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When Exit is an Option: Effects of indiscriminate violence on attitudes among Syrian refugees in Turkey

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Abstract

Most research on the effects of violence on civilian attitudes and behavior during civil war presumes that civilians are trapped in the conflict zone, with incumbents and insurgents competing for their loyalties. Yet in many cases – such as the current conflict in Syria, which we examine – large numbers of civilians leave the conflict zone, at least temporarily. How does indiscriminate violence affect civilian attitudes when exit is an option? Using a natural experiment owing to the inaccuracy of barrel bombs, we examine the effect of having one’s home destroyed on a cluster of attitudes of Syrian refugees in Turkey related to their personal security, side-taking and social engagement. While losses from barrel bombing represent only one component of wartime harm, they nevertheless have profound effects. Specifically, civilians who lose a home to barrel bombing are more likely to see the Assad regime as a greater threat to themselves personally and to the whole of Syria. Such harm does not, however, increase civilians’ support for the opposition, who failed to protect them. Instead we show that such violence increases parochial forms of solidarity and social engagement within the refugee community. Altogether this suggests that, when civilians can escape the conflict zone, they no longer need to choose sides as they seek safety, but rather may object to all armed groups. One implication of this logic is that for the armed groups – and particularly the incumbent in this case – indiscriminate violence may be a tragically effective tool for driving out populations that could have otherwise provided support and cover to the opposition. This argues for a “draining the sea” logic to indiscriminate violence, distinct from the logic described in the cases of captive civilian populations.

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1 Introduction

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

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

Using original survey data collected in four provinces of Turkey in the fall of 2016, we aim to better understand Syrian civilians' attitudes on a cluster of issues related to their personal security, their political allegiances and their social cohesion. While we also describe these attitudes in the overall population of refugees from which we sampled, we are principally interested in learning about the effects of violence or harm on these positions. We employ a tragic quasi-experimental approach that arises due to the technical nature of barrel bombs. Because these terrifying weapons cannot be finely targeted, the damage they cause is effectively random within a sufficiently small area targeted by the regime. This makes barrel bomb related harms well-suited to quasi-experimental research. Due to inferential reasons described below, our primary results focus on the effects of losing one's home to barrel bombing, above and beyond the many other harms that Syrian refugees we interview have suffered. We describe the other types of violence our respondents lived through in order to provide a better understanding of Syrian refugees' overall wartime experiences. However, we are not able to make credible causal claims about their effects. Focusing on destruction of homes by barrel bombs is admittedly narrow; nevertheless this approach uniquely allows us to test the implications of competing arguments as to how indiscriminate violence affects civilian attitudes towards armed groups in a context where civilians leave the conflict zone.

This paper is not the first to utilize a quasi-experimental design based on the conditional-randomness of violence in order to credibly estimate its effects. For example, Lyall (2009) examines how downstream insurgent attacks from a given location may be influenced by the previous use of indiscriminate violence at that location, and Blattman (2009) examines the effect of indiscriminate violence on political engagement. Bauer et al. (2016a) provides a thorough review of work, some of it quasi-experimental, on how violence influences cooperation and social cohesion. Particularly relevant in theories of civil war violence, however, is the question of how indiscriminate violence influences support for armed groups. While prior work has examined this relationship using surveys (e.g. Lyall et al., 2013) and survey experiments (e.g. Fair et al., 2016) ours is one of the few to use a quasi-experimental design to estimate the effects of violence on civilian attitudes towards warring groups.

More importantly, we believe our main contribution is the paper's emphasis on conflict conditions where "exit is an option" for civilians, and dynamics are thus not principally driven by the idea that armed groups compete for civilian cooperation and loyalty. We know of no prior work emphasizing the ability to exit from the war theater and what such an exit opportunity means

for how civilians react to indiscriminate violence. In the “captive civilian” model, armed groups compete for the cooperation of a fixed population, and populations must choose sides to achieve their own security and personal aims (Kalyvas, 2006). By comparison, in the “exit as an option” setting, we argue both that incentives for armed groups to use indiscriminate violence, and the ways in which civilians react to that violence, can change drastically. Even among refugees, who have by definition escaped the conflict zone, it is reasonable to expect that experiencing higher levels of incumbent indiscriminate violence drives up support for the insurgents, just as in the “captive civilian” model. Alternatively, it is reasonable to argue that civilians with an exit option instead turn away from all armed groups – both those who attacked them, and those who failed to protect them. Our principal contribution is an empirical test of these two alternatives, using a quasi-experimental approach to determine which response dominates.

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

In what follows, we first describe the literature on the effects of violence, discussing both the central theory relevant to this paper regarding indiscriminate violence, and the wide range of relevant empirical findings (Section 2). In Section 3 we turn to the context of the Syrian civil war. Section 4 describes our methodology, including the survey, our identification strategy, and estimation procedures. Results are given in Section 5 and discussed in 6, together with robustness tests and discussions of threats to the identification strategy. We conclude, suggesting both academic and policy implications of these results, in Section 7.

2 Background

This study contributes to an already rich theoretical and empirical literature on civil conflict. Below we survey the relevant literature on indiscriminate violence, first defining the term and then presenting arguments about its strategic logic, efficacy, and effects on behaviors and attitudes. We conclude the section by summarizing the gaps in these accounts as we see them and how our work offers a novel empirical and methodological opportunity to address these gaps.

2.1 Theory: Logic of Indiscriminate Violence

Scholars of civil conflict now routinely draw a conceptual distinction between “selective violence” –instances when combatants and/or the civilians that have materially supported combatants are targeted based on personalized information about their individual actions Kalyvas (2006)– and “indiscriminate violence”. Indiscriminate violence, on the other hand, is often used interchangeably with the term civilian victimization. It is variably defined as targeting everyone in a particular area with no effort to determine guilt or innocence (Downes, 2007), targeting violence without making credible efforts to distinguish between combatants and civilians Lyall (2009), and targeting violence based on guilt-by association or concepts of collective guilt (Kalyvas and Kocher, 2007).

In the Syrian case, it is unlikely that barrel bombing is meant to kill insurgents directly. This is evidenced by the fact that barrel bombing is most intense in the communities away from the front lines on which combat is occurring. If the point is not to kill insurgents, what then is the logic of such indiscriminate violence? In the rich theoretical framework articulated by Kalyvas (2006), it is assumed that civilian populations are largely static. This drives a contest for their support and cooperation on the part of armed groups, and forces civilians to chose which armed group they should provide with information and/or material support in return for protection or other benefits. In such circumstances, the strategic value of indiscriminate violence is limited: selective violence is far more useful, and the information needed to perpetrate it can be obtained from cooperative informants. One remaining place for indiscriminate violence, Kalyvas argues, is in territories where one side (the incumbent or insurgent) has almost no control or information. As examined empirically in Kalyvas and Kocher (2007), these are areas where selective violence is not possible and where an attacker may hope to turn civilians against the party holding the territory by showing they are incapable of providing protection (or perhaps are even responsible for provoking violence).

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

2.1.1 Exit as an Option

Conflicts such as Syria and Darfur (Hazlett, 2016) that involve massive population movements may be labeled by some as “eliminative”, removing them from the scope of the standard logic of violence proposed in Kalyvas (2006). However, we argue that (a) attempting to describe these as “coercive” or “eliminative” is not straightforward, and (b) an alternative feature that drives the strategic logic of indiscriminate violence is whether or not exit is an option for civilians. From this, a different logic of indiscriminate violence can be theorized.

In the Syria case, we argue that the regime’s purpose in using violence against certain populations may be better described as a “draining the sea” tactic (Valentino et al., 2004), designed to sever the insurgents from their potential safe-havens and bases of support. Although governments may worry that excessive violence could backfire and drive civilians into the arms of the insurgents, as Kalyvas (2006) notes, “it usually is easier to prevent or deter cooperation through violence than it is to win it” (385). The cases of mass displacement seem to blend coercion and elimination: they use violence coercively, not only to cutoff civilian support for the rebels initially, but then to drive out civilians – even if temporarily – from the battle zone. Yet the *goal* is not to eliminate these populations as such, and the incumbent may even hope that displaced civilians return once the area is cleared of insurgents.

If a civilian exit option gives the regime strategic reasons to use indiscriminate violence as a means of waging counter-insurgency by “draining the sea”, the same applies for insurgents. They

too may wish to use indiscriminate violence to drive out incumbent supporters from areas they seek to control. As for civilians, which we focus on here, the logic linking their victimization to who they support can change radically in these conditions. When exit is an option, how will civilians react to indiscriminate violence? Considerations of emotion, trauma, and desire for revenge could suggest that civilians turn towards whatever group opposes the perpetrators of violence. In our case, this would suggest that regime indiscriminate violence leads to support for the opposition. On the other hand, with exit as an option, there may no longer be an individual strategic logic to taking sides. Civilians might turn against all parties – the regime for causing violence, and the opposition for failing to protect them from or for provoking such violence. The issue of how civilians react to indiscriminate violence when they can exit is an open empirical question that we seek to answer here.

2.2 Relevant Empirical Work

Wartime Efficacy

Beyond work that seeks to identify the logic and strategic reasoning behind civilian victimization, a number of other studies estimate the impact of indiscriminate tactics to ascertain whether or not they achieve their desired objective. The driving question here is if strategies based on indiscriminate civilian victimization (whether in the form of aerial-bombardment or scorched earth campaigns) can help incumbents or insurgents to win wars. For example, Dell and Querubin (2016) find that U.S. aerial bombardment in the Vietnam war was counter-productive: it increased the military and political activities of the insurgency, weakened local governance, and reduced non-communist civic engagement. In a closely related study, Kocher et al. (2011) find that aerial bombardment increased Viet-Cong insurgent activities in bombed areas. In contrast, Downes (2007) finds that indiscriminate violence can be an effective wartime strategy when structural conditions are favorable: i.e. the underlying population supporting the insurgents is small, their geographic area of control is constricted, and civilian loyalties are rigid. Lyall (2009) comes to a related finding, citing the fact that Chechen villages that were indiscriminately shelled by Russians experienced a substantial reduction in subsequent insurgent attacks. He attributes this reduction in insurgent attacks to the fact that indiscriminate violence can disrupt insurgent supply chains as well as restructure “the relationship between insurgents and populace by underscoring that the insurgency cannot credibly protect the population” (339).

In a response to Lyall (2009), Souleimanov and Siroky (2016) show that in the case of Chechnya, insurgent retaliation was often delayed by months and rarely took place in the same location where the incumbents directed their original indiscriminate attacks. Instead, insurgent responses to violence initiated a wartime game of “whack-a-mole”. Souleimanov and Siroky (2016) further urge a clearer distinction between non-targeted, collective violence carried out “without regard to prior local activity such as insurgent targeting of local counterinsurgency forces” (what they call “random indiscriminate”) and similar violence that “is triggered by the insurgency’s prior targeting of local counterinsurgency forces” (what they call “retributive indiscriminate”). In our case the key characteristic of incumbent violence we must assume is more simply that of “conditional randomness”: some areas may be targeted for more or less violence based on presumptions of where the insurgency is, who supports it, or other factors. Nonetheless, there must exist a geographic level below which there *is* randomness in the exact individuals or households hit by barrel bombs. That said, understanding whether civilians believe that their areas may be targeted due to prior insurgent activities is of central importance here, and one possible explanation we consider for our results is that civilians might blame insurgents for bringing the regime down on them.

Behavioral and Attitudinal Responses

Beyond studies that focus on the strategic logic and efficacy of indiscriminate violence, a number of studies further take individual behaviors and attitudes into account. On the behavioral side,

and closely related to many of the studies cited above, is an important body of work focused on combatants' shifting alliance patterns and civilians' rational calculations in this context. In these accounts, combatants and civilians are cast as utility-maximizing agents. Alliances and loyalties shift in response to the perceived rewards (including material support and basic security) and punishments (violence or its threat) that actors anticipate (Christia, 2012).

Other relevant studies are interested in how exposure to violence shapes various types of "civic" behaviors and attitudes including participation in civil society, political engagement and altruistic actions and beliefs. For example, Voors et al. (2010) presents evidence that exposure to increased violence results in more altruistic behavior to one's neighbors in Burundi. Bellows and Miguel's study in Sierra Leone (2009) finds that individuals whose households directly experienced more intense war violence are more likely to attend community meetings, to join political/community groups, and to vote. Although not directly related to civil wars, Bateson (2012) similarly finds that crime victims tend to be more involved in both civic and political life. Tangentially related empirical studies on the effects of violence on attitudes of trust also exist. For example, Cassar et al. (2011) find that exposure to conflict and intense fighting undermines trust in Tajikistan and Uganda respectively. Finally, in her work on the Spanish civil war, Balcells (2012) finds that victimization matters for political attitudes, identities and behaviors. Those victimized by violence reject the perpetrators' identities, and this rejection can be passed through to subsequent generations who do not themselves experience the trauma directly.

A set of even more nuanced findings that distinguishes between political engagement vs. other types of social engagement emerges from Blattman's study of ex-combatants in Northern Uganda. Those conscripted through abduction are found to exhibit a substantial increase in voting and community leadership, but not nonpolitical forms of social engagement (Blattman and Annan, 2008). Blattman attributes these increases in political engagement to a process by which witnessing elevated levels of violence leads to personal growth and political activation.

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

2.3 Implications

To conclude, we note several remaining gaps in the literature that seem especially relevant to the study of indiscriminate violence.

First, and most broadly, what are the implications of taking seriously the possibility of exit from the conflict zone? On the one hand, we might very reasonably expect that individuals who suffer higher levels of indiscriminate harm increase their support for the opposition even if they can leave the conflict zone. This could involve behavioral support, but also the expression of political support, alignment of policy preferences with the opposition, or feeling that the opposition represents them. Alternatively, when exit is an option – and particularly among refugees who *have* actually exited – incumbent indiscriminate violence may not drive individuals into the arms of the opposition, as there is no security rationale for such a response. Support for the opposition

therefore could remain unchanged and/or the opposition’s failure to protect – and perhaps their perceived role in provoking incumbent violence – could lead individuals away from the opposition as well. Our aim in this paper is to offer quasi-experimental evidence to shed empirical light on the question of which of these reactions dominates, at least in the Syrian case.

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

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

Understanding civilian attitudes is important for several reasons, especially in the Syrian context. First, about 90% of refugees in our sample consider returning to Syria likely, 91% have family members in Syria, and 89% of them follow the news from Syria. Moreover, 11% of them have visited their home in Syria after they left.¹ These figures strongly suggest that our sample of refugees are deeply involved in the conflict and are likely to be part of a post-conflict Syria in the future. Beyond this, civilians’ attitudes are crucial because returning civilians can act as spoilers and jeopardize the durability of peace settlements. Even when elites appear to be in control of the peace process, they are often only appealing to what they believe civilians want (Hoddie and Hartzel, 2010). Furthermore, the monetary support and remittances that civilian refugees provide, can serve as a continuing form of material support for certain groups, even if refugees themselves never return (Lindley, 2010). Finally, a study of refugee attitudes provides important additional insights into how refugees’ everyday lives have taken shape in Turkey itself and how these configurations can impact social cohesion in the host country.

3 The Syrian Civil War

3.1 Background on the Civil War

The Syrian civil war was sparked by protests that began in March 2011, when children aged between 9 and 15 were detained and reportedly tortured for writing graffiti denouncing the Assad regime on the walls of their school in Der’a (McHugo, 2014). On March 15, during demonstrations calling for the children’s release, four people were shot dead by the security forces, causing demonstrations to escalate. Protests soon spread to other cities, such as Hama and Homs, and were met with a harsh response from the regime (Hokayem, 2013). In early April, Assad introduced a number of minor reforms in order to quell the growing unrest: He replaced the governor of Der’a, lifted the Emergency Law which was in effect since 1963, and also gave citizenship to about 300,000 Syrian

¹This number should be considered as a lower bound on the actual number of refugees who have visited home because the refugees are not allowed to visit home except with special permissions under the terms of the so-called “Temporary Protection Regime in Turkey”, and therefore likely not to report their visits back to Syria.

Kurds (Asrar, 2011). These reforms proved insufficient, and by the end of July 2011 around 2,000 people were reported to have died as result of mounting violence (McHugo, 2014). Also in July of that year, a number of Syrian Army officers deserted to form the Free Syrian Army (FSA). This significant development in the organization of the opposition was followed by the formation of a political body called Syrian National Council in Istanbul, in October 2011. In 2012, fighting spread to Aleppo, the commercial center of Syria and its second largest city .

By July 2012, the initial protests that were largely peripheral, organized by lower and middle classes, and fueled by mostly local grievances had turned into a brutal civil war, fought between the Syrian government forces and multiple rebel factions, including both secular and Islamist groups (McHugo, 2014). In March 2013, Islamist rebel forces took control of Raqqa, making it the first major city to come under insurgent control in the civil war. Around this time, the regime also started to withdraw its forces from eastern Syria. The war entered a new phase in the summer of 2014, when the Islamic State of Iraq and Syria (ISIS) captured the Iraqi city of Mosul in June and declared an Islamic caliphate with the Syrian city of Raqqa as its de-facto capital. After ISIS started to attack Kurdish forces dominated by the Democratic Union Party (PYD) in northern Syria, Kurds became a fourth major player in the war (Phillips, 2016).

As of the summer of 2014, while the government was in control of Damascus and the coastal cities of Latakia, Banyas, and Tartous, the city of Aleppo was divided between government the opposition groups. To the east of Euphrates river, the regime had no forces present, and the Syrian Kurds and various Islamist groups battled for control. In March 2015, the city of Idlib was captured by Islamist rebel groups, and ISIS captured the desert city of Palmyra in May 2015. These losses forced Assad to concede the Syrian army faced a manpower shortage (Asrar, 2015) and in the fall of 2015, Russia, a staunch supporter of the Assad regime, started its first air strikes to bolster regime control from its Khmeimim airbase near Latakia. These strikes aimed at both ISIS and non-ISIS opposition groups, bombing targets in Homs, Hama, and Quneitra provinces (Phillips, 2016). By early 2016, Russian intervention had reset the military balance in Syria (Casagrande and Cafarella, 2016) and the Assad regime gained ground throughout the year. Around this time, the war also had clearly become a proxy conflict between actors supporting the regime (Russia, Iran, and Hezbollah) and those who support the opposition (US, Turkey, Saudi Arabia and Qatar). At the end of 2016, the regime recaptured the entire city of Aleppo (BBC, 2016).

Indiscriminate Violence during the Syrian Civil War

Large-scale violence directed against civilian populations is a common feature in civil wars, especially when states attempt to defeat rebel groups that they believe are embedded in or supported by civilian communities (Valentino et al. (2004), Colaresi and Carey (2008), Downes (2007)). The Syrian civil war is no exception, with the Assad regime using widespread shelling and bombing—especially barrel bombing. Barrel bombs are improvised explosive devices (IEDs) dropped from helicopters. They are typically oil barrels, fuel tanks or gas cylinders packed with explosives, fuels and metal fragments like nails and machine parts to increase their lethality. During our preliminary fieldwork for this study in the fall of 2015, medical responders in Syria informed us that the radius of damage from a barrel bomb is normally about 500 meters, with some people surviving unscathed, some injured to various degrees and others killed.

Among all governorates in Syria, Aleppo has suffered the most concentrated number of barrel bombings and civilian deaths. According to our conversations with the director 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.

3.2 Studying Syrian Refugees in Turkey

Turkey hosts the largest number of Syrian refugees (UNHCR, 2015) and is the primary destination for Syrians exposed to indiscriminate violence in the civil war, and especially to barrel bombs.

Conducting research with highly mobile and vulnerable populations is challenging, and studying Syrian refugees in Turkey is made more difficult for several reasons. Unlike most refugee crises, where the response is coordinated by the United Nations High Commissioner for Refugees (UNHCR), the Turkish government has insisted on directly controlling the refugee registration and management process within its own borders through two state-controlled bodies, the Prime Ministry Disaster and Emergency Management Authority (AFAD) and the Directorate General of Migration Management (DGMM). According to the statistics provided by DGMM, as of June 15, 2017, there are 3,049,879 registered Syrian refugees in Turkey. About 8% of them are settled in the 24 camps run by the Turkish government, while the vast majority reside among the Turkish population in urban areas. About 60 % of non-camp refugees are living in 4 provinces of Turkey: İstanbul, Gaziantep, Hatay, and Şanlıurfa.²

AFAD tightly controls access to the refugee camps and completely prohibits researchers from entering. AFAD and DGMM also refuse to release any detailed data on the origin areas or settlement patterns of refugees entering Turkey.³ Finally, in the spring of 2015, the Interior Ministry announced that academic research with Syrian refugee populations in Turkey was subject to the approval of the ministry. This approval requirement was lifted by the end of 2015. Taken together, these regulations and prohibitions have limited access to information about barrel bombed populations.

The absence of official data on refugee movements required us to obtain much of our preliminary information through key informant interviews at organizations serving Syrian refugee communities in Turkey. These organizations included legal and psycho-social support centers, schools and medical facilities. We conducted this field-work in the fall of 2015 and learned that refugees from Aleppo are heavily concentrated in a subset of neighborhoods in the Turkish cities of Kilis, Gaziantep and, to a lesser extent, İstanbul, Hatay, and Şanlıurfa. The data we collected subsequently and present here is, to our knowledge the largest and most comprehensive survey of Syrian refugees in Turkey.

4 Methods

4.1 Identification Strategy

As our identification strategy shapes much of our research design, it is helpful to begin this section with it. On the one hand, a careful identification strategy is needed to differentiate claims of the type that “violence causes a higher proportion of people to have a given attitude” from findings that “those people most subject to violence are also those more likely to have a given attitude.” On the other hand, such an approach admittedly limits the types of questions we can hope to credibly answer. In our case, it restricts us to examining the impacts of barrel bomb related harms.

Barrel Bombing and Conditional Randomness

Our identification strategy rests on a claim that *while barrel bombs can be targeted to one neighborhood versus another, their effects are indiscriminate within that neighborhood*. In other words, conditional on the neighborhood, we expect barrel bomb related harms to be distributed effectively

²The province-level numbers of registered Syrian refugees in Turkey are available at http://www.goc.gov.tr/icerik6/gecici-koruma_363_378_4713_icerik.

³The one exception is the report published by AFAD (2013) based on a survey conducted in 2013, long before barrel bombings were being used heavily in the Syrian civil war.

at random. Our principal argument for this claim is that the military targets neighborhoods for barrel bombing, but the targeting of specific individuals, families, or even buildings within these neighborhoods is limited for two main reasons: (a) *technical limitations* and (b) *strategic logic*.

First, regarding *technical limitations*, barrel bombs are imprecise weapons. They are dropped from helicopters and as they fall, their trajectory is unpredictable. While they may be targeted to the area of a block or so, precision targeting beyond this is not practical. As mentioned above, even once they hit a particular spot, our discussions with first responders during our qualitative work in 2015 revealed that there is a radius of 500 meters over which who is injured and what is destroyed (or not) is seemingly unpredictable.

Second, regarding *strategic logic*, we argue that the principal aim of barrel bombing was to make the area inhospitable to civilians so they either withdraw support for the opposition or leave the city. Killing rebels was not the main goal of these bombings so there was not a reason to target them. As mentioned above, one piece of evidence for this is that the barrel bombings did not actually focus on the front lines where active fighting was occurring and where rebels were known to be operating. Rather, areas away from the front line were most heavily targeted, in an effort to clear them out. Even if the regime did intend to target rebels in these areas, they know rebels can move from building to building, and rebels can see and hear the barrel bombs being dropped, making it generally unhelpful to target specific sub-sets of buildings thought to hold rebels. Indeed, the inability to know where a barrel bomb will drop (when seeing and hearing it from the ground) prevents people from effectively avoiding harm – and makes them all the more terrifying.

4.1.1 Barrel bomb related harms

We measured a number of barrel bomb related harms. These included:

- Was the neighborhood you come from barrel bombed at some point? (Y or N): *bbomb_neighborhood*
- Were you present in that neighborhood during the time of any barrel bombing? (Y or N): *present_bbomb*
- Were you yourself physically injured by the barrel bombings? (Y or N): *injured_bbomb*
- Thinking about your spouse, siblings, children and parents only, how many of these family members were injured due to barrel bombing? *bbomb_fammember_injured*
- Thinking about your spouse, siblings, children and parents only, how many of these family members were killed due to barrel bombing? *bbomb_fammember_killed*
- Was your home at that time destroyed or damaged so badly [by barrel bombs] as to make it unlivable? (Y or N): *bbomb_house_destroyed*
- Was your place of business destroyed by barrel bombing? (Y or N): *bbomb_business_destroyed*
- Can you please tell me what other assets belonging to you or your family were destroyed by barrel bombing? *bbomb_assets_destroyed*: (open-ended)
- Thinking about your spouse, siblings, children and parents only, were your family members injured due to indiscriminate violence (barrel bombs, shelling or rocket attacks)? (Y or N): *familyinjured_indisviolence*
- Thinking about your spouse, siblings, children and parents only, were your family members killed due to indiscriminate violence (barrel bombs, shelling or rocket attacks)? (Y or N): *familykilled_indisviolence*

We list all these harm-related questions for completeness, but our analysis in this paper focuses on *bbomb_house_destroyed* as the harm uniquely suited to credible causal inference. This is because

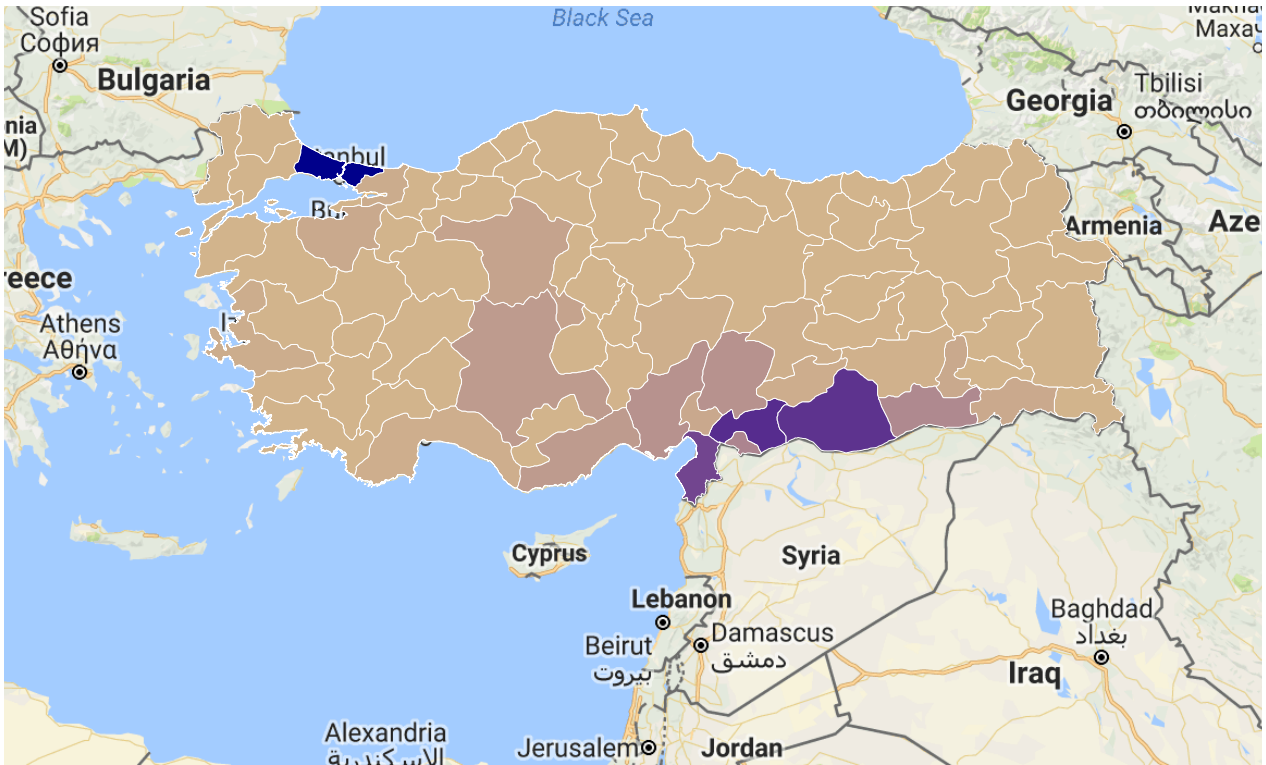
a person’s house is destroyed (or not) based on its location relative to where barrel bombs happen to strike. This does not depend upon how risk tolerant a person is (the way *present_bbomb* could be), or the behavior of one’s family members (the way the family-related harms are). A complete description of the concerns that prevent us from using each of the measures above (besides *bbomb_house_destrtoed*) is given in A.1.

Accordingly, our primary results and our conclusions will thus be driven by our analysis of the effects of *bbomb_house_destroyed*. While we will limit our analysis in this way to maximize the credibility of our identifying assumptions, this does admittedly narrow our understanding. In our discussion below, we perform supplementary analyses of several types with the aim of better clarifying our results and what may account for them. We emphasize that other harms beyond the narrowly defined harm of having one’s home destroyed clearly matter, both in human terms and in terms of their effects on civilian attitudes. We provide a description of the prevalence of numerous other harms suffered by our sample population (see Table 1).

4.2 Survey Sampling

Our sampling strategy involved three stages: First, we sampled Turkish provinces with the highest number of Syrians present: İstanbul, Hatay, Şanlıurfa and Gaziantep.⁴ Syrians in these provinces comprise about 60% of all out-of-camp Syrians living in Turkey. Figure 1 shows all provinces of Turkey, with the number of Syrian refugees in each indicated by color. The four darkest areas are those in which we sampled. The province with the fifth highest number of Syrians is Adana, which was hosting 149,188 refugees at the time of our survey, less than half of the number in Gaziantep.

Figure 1



In earlier fieldwork in these provinces, we found that while Syrians residing in Gaziantep are mostly Sunni Arabs, Syrian Kurds live in Şanlıurfa, and Alawite Syrians are more likely to reside in Hatay. Gaziantep, Hatay, and Şanlıurfa share a border with Syria, while İstanbul is far away from the border between Turkey and Syria.

⁴According 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. The number of registered Syrians in Turkish provinces which are updated regularly is available at http://www.goc.gov.tr/icerik6/gecici-koruma_363_378_4713_icerik.

Each Turkish province is divided administratively into smaller units, called districts. The second stage of our sampling involved choosing districts in these four provinces to interview Syrians. We sampled districts that have the highest concentration of Syrians according to the information we collected during our interviews in 2015 with NGOs that assist Syrian refugees in these provinces. In Gaziantep, we sampled Şahinbey and Şehitkamil districts. In Şanlıurfa, we sampled Haliliye and Eyyubiye districts. In Hatay, we sampled Antakya. In Istanbul, we sampled in five districts: Bağcılar, Esenler, Fatih, Küçükçekmece, and Sultanbeyli. The number of Syrians at the district level in Turkey is not publicly available. However, according to a recent report on Syrian refugees in Istanbul (Kaya and Kirac, 2016), as of July 2015, Küçükçekmece (27,419), Bağcılar (25,406), and Fatih (23,800) were the top three districts with the highest number of Syrians in Istanbul, while Sultanbeyli (14,661) and Esenler (12,000) are amongst the top 10. These numbers further confirm that our choice of districts is in line with findings from other scholarly research on the topic.

In the third stage, within each neighborhood with a heavy concentration of Syrians, our enumerators randomly chose a street and then randomly selected households on that street. Within a household, if the male head of the household was at home, he was asked to participate in the survey. If the male head of the household was not at home or (in a rare number of cases) if the female head of household opened the door, she was asked to participate. If the respondent agreed to participate, the enumerators proceeded to the survey. Having completed at most 10 interviews per street, our enumerators randomly picked another street and made the next round of interviews using the same sampling procedure.

While showing some variation across provinces, our response rate was 34 percent⁵. The questions we asked on the survey – and the subset of questions used for analysis – are the result of the identification strategy we employ to ensure that individuals who do and do not experience a given harm are otherwise comparable. A complete copy of our survey instrument is available at [*website available upon publication*].

Outcome Measures

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

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

⁵American Association for Public Opinion Research (AAPOR) Response Rate 1 formula.

Geographic variables used for identification

In order to identify the location of respondents' homes in Syria as accurately as possible, we asked them in which governorate, city and neighborhood they used to live. If they are from a rural area, we asked them which governorate and village they are from.⁶ Using the information about respondents' original homes, we matched each respondent to one of the administrative units in Syria, relying on the list provided by United Nations Cartographic Section (UNCS) and United Nations Office for Coordination of Humanitarian Affairs (OCHA)⁷. For our respondents from the capital cities of each governorate such as Aleppo or Ar-Raqqa, these units are neighborhoods in these cities. For our respondents from outside the cities in each governorate, these units are either small towns or villages. Overall, we were not able to match 135 respondents to a unit because either the respondent failed to provide any information or we were not able to match respondent's answer to the list of administrative units available to us.

4.3 Estimation Procedures

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

Conditioning is done straightforwardly by matching on location indicators and gender, using the *Match* package (Sekhon, 2011). Because location and gender are discrete, matching is exact and there is no need for a bias correction due to inexact matches. This produces an average treatment effect on the treated (ATT) estimate, as each treated unit is matched to control units, or otherwise dropped if no control is available. The Abadie-Imbens standard errors for matching (Abadie and Imbens, 2006) are used to construct conventional 95% confidence intervals.

Conventional $(1-\alpha)\%$ confidence intervals allow one to determine whether the null hypothesis that the average effect is zero can be rejected at the α level, by determining whether these confidence intervals exclude zero. We show these conventional confidence intervals for the null of no average effect in the usual way; but because our estimator leaves relatively few matched units with which to construct each difference-in-means estimator, we also sought to employ a more robust inferential approach. To this end, we also show the 90% and 95% boundaries of a null distribution generated by permutation inference.

Specifically, consider first a dataset consisting of the matched pairs found by matching (with the weights implied by the matching procedure, for multiply-matched units). In each pair (i.e. one row of the matched dataset), one unit was originally harmed ("treated") and the other was not, and the standard ATT estimate is constructed by taking the difference in outcomes within each pair, then averaging these differences together across all pairs (weighting each pair using the given weights). To construct a distribution of the effect estimates one would see had there been no effect within any pair (known as the "sharp" null of no treatment effect within pair), we randomly re-assign harm within each matched pair. Each time we randomly reassign which of the units is considered the harmed one for all matched pairs, we can recompute an effect estimate one would expect to see under this null.

We do this 10,000 times, producing a distribution of outcomes under this null. The 95% two-sided null interval is then constructed using the 2.5th and 97.5th percentiles of this distribution. The same process can be used by showing the 5th and 95th percentiles to construct the 90% two-sided

⁶We also asked our respondents to locate their home using Google Maps on our enumerators' smart phone, to obtain GPS coordinates for their home in Syria. Unfortunately only 18% of respondents were willing and able to do this.

⁷The list of administrative units in Syria and their maps are available at <https://data.humdata.org/dataset/syrian-arab-republic-administrative-boundaries-populated-places>

null interval.

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

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

5 Results

5.1 Descriptives

We first describe the full sample and then the restricted sample required by our identification strategy.

Full Sample

Our sampling resulted in surveying 1,120 Syrians. The geographic distribution of the sample by Syrian governorate is in Figure 2. Our sample is diverse in terms of the governorates the respondents are from. While 60 percent of the respondents are from Aleppo, 13 percent are from Ar-Raqqa, and 11 percent are from Idleb. 28 percent of our respondents from Aleppo came from barrel bombed neighborhoods. In Ar-Raqqa and Idleb, those from barrel bombed neighborhoods constitute 21 and 44 percent of the respondents, respectively.

Our sample is relatively well balanced on gender, with 38 percent of our sample being female, despite the difficulty of interviewing Syrian refugee women that has affected other surveys of Syrians, in which the gender ratio is highly imbalanced (Giebler, 2015). Figure 3 shows the distribution of age in our sample together with 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 specified security concerns, as opposed to other reasons, such as economic considerations, family reunification, escaping to Europe or avoiding conscription. Our sample also shows significant variation on the time of leaving Syria. While basically all our respondents left Syria after the start of the civil war, 80% of them left Syria in 2013 or afterwards, 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).

Figure 2

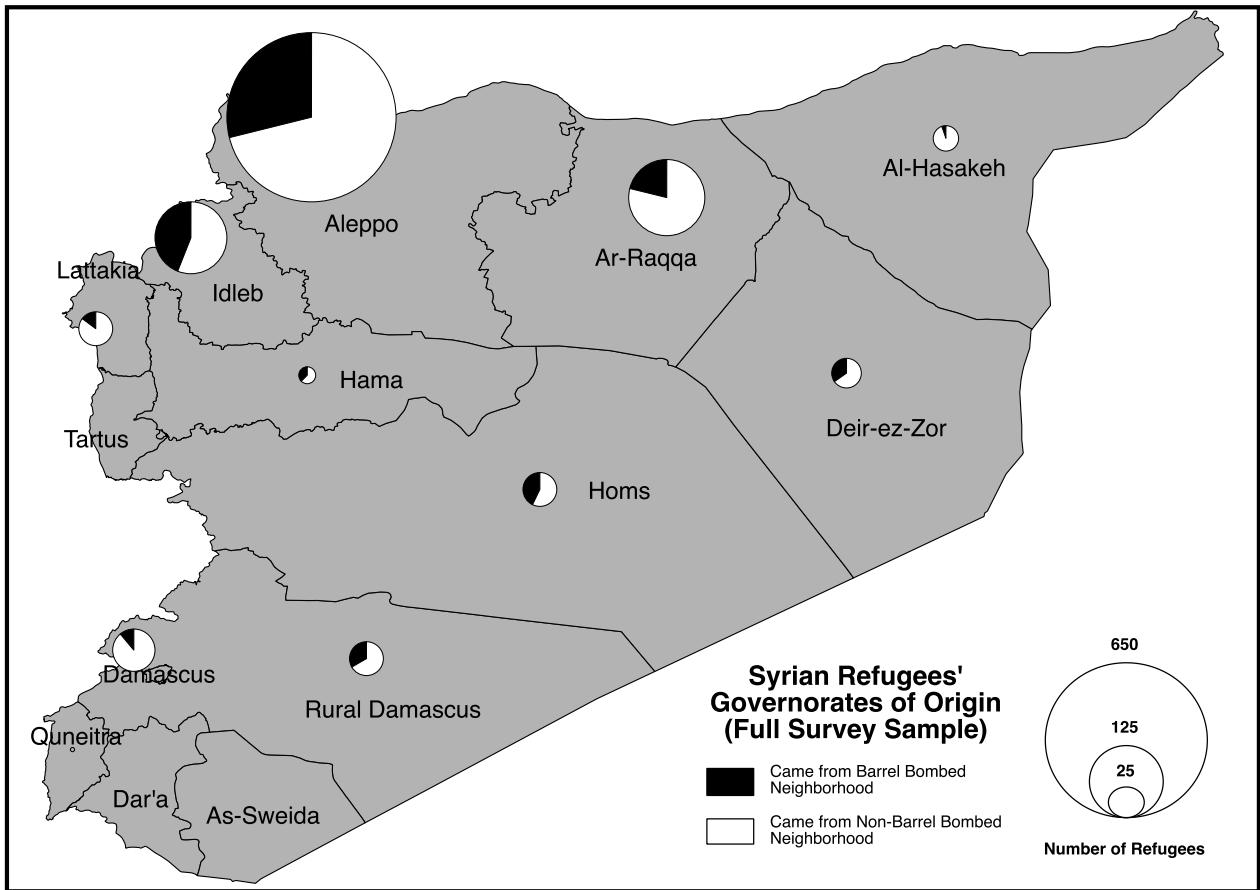
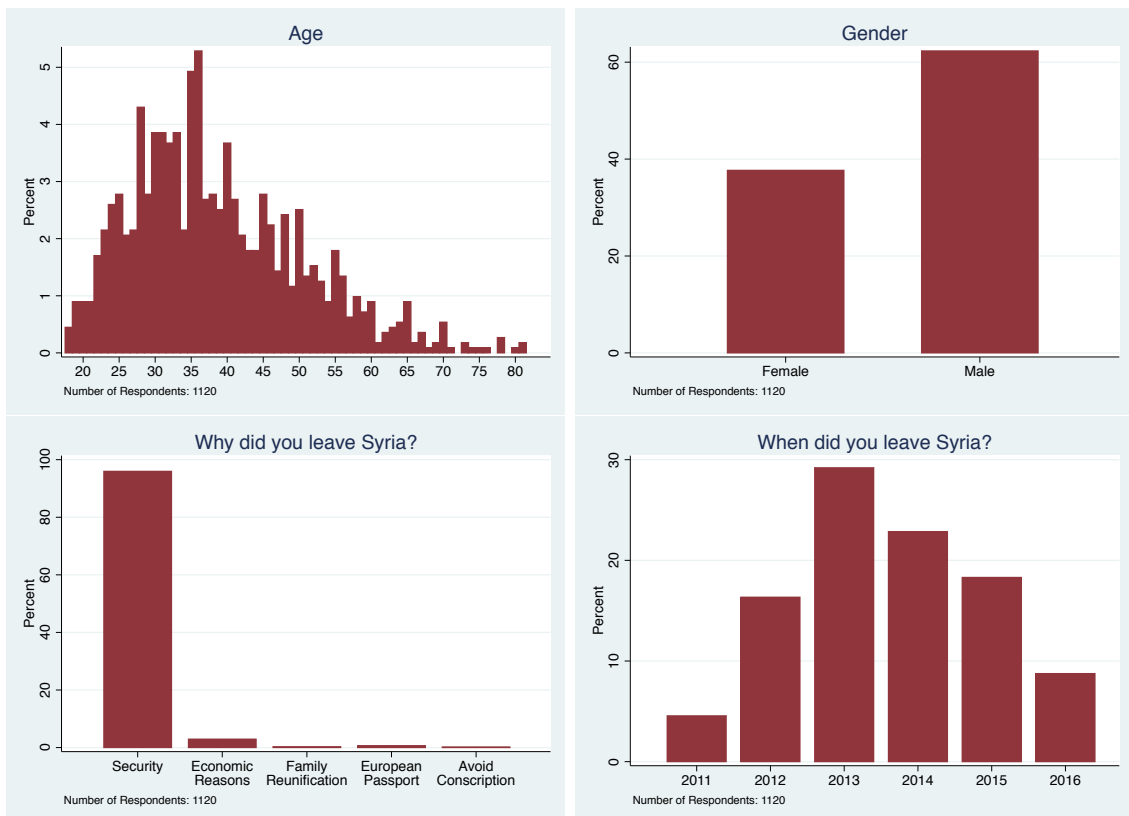


Figure 3: Key Demographic Descriptives



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

Distribution of Violence

While our identification strategy requires focusing narrowly on one type of violence, for descriptive purposes we report the levels of violence experienced by participants in Table 1. Some forms of violence we asked about were extremely low and so not reported here: fewer than one third of one percent reported an injury or death due to insurgent violence, either in their family or neighborhood. Rates were similarly low for questions about torture and injury or death due to sniper fire.

We also inquired about family members injured or killed by barrel bombs, shelling, and rockets. Since the rates were low for all three, we join them together in *familyinjured_indisviolence* and *familykilled_indisviolence*. Suicide bombing rates were higher, and what we report here combines family members killed or injured by suicide bombing.

Table 1: Experiences with Violence

	Full Sample	Matched Sample	Home Destroyed	Home Not Destroyed
<i>injured_bbomb</i>	0.015	0.012	0.041	0
<i>bbomb_fammember_injured_bin</i>	0.035	0.035	0.092	0.012
<i>bbomb_fammember_killed_bin</i>	0.026	0.035	0.092	0.012
<i>familyinjured_indisviolence</i>	0.085	0.059	0.061	0.058
<i>familykilled_indisviolence</i>	0.066	0.041	0.061	0.033
<i>suicide_bombing</i>	0.070	0.053	0.029	0.058

Description of exposure to violence other than having one’s home destroyed by barrel bombs. Results are given first for the full sample (column 1). For comparability, we also provide means after restricting to the matched sample that is used in analyses below (column 2). We further split this sample out into those who had their homes destroyed by barrel bombs, and those who did not (“Treated” and “Control”)

We note that we do not expect these characteristics to be “balanced” across those who do and do not have their homes destroyed for two distinct reasons. First, we are not yet conditioning on neighborhood, as we do in our primary analysis, which would be required to avoid confounding and to expect balance. Second, even if we did condition on neighborhood, these variables are potentially affected by having one’s home destroyed. For example, those whose homes are destroyed may be more likely to leave, making them less susceptible to subsequent suicide bombings. That is, these other forms of violence are potentially “post-treatment” and thus are not expected to be balanced.⁸

Effective Sample: Respondents From Barrel Bombed Neighborhoods

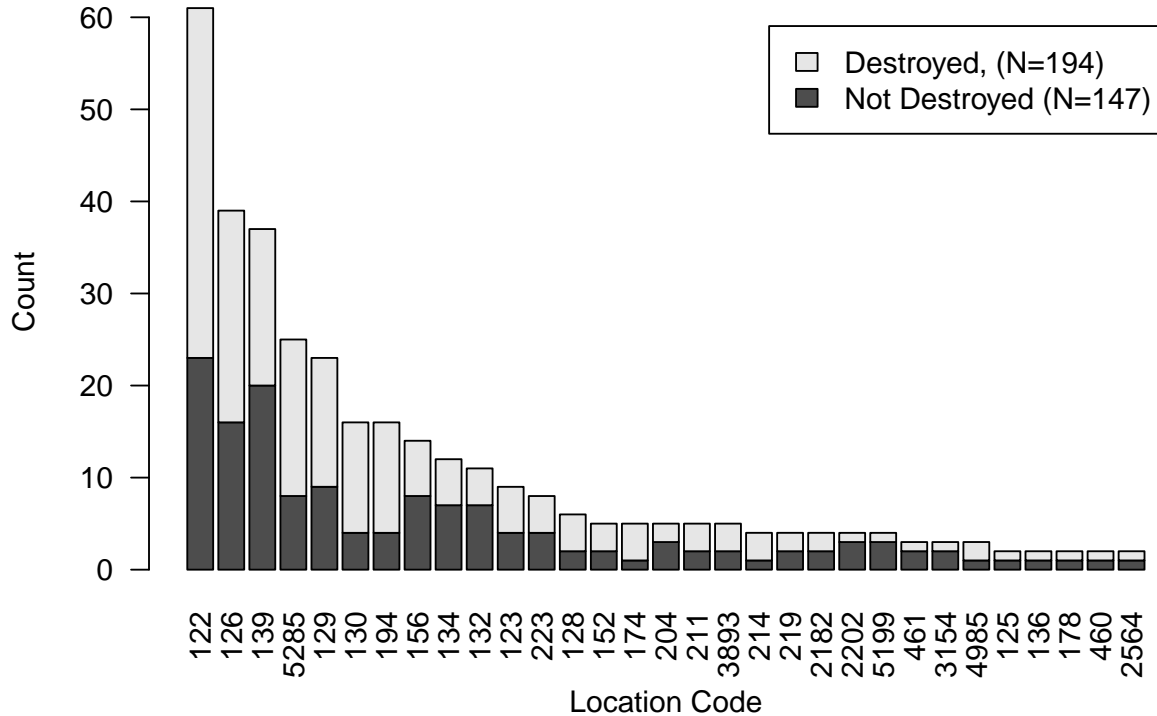
Our identification approach requires restricting the sample to those whose neighborhoods were reportedly barrel bombed. This reduces our sample size to 493. It also requires dropping those for whom we were unable to get sufficiently good information on the location of their neighborhood, which drops a further 53 observations. Beyond this, the process of conditioning on location and gender by matching also effectively limits the sample about which we are making an inference: only those unit-gender strata in which there is at least one individual whose house was destroyed and one whose was not remain in the matched sample. The resulting sample has 341 respondents, spread out across a variety of neighborhoods with similar proportions of treated and control in each (see Figure 4). This effective sample is 37.5% women, with a mean age of 39. About 95% of them have left Syria in 2012 or later. The majority (76%) of these individuals are from the governorate of Aleppo, and out of these, 88% are from the city of Aleppo.⁹ 12% of our restricted

⁸Such potentially post-treatment variables also cannot be conditioned upon, which prevents us from conducting analyses within sub-groups based on these or other potentially post-treatment variables.

⁹See Figure 9 for the neighborhoods of Aleppo city that are included in our sample.

sample are from Idleb, and 4.5 % are from Ar-Raqqqa.

Figure 4: Effective Sample by Geographic Unit



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

5.2 Effects of Harm

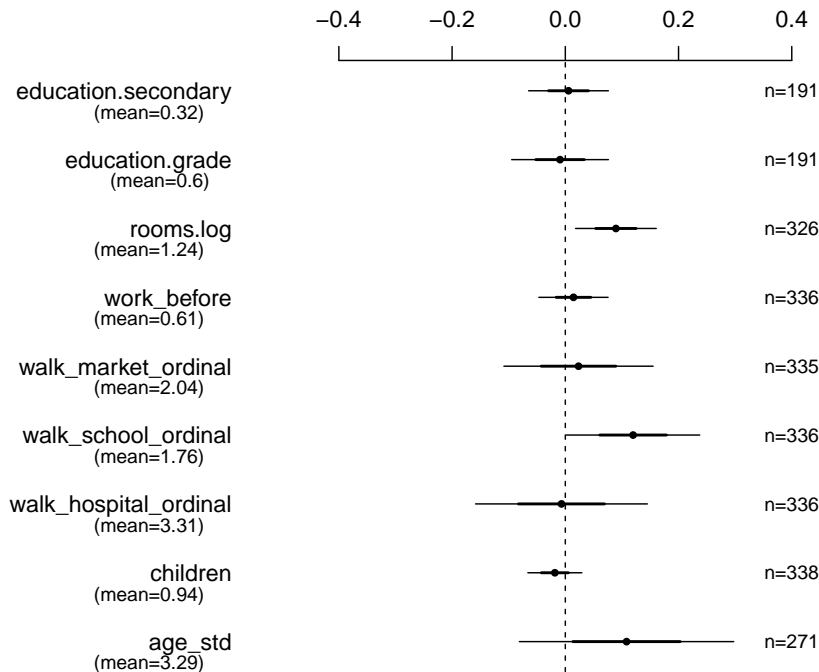
We now turn to our quasi-experimental strategy for estimating the effect of having one's house destroyed by barrel bombing.

Conditional Balance

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

However, here our assumption is not that barrel bombing is unconditionally random, but rather that those within a given neighborhood (and gender) have equal chances of having their home destroyed. Thus the appropriate balance test is a conditional one: conditional on neighborhood and gender, do those whose homes were destroyed look similar to those whose homes were not? To test this, we condition on neighborhood and gender by exact matching (see 5.3), and consider each pre-treatment covariate as if it were an outcome. There should be no "effect" of having one's house destroyed on these covariates, if the identification assumption holds.

Figure 5: Conditional Balance/ Placebo Test



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

We use this procedure to check conditional balance on all the covariates we have that (a) show some variation (at least 5% in the minority category for binary variables), and (b) are convincingly “pre-treatment”, i.e. we are sure they are unaffected by barrel-bombing. These include the (log) number of rooms in the house (*rooms.log*), whether the person worked prior to leaving (*work_before*), ordinal measures of how long it takes to walk to the nearest market, school, or hospital (*walk_market*, *walk_school*, *walk_hospital*)¹⁰, number of children prior to the crisis (*children*), and *age_std* (*age* in years, standardized to improve visualization). We note that such a conditional balance test is also effectively a placebo test, where pre-treatment covariates are used as if they were outcome variables that we know should not be effected by the treatment. We also include grade school and secondary education (*education.secondary* and *education.grade*). We exclude higher levels of education because they show little variation, and also risks being post-treatment, whereas primary and secondary education are most likely unaffected by barrel bombing in our sample since all participants are at least 18 years of age.

Our conditional balance/ placebo tests referenced in (Figure 5) are largely consistent with a case in which home destruction by barrel bombing is random conditional on neighborhood. Of course, they cannot show if unobservables are also balanced, and hence are not dispositive. We note potentially concerning imbalances on two variables. First, homes with more rooms were more likely to be destroyed. This is sensible, simply because homes with more rooms are larger and thus more likely to be hit. It could of course generate an imbalance (and bias) with those who have their homes destroyed more often being among the better off socio-economically. Intending to investigate this, we asked people for their socio-economic status in Syria (low, low-middle, middle, upper-middle, or upper). While those of higher income are no more likely to have their homes destroyed, we came to realize these variables are potentially post-treatment, since losing one’s home could affect one’s answer on this question. Instead, we place higher faith in the education

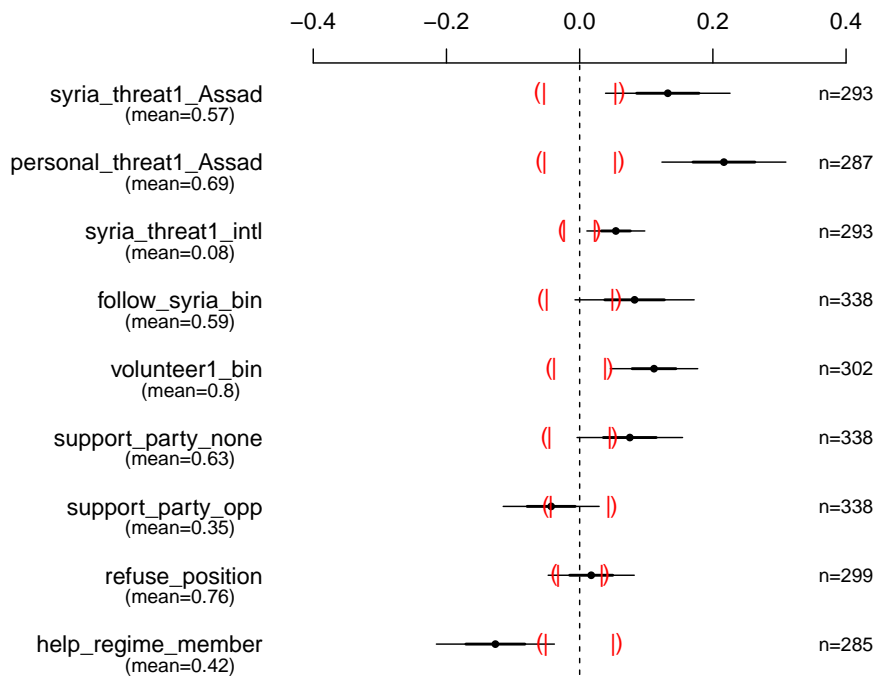
¹⁰Each of these *walk_x.ordinal* variables is coded as 1 (less than 5 minutes walk); 2 (5-15 minutes walk); 3 (15-30 minutes walk); and 4 (more than 30 minutes walk).

variables as proxies for socio-economic status, which prove to be well balanced.

The other variable not well balanced is the distance to the nearest school. Those exposed to barrel bombing tend to have lived slightly farther from the nearest schools. We are not entirely sure how to interpret this result. One plausible explanation relates to wartime education programming. In the phase of the war when these bombings occurred, many schools were run by the community and sometimes moved from their original location. As schools were scattered and moved to avoid violence, efforts may have been made to place them in areas thought to be relatively safer, even within a given neighborhood, such as buildings with underground space. Such wartime activity may create the observed imbalance. However, we have no expectation that distance to these school locations would be associated with the outcome so as to make it a confounder. Furthermore, in the sensitivity analysis in Section 6 we show that the confounding that would be due to such a covariate – or to an unobservable similarly related to both bombing and outcomes – would have very little impact on our estimates.

5.3 Effect Estimates

Figure 6: Effect Estimates: Home Destroyed by Barrel Bombing



ATT estimates for the effect having house destroyed on attitudes related to security. Conditioning on neighborhood and gender is done by exact matching. Confidence intervals by classical inference (whiskers) and boundaries of the sharp-null distribution as described in the text.

Our main effect estimates are shown in Figure 6. These plots allow for inference both by conventional tests and permutation inference at the same time. We show ATT estimates with conventional 95% confidence intervals so readers can perform the usual visual test, seeing if these intervals include zero or not. However our intended analyses rely on permutation inference. For this, we indicate the 90% and 95% percentiles of the permutation null (the “sharp null”) around 0 using “|” and “)” symbols respectively. Point estimates (solid dots) that fall with the 90% (“|”) indicators are points for which we cannot reject the null hypothesis of no effect with p-values of 0.10. Point estimates outside the 95% interval (red “(” and “)”) indicators are those for which we can reject the null at the conventional p-values of 0.05 or less. And those point estimates falling between the “|” and “)” markers are those where the p-value would fall in the marginal range

between 0.05 and 0.10. We make our inferences based on the permutation test rather than the classical one, as it is assumption free. However, for the most part the methods agree, and we report both p-values below.

We note first that those who lost homes to barrel bombing are 13 percentage points more likely to find Assad as the number one threat to the country, and 22 percentage points more likely to say Assad is the number one threat to them personally, both of which are highly significant by either the conventional test or permutation inference ($p < 0.005$ for all tests). We consider this result unsurprising, and largely a sanity check. Along similar lines, those who lose homes to barrel bombs prove far less willing to provide lifesaving support to a regime member, by 12.7 percentage points ($p < 0.0001$ by permutation; $p = 0.003$ by conventional).

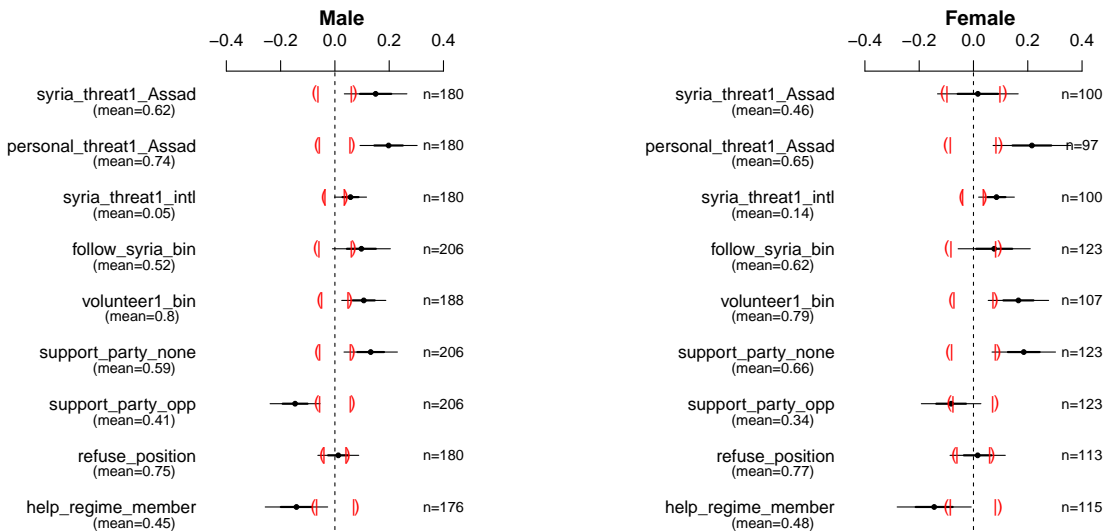
Those who lose homes to barrel bombing are also 5 percentage points more likely to say that international powers are the primary threat to Syria, also statistically significant by both methods ($p = 0.023$ by permutation; $p = 0.027$ by conventional).

Next, we find that those who lost homes to barrel bombs are more likely to follow Syria “very closely” or “somewhat closely” in the news by 8.25 percentage points. The result is strongly significant by permutation test ($p = 0.006$), though only marginally significant by conventional test ($p = 0.065$).

We see no estimate approaching significance for *refuse_position*, indicating no change in the permissibility of remaining neutral in the conflict. However, we see that those losing homes to barrel bombings are much more likely to report supporting their community through volunteer work, by 11 percentage points ($p < 0.001$ by permutation and conventional tests).

Regarding our key variables, support for the opposition (*support_party_opp*) is 0.043 percentage points *lower* among those who lost homes to barrel bombs. The result is only marginally significant by permutation test ($p = 0.098$) and worse by the conventional test ($p = 0.24$). However there is certainly no evidence for *increased* support for the opposition as might be expected in a “captive civilian” scenario”. Likewise, those who lost homes to barrel bombs are 7.5 percentage points *more* likely to say that “no party” represents their views, strongly significant by permutation test ($p = 0.006$) and marginally significant by the conventional test ($p = 0.057$).

Figure 7: Effect Estimates by Gender



ATT estimates for the effect having house destroyed on attitudes related to security. Conditioning on neighborhood and gender is done by exact matching. Confidence intervals by classical inference (whiskers) and boundaries of the sharp-null distribution as described in the text.

5.4 Results by Gender

We further split the sample by gender and conduct the same analysis in both subsets. We did not do this to test some hypothesis about differences in gender, because we had no such hypotheses. Rather, the purpose is to (a) see if the results would “replicate” well in these two independent samples, and (b) as discussed below, men likely face more severe problems of selection into our sample, which makes this comparison useful to rule out such selection as the source of our effects. Results are shown in Figure 7. As these samples are smaller than the full sample – particularly for women who make up 39% of our sample – some variation and loss of statistical power are expected. Regardless, the overall pattern of results is extremely similar. While women do not show the same increase in reporting Assad to be the greatest threat to Syria, they do show the increased personal threat from Assad, and very similar increased international threat. They have similar effects to men on all other variables, including the negative effect (though less significantly) on support for the opposition party, and large, highly significant positive effects on support for “no party”.

6 Discussion of Findings

We begin with the “unsurprising” findings that are nevertheless reassuring. Having one’s house destroyed by a barrel bomb unsurprisingly proves to have a large effect on *security* threat-perceptions. Those whose houses are destroyed by barrel bombs are much more likely to see the Assad regime as the greatest threat to their personal safety (*personal_threat1_assad*) and are also more likely to see the Assad regime as the greatest threat to Syria (*Syria_threat1_assad*), a share which is largely drawn from the proportion that would otherwise see ISIS as the greatest threat (*ISIS_threat1_assad*), which drops by almost as much. The proportion seeing the other groups we asked about (the opposition, international powers, other armed groups) as the greatest threat were all very low.

Turning to the findings more central to the theoretical considerations of “exit as an option”, we also find effects of this form of violence on attitudes about allegiances, side-taking and community solidarity. In a theory where civilians are treated as captive and must chose sides, one would expect that having your home destroyed increases the probability of seeing the opposition as the political group most closely representing you. Importantly, our effect estimate lies in the opposite direction, with a statistically insignificant negative effect. This is more consistent with the idea that, when exit is an option, civilians respond to such losses not by flocking to the opposition, but by either remaining unchanged in their support or even withdrawing it.

Further, we find a significant rise in the proportion of people finding that *no group* represents their interest. Again this is consistent with withdrawal of support in response to violence, which civilians can get away with when they exit the war theater, but which they may not be able to afford if captive in the conflict zone. Similarly, while captive civilians are expected to choose sides, our estimate for *refuse_position* is near zero (and fairly precisely estimated). We argue that the expected direction of any bias given our design likely would be against the observed effect. Presumably, if some individuals are more targeted by the regime, these individuals are apt to be the more pro-opposition. Hence, if some characteristic makes some individuals (within a given neighborhood) more likely to have their homes destroyed, we would expect these individuals to be more pro-opposition. Such a confound would produce an apparently positive relationship between home destruction and pro-opposition attitudes, while we observe the opposite. We further examine such potential violations and robustness to them in Section 6.1

Collectively, we interpret these findings as evidence that, when civilian exit is an option, civil war dynamics are altered in important ways. First, with respect to debates on the effectiveness of indiscriminate violence during counterinsurgency campaigns, we find evidence that indiscriminate violence indeed pushes civilians away from the party who perpetrated it. More importantly, however, because they were able to flee and hence to protect themselves from future predation by all sides, refugees subjected to government barrel bombing are free to simply withdraw their support from all armed groups. A dominant theme in civil war research sees macro-level dynamics

emerging from a patchwork of shifting local bargains and alliances (e.g. Kalyvas, 2006; Christia, 2012). These alliances, while fluid, are theorized to result from groups and individuals deciding who to side with, in part out of security considerations. While an apt description when civilians must make these choices, these very conditions are undermined when civilians can exit the area of danger.

We do not believe that this lack of support for any political group should be interpreted as political apathy for two reasons. First, those who lost a home to barrel-bombing are no less likely to follow the news on events back home in Syria, a sign of continued political engagement. Second, we find that refugees facing additional losses from violence are more likely to show heightened community solidarity *within* the refugee community as evidenced by their reported increase in volunteer-ship. Such civic-mindedness among the harmed, does not, however extend to regime members. Having one’s house destroyed by barrel bombs decreases willingness to provide life-saving assistance to a regime member in need (*help regime member*) by nearly 13 percentage points – almost a quarter of its mean. To this end, our paper also contributes to a growing literature on the effects of exposure to violence on civic and social behavior. As Bauer et al. (2016b) reviews numerous findings that violence increases in-group social cohesion, we also find increased volunteer-ism. This apparent increase in altruism, however, does not extend to regime-members – this may be a bridge too far for those who have suffered at the hands of the regime, or it may reflect that the apparent increase in altruism after violence is of a parochial nature (also reviewed in Bauer et al. (2016a)).

6.1 Robustness and Threats to Validity

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

Remaining Confounders

Here we discuss threats due to possible remaining confounders, first by qualitatively considering what types of confounders could explain away our results, and second with a formal sensitivity analysis.

The strategy we have pursued recognizes that some neighborhoods surely are more/less subject to violence, while also being more/less supportive of the opposition, for example. Conditioning on the neighborhood unit as described attempts to address this type of concern. However, our identification strategy would fail – and our results may be due to confounding bias – if on a *within-neighborhood basis*, an unobserved characteristic makes some individuals more (less) likely to lose their home, *and* less (more) supportive of the opposition. We consider, first qualitatively and then with a sensitivity analysis, whether such a confounder seems likely, and what it might be. If the neighborhood units we used are too large and targeting is possible within these areas, it is easy to think of confounders such that individuals whose homes are more likely to be destroyed are *more* likely to be pro-opposition. For example, being pro-opposition itself could lead one’s building (or other sub-neighborhood area) to be known as sympathetic to the opposition, and thus subject to greater targeting. This would drive a positive relationship between having one’s home destroyed and being pro-opposition. We observe just the opposite. It is more difficult to think of confounders that would make those who are more likely to lose their homes also be *less* supportive of the opposition. Similarly, it is difficult to think of confounders that would make those who are more likely to lose their homes also be *more* supportive of “no” party. Thus, while we cannot rule out all possible unobserved confounders, we struggle to identify a likely candidate that could produce the results we observe.

In addition to this qualitative reasoning about potential remaining confounders, a formal sensitivity analysis can help to characterize the types of confounders that would be problematic. The analysis we use here follows Cinneli and Hazlett (2017) and rests on very few assumptions, but

specifically analyzes the sensitivity of coefficients from a linear regression. We specify a model with location fixed effects, and gender fixed effects. As covariates and for benchmarking purposes, we include all of the pre-treatment covariates used above for balance testing. Such a model, while different from the matching estimators used above, estimates that having one’s home barrel bombed is associated with 7.7 percentage points lower support for the opposition, and 7.2 percentage points higher support for “no party”, very similar to the matching estimates (7.5 and 4.2 respectively). Figure 8 shows a sensitivity plot for the opposition support outcome. In brief, the coordinates given by the horizontal and vertical axes indicate how strongly a hypothesized confounder is related to the treatment (horizontal) and the outcome (vertical). The contour lines are labeled to show the treatment effect estimate after adjusting for the bias that would be due to such a confounder. The points shown on the plot are benchmarks, which indicate how an unobserved confounder would alter our estimate if it has been as strongly related to the treatment (horizontal position) or outcome (vertical position) as the covariate named next to each point. For readability, we show only the several benchmarks that are most troubling or otherwise of interest, as they are all clustered in the bottom left.

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

- an unobserved confounder that is as strongly associated with the treatment as “children” ($R_d^2 = 0.024$) would have to be at least 1672 times more strongly associated with the outcome (reaching $R_y^2 = 0.208$) in order to reduce the estimated effect of *bbomb_house_destroyed* to 0
- an unobserved confounder as strongly associated with the outcome as “walk_market_ordinal” ($R_{2y} = 0$) would have to be at least 40 times as strongly associated with the treatment (reaching $R_d^2 = 0.976$) in order to reduce the estimated effect of *bbomb_house_destroyed* to 0

We regard these results as relatively strong in a context where we know the fundamental inaccuracy of barrel bombs makes it very unlikely that any unobservable could be very strongly related to the treatment. This does not mean our result is impervious to unobserved confounding. If some confounder akin to neighborhood exists and remains unaccounted for, it would have the power to change our answer. However, it is difficult to think of such a confounder, particularly one whose direction would produce the effect we observe rather than its opposite.

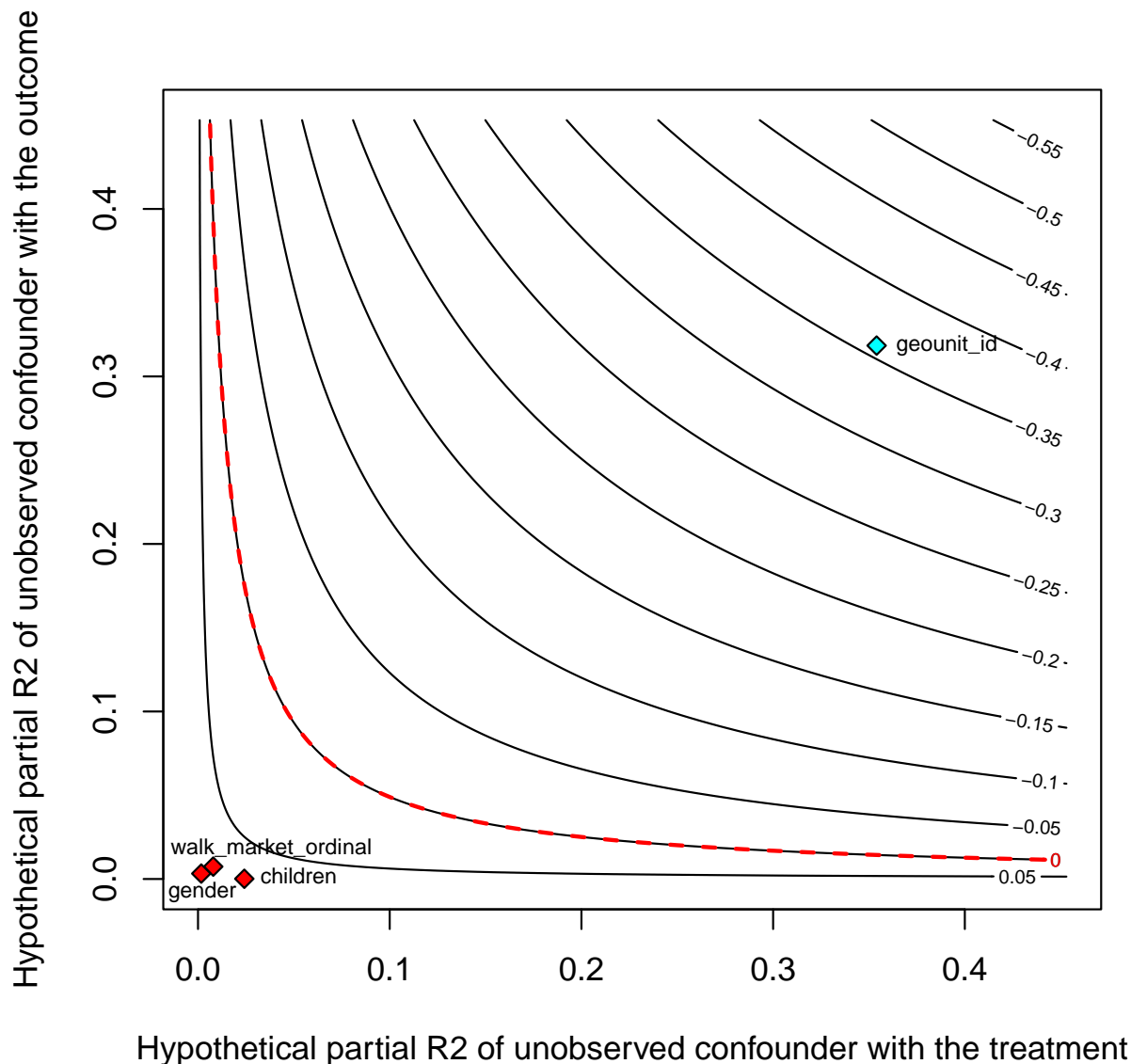
Risk of differential selection into sample

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

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

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

Figure 8: Sensitivity Analysis for Effect of Harm on Supporting “No Party”



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

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

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

(3) Another, more complicated class of cases are those in which barrel bombing *heterogeneously* or even non-monotonically affects attitudes. Specifically, the effects of losing one’s home on attitudes would have to depend upon individuals’ prior attitudes toward the opposition. Among the simplest example we can construct is a scenario that involves three assumptions: (i) First,

suppose there are two types of people, defined by their reactions to losing their homes: those who will become more pro-opposition, and those who will not change. (ii) Second, suppose that individuals whose (prior) level of pro-opposition support is high, also have a higher probability of being the first type, i.e. they react to losing their homes by becoming more pro-opposition. (iii) Third, assume that becoming even more pro-opposition increases one’s chances of joining opposition insurgents rather than becoming a refugee. The consequence of these three assumptions would be that those who had their homes destroyed *and* who were already opposition supporters (and became even more pro-opposition once bombed) will be under-represented in the sample, driving down the apparent opposition support among those *in the refugee sample* whose homes have been destroyed.

How would each of these affect our estimates? In scenario (1), if it is the pro-opposition individuals who are reluctant to leave Syria unless prompted by losing their home, this is unproblematic: the bias generated would be opposite to our estimate. However, if it is instead the relatively pro-regime individuals who are reluctant to leave, this would pose a problem, as it could be an alternative explanation for our findings on *support_party_none* and *support_party_opp*. We cannot entirely rule this out, though this scenario seems unlikely to us – relatively pro-regime individuals would not be apt to stay in the highly opposition-dominated areas our participants come from. They may leave to other areas rather than Turkey, but then their chances of coming to Turkey at some later point would not likely depend upon their house being barrel bombed.¹¹

Regarding scenarios (2) and (3) above, again we cannot entirely rule out the second or third possibilities above, but we argue they are unlikely to account for our results for several reasons. First, in both of these, the effect of the missing-opposition-supporters on the estimate would be mitigated or reversed by the effect of increased opposition support among those who were not yet at the threshold to become fighters. Second, if the most pro-opposition individuals who also had their home destroyed are simply missing from the sample, one would also expect a (false) result on *refuse_position* because many of those who would say that it is unacceptable to refuse taking a position would be missing from the treated sample. But this effect was close to zero and among our most precisely estimated at that. Third, our strongest argument against selection processes widely affecting our estimates lies in the similarity of results by gender. Figure 6) referenced above shows all the effect estimates separately for male and female respondents. The striking similarity of results for men and women is reassuring from a statistical perspective. Moreover, we conducted this analysis because if there are selection processes that drive some individuals to stay behind in Syria in support of an armed group, they surely would have a greater impact on males in our sample, as they make up a strong majority of those fighting. Nevertheless, we see very similar results for men and women, including on *support_party_none* and *support_party_opp*.

7 Conclusion

The effects of indiscriminate violence on civilians – especially in conflicts where the objective is not to control the population but to remove it – are not well understood. We have studied the effects of indiscriminate violence “when exit is an option” by conducting a survey of Syrian refugees in Turkey. Using a quasi-experimental research design, based on the indiscriminate use of barrel bombs, our goal is to understand how losses shape attitudes.

Among a captive population, when faced with violence by one party (the regime, here), casting one’s political (or military) support towards the opposing party sounds plausible. One might suspect that even where populations can escape instead of take sides, a reactionary and/or anger-driven side-taking may occur nevertheless. However, instead, we find that when civilians can escape, there is no need to participate in such a forced choice: anti-regime need not be pro-opposition, pro-ISIS, or anything else. We do not even find significant evidence that suffering this

¹¹Furthermore, the negative relationship we see between losing one’s home and *help_regime_member* would be surprising if this scenario is driving our effect estimate. At a minimum, the effect estimate on *help_regime_member* we have would be a conservative one if this selection process is occurring to any degree.

type of violence causes victims to view those who opt to remain neutral less favorably. While those whose homes are destroyed in regime-inflicted barrel bombing are more likely to see the regime as the greatest threat, both to the country and to their own security, we argue that for civilians who can remove themselves from the conflict zone, the usual logic that forces communities to politically align with one armed group or another is not in operation. Rather, it is entirely possible for civilians harmed by one side to withdraw support from both sides, not negatively judge those who remain neutral, and instead engage in civic activities that benefit the civilian refugee community.

Despite rich literatures on both civil war violence and refugee studies, disciplinary boundaries have hindered the development of research agendas that examine attitudes in cases where civil war violence and mass migration intersect. With global refugee flows now at their peak, and much of this movement driven by civil conflict and failing states, future research should devote closer attention to how porous borders and the option of exit shape both the strategic logic of conflict and the attitudes of civilians displaced by violence.

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A Appendix

A.1 Barrel Bombing Harms other than House Destroyed

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

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

Third, *bbomb_neighborhood* cannot be used as a “treatment” in our main analysis, as we work only with neighborhoods that have been barrel bombed. Relatedly, we collected the variable *present_bbomb* for purposes of potentially identifying the individuals who were present during barrel bombing and thus stood equal risk of harms such as injury. We later came to realize this is an unwise strategy.¹² We also do not use *present_bbomb* as a “treatment” harm because it allows self-selection, with some taking greater risks and choosing to remain present while others do not.

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

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

The *bbomb_house_destroyed* variable sidesteps this problem, because a person’s house stays whether the person leaves or not. Within neighborhoods that are attacked, we argue, which houses are destroyed is effectively random. This holds whether the person is more risk-avoidant (and left already) or more risk-tolerant (and stayed longer).¹³ Given the various identification and

¹²Conditioning on *present_bbomb* would be problematic for two reasons. First, our question asked people whether they were present during *any* barrel bombing in their neighborhood, not necessarily the barrel bombing that destroyed their house. For those who did not lose their house, there is no way of asking whether they were present during the barrel bombing that “would have” destroyed their house. Second, it is potentially post-treatment. For example, many people moved multiple times during the conflict. At some point, those who did not lose their home may move back, and be present for barrel bombing. Others may have lost their home while away, discouraging them from ever moving back and thus being present during barrel bombing there.

¹³This does imply that we assume people know whether their house was destroyed or not, through their contacts, even if it was destroyed after leaving. Because there is generally considerable communication with those who stayed behind,

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

we find this assumption plausible

Figure 9

