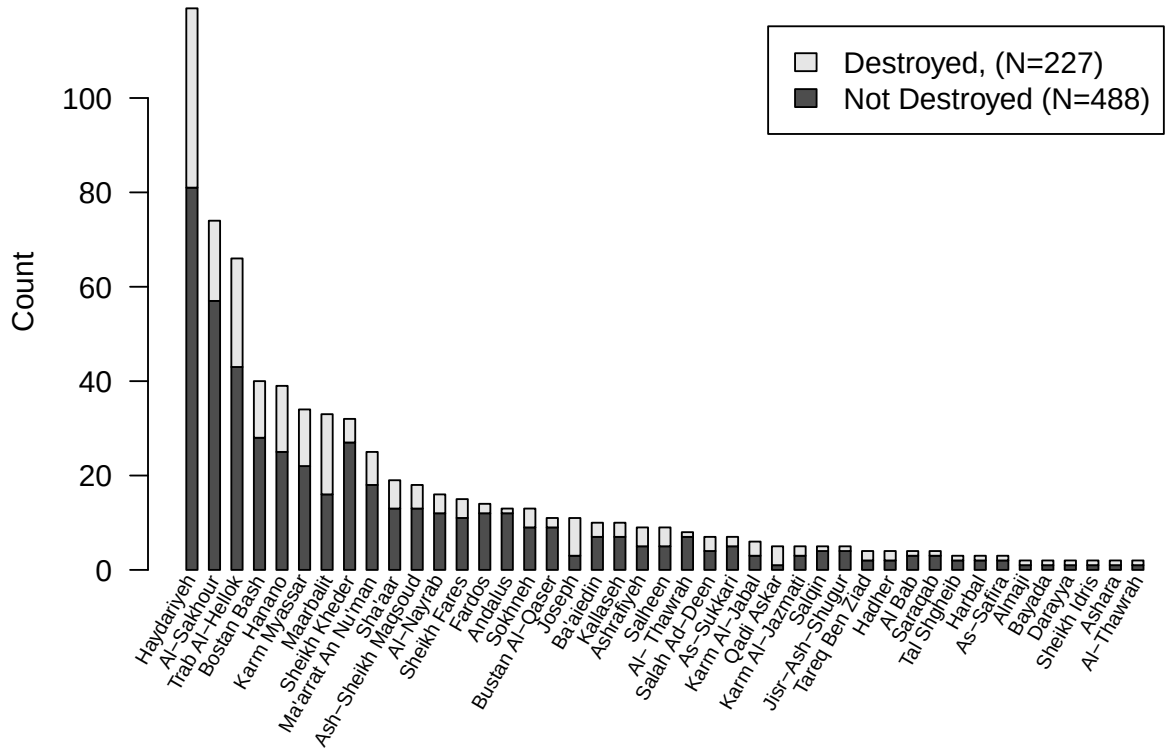
5.4 Main Results

As is often the case with matching estimators, the estimate 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, or ATT). The post-matching sample contains 227 individuals whose homes were destroyed by barrel bombs, after dropping 37 individuals from the "treated" group for whom a match could not be found. The "control" group is drawn from 488 unique observations. The total number of unique observations found in each geographic location is shown in Figure 3.

Urban neighborhoods, mostly in Aleppo, make up 27 of the 43 neighborhoods in the post-matching sample, while the remaining 16 neighborhoods are in small provincial towns. About 83% of the unique observations after matching come from Aleppo, and out of these, 97% are from the city of Aleppo. The map in Figure 4 shows the number of (unique) individuals contributing to the estimate falling in various neighborhoods of Aleppo city. The proportion of refugees in the matched sample from Aleppo is large because among all governorates in Syria, Aleppo suffered the

[20] We thank an anonymous reviewer for raising this possibility.

Figure 3: Respondents by neighborhood after matching



Note: Barplot showing number of (unique) respondents included in the data from each neighborhood, after matching on location and gender, for those who lost homes (grey) or did not (black) due to barrel bombing.

most concentrated number of barrel bombings.[21] 12% of the unique observations after matching come from Idleb, and 3% are from Ar-Raqqa. 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).[22]

Figure 5 shows effect (ATT) estimates for losing one's home to barrel bombs, having matched on neighborhood and gender. Whiskers show 90% (thick) and 95% (thin) confidence intervals, using the Abadie and Imbens (2006) standard errors. Numerical results are available in Appendix Table C.1.

The first two results regard effects on perceived threats. Those who lost homes to barrel bombing are 20 percentage points more likely to find Assad to be the number one threat to the country, and 15 percentage points more likely to say Assad is the number one threat to them personally ($p < 0.001$). Given the paper's theoretical motivation to understand whether civilian

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

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

Figure 4: City of Aleppo: Locations included in matched sample

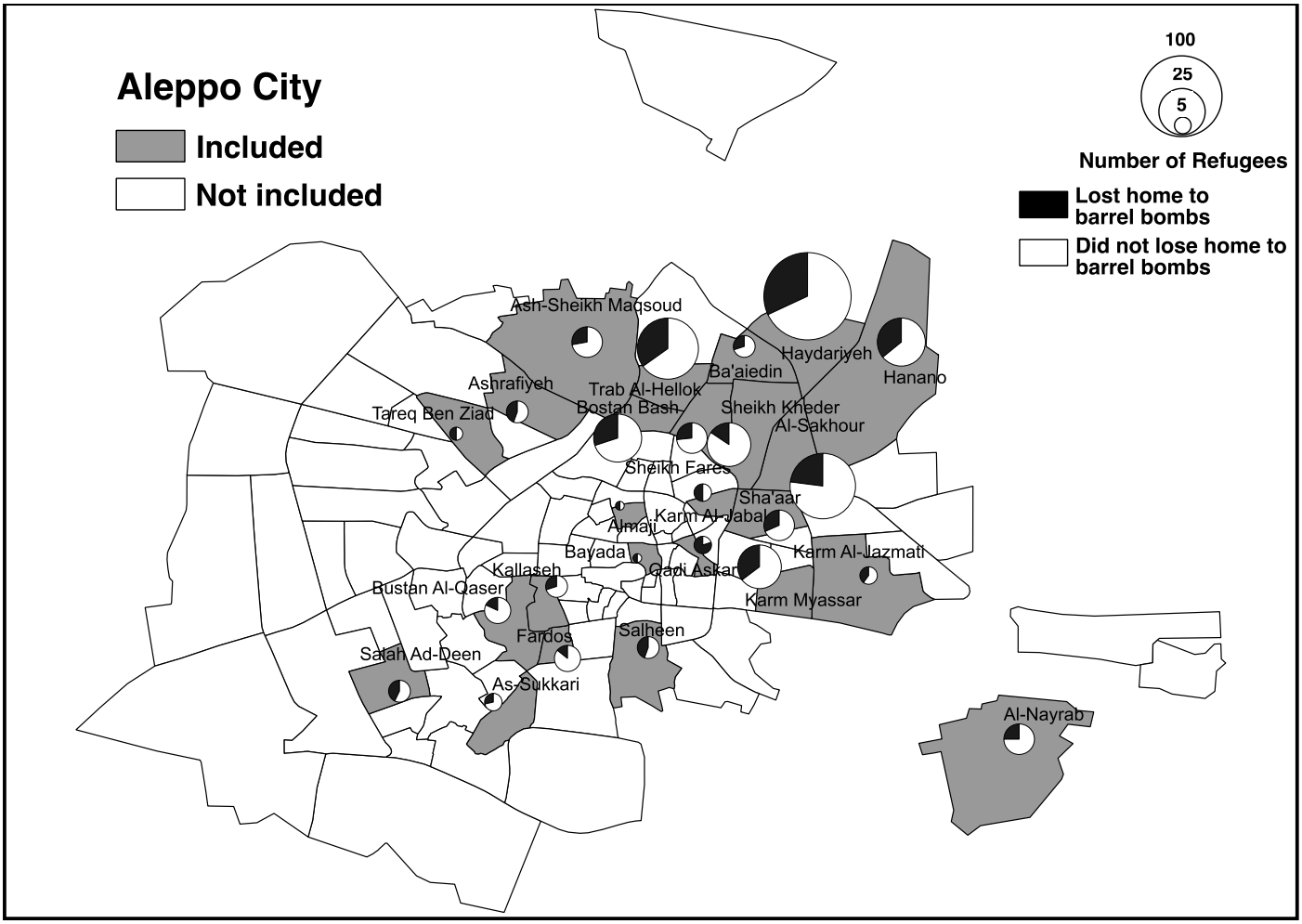
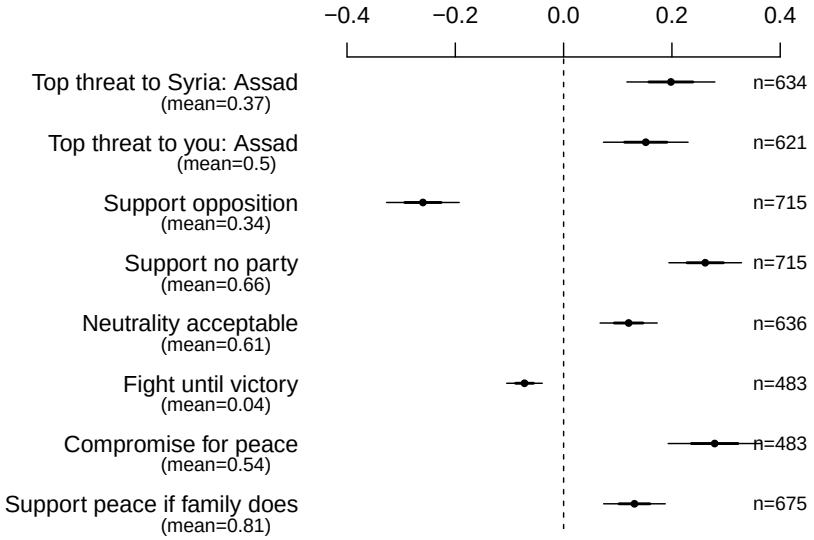


Figure 5: Main Estimates: Effects of Home Destroyed by Barrel Bombing



Note: ATT estimates for the effect of having house destroyed on attitudes related to perceived threat and security. Conditioning on neighborhood and gender is done by exact matching. Whiskers show the 90% (thick) and 95% (thin) confidence intervals using Abadie-Imbens standard errors.)

victimization turns people towards or away from the opposition, it would also be interesting to see the effect on naming the opposition as the primary threat to Syria. However, fewer than 1% do so

in the entire sample.

The remaining results shown in Figure 5 are about loyalties and attitudes towards compromise and peace. Regarding loyalties, support for the opposition is 26 percentage points *lower* among those who lost homes to barrel bombs ($p < 0.001$). No other party appears to gain that lost support. Instead, a corresponding increase is seen for "no party": those who lost homes to barrel bombs are 26 percentage points *more* likely to say that "no party" represents their views ($p < 0.001$).[23] With respect to attitudes towards compromise and peace, when asked if it is acceptable for other civilians to refuse to take a position in the current conflict, those who lost homes to barrel bombs are actually 12 percentage points ($p < 0.001$) *more* likely to report that neutrality is acceptable. This group is also 7 percentage points ($p < 0.001$) less likely to demand that the opposition fights until an outright military victory. Perhaps most notably, those who lose homes to barrel bombing are a surprising 28 percentage points more likely to support plans that call for compromise in order to achieve peace ($p < 0.001$). Finally, individuals who lost homes to barrel bombs were 13 percentage points (SE=0.003) more likely to agree or strongly agree to call for peace made by a family member under this condition compared to those who did not lose their home to barrel bombing ($p < 0.001$)[24]. Collectively this is evidence of a pro-peace effect of losing one's home to barrel bombing. We note that this set of findings runs contrary to the biases that would most naturally be expected if the "no within-neighborhood targeting" assumption fails: within neighborhood, if there is variation in the risk of having one's home destroyed, one would expect it was those who were more militant and resistant to peace who would be targeted, whereas we find the opposite.

Several additional analyses have been reserved for the Appendix. While the above analyses have conditioned at the finest geographic level available, the size of these units can vary considerably. Limiting our analysis only to the urban areas (removing the 16 non-urban units that make it through the matching process) therefore allows finer conditioning that makes targeting within these units even more difficult. As shown in Appendix Figure E.5, the results are very similar, supporting the same substantive conclusions. Similarly, limiting the analysis to the neighborhoods at or below the median size again produces similar results (Figure E.6). We note that we might have reasonably focused only on the urban areas in order to maximize the credibility of the no within-neighborhood targeting assumption, but chose to use the more inclusive estimates as our main results, particularly since they do not materially differ. Given the composition of the opposition has

[23] As readers will recall, this was an open-ended question. Since fewer than 1% said Assad most closely represents them, we do not show effects for this.

[24] The purpose of this question was to disarm a possible social prohibition against calling for peace in this community, by proposing that a family member already supports the idea.

changed over time, we also look at whether the effect of home loss varies by the respondents' year of departure from Syria (Figure H.9 in the Appendix). Our results are remarkably similar across different windows of time. Finally, we also conduct sensitivity analyses (Cinelli and Hazlett, 2020) which show that our conclusions are very unlikely to be driven entirely by residual confounding (Section F in the Appendix).

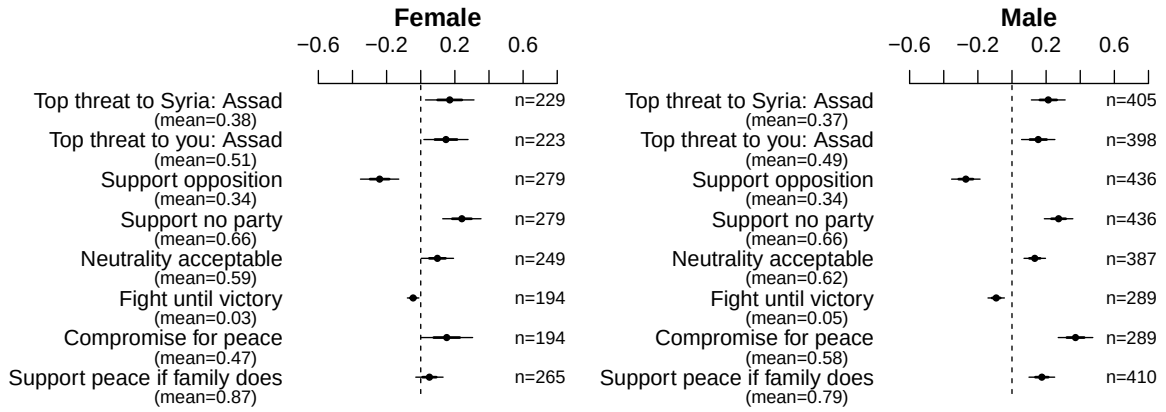
5.5 Selection Into the Sample and Internal Validity

One concern to keep in mind in this study, as in any study of selected populations such as refugees, is the role of "selection into the sample". Here, the selective nature of sampling encompasses a number of choices and characteristics besides agreeing to participate in the survey once asked: our participants are drawn from a group for whom every member (i) *remained alive* at the time of the survey; (ii) opted to *leave Syria, and specifically to come to Turkey* at some point prior to the survey; and (iii) opted to *remain in Turkey* at least until the survey was conducted. Such selection forces are certainly in play, so the question is what this means for the inferences we seek to make about the sample in hand. Specifically, the concern is whether selection into the sample results only in changing the population *about whom we make an inference*, or if it also threatens the internal validity of the effect we estimate within this population. While we cannot rule out the threat to internal validity entirely, in Appendix E.2, we discuss several scenarios in which seemingly plausible selection processes would or would not bias our estimate and the possible directions of these biases.

A related analysis we conduct to rule out threats to internal validity begins with the supposition that, if selection into the sample biases the effect estimate, it quite possibly does so in different ways for women than for men, since women and men face very different reasons and pressures in deciding whether to leave Syria and stay in Turkey.[25] In Figure 6, we split the sample by gender and conduct the same analysis in both subsets. Some variation and loss of statistical power are expected, but the pattern of results is very similar across genders. This suggests that if selection into the sample is biasing our estimates of the effects within this group, it either does so very little, in the same ways for men and women, or in ways that happen to perfectly counteract existing differences in the true effects between the genders, across all eight outcomes.

[25] See e.g. Pearlman, 2016. For a report that looks at the composition of Syrian refugees travelling to Europe in 2015, and shows that the majority of these refugees are young and male, see REACH (2015). Based on about 170 interviews conducted in Turkey, Schon (2019) argues that Syrian men and women exhibit different migration timing patterns, possibly because of forced conscription in Syria.

Figure 6: Effect Estimates by Gender



Note: ATT estimates for the effect of having house destroyed on attitudes related to security, by gender. Conditioning on neighborhood is done by exact matching. Whiskers show the 90% (thick) and 95% (thin) confidence intervals.)

5.6 Mechanisms: Disengagement and Pessimism about an Opposition Victory

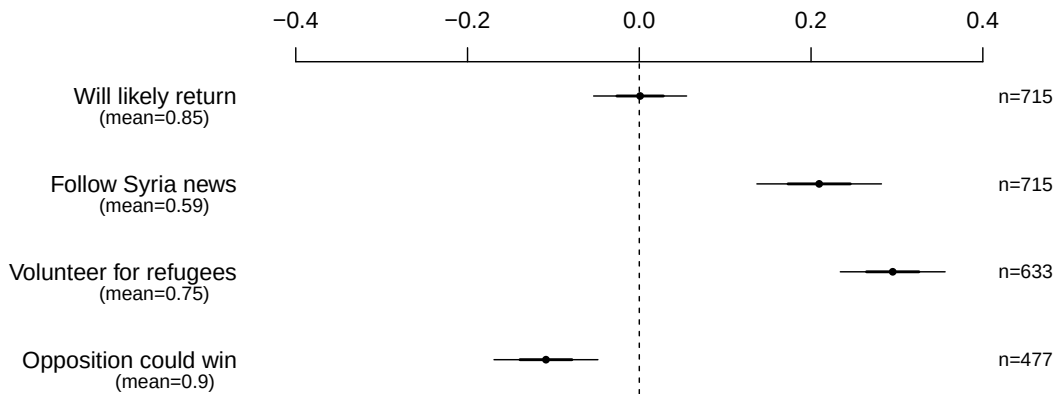
Why might refugees react to regime-caused destruction in this way, turning against perpetrators and not only failing to show increased support for the opposition, but showing *reduced* support for them and an increased desire for peace?

First, note that we can rule out that individuals were directly attacked by the opposition (See Section 5.2). Second, a key question is whether we are measuring here the effect of only losing one's home to barrel bombs, or the combined effect of that loss together with any other loses that it makes more likely as a consequence. In Appendix D.1, we show the estimated effect of losing one's home to barrel bombing on a range of other experienced harms. As might be expected, losing one's home to barrel bombs makes it more likely that civilians will experience other barrel bomb related harms, namely having family members killed or injured by barrel bombs, being injured oneself by barrel bombs. These increases are fairly small, all falling in the range of 5-10 percentage points. Moreover, the fraction of the sample experiencing these harms remains quite low (see Figure D.2 in the Appendix), with only 2% being injured by barrel bombs, and 4-6% experiencing the other harms. In this way, while losing one's home to barrel bombs can be linked to these other harms, on the whole very few people experience them, and thus mechanically they can explain very little of the variation in the outcomes.

We next consider two other possible explanations for our results. First, civilians who lose homes could disengage from community concerns or political events in their homeland, perhaps but not necessarily due to social apathy, withdrawal, or even psychological consequences such as

depression and PTSD (see e.g. Pham et al., 2009). Such disengagement could lead individuals to feel indifferent towards or unrepresented by any party to the conflict and, what is more, those who lose homes might simply be less likely to report wanting to return to Syria. Second, victimized civilians could become pessimistic about the likelihood of an opposition victory, leading them to pragmatically withdraw support from the opposition and develop a preference for peace and non-violent compromise.

Figure 7: Effect of barrel bomb home destruction on mechanism variables



Note: ATT estimates for effects of losing one's home to barrel bombs on engagement with events in Syria and views on opposition victory. All procedures are identical to those used to estimate the main effect estimates above; only the outcome variables differ.

Figure 7 shows the estimated effects of losing one's home to barrel bombs on a serious of intermediate outcomes related to the mechanisms just described. These are estimated using the same procedure—matching on gender and neighborhoods—as the main effect estimates, and for the same reasons.

We find little to no evidence of disengagement. Those who lost homes to barrel bombs are just as likely to say they will return to Syria as those who did not. They also report following what is happening in Syria "very closely" or "somewhat closely" in the news 21 percentage points *more* often, and are 30 percentage points more likely to report supporting their fellow refugee community through volunteer work, compared to those who did not lose their homes to barrel bombs. We thus find no evidence for a loss of interest in what is happening in Syria among those who lost their homes, and further see an apparently pro-social response towards other fellow refugees. The latter result is 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).

Turning to the second mechanism, we find that losing one's home to barrel bombing does

appear to diminish faith in the opposition's ability to win the war (Figure 7).[26] Those who lost their homes to barrel bombs were 11 percentage points less likely to say that they felt the opposition had at least an even chance of achieving a military victory.[27] This reduction in optimism is seen against a high level of average optimism at the time, with 90% of the matched sample believing that the opposition had at least an even chance of victory. This suggests the possibility that those who lost their homes turn away from the opposition in part because of this loss of faith in their chances for victory. Such a mechanism differs from those identified in the literature, which has largely focused on the opposition's inability to provide protection (Lyall, 2009) and a belief that opposition activity provoked regime bombings (Schubiger, 2021; Pechenkina et al., 2019). That said, we cannot distinguish among the various causal orderings: reduced optimism about the opposition's chances of success could lead to withdrawal of support, withdrawal of support could occur first by another means and then get retrospectively justified by claims that the opposition were not going to win anyway, or both the withdrawal of support and the diminished faith in their chances of success could be the results of some other process, without one causing the other.

6 Discussion

Among refugees, the experience of regime-caused violence before leaving is expected to increase antipathy towards the regime. Like others who have studied the consequences of violence during counterinsurgency campaigns—albeit violence carried out by external actors (Anderson, 2005; Hashim, 2006)—we find the expected evidence that incumbent-inflicted violence makes civilians feel more threatened by its perpetrators.

By contrast, the consequence of regime-inflicted violence on attitudes toward the opposition are more difficult to predict. We could reasonably expect either that losses due to regime bombing fuel support of the opposition, or that civilians who lose homes from regime inflicted violence choose to turn away from the opposition as well. We find strong evidence of the latter in this case. Among Syrian refugees we surveyed in Turkey, those who lost their homes due to regime-led barrel bombing were far less supportive of the opposition, with a concomitant increase in the proportion saying that "no party" represents their views. Those refugees who lose their homes to barrel bomb attacks are also more open to compromise, want fighting to end, are accepting of other community members who wish to remain neutral and are less adamant that they take a side — all of which points to a pro-peace response to experiencing the type of violence we study. All

[26] We thank an anonymous reviewer for raising this point.

[27] We get similar results when we look at the effect of losing home on alternative codings of this dependent variable with cutoffs at "somewhat likely" or "very likely".

of these estimates are very similar by gender. While we argue there is relatively little room for confounding given our reliance on within-neighborhood comparisons and the inaccuracy of barrel bombs, even confounding as strong as any of the observed variables, such as distance to the nearest hospital, would not substantially change the estimate.

Further, the two main mechanisms we anticipated could generate this response were a lack of engagement and pessimism about the opposition's chances for victory. We find no evidence that our effects are driven by social withdrawal or apathy, as individuals who lost their homes are also reportedly more likely to follow the conflict in the news, to volunteer to help refugees locally, and have similar intentions of returning to Syria. Pessimism about an opposition victory, however, does seem to provide a plausible explanation for the anti-opposition effect of exposure to regime-caused violence. For those who lose a home, the opposition's failure to protect their property and person may ignite pragmatic calculations about the opposition's lack of military efficacy, which then leads them to withdraw their support from the opposition and become more open to compromise and peace.

Here, it is worth noting that although losing a home to barrel bombing does appear to lead to a greater desire for peace and compromise, it does not necessarily translate into pro-social attitudes towards members of the regime itself. We note here that those who lose homes to barrel bombs also prove less willing to provide life saving support to a regime member in need, by 11 percentage points ($p < 0.003$). As this result suggests, there will likely be steep hurdles when it comes to fully reconciling relations between the victims and perpetrators of violence and to successfully reintegrating the refugee population back into Syrian society.

6.1 Conclusions, limitations and next steps

Civilians, including displaced civilians, play a key role in conflict dynamics. Indeed, as is well established in the literature, most of the people who support insurgencies are civilians, not combatants (Kao and Revkin, 2021; Petersen, 2001; Arjona, 2016). As our work highlights, practitioners of peace thus potentially face an uncomfortable trade-off when confronted with widespread violence against civilian populations by incumbent regimes: allowing or assisting people to leave the conflict zone is the life-saving option, but it does mean that the perpetrators of violence can be "successful", both in removing the population from the war zone and in weakening support for armed opposition. This finding serves as an important reminder that violence does sometimes achieve its intended purposes, making it all the more important for the advocates of peace to seek a timely end to violence, especially violence directed at civilians.

This study is not without its shortcomings. First, we have endeavoured to minimize the scope for confounding through both our research design and sensitivity analysis but we cannot rule it out entirely. Furthermore, our results do not speak directly to how other civilian groups, such as those who have remained in Syria, or who have gone to places other than Turkey, might react to regime-inflicted indiscriminate violence. Related, while we have sought to examine the threats posed by selection into our sample, not all concerning possibilities can be entirely ruled out. Finally, our analysis was made simpler by the fact that the refugees we worked with faced violence almost entirely committed by or on behalf of the regime, and not the opposition. Cases where individuals face violence from multiple sides (as seen in other areas of the Syrian conflict), may generate a different type of response, and would be equally worth examining.

Nevertheless, our findings provide an additional building block in our knowledge of how civilians respond to violence. For refugees who have left the war-zone, civilian victimization may have a more ambiguous impact on the formation of loyalties than is found in much of the existing civil conflict literature, which focuses almost exclusively on civilians captive in the war zone. Among those who have fled, our results show that exposure to violence leads to a decreased propensity for side-taking and an increased preference for compromise, potentially driven by pessimism about the likelihood of an opposition victory. Particularly interesting avenues for future work could include other cases of conflict in which civilians were able to and did flee the conflict zone. Indeed, the enormous numbers of refugees around the world – many forced out of their homes by civil conflict and civilian victimization – demand that we begin to better understand the dynamics of 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.

References

Abadie, A. and Imbens, G. W. (2006). Large sample properties of matching estimators for average treatment effects. *econometrica*, 74(1):235–267.

AFAD (2013). Syrian refugees in turkey, 2013. Available online at `https://www.afad.gov.tr/upload/Node/2376/files/61-2013123015505-syrian-refugees-in-turkey-2013_print_12_11_2013_eng.pdf`.

AFAD (2017). Field survey on demographic view, living conditions, and future expectations of syrians in turkey. Available online at `https://www.afad.gov.tr/suriye-raporlari`.

Amnesty International (2015). Death everywhere: War crimes and human rights abuses in aleppo, syria. Available online at `http://www.amnestyusa.org/sites/default/files/embargoed_5_may_aleppo_report_death_everywhere.pdf`.

Amnesty International (2020). The circle of hell: Barrel bombs in aleppo, syria. [Online at `https://www.amnesty.org.uk/circle-hell-barrel-bombs-aleppo-syria`; accessed 21-March-2022].

Anderson, D. (2005). *Histories of the Hanged: The Dirty War in Kenya and the End of Empire*. W.W. Norton.

Arjona, A. (2016). *Rebelocracy: social order in the Colombian civil war*. Cambridge studies in comparative politics. Cambridge University Press, New York, NY.

Bakke, K. M., O'Loughlin, J., and Ward, M. D. (2009). Reconciliation in conflict-affected societies: Multilevel modeling of individual and contextual factors in the north caucasus of russia. *Annals of the Association of American Geographers*, 99(5):1012–1021.

Balcells, L. (2012a). The Consequences of Victimization on Political Identities: Evidence from Spain. *Politics & Society*, 40(3):311–347. Publisher: SAGE Publications Inc.

Balcells, L. (2012b). The consequences of victimization on political identities. evidence from spain. *Politics and Society*, 40(3):309–345.

Balcells, L. and Stanton, J. A. (2021). Violence Against Civilians During Armed Conflict: Moving Beyond the Macro- and Micro-Level Divide. *Annual Review of Political Science*, 24(1):45–69.

Bauer, M., Blattman, C., Chytilová, J., Henrich, J., Miguel, E., and Mitts, T. (2016). Can war foster cooperation? *The Journal of Economic Perspectives*, 30(3):249–274.

Benmelech, E., Berrebi, C., and Klor, E. F. (2015). Counter-Suicide-Terrorism: Evidence from House Demolitions. *The Journal of Politics*, 77(1):27–43.

CAERUS (2014). Mapping the conflict in aleppo, syria. Available online at `https://www.americansecurityproject.org/mapping-the-conflict-in-aleppo-syria/`.

Canetti, D., Elad-Strenger, J., Lavi, I., Guy, D., and Bar-Tal, D. (2017). Exposure to Violence, Ethos of Conflict, and Support for Compromise: Surveys in Israel, East Jerusalem, West Bank, and Gaza. *Journal of Conflict Resolution*, 61(1):84–113. Publisher: SAGE Publications Inc.

Canetti-Nisim, D., Halperin, E., Sharvit, K., and Hobfoll, S. E. (2009). A New Stress-Based Model of Political Extremism: Personal Exposure to Terrorism, Psychological Distress, and Exclusionist Political Attitudes. *Journal of Conflict Resolution*, 53(3):363–389.

Cederman, L.-E., Hug, S., Schubiger, L. I., and Villamil, F. (2020). Civilian Victimization and Ethnic Civil War. *Journal of Conflict Resolution*, 64(7-8):1199–1225.

Ceyhun, Huseyin Emre (2017). Refugees in jordan and lebanon: Life on the margins. Available online at `http://www.arabbarometer.org/content/arab-barometer-iv-syrian-refugees-lebanon-and-jordan`.

Christia, F. (2012). *Alliance formation in civil wars*. Cambridge University Press.

Cinelli, C. and Hazlett, C. (2020). Making sense of sensitivity: Extending omitted variable bias. *Journal of the Royal Statistical Society: Series B (Statistical Methodology)*.

Condra, L. N. and Shapiro, J. N. (2012). Who Takes the Blame? The Strategic Effects of Collateral Damage: WHO TAKES THE BLAME? *American Journal of Political Science*, 56(1):167–187.

Corstange, D. (2019). The syrian conflict and public opinion among syrians in lebanon. *British Journal of Middle Eastern Studies*, 46(1):178–200.

Corstange, D. (2020). National and subnational identification in the syrian civil war. *Journal of Politics*, 82(3):1176–1181.

Costalli, S. and Ruggeri, A. (2015). Forging political entrepreneurs: Civil war effects on post-conflict politics in Italy. *Political Geography*, 44:40–49.

Dell, M. and Querubin, P. (2017). Nation building through foreign intervention: Evidence from discontinuities in military strategies. *The Quarterly Journal of Economics*, 133(2):701–764.

Downes, A. A. (2007). Draining the sea by filling the graves: Investigating the effectiveness of indiscriminate violence as a counterinsurgency strategy. *Civil Wars*, 9(4):420–444.

Dyrstad, K. and Binningsbø, H. M. (2019). Between Punishment and Impunity: Public Support for Reactions against Perpetrators in Guatemala, Nepal and Northern Ireland. *International Journal of Transitional Justice*, 13(1):155–184.

Fair, C. C., Littman, R., Malhotra, N., and Shapiro, J. N. (2016). Relative poverty, perceived violence, and support for militant politics: Evidence from pakistan. *Political Science Research and Methods*, pages 1–25.

Fortna, V. P., Lotito, N. J., and Rubin, M. A. (2018). Don't Bite the Hand that Feeds: Rebel Funding Sources and the Use of Terrorism in Civil Wars. *International Studies Quarterly*, 62(4):782–794.

Gibbons-Neff, T. (2015). Dropping a barrel bomb? apparently there's an app for it. *Washington Post*.

Giebler, H. (2015). Listen to syrians. Available online at `https://diary.thesyriacampaign.org/what-refugees-think/`.

Goodwin, J. (2001). *No Other Way Out: States and Revolutionary Movements, 1945-1991*. Cambridge University Press.

Goodwin, J. (2006). A Theory of Categorical Terrorism. *Social Forces*, 84(4):2027–2046. Publisher: Oxford University Press.

Gutiérrez-Sanín, F. and Wood, E. J. (2017). What Should We Mean by "Pattern of Political Violence"? Repertoire, Targeting, Frequency, and Technique. *Perspectives on Politics*, 15(1):20–41.

Hadzic, D., Carlson, D., and Tavits, M. (2020). How Exposure to Violence Affects Ethnic Voting. *British Journal of Political Science*, 50(1):345–362.

Hall, J., Kovras, I., Stefanovic, D., and Loizides, N. (2018). Exposure to Violence and Attitudes Towards Transitional Justice. *Political Psychology*, 39(2):345–363. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/pops.12412.

Hashim, A. (2006). *Insurgency and Counterinsurgency in Iraq*. Cornell University Press.

Hazlett, C. (2016). Angry or weary? the effect of physical violence on attitudes towards peace in darfur. *Working Paper*.

Hirsch-Hoefler, S., Canetti, D., Rapaport, C., and Hobfoll, S. E. (2016). Conflict will Harden your Heart: Exposure to Violence, Psychological Distress, and Peace Barriers in Israel and Palestine. *British Journal of Political Science*, 46(4):845–859. Publisher: Cambridge University Press.

Hockenos, P. (2003). *Homeland Calling: Exile Patriotism and the Balkan Wars*. Cornell University Press.

Hoddie, M. and Hartzel, C. A., editors (2010). *Strengthening peace in post civil war states: transforming spoilers into stakeholders*. University of Chicago Press.

Hokayem, E. (2013). *Syria's Uprising and The Fracturing of the Levant*. Routledge.

Huddy, L., Feldman, S., Capelos, T., and Provost, C. (2002). The Consequences of Terrorism: Disentangling the Effects of Personal and National Threat. *Political Psychology*, 23(3):485–509. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/0162-895X.00295.

Kalyvas, S. N. (2006). *The logic of violence in civil war*. Cambridge Univ Press.

Kao, K. and Revkin, M. R. (2021). Retribution or Reconciliation? Post-Conflict Attitudes toward Enemy Collaborators. *American Journal of Political Science*, n/a(n/a).

Kocher, M. A., Pepinsky, T. B., and Kalyvas, S. N. (2011). Aerial bombing and counterinsurgency in the vietnam war. *American Journal of Political Science*, 55(2):201–218.

Kondra, L. and Shapiro, J. (2012). Who takes the blame? the strategic effects of collateral damage. *American Journal of Political Science*, 56(1):167–187.

Levy, J. S. and Morgan, T. C. (1986). The war-weariness hypothesis: An empirical test. *American Journal of Political Science*, pages 26–49.

Liendo, N. and Braithwaite, J. M. (2018). Determinants of Colombian attitudes toward the peace process. *Conflict Management and Peace Science*, 35:622–636.

Lindley, A., editor (2010). *The early morning phone call: Somali refugees' remittances*. Berghahn Books.

Lyall, J. (2009). Does indiscriminate violence incite insurgent attacks? evidence from chechnya. *Journal of Conflict Resolution*.

Lyall, J., Blair, G., and Imai, K. (2013). Explaining support for combatants during wartime: A survey experiment in afghanistan. *American Political Science Review*, 107(4):679–705.

McHugo, J. (2014). *Syria: From the Great War to Civil War*. Saqi Books.

Mironova, V., Mrie, L., and Whitt, S. (2019a). Commitment to rebellion: Evidence from syria. *Journal of Conflict Resolution*. Available at https://journals.sagepub.com/doi/full/10.1177/0022002719867472.

Mironova, V., Mrie, L., and Whitt, S. (2019b). Risk tolerance during conflict: Evidence from aleppo, syria. *Journal of Conflict Resolution*. Available at https://journals.sagepub.com/doi/full/10.1177/0022343318824632.

Nussio, E., Rettberg, A., and Ugarriza, J. E. (2015). Victims, Nonvictims and Their Opinions on Transitional Justice: Findings from the Colombian Case. *International Journal of Transitional Justice*, 9(2):336–354.

Pearlman, W. (2016). Narratives of fear in syria. *Perspectives on Politics*, 14(1):21–37.

Pechenkina, A. O., Bausch, A. W., and Skinner, K. K. (2019). How do civilians attribute blame for state indiscriminate violence? *Journal of Peace Research*, 56(4):545–558.

Petersen, R. D. (2001). *Resistance and rebellion: lessons from Eastern Europe*. Cambridge University Press, Cambridge ; New York.

Pham, P., Vinck, P., and Stover, E. (2009). Returning home: forced conscription, reintegration, and mental health status of former abductees of the lord's resistance army in northern uganda. *BMC psychiatry*, 9(1):23.

REACH (2015). Migration trends & patterns of syrian asylum seekers travelling to the european union. Available online at https://reliefweb.int/report/world/migration-trends-patterns-syrian-asylum-seekers-travelling-european-union-assessment?gclid=Cj0KCQiA4aacBhCUARIsAI55maEdwsmG4UcBZu1DlQZQSaeaXPMHQlNHHab7bIwCBLg9MFH6QqK8trMaAsJgEwcB.

Schon, J. (2019). Motivation and opportunity for conflict-induced migration: An analysis of syrian migration timing. *Journal of Peace Research*, 56(1):12–27.

Schubiger, L. I. (2021). State Violence and Wartime Civilian Agency: Evidence from Peru. *The Journal of Politics*, 83(4):1383–1398.

Schutte, S. (2017). Violence and civilian loyalties: Evidence from afghanistan. *Journal of Conflict Resolution*, 61(8):1595–1625.

Sekhon, J. S. (2011). Multivariate and propensity score matching software with automated balance optimization: the matching package for r. *Journal of Statistical Software*, 42(7):1–52.

SNHR (2016). Multiple evidences indicating that russian and syrian forces deliberately targeted the u.n. aid convoy in aleppo. Available online at http://sn4hr.org/wp-content/pdf/english/Russian_and_Syrian_forces_bombed_a_convoy_of_UN_in_Aleppo_en.pdf.

SNHR (2017). The syrian regime has dropped nearly 70,000 barrel bombs on syria: Ruthless bombing. Available online at http://sn4hr.org/wp-content/pdf/english/The_Syrian_Regime_Has_Dropped_Nearly_70,000_Barrel_Bombs_en.pdf.

SOHR (2020). On international human rights day: Millions of syrians robbed of "rights" and 593 thousans killed in a decade. [Online at https://www.syriahr.com/en/195385/; accessed 21-March-2022].

Souleimanov, E. A. and Siroky, D. (2016). Random or retributive?: Indiscriminate violence in the chechen wars. *World Politics*, 68(4):677–712.

Steele, A. (2017). *Democracy and displacement in Colombia's civil war*. Cornell University Press, Ithaca.

Straus, S. (2015). *Making and unmaking nations: The origins and dynamics of genocide in contemporary Africa*. Cornell University Press.

Tellez, J. F. (2018). Worlds apart: Conflict exposure and preferences for peace. *Journal of Conflict Resolution*, pages 1–24.

Tyner, E. (2016). Do territorial control and the loss of territory determine the use of indiscriminate violence by incumbent actors? an examination of the syrian civil war in aleppo over 45 weeks. *Journal of Terrorism Research*, 7(1):52–66.

UNHCR (2022a). Syria emergency. [Online at https://www.unhcr.org/uk/syria-emergency.html; accessed 21-March-2022].

UNHCR (2022b). Syria regional refugee response. [Online at https://data2.unhcr.org/en/situations/syria; accessed 21-March-2022].

UNHCR (2022c). Update on unhcr's operations in the middle east and north africa. [Online at https://www.unhcr.org/6228a5bb4.pdf; accessed 21-March-2022].

UNSC (2014). Resolution 2139. Available online at https://www.securitycouncilreport.org/atf/cf/%7B65BFCF9B-6D27-4E9C-8CD3-CF6E4FF96FF9%7D/s_res_2139.pdf.

Villamil, F. (2021). Mobilizing memories: The social conditions of the long-term impact of victimization. *Journal of Peace Research*, 58(3):399–416. Publisher: SAGE Publications Ltd.

Vinck, P., Pham, P., Stover, E., and Weinstein, H. (2007). Exposure to war crimes and implications for peace building in northern uganda. *JAMA*, 298(5):543–554.

Voors, M., Nillesen, E., Verwimp, P., Bulte, E., Lensink, R., and van Soest, D. (2010). Does conflict affect preferences: Results from field experiments in burundi. *MICROCON Working Paper*, 21.

Wood, E. J. (2003). *Insurgent collective action and civil war in El Salvador*. Cambridge University Press.

Wood, E. J. (2008). The Social Processes of Civil War: The Wartime Transformation of Social Networks. *Annual Review of Political Science*, 11(1):539–561. _eprint: https://doi.org/10.1146/annurev.polisci.8.082103.104832.

Threat Perceptions, Loyalties and Attitudes Towards Peace: The Effects of Civilian Victimization among Syrian Refugees in Turkey

Appendix

Contents

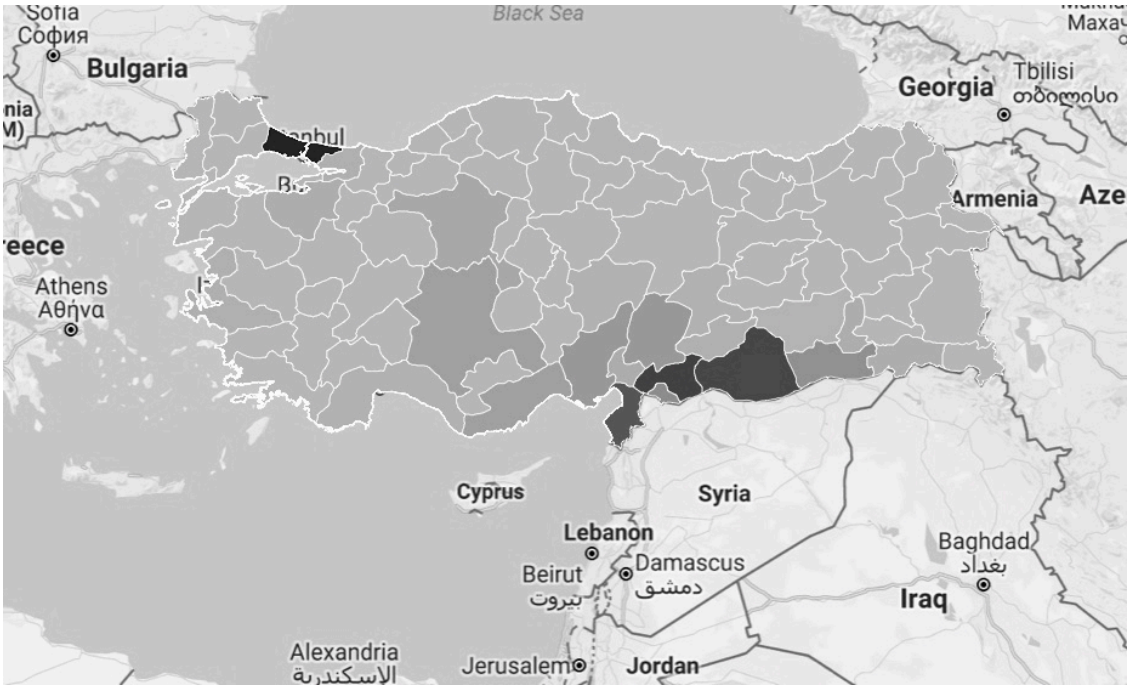
List of Figures

List of Tables

A Survey Sampling and Research Ethics

We conducted our survey in the summer and fall of 2016. The aim of our sampling strategy was to sample out-of-camp Syrian refugees living in the four Turkish locations where we worked. This involved three stages: First, we selected Turkish provinces with the highest number of Syrians present: İstanbul, Hatay, Şanlıurfa and Gaziantep.[1] Syrians in these provinces comprise 50 percent of all out-of-camp Syrians living in Turkey. Figure A.1 shows all provinces of Turkey, with the number of Syrian refugees in each indicated by color. The four darkest areas are those selected. In earlier fieldwork in these provinces, we learned that the vast majority of refugees from Syria are Sunni Muslims. The Syrians residing in Gaziantep are mostly Sunni Arabs, while many Syrian Kurds live in Şanlıurfa, and the smaller population of Alawite Syrians are more likely to reside in Hatay or Istanbul. Given the political complexities and sensitivities around religious identity in Turkey, we were unable to ask our respondents directly about sect. We were, however, able to ask respondents about the languages they spoke, namely whether or not they spoke Kurdish.

Figure A.1: Distribution of Syrian Refugees in Turkey



Next, we chose 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. Finally, 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

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

survey if he was at home.[2] If the male head of the household was not at home or (in rare 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.

Research with refugee populations in areas proximate to conflict zones pose unique risks and challenges (Cronin-Furman and Lake, 2018; Jacobsen and Landau, 2002) that we considered seriously when designing our work. We completed IRB review for our research at both US and Turkish institutions. We conducted several rounds of qualitative work prior to our survey to assess Syrians' willingness to discuss their attitudes to the conflict and experiences of violence without inflicting duress. Our survey itself was implemented with the aid of a research firm, Infakto, which has extensive experience surveying Syrians in the areas where we worked. Together with their staff and our translators, we ensured the cultural appropriateness, political acceptability, and personal sensitivity of the questions asked. We also conducted thorough in-person training with the enumerators to ensure their compliance with best practices, particularly informed consent, detecting signals of discomfort, and the ability of participants to drop out of the survey at any time. Questions regarding personal, family, or community experiences with violence were kept as minimal, brief, and as non-specific as possible. In addition, respondents were offered a card with the details of an organization that offers free psycho-social support services for refugees and their families across Turkey.

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. Istanbul still had a considerable number of unregistered Syrians at the time of our survey. Hatay has its own local Alawite population and therefore in general had more tense relations with the Arab Sunni refugees. These two factors may be the reasons for the lower response rates in Istanbul and Hatay, respectively.

[2]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 norms.

B Survey Questions and Variables

Here we describe the questions for our outcomes of interest and mechanisms together with the names of the variables (italicized) that we construct using the answers to those questions. For questions with categorical outcomes, binary responses were constructed, and we used the resulting variables when they were relevant to our research question and contained at least 5% of the responses.

Primary outcomes

Our first two primary outcomes are designed to measure threat perception, with the expectation that individuals who lost homes to barrel bombing will find the Assad regime to be a greater threat to themselves and to the country,

- In your opinion, which of the following groups I will read is the biggest security threat to the country of Syria?: (i) ISIS, (ii) Assad regime, (iii) Opposition groups, (iv) Other armed groups, (v) International powers. [Variable name: *Top threat to Syria: Assad*]
- In your opinion, which of these do you consider the biggest security threat to you personally in a future Syria?: (i) ISIS; (ii) Assad regime; (iii) Opposition groups; (iv) Other armed groups; (v) International powers. [Variable name: *Top threat to you: Assad*]

The next and arguably most important outcome measure assesses loyalties, looking at the degree of support for the warring parties using the following open-ended question,

- Which party to the conflict do you think most closely represents your interests? (Open-ended). [Variable names: *Support opposition*; *Support no party*]

  With this question, respondents were free to name any party to the conflict they wished.

Our other primary outcomes assess attitudes towards compromise, neutrality and peace.

- 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?: (i) Accept a peace settlement that ends the fighting, regardless of who maintains control; (ii) 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; (iii) Accept a peace settlement that ends the fighting, but only if the regime is removed entirely from Syria; (iv) No political settlement is acceptable, fighting should continue until an outright military victory. We construct two variables from this question. The first, which groups responses (i) and (ii), measures willingness to compromise in the name of peace. The second, which equals one if the respondent answered (iv), measures a desire to continue fighting. [Variable names: *Compromise for peace; Fight until victory*]
- 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). [Variable name: *Neutrality acceptable*]

- If a family member of yours spoke out publicly calling for an end to fighting, to what extent would you agree?: (i) strongly agree; (ii) agree; (iii) indifferent; (iv) disagree; (v) strongly disagree. [Variable name: *Support peace if family does*, equalling one when response was "agree" or "strongly agree"]

We structured the last question above, *Support peace if family does*, in this way because we were concerned there may be a social prohibition on calling for peace, which might be disarmed somewhat by asking whether the respondent would agree with a family member who called for it.

Mechanism measures

Further analyses considers precisely why and how losing a home to barrel bombing might matter for threat perceptions, loyalties and attitudes towards peace.

We consider evidence for two possible mechanisms behind our results, one focused on respondents' overall level of engagement, including intentions to return, and another focused on pragmatic views about the likelihood of an opposition victory.

First, a reason victimized individuals may report being less supportive of the opposition or continued fighting might be simply that they are less socially engaged and active overall, or have little intention of ever returning to Syria, and therefore are indifferent about the actors in the conflict. To consider this general issue of engagement, we asked:

- Do you think it will ever be possible for you to return to the area in Syria where your home is/was? (Certainly, very likely, somewhat likely, not very likely, not at all likely)[Variable name: *Will likely return*, equalling one when response was "certainly" or "very likely" or "somewhat likely"]
- How closely do you follow the news from Syria? (Very closely, somewhat closely, not too closely, not at all closely) [Variable name: *Follow Syria news*, equalling one when response was "very closely" or "somewhat closely"]
- Do you do any volunteer work for services that help Syrian refugees? (Yes or No). [Variable name: *Volunteer for refugees*]

Second, we examine whether or not respondents' assessment of the opposition's ability to achieve victory might matter, as this could potentially alter considerations about the benefits and drawbacks of supporting the opposition and favoring peace.

- How likely do you think it is that the opposition will achieve victory over the regime and take control of the country away from the Assad regime? (Very unlikely, somewhat unlikely, even chances, somewhat likely, very likely) [Variable name: *Opposition could win*, equalling one when response was "even chances", "somewhat likely" or "very likely"]

C Main Results Table

The table below presents the numerical results of the matching estimates reported in the main text.

Table C.1: Numerical results from matching estimates

	Estimate	*CI*95(*low*)	*CI*95(*high*)	*mean*	*n*(*treated*)	*n*(*control*)
Top threat to Syria: Assad	0.20	0.12	0.28	0.37	200	434
Top threat to you: Assad	0.15	0.08	0.23	0.50	194	427
Support opposition	-0.26	-0.33	-0.19	0.34	227	488
Support no party	0.26	0.20	0.33	0.66	227	488
Neutrality acceptable	0.12	0.07	0.17	0.61	204	432
Fight until victory	-0.07	-0.10	-0.04	0.04	132	351
Compromise for peace	0.28	0.19	0.36	0.54	132	351
Support peace if family does	0.13	0.08	0.19	0.81	222	453

Note: Numerical results corresponding to coefficient plots in Figure 5. *Estimate* is the estimated average treatment effect on the treated after matching (exact matching on neighborhood and gender). The lower and upper limits of the 95% CI given by *CI95(low)* and *CI95(high)* respectively employ the Abadie-Imbens standard error. The overall mean of the outcome variable is given by *mean*, and the unique number of treated and control units that enter into the matched comparison are given by *n(treated)* and *n(control)*.

D Barrel Bombing Harms other than House Destroyed

In addition to whether or not a respondent's home was destroyed, we asked the following questions on barrel bomb-related harms and created variables using respondents' answers to these questions. Unless otherwise noted, the response options for the questions are *yes, no, don't know/refuse/no response*. Summary statistics for these variables are provided in Table D.2.

- 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 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*, equalling 1 for respondents who listed at least one asset.

- 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)*

As noted in the main text, none of these harms would be suitable for analysis (as other exposures/treatments) using the identification opportunity we employ, which attempts to eliminate confounding based on the inaccuracy of barrel bombs. We now explain why each of these outcomes would not be suitable for this strategy.

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.

Second, using *Business destroyed due to barrel bomb* poses several problems. You can only lose a business if you have one. Even if the destruction of a given business happened at random, this variable would still be confounded with our outcomes because to have a "1" on it means you had a business to lose.

Third, *Barrel bombed neighborhood* cannot be used as a "treatment" in our main analysis, as we must make comparisons only within each neighborhood. 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 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 post-treatment, since one could have lost their home to barrel bombing (the treatment) while away, in which case one would not move back home to be present during further barrel bombing. In addition to not conditioning on *Present during barrel bomb*, we also cannot use *Present during barrel bomb* as our main "treatment" 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 assets can have more destroyed, creating a confound similar to that described for *business destroyed* above. 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.

Relying upon *House destroyed due to barrel bomb* sidesteps all of these problems, because a person's house stays whether the person leaves or not. Within neighborhoods that are attacked, we have argued that 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). As noted in the text, this does assume that 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. 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.

Table D.2: Other Bombing Harms

Types of Harm	Mean	Std.Dev.	N
Barrel bombed neighborhood	0.37	0.48	1343
Present during barrel bomb	0.21	0.41	1384
Injured due to barrel bomb	0.02	0.12	1384
Family members injured due to barrel bomb	0.03	0.18	1384
Family members killed due to barrel bomb	0.03	0.16	1384
Business destroyed due to barrel bomb	0.08	0.26	1384
Assets destroyed by barrel bomb	0.23	0.42	1384
Family injured (indiscriminate violence)	0.11	0.31	1055
Family killed (indiscriminate violence)	0.09	0.28	1052

Note: Descriptive statistics on other harms

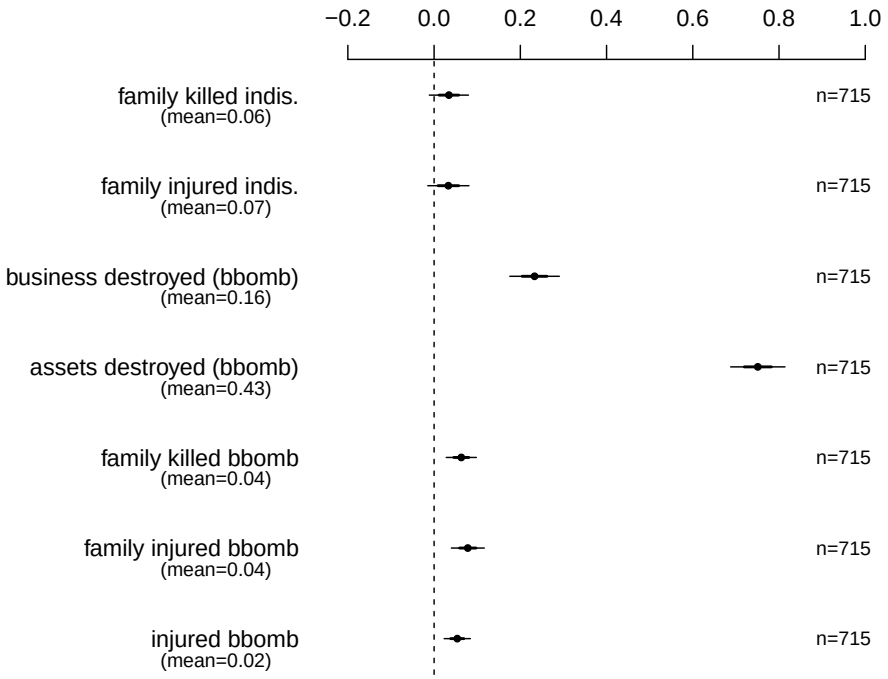
D.1. Other harms as outcomes

As noted in the text, losing one's home to barrel bombs may have an influence on the main outcomes of our study not only directly but through increasing the risk of other harms. We can examine the effect of losing one's home on these other harms using the same strategy used for the main

outcomes—comparing respondents of the same gender and neighborhood who did and did not lose their homes to barrel bombs. This comparison limits us to types of harms that are measured below the neighborhood level, and which show sufficient variation. For simplicity, we use binary forms of each variable, indicating whether they were zero or non-zero, as nearly all variation is found here.

That said, we also note that with the exception of losing one assets to barrel bombs, these other harms are quite rare, with some experienced by just a few percent of individuals. The increased probability of experiencing these harms is also fairly small with point estimates below 10 percentage points. Accordingly, they cannot explain much variance in the main outcomes we study in the paper. Losing assets to barrel bombs, however, is very highly correlated with losing one's home, as might be expected. It is thus reasonable to think of most individuals who lost their homes to barrel bombs as also being far more likely to lose other assets as well.

Figure D.2: Effect of losing one's home to barrel bombing on other harms

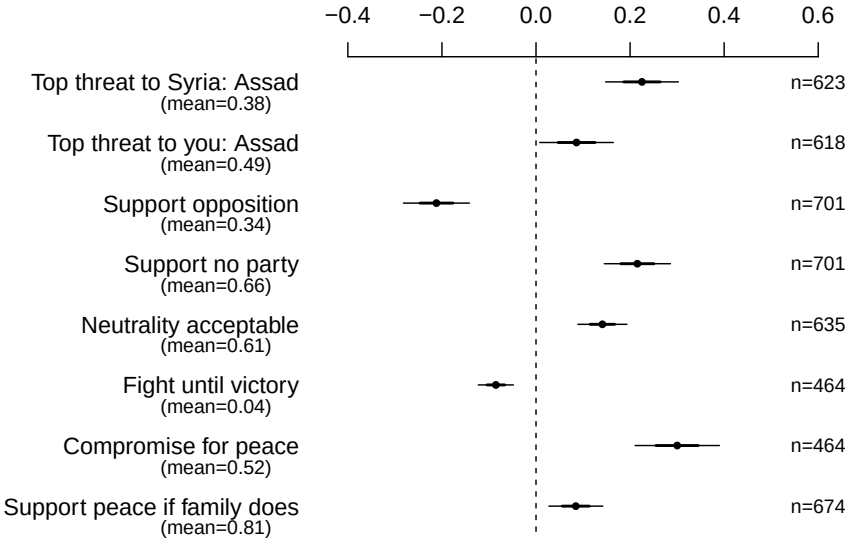


Note: Effect of losing one's home to barrel bombing on other harms, estimated by using matching to compare those who did and did not lose their homes to barrel bombs, within neighborhood and gender.

Finally, we show results applying our estimation strategy to two alternative "treatment" variables: *present during barrel bomb* and *assets destroyed due to barrel bomb*. As noted above, there are good reasons to worry about confounding bias in these estimates. While barrel bomb destruction may be effectively random within small enough geographic areas, whether on is present or not when one's home is destroyed (if it is) is a consequence of other characteristics of that person that might themselves be related to the outcomes. And the assets one can lose to barrel bombing depend in part on the assets they have, introducing "wealth" as a potential confounder. Nevertheless, the results for *present during bbomb* are similar to those of losing one's home to barrel bombs, with an

identical pattern of sign and significance across the eight outcomes.

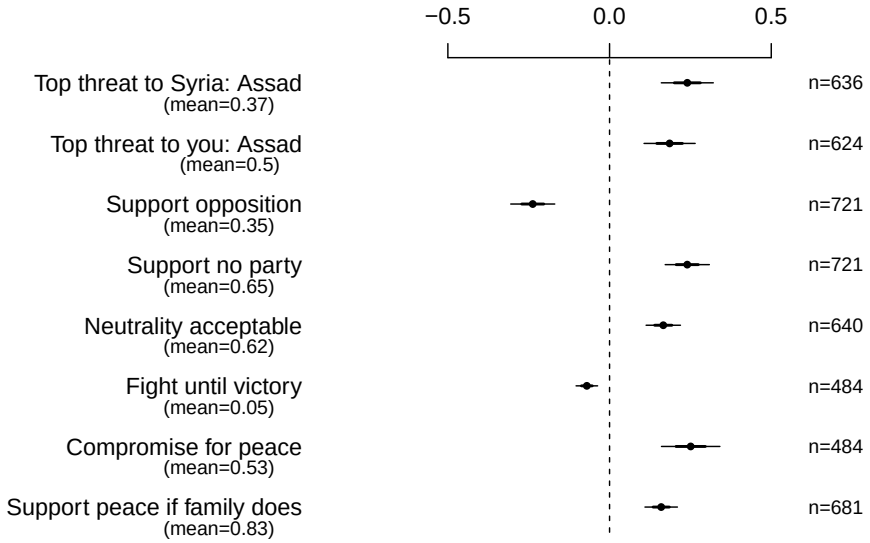
Figure D.3: Estimates for being present during barrel bomb attack on the main outcomes



Note: Relationship between being present during barrel bomb attack and the eight main outcomes, estimated by using matching to compare those who were and were not present, within neighborhood and gender.

Regarding losing one's assets to barrel bombs, as noted above, this is very closely associated with losing one's home, with a correlation of 0.78. It is thus unsurprising that taking it as treatment produces essentially identical results.

Figure D.4: Estimates for losing one's assets to barrel bombing on the main outcomes



Note: Relationship between losing assets (binary indicator) to barrel bomb destruction and the eight main outcomes, estimated by using matching to compare those who did and did not lose assets, within neighborhood and gender.

E Discussion of Threats to Validity

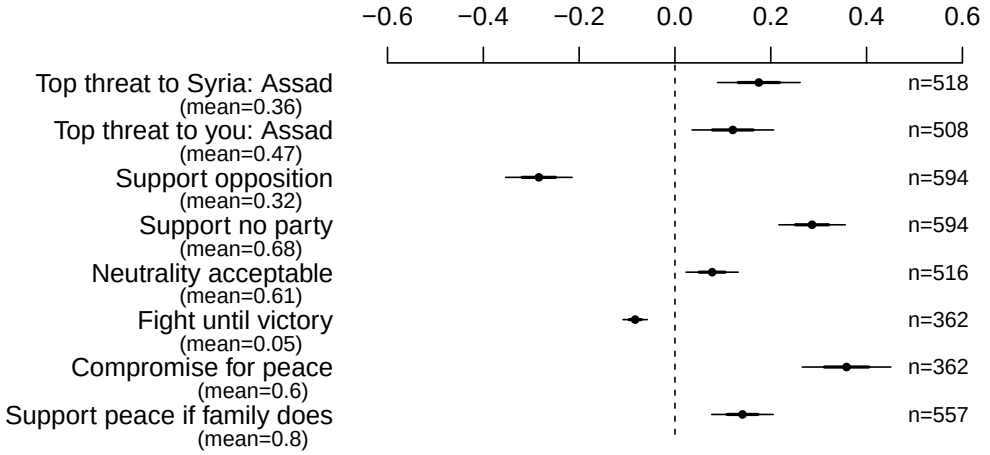
We consider here two main threats to validity: selection into "treatment" issue (i.e. confounding concerns, and sample selection issues ("selection into the sample") of the sort that could cause bias.

E.1. Selection into treatment

As discussed at length in the main text, we endeavor to limit the scope for confounding through our within-neighborhood comparison and our understanding that barrel bombs cannot be well targeted within neighborhoods of these sizes. We have also discussed the expected direction of bias due to unobserved confounding and present the sensitivity analysis of our results to potential unobserved confounding below in Section F. Here we continue this discussion.

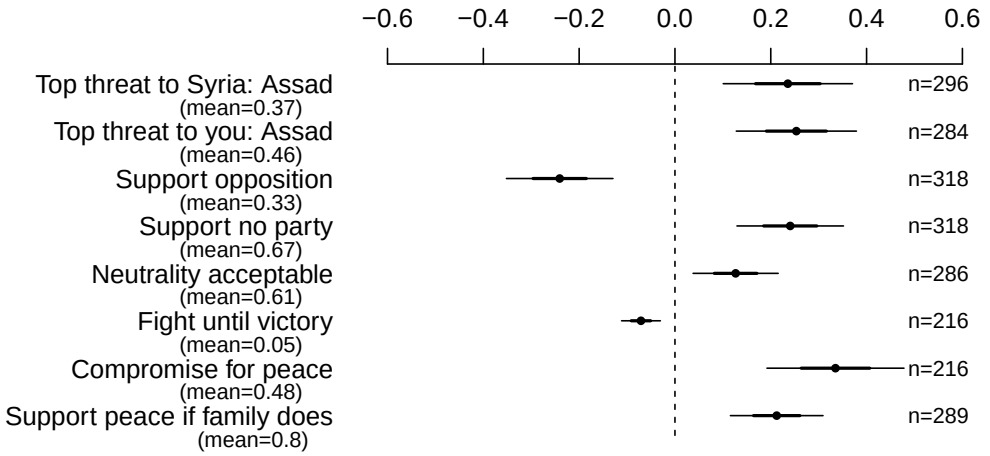
Results for urban and smaller neighborhoods. As noted in the text, our identification strategy is most credible in smaller neighborhoods, as conditioning on the smaller areas makes the indiscriminacy of barrel bombing damage within that area more certain. Because some of the neighborhoods in our sample 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. Figure E.5 shows the resulting estimates, which are substantively identical to those in the full analysis in the main text. Similarly, we show results when restricting analysis to the neighborhoods at or below median size in Figure E.6. In both cases, we see that the effects remain similar in magnitude and significance as in the full sample.

Figure E.5: Estimated effect of house destruction: Urban neighborhoods 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. Whiskers show the 90% (thick) and 95% (thin) confidence intervals.

Figure E.6: Estimated effects of house destruction: Smaller-than-median neighborhoods



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. Whiskers show 90% (thick) and 95% (thin) confidence intervals.

E.2. Selection into the sample concerns

The ideal sampling strategy for our study would be to randomly sample households that had lived in each neighborhood where bombing occurred. Unfortunately, this was not feasible, and we must instead work with samples from the refugee population in Turkey.[3] This introduces additional concerns, as individuals select into the populations from which we can sample them. Such a situation is common in research on refugees or other highly selected populations. In some circumstances this only "changes the population about which we make an inference". For example, suppose 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. Such challenges to external validity or generalization are acceptable for many research purposes, including ours, which are concerned only with the internal validity of the estimate within the group studied.

In more pernicious cases however, the selection process can also bias estimates of effects even for the units in our population. Such problematic cases can arise due to "differential selection", cases where the selection process looks different for those who lost their homes to barrel bombing than it does for those who did not. This is problematic because it destroys the comparability we require between the treated and control groups within neighborhood.

We consider two main differential selection scenarios that we judged to be plausible given qualitative information about our setting. First, suppose that typically, hardcore opposition supporters are most reluctant to leave Syria and/or if they do leave Syria for Turkey they are less likely to

[3]We tested a respondent-driven sampling approach intended to sample from neighborhoods of origin in this way, but it proved infeasible.

remain there. However, if their home is destroyed, it may push them to leave, with some chance of coming to Turkey, and/or it encourages them to stay in Turkey longer. If this occurred, the respondents who lost their homes (in a given neighborhood) would contain a larger fraction of the "hardcore opposition supporters" than the group without their homes destroyed. However, this would lead home destruction to be associated positively with opposition support, whereas our finding is the opposite. This particular case would therefore not pose a challenge to our research conclusion, only suggest that the actual effect of home destruction on withdrawing support is perhaps even larger than we claim.

A second scenario we consider is created when (i) losing one's home to barrel bombing makes people more pro-opposition on average regardless of their initial attitudes, while (ii) those who are most highly pro-opposition (potentially after losing their home) are less likely to appear in our sample, e.g. because they are less likely to leave Syria and/or are more likely to return quickly to Syria. This would imply that in our sample we are missing a group of highly pro-opposition individuals who also lost their homes, leading to a bias in the direction of our research conclusion. We cannot entirely rule out such a concern, though two observations help to suggest it may have limited impact. First, this bias may not be as large as we might fear because (i) it is generated only by individuals for whom losing their homes to barrel bombs was a *necessary and sufficient* condition for them to come to (and stay in) Turkey, and (ii) even those individuals contribute to bias in the estimate only to the degree that losing their homes to barrel bombs increased their support for the opposition. Those individuals who already do not plan to come to/ stay in Turkey, or who would decide not to come to Turkey even after the added offense of losing their homes, will not contribute to the bias. Since this selection problem operates only through such a sub-population, and only insofar as their support for the opposition is increased, its biasing effect on the overall estimate would be attenuated. Second, as described in the paper, we see extremely similar estimated effects of losing one's home for women as for men, on every outcome (Figure 6). This is difficult to explain if selection pressures are substantially biasing our results because the nature of the decision to come to Turkey is thought to vary so greatly by gender, with women more often expected to bring their family to safety in neighboring countries and men more often expected to join the fight or to leave Syria for Europe in order to start the asylum process with the goal of their family members joining them eventually (REACH, 2015).

F Confounding and Sensitivity Analysis

While we have sought to reduce the threat of confounding by conditioning on geographic areas small enough that barrel bombs cannot effectively be targeted within them, we do not claim to have confidently ruled it out, and provide here results that show how estimates would look under varying degrees of confounding. Suppose that (even restricting to the urban or smaller-than-median neighborhoods) at least a weak, probabilistic degree of targeting is possible within these areas. If so, it is easy to think of potential 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 in our results. It is more difficult, however, to think of confounders that would make those who are more likely to lose their

homes also be *less* supportive of the opposition, as needed to "explain away" the observed result. 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".

Formal Sensitivity Analysis

To formally examine how strong confounding would have to be to account for our estimates, we employ the method of Cinelli and Hazlett (2020), which provides sensitivity statistics for linear regression models.[4] Table F.3 shows estimates for a simple linear model (OLS) that mimics the matching estimator used above by including location and gender fixed effects to realize the within neighborhood and gender comparison, but that also includes the set of pre-treatment covariates used above for balance testing. The addition of these covariates does not change any estimate by more than a half a percentage point. For each outcome, the OLS-based effect estimate is similar to those shown in Figure 5 from matching in main text in terms of magnitude, sign, and statistical significance. In addition to the basic OLS estimates, Table F.3 is augmented by several quantities that describe the sensitivity of the result to unobserved confounding.

Table F.3: Regression estimates for all main outcomes with sensitivity statistics

	Estimate	SE	t-value	RV(%)	$RV_{\alpha=0.05}$	dof
Top threat to Syria: Assad	0.20	0.05	4.36	16.07	9.18	619
Top threat to you: Assad	0.15	0.05	2.75	10.55	3.14	610
Support opposition	-0.16	0.05	-3.34	12.34	5.29	643
Support no party	0.17	0.05	3.40	12.54	5.50	643
Neutrality acceptable	0.23	0.03	8.04	27.36	21.49	627
Fight until victory	-0.04	0.03	-1.52	5.89	0.00	623
Compromise for peace	0.31	0.05	6.58	23.12	16.86	623
Support peace if family does	0.09	0.04	2.04	7.86	0.28	619

Note: OLS regression results with sensitivity statistics. Pre-treatment covariates previously used for balance checking are included in the regression, together with the gender and neighborhood ID fixed effects required for identification. The treatment variable is always the indicator for having one's home destroyed by barrel bombs. Estimates are similar to the matching results shown in Figure 5 in the main text. The RV and RV_α describe the degree of confounding that would be required to overturn the result, described in the text. The dof is the model residual degrees of freedom. The sample used begins with the set of 1096 observations meeting the same criteria required prior to matching, namely that the neighborhood and gender data are non-missing. The further reductions in sample size are due to missingness.

To explain the sensitivity results, we focus first on the effect of having one's home destroyed by barrel bombs on support for the opposition, since this is central to our theoretical question and results.[5] Table F.3 reports that the model estimates 16 percentage point lower support for the opposition (95% CI of [-.26, -.06], $t = -3.23$, $p = 0.001$) among those whose homes were destroyed

[4]The approach has similarities to sensitivity analyses used in prior quasi-experimental work on violence—such as Blattman (2009), which employs the method of Imbens (2003)—but has a number of advantages. First, this approach does not require functional form assumptions on the treatment model, nor distributional assumptions on confounding variables. Second, for reasons described in Cinelli and Hazlett (2020), procedures used for "benchmarking" with observed covariates in numerous existing approaches can be misleading.

[5]The results are nearly identical (with the opposite sign) for *support no party*, since these two are effectively counterparts.

by barrel bombs. The "robustness value" (RV) of 12% characterizes how this effect will change under confounding. It has the interpretation that confounders explaining any less than 12% of the residual variance of both *Home destroyed due to barrel bomb* (the exposure) and of *Support opposition* (the outcome) would not be sufficient to explain away all of the observed relationship. Similarly, if we wish to know how much confounding it would take to no longer find a statistically significant result, the $RV_{\alpha=0.05}$ value of 5.29% summarizes the answer: Confounding would have to explain at least 5.29% of the residual variance of both the treatment and outcome in order to reduce the estimated effect to the boundary of insignificance at $\alpha = 0.05$. Table F.3 presents these estimates and summary sensitivity statistics for all eight of our outcome variables. As summarized by the RV, results for other outcomes are largely similar to those for *Support opposition* or *Support no party* in that confounding of a similar strength would be required to overturn the result.

There is no particular RV or other diagnostic that certifies a result as "robust", since they only describe the confounding required to change the result and can not say anything about the confounding present. Here, it is difficult to imagine what unobservables could explain 12% of the residual variation in the indicator for having one's home destroyed, given that there is so little ability to target home destruction conditionally on neighborhood. Note that a confounder that explains 12% of residual variance has a correlation with home loss (and the outcome) of $\sqrt{12\%} \approx 35\%$ after accounting for covariates—a substantially strong relationship. Such exercises do not rule out the possibility of confounding, but they do refine critiques as to what would be needed to alter our conclusions.[6]

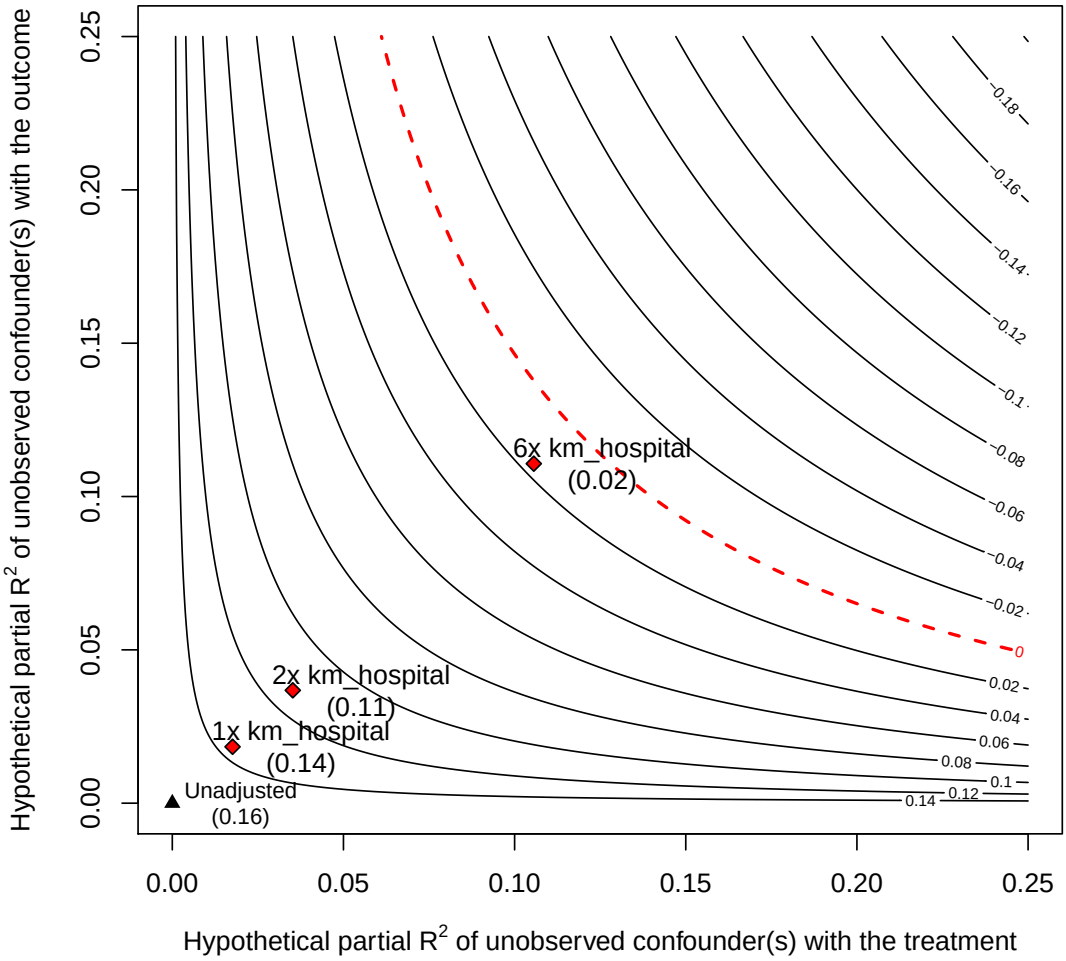
Finally, for confounding to change our conclusions, it would also have to explain far more variation in exposure and outcome than even the most predictive observed covariates. The variable *km to hospital* has the most worrying apparent imbalance (and thus strength of relationship to the exposure), and it explains a larger share of residual variation in the outcome (*Support opposition*) than any other covariate. Suppose for a moment that confounding is "as strong as" *km to hospital*, meaning it explains as much residual variation in exposure and the outcome. This would nevertheless only shrink the implied estimate on *Support opposition* from -0.16 down to -0.14 percentage points, with an adjusted t-statistic of -2.77. In fact, to completely eliminate the estimate, confounding would have to explain almost seven times as much of the residual variance in exposure and the outcome as *km to hospital*. Similar analyses can be repeated with other variables, but *km to hospital* proves to be the hardest such test.

F.1. Visualization and Further Explanation of Sensitivity Analysis

To better visualize and explain the results of our sensitivity analysis, the contour plot in Figure F.7 shows the adjusted estimate for confounding that explains any combination of the treatment (*House destroyed due to barrel bomb*, horizontal axis) and the outcome (*Support opposition*, vertical axis). The triangular point labeled *Unadjusted* shows the estimate one would typically report, as though unobserved confounding is exactly zero, in which case we estimate an effect of 16 percentage points. Confounding that explains as much of the treatment as shown on the horizontal axis, and as much of the outcome as shown on the vertical axis, would produce the adjusted estimate shown by

[6] We expand on this analysis in Appendix F.1 with a contour plot (Figure F.7) showing the adjusted estimate for unobserved confounding that explains any combination of the treatment and outcome.

Figure F.7: Sensitivity Analysis for Effect of Harm on Support for Opposition



Note: Contour plot showing sensitivity to unobserved confounding, controlling for all variables used in balance tests. The triangular point marks the "usual" estimate, assuming zero unobserved confounding. Presuming that confounding is "no worse than" *km hospital*, the point "1x km_hospital" shows that the implied effect estimate would remain nearly unchanged at 14 percentage points. If confounding is allowed to be "twice as bad" as *km hospital*, an estimate of 11 percentage points would remain, and it would be only just statistically significant, with $t = 2.11$. The effect would be non-significant but remain above zero until confounding exceeds six times the strength of *km_hospital*.

the corresponding "height" of the contour line at those coordinates. For example, the red dashed contour plot of zero effect runs very near the intersection of 0.10 on the horizontal axis and 0.20 on the vertical axis. This means that a confounder that explains approximately 10% of the residual variance in barrel bombing and approximately 20% of the residual variation in the outcome would imply an effect near zero.

To aid in interpreting these results, we can determine how strong confounding would need to be to alter our results, by comparison to the observed covariates. Specifically, we can pose an assumption regarding the relative strength of unobserved confounding compared to an observed covariate, in explaining the treatment and outcome. We can then derive a corresponding bound on the maximum amount of confounding that can exist, and see how this alters our result. This is similar in spirit to exercises of leaving covariates out or showing "benchmarks" using observed covariates, but corrects for problems with those procedures. We refer readers to Cinelli and Hazlett (2020) for technical details.

The most worrying potential source of confounding is any scope that remains for targeting of the barrel bombs within neighborhood. Such potential for targeting may be picked up in the imbalance we detected on *km hospital*, which was among the most imbalanced variables and (in the regressions used for sensitivity analyses) explains a larger share of the outcome (*Support Opposition*) than any other covariate. It is thus useful to consider *km hospital* as a benchmark, asking how much stronger confounding would have to be than this variable, in terms of the residual variance it explains in treatment and outcome, in order for our results to change. Consider first confounding that is assumed only to be "no stronger than" *km hospital*, in that it explains no more of the residual variance in who lost their homes or in the outcome than does *km hospital*. Figure F.7 shows that confounding this strong would imply only a slightly reduced effect estimate (from 16 down to 14 percentage points), marked by *1x km_ hospital*. Confounding more than twice as strong as *km hospital* would reduce the estimate to 11 percentage points, which we remark on because such a point would still prove statistically significant ($t = 2.11$),[7] but confounding three times as strong would imply a non-significant results ($t = 1.83$). Finally, confounding six times stronger than *km hospital* would leave a point estimate just above zero. Note that we can repeat this analysis using any covariate, but report only this one as it is by far the most powerful and thus the toughest case.

Summarizing the results of our sensitivity analyses, first, the RV of 12% means that if confounding is responsible for the entire observed association between barrel bombing and lower support for the opposition, it would need to explain a fairly substantial proportion of the residual variance of exposure and outcome (e.g. 12% of both, or other equivalent combinations given by the contour on Figure F.7). Second, we need not assume precisely zero confounding, but rather very considerable confounding—several times stronger than *km hospital*—would be required to substantially alter the effect estimate. These results give guidelines for thinking about whether a particular confounder that readers propose would be sufficient to alter our conclusions. Our results are not beyond criticism, but critiques can most usefully be made if they suggest confounders that can arguably satisfy these criteria.

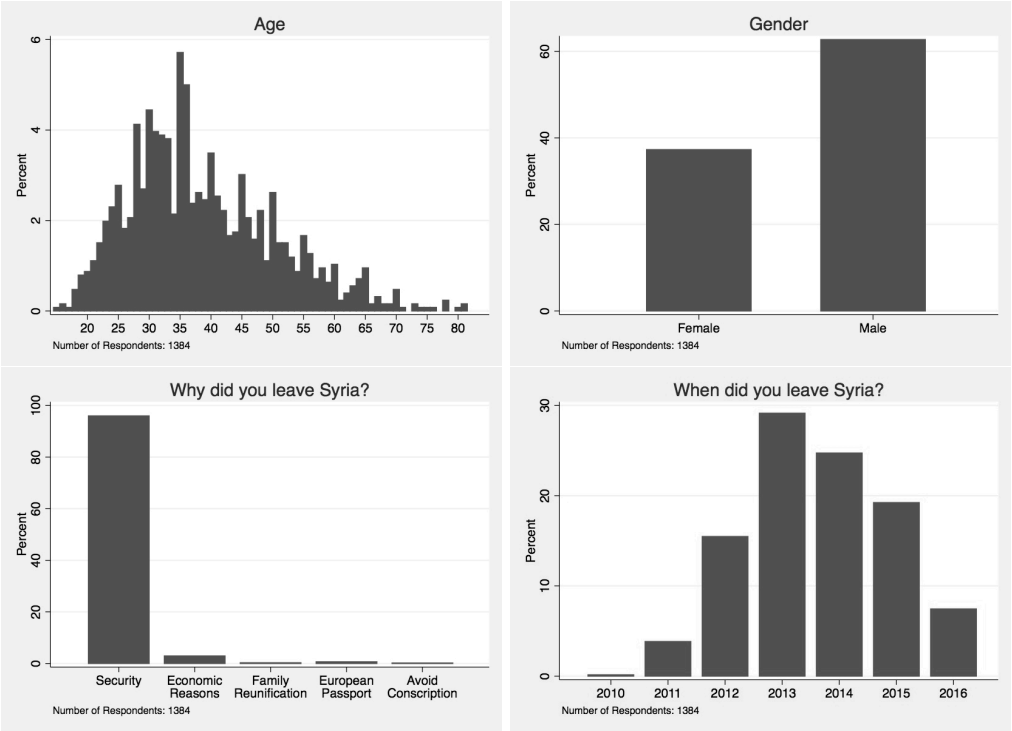
G Sample Composition and Benchmark Comparisons

Figure G.8 shows the distribution of age in our sample together with several other demographics. 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 (96%)

[7] Each hypothesized level of confounding also has implications for the standard error estimate. Adjusted t-statistics take into account both the adjustment to the point estimate and standard error. See Cinelli and Hazlett (2020) for details.

specified security concerns, as opposed to other reasons, such as economic considerations, family reunification, escaping to Europe, or avoiding conscription. 80% 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% of our sample are Kurdish speakers.

Figure G.8: 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*).

Below, we provide descriptive statistics that compare our sample to the Syrian population before the war, to another survey of Syrian refugees in Turkey, and to surveys of Syrian refugees in neighboring Lebanon and Jordan. The descriptive statistics in Table G.4 focus on gender, employment and education. To make the comparison meaningful, we focus on data of the Syrian population immediately before the war, and use data from surveys of Syrian refugees conducted around the same time as we did our survey in 2016. Since our survey covers only refugees who live outside the camps in Turkey, in the case of Jordan which hosts Syrians both inside and outside the camps, we report statistics for out-of-camp Syrians. Our data source for Syrians before the war is the World Development Indicators, which has data on the share of women in the population, and employment to population ratio in 2010, and secondary school and college completion rates in 2009.[8] The data on Syrian refugees in Lebanon are from the survey conducted in 2015 by Corstange (2020). The data on Syrian refugees in Jordan come from multiple sources: The proportion of female refugees is from UNHCR Statistical Report (UNHCR, 2016) conducted in May 2016. The employment data

[8]The link to the data set for World Development Indicators is available at https://data.worldbank.org/country/SY.

are from Krafft et al. (2019), which uses data from the 2016 Jordan Labor Market Panel Survey. The education data are from a survey conducted among non-camp Syrian refugees in Jordan by Care International (Care International, 2016). The survey data for out-of-camp refugees in Turkey are from the survey conducted by Disaster and Emergency Management Authority in Turkey in 2016-2017 (AFAD, 2017), which has surveyed refugees in 9 different provinces, and hence has a wider geographic coverage than our survey, which interviewed refugees in 4 provinces.

Our sample oversamples men relative to women because of the difficulties of surveying women in the households we selected, as discussed above in Section A. Compared to another survey of Syrian refugees in Turkey, the work status of the refugees in our sample is quite similar, except a smaller share of men in our sample had a paying job at the time of our survey. This is not surprising since our interviewees are those who were at home at the time of the survey, and therefore less likely to be working. But the overall share of those with a paid job is similar to the Syrian refugees in Jordan. Our sample is slightly less educated than the Syrian population before the war, and Syrian refugees in Lebanon and Jordan.

Overall, the differences between the demographic characteristics of our sample and the Syrian population before the war and the rest of the Syrian refugees in Turkey, Lebanon and Jordan are small.

Table G.4: Basic Demographic Benchmarks

	Syria	***Lebanon***	***Jordan***	***Turkey***			
	2009-2010	2015	2016	Refugees in 2016-17	Full Sample	Treatment Group	Matching Sample
Statistics							
Proportion Female (15+)	0.50	0.40	0.53	0.49	0.37	0.36	0.36
Employment to population ratio (15+)	0.39				0.53	0.61	0.59
Employment to population ratio (15+ male)	0.68				0.79	0.88	0.85
Employment to population ratio (15+ female)	0.10				0.11	0.13	0.12
Proportion with a paid job, Total			0.20	0.23	0.16	0.21	0.18
Proportion with a paid job, Male			0.40	0.37	0.21	0.28	0.23
Proportion with a paid job, Female			0.03	0.09	0.08	0.10	0.09
Secondary school completion, Total	0.22	0.20	0.27	0.20	0.12	0.08	0.10
Secondary school completion, Male	0.25	0.21	0.27		0.12	0.06	0.09
Secondary school completion, Female	0.19	0.18	0.26		0.11	0.11	0.10
College completion, Total	0.12	0.02	0.07	0.08	0.06	0.04	0.04
College completion, Male	0.14	0.03	0.09		0.06	0.04	0.05
College completion, Female	0.10	0.02	0.05		0.05	0.02	0.03

Note: The data for Syria are from the World Development Indicators, which is available at https://data.worldbank.org/country/SY. The data for Lebanon are from Corstange (2020). The data for Syrian refugees in Jordan come from multiple sources: The proportion of female refugees is from UNHCR (2016). The employment data are from Krafft et al. (2019). The education data are from Care International (2016). The data for refugees in Turkey in 2016-17 are from "Field Survey on Demographic View, Living Conditions and Future Expectations of Syrians in Turkey", conducted by Disaster and Emergency Management Authority in Turkey (AFAD, 2017). Full sample includes all refugees surveyed. Treatment group includes only the treated units. Matching sample includes all the respondents used in the analysis with matching estimates, including the control cases.

Table G.5 compares our respondents' province of origin to another survey of Syrians in Turkey, both for the whole sample and for respondents grouped by their year of departure from Syria. There are no major differences between the two surveys. Our full and matched sample both have more respondents from Aleppo. This is expected since our primary goal in choosing our survey locations was to reach Syrians who suffered from barrel bomb attacks, and Aleppo has been one of the main targets of these attacks. Moreover, three provinces in our survey (Hatay, Gaziantep, Sanliurfa) are bordering Aleppo governorate. While the share of respondents from Damascus and Idleb are very similar between both surveys, our sample had fewer respondents from Hama and Homs.

In Table G.6, we present descriptive statistics of our respondents by their year of departure from Syria. Slightly more than half of our respondents have left Syria in 2013-2014. Overall, there are no major differences between those who left early in the war and those who left later. Those who left early before 2013 are slightly less educated, and more likely to be male than those who left after 2014. Looking at the respondents' exposure to barrel bombing over time, we observe small increases over time in the share of respondents whose neighborhoods are barrel bombed, whose houses are destroyed as a result of barrel bomb attacks, and who were present during barrel bombing. This pattern is consistent with reports from the conflict (SNHR, 2017) that show a rise in the use of barrel bombs from 2012 to 2016.

Table G.5: Respondents' Province of Origin, by Year of Departure from Syria

Year of departure	Refugees in 2016-17	Full Sample	Matched Sample	Full Sample	Matched Sample	Full Sample	Matched Sample	Full Sample	Matched Sample
				Before 2013		2013-2014		After 2014	
Province of Origin									
Al-Hasakeh	3.1	1.4	0	2.6	0	0.8	0	1.6	0
Aleppo	53.2	66.7	80.2	64.3	72.2	73.6	84.3	54.6	76.6
Ar-Raqqa	4.6	10.2	1.3	8.2	0	9.4	2.4	13.2	0
As-Sweida	0.2	0	0	0	0	0	0	0	0
Damascus	5.3	5.4	0.4	4.1	0	5.1	0.8	6.0	0
Deir ez-Zor	2.0	1.9	0.4	0.4	0	0.8	0	5.1	1.6
Hama	6.4	0.7	0	0.4	0	0.7	0	0.8	0
Homs	9.8	2.1	1.8	1.5	0	0.3	0	6.2	6.3
Idleb	9.0	9.0	15.9	11.9	27.8	8.1	12.6	8.9	15.6
Lattakia	5.3	2.0	0	6.0	0	0.5	0	1.9	0
Quneitra	0.1	0.1	0	0	0	0.1	0	0.3	0
Tartus	0.3	0	0	0	0	0	0	0	0
Dar'a	0.6	0	0	0	0	0	0	0	0
N	2242	1384	227	269	36	745	127	370	64

Note: The data for refugees in Turkey in 2016-17 are from "Field Survey on Demographic View, Living Conditions and Future Expectations of Syrians in Turkey", conducted by Disaster and Emergency Management Authority in Turkey (AFAD, 2017), and which surveyed 2242 heads of households who live outside the camps. These heads of households gave information about all members of their households, which equals 9617 refugees outside the camps. Full sample includes all refugees surveyed. The matched sample is derived from the locations of the treated units for whom matches were found; the results would be identical when including their corresponding control pairs due to exact matching on neighborhood.

Table G.6: Descriptive characteristics of sample, by year of departure from Syria

	Before 2013			*2013-2014*			*2015-2016*		
	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N
Male	0.68	0.47	269	0.61	0.49	745	0.62	0.49	370
Age	39.78	12.22	258	38.74	11.99	665	37.47	11.25	337
Worked in Syria	0.60	0.49	267	0.51	0.50	733	0.55	0.50	366
Kurdish	0.14	0.35	269	0.13	0.33	745	0.11	0.32	370
Education	1.50	0.95	229	1.53	0.99	577	1.77	1.16	296
Has children	0.92	0.27	269	0.92	0.26	745	0.92	0.27	370
Rooms	3.12	2.19	256	3.12	1.55	716	3.04	1.18	362
Lived in an urban area in Syria	0.69	0.46	265	0.70	0.46	725	0.60	0.49	363
Has family members in Syria	0.89	0.31	269	0.89	0.31	745	0.91	0.28	370
Settled in Istanbul	0.14	0.35	269	0.20	0.40	745	0.20	0.40	370
Distance to market	0.93	0.78	268	0.90	0.79	739	0.87	0.78	369
Distance to school	0.64	0.52	268	0.65	0.54	741	0.61	0.55	370
Distance to hospital	1.40	0.94	268	1.42	0.92	738	1.55	0.95	369
Year left Syria	2011.79	0.43	269	2013.46	0.50	745	2015.28	0.45	369
Barrel bombed neighborhood	0.30	0.46	260	0.37	0.48	724	0.41	0.49	359
House destroyed due to barrel bomb	0.19	0.39	269	0.21	0.41	745	0.25	0.44	370
Present during barrel bomb	0.12	0.32	269	0.22	0.42	745	0.25	0.43	370
Injured due to barrel bomb	0.01	0.09	269	0.01	0.12	745	0.02	0.15	370
Number of family members injured due to barrel bomb	0.01	0.11	269	0.03	0.17	745	0.06	0.24	370
Number of family members killed due to barrel bomb	0.03	0.16	269	0.02	0.15	745	0.03	0.17	370
Business destroyed due to barrel bomb	0.04	0.21	269	0.09	0.28	745	0.08	0.26	370
Assets destroyed due to barrel bomb	0.19	0.40	269	0.21	0.41	745	0.28	0.45	370
Top threat to Syria: Assad	0.34	0.47	240	0.34	0.47	682	0.38	0.49	345
Top threat to you: Assad	0.52	0.50	237	0.47	0.50	677	0.48	0.50	344
Support opposition	0.54	0.50	269	0.50	0.50	745	0.49	0.50	370
Support no group	0.45	0.50	269	0.50	0.50	745	0.51	0.50	370
Neutrality acceptable	0.33	0.47	250	0.33	0.47	695	0.34	0.47	345
Compromise for peace	0.42	0.49	228	0.47	0.50	554	0.39	0.49	290
Fight until victory	0.11	0.32	228	0.08	0.27	554	0.06	0.24	290
Support peace if family does	0.73	0.44	257	0.78	0.42	718	0.80	0.40	353
Follow Syria news	0.54	0.50	269	0.49	0.50	745	0.51	0.50	370
Volunteer for refugees	0.57	0.50	234	0.65	0.48	658	0.66	0.47	333
Will likely return	0.89	0.31	255	0.87	0.33	720	0.87	0.33	643
Opposition could win	0.90	0.30	225	0.88	0.32	549	0.90	0.29	533

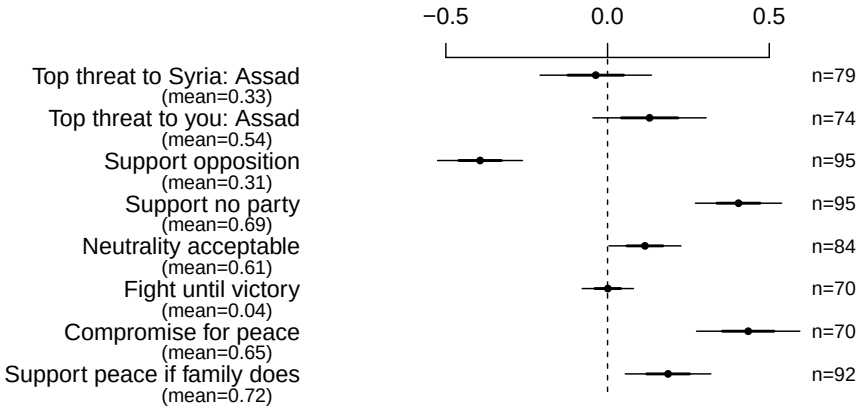
Note: Descriptive statistics on demographics, exposure to bombing, outcome variables, and variables for potential mechanisms.

H Effects by year-left-Syria

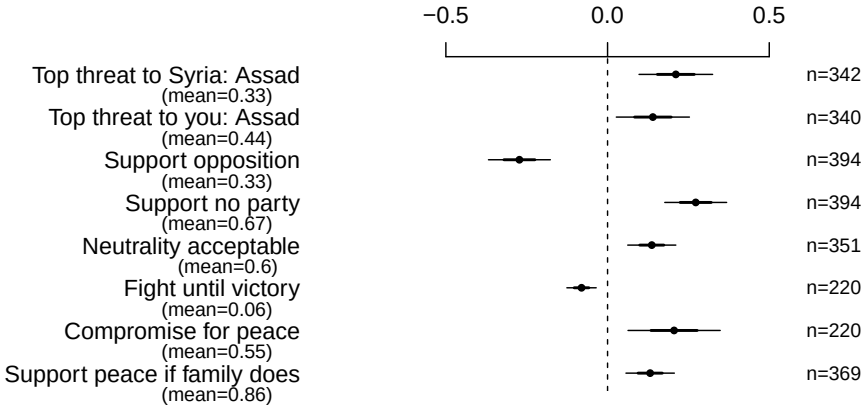
Here we report results separately for civilians who left Syria in three different time periods, marked by different dynamics and the changing character of the opposition forces. We note that losing one's home to barrel bombing can itself influence when a person leaves, so that conditioning on the year leaving may potentially introduce complexities due to conditioning on a post-treatment variable.

Results are shown in Figure H.9 below. The smaller sample sizes available in each cohort make comparisons more uncertain. Nevertheless, the apparent pattern of results is largely similar across these cohorts. Further, despite the smaller sample sizes in each cohort, three results are always significant and in the same direction: support for the opposition is lower, support for "no party" is higher, and the willingness to "compromise for peace" is higher among those who lost their homes to barrel bombs compared to those who did not.

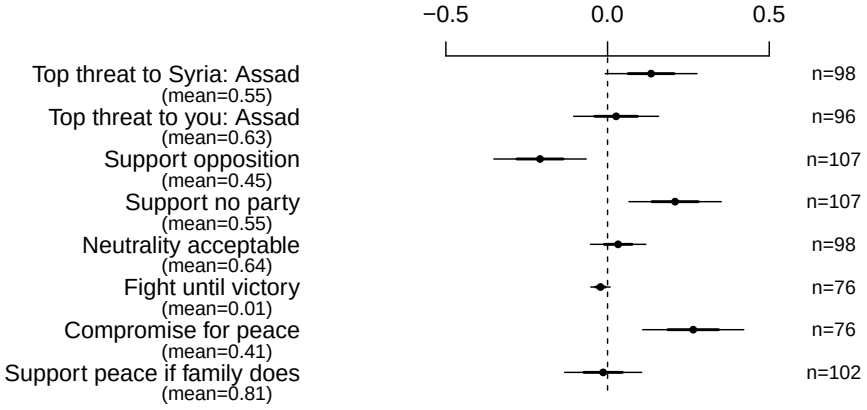
Figure H.9: Effects of home destroyed by barrel bombing, by year left Syria



(a) Left Syria before 2013



(b) Left Syria 2013-2014



(c) Left Syria 2015-2016

Note: Main ATT estimates with matching on neighborhood and gender, as in main text, for groups leaving Syria in different years.

H.1. Sensitivity analysis by year of departure

We provide separate sensitivity analyses for the three analyses above based on year of departure. Though handling eight outcomes for each of three sub-samples procludes an in-depth analysis with benchmarking etc., we provide the basic sensitivity statistics corresponding to the same OLS models used in the sensitivity analyses above.

Finally, we provide a sensitivity analysis that restricts the sample to only those individuals from Aleppo. The results are again similar.

Table H.7: Sensitivity analysis for those leaving Syria before 2013

	Estimate	SE	t-value	RV(%)	$RV_{\alpha=0.05}$	dof
Top threat to Syria: Assad	0.21	0.10	2.16	16.07	1.40	151.00
Top threat to you: Assad	0.13	0.11	1.20	9.31	0.00	150.00
Support opposition	-0.33	0.10	-3.41	23.58	10.69	160.00
Support no party	0.34	0.10	3.56	24.44	11.71	160.00
Neutrality acceptable	0.30	0.06	4.92	32.47	20.98	155.00
Fight until victory	-0.05	0.06	-0.84	6.47	0.00	158.00
Compromise for peace	0.53	0.10	5.50	35.20	24.32	158.00
Support peace if family does	0.14	0.09	1.57	11.93	0.00	153.00

Table H.8: Sensitivity analysis for those leaving Syria in 2013-2014

	Estimate	SE	t-value	RV(%)	$RV_{\alpha=0.05}$	dof
Top threat to Syria: Assad	0.20	0.06	3.19	14.41	5.81	421.00
Top threat to you: Assad	0.12	0.07	1.70	7.98	0.00	415.00
Support opposition	-0.15	0.06	-2.36	10.68	1.86	436.00
Support no party	0.15	0.06	2.41	10.90	2.11	436.00
Neutrality acceptable	0.22	0.04	6.24	26.00	18.65	426.00
Fight until victory	-0.04	0.04	-1.30	6.16	0.00	418.00
Compromise for peace	0.30	0.06	4.69	20.45	12.44	418.00
Support peace if family does	0.11	0.05	2.03	9.41	0.32	423.00

Table H.9: Sensitivity analysis for those leaving Syria in 2015-2016

	Estimate	SE	t-value	RV(%)	$RV_{\alpha=0.05}$	dof
Top threat to Syria: Assad	0.21	0.08	2.59	17.09	4.33	190.00
Top threat to you: Assad	0.18	0.09	2.09	14.14	0.83	188.00
Support opposition	-0.11	0.08	-1.32	9.07	0.00	194.00
Support no party	0.11	0.08	1.39	9.49	0.00	194.00
Neutrality acceptable	0.19	0.05	3.83	24.19	12.56	190.00
Fight until victory	-0.06	0.04	-1.48	10.23	0.00	187.00
Compromise for peace	0.24	0.08	3.08	20.13	7.74	187.00
Support peace if family does	0.07	0.06	1.18	8.23	0.00	188.00

Table H.10: Sensitivity analysis for individuals from Aleppo

	Estimate	SE	t-value	RV(%)	$RV_{\alpha=0.05}$	dof
Top threat to Syria: Assad	0.20	0.06	3.64	16.33	7.86	415.00
Top threat to you: Assad	0.16	0.07	2.41	11.26	2.18	408.00
Support opposition	-0.15	0.06	-2.62	11.76	3.06	437.00
Support no party	0.16	0.06	2.73	12.23	3.58	437.00
Neutrality acceptable	0.23	0.03	6.66	27.42	20.25	428.00
Fight until victory	-0.03	0.04	-0.80	3.81	0.00	425.00
Compromise for peace	0.34	0.06	5.84	24.61	17.11	425.00
Support peace if family does	0.10	0.05	1.95	9.06	0.00	419.00

I Regression tables with covariate coefficients

In this section, we provide regression results showing coefficients on control variables. We note that such analyses can be difficult to interpret for several reasons, including the simple fact that these covariates still have confounded relationships with the outcome. These covariates are not akin to our main "treatment", in that we have argued that the choice to condition on neighborhood and gender eliminates or greatly reduces confounding between that variable and each outcome. By contrast these covariates are not in general expected to be made unconfounded by our strategy according to our assumptions.[9]

We use the same specifications and data employed for the OLS-based sensitivity analyses above, with the indicators for whether the individual left Syria before 2013, 2013-2014, or after 2014. As noted in the prior section, these year indicators may be problematic to condition on by including them in the model, because they are potentially post-treatment. Nevertheless, they make no discernable difference to any of the results.

Results are shown below,[10] suppressing coefficients on the neighborhood fixed effects as they are numerous and uninformative. We note that, as was the case in the OLS models used for sensitivity analysis, these results are very similar to the estimates used in our primary analyses, which used matching, only on neighborhood and gender.

[9] We also note that in many settings, the causal structures one may argue for so as to claim that the covariates are suitable control variables will also lead to the conclusion that the estimated coefficients for those control variables do not have the expected causal interpretation, a symptom of the "table 2 fallacy" (see e.g. Westreich and Greenland (2013)). We refrain from a discussion of how this fallacy might bias coefficients in our case, because this requires considerable additional discussion of the causal structure of the problem and because the simple confounding problem just mentioned is more than sufficient to warn against straightforward interpretatino of these coefficients.

[10] Table made using the `stargzer` package Hlavac (2022)

Table I.11: Extended regression results

	Dependent variable:							
	Assad threat: Syria	Assad threat: you	Support opposition	Support no party	Neutrality acceptable	Fight to victory	Compromise for peace	Support peace if family does
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
House destroyed	0.192***	0.130**	−0.171***	0.171***	0.241***	−0.041	0.325***	0.080*
	(0.047)	(0.054)	(0.050)	(0.050)	(0.029)	(0.028)	(0.048)	(0.043)
Age (std)	−0.008	−0.011	−0.021	0.022	−0.007	0.006	−0.044**	0.002
	(0.018)	(0.020)	(0.019)	(0.019)	(0.011)	(0.011)	(0.018)	(0.016)
Kurdish	−0.128*	−0.037	0.024	−0.031	0.019	0.004	0.093	−0.204***
	(0.069)	(0.080)	(0.075)	(0.075)	(0.046)	(0.043)	(0.074)	(0.066)
Children	−0.006	−0.048	0.042	−0.048	0.016	−0.063	0.113	−0.035
	(0.067)	(0.077)	(0.072)	(0.072)	(0.042)	(0.041)	(0.070)	(0.063)
Work before	−0.079*	−0.007	−0.089*	0.080*	0.090***	0.040	−0.038	0.0004
	(0.045)	(0.052)	(0.049)	(0.049)	(0.029)	(0.028)	(0.048)	(0.043)
Rooms (log)	0.058*	0.032	−0.002	0.003	−0.012	−0.062***	0.091***	0.102***
	(0.033)	(0.038)	(0.035)	(0.035)	(0.021)	(0.020)	(0.035)	(0.030)
Hospital km	0.031*	0.009	−0.067***	0.065***	0.008	−0.057***	0.007	0.070***
	(0.018)	(0.021)	(0.020)	(0.020)	(0.012)	(0.011)	(0.019)	(0.017)
School km	0.023	0.082**	−0.001	0.006	0.051***	−0.015	0.052	−0.026
	(0.031)	(0.035)	(0.032)	(0.032)	(0.019)	(0.018)	(0.032)	(0.028)
Market km	−0.093***	−0.028	0.045*	−0.044*	−0.011	0.112***	−0.090***	−0.050**
	(0.022)	(0.025)	(0.024)	(0.024)	(0.014)	(0.014)	(0.024)	(0.021)
Grade school	0.006	−0.053	0.008	−0.002	0.046	−0.005	0.098	0.074
	(0.069)	(0.079)	(0.075)	(0.075)	(0.044)	(0.042)	(0.073)	(0.065)
Secondary school	−0.006	0.019	0.135*	−0.137*	−0.021	0.052	−0.071	0.056
	(0.074)	(0.084)	(0.080)	(0.080)	(0.047)	(0.045)	(0.078)	(0.069)
Left after 2014	0.070	0.115**	0.028	−0.013	−0.058**	−0.004	−0.077	0.049
	(0.044)	(0.051)	(0.048)	(0.048)	(0.028)	(0.027)	(0.047)	(0.042)
Left before 2013	0.022	0.060	−0.037	0.034	−0.032	0.010	−0.023	0.017
	(0.044)	(0.050)	(0.046)	(0.046)	(0.028)	(0.026)	(0.045)	(0.041)
Gender	0.093*	0.076	0.182***	−0.175***	0.027	−0.043	0.057	−0.067
	(0.048)	(0.055)	(0.051)	(0.052)	(0.031)	(0.029)	(0.051)	(0.046)
Observations	804	795	832	832	813	805	805	806

Note: *p<0.1; **p<0.05; ***p<0.01

References

AFAD (2017). Field survey on demographic view, living conditions, and future expectations of syrians in turkey. Available online at https://www.afad.gov.tr/suriye-raporlari.

Amnesty International (2015). Death everywhere: War crimes and human rights abuses in aleppo, syria. Available online at http://www.amnestyusa.org/sites/default/files/embargoed_5_may_aleppo_report_death_everywhere.pdf.

Blattman, C. (2009). From violence to voting: War and political participation in uganda. *American Political Science Review*, 103(02):231–247.

Care International (2016). Six years into exile: The challenges and coping strategies of non-camp syrian refugees in jordan and their host communities. Available online at https://data.unhcr.org/en/documents/download/51182.

Cinelli, C. and Hazlett, C. (2020). Making sense of sensitivity: Extending omitted variable bias. *Journal of the Royal Statistical Society: Series B (Statistical Methodology)*.

Corstange, D. (2020). National and subnational identification in the syrian civil war. *Journal of Politics*, 82(3):1176–1181.

Cronin-Furman, K. and Lake, M. (2018). Ethics abroad: Fieldwork in fragile and violent contexts. *PS: Political Science and Politics*, 51(3):607–614.

Hlavac, M. (2022). stargazer: Well-formatted regression and summary statistics tables; r package version 5.2. 3. *Social Policy Institute: Bratislava, Slovakia*.

Imbens, G. W. (2003). Sensitivity to exogeneity assumptions in program evaluation. *American Economic Review*, 93(2):126–132.

Jacobsen, K. and Landau, L. (2002). The dual imperative in refugee research: Some methodological and ethical considerations in social science research on forced migration. *Disasters*, 27(3):95–116.

Krafft, C., Sieverding, M., Salemi, C., and Keo, C. (2019). Syrian Refugees in Jordan: Demographics, Livelihoods, Education, and Health. In *The Jordanian Labor Market: Between Fragility and Resilience*, pages 141–172. Oxford University Press.

REACH (2015). Migration trends & patterns of syrian asylum seekers travelling to the european union. Available online at https://reliefweb.int/report/world/migration-trends-patterns-syrian-asylum-seekers-travelling-european-union-assessment?gclid=Cj0KCQiA4aacBhCUARIsAI55maEdwsmG4UcBZu1DlQZQSaeaXPMHQlNHHab7bIwCBLg9MFH6QqK8trMaAsJgEALwwcB.

SNHR (2017). The syrian regime has dropped nearly 70,000 barrel bombs on syria: Ruthless bombing. Available online at http://sn4hr.org/wp-content/pdf/english/The_Syrian_Regime_Has_Dropped_Nearly_70,000_Barrel_Bombs_en.pdf.

UNHCR (2016). Statistical report on unhcr registered syrians as of 15 may 2016. Available online at https://data.unhcr.org/en/documents/details/43466.

Westreich, D. and Greenland, S. (2013). The table 2 fallacy: presenting and interpreting confounder and modifier coefficients. *American journal of epidemiology*, 177(4):292–298.