

Humanitarian Aid and Civil War

Quasi-Experimental Evidence that Aid to Syrian Refugees in Lebanon Did Not
Impact Insurgent Mobilization *

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February 25, 2018

Word Count: Approximately 12,500 words

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Contents

1	Introduction	1
2	Humanitarian Aid, Refugees, and Conflict	6
2.1	The Correlates of Conflict in Refugee Crises	7
3	The Syrian Refugee Crisis in Lebanon	11
4	Experimental Design	15
4.1	Sharp Regression-Discontinuity Design	18
4.2	Data Collection	19
4.3	Study Population	20
5	Sample Descriptives	22
6	Internal Validity	27
6.1	Randomization Checks	29
6.2	Manipulation of the Forcing Variable	32
6.3	Measuring Insurgent Mobilization	36
6.4	Statistical Power and Minimum Detectable Effect Sizes	40
7	Results	42
7.1	Refugees and Mobilization	42
7.2	Aid and Mobilization	45
8	Conclusion	53

Abstract

Understanding when refugee crises and humanitarian aid affect political violence is critical for both theory and policy regarding the world's many displacement crises. Based on original survey data of 1,358 Syrian households in Lebanon, we examine whether refugees are prime candidates for recruitment into insurgent groups and whether humanitarian aid to refugees impacts their choice to join insurgent groups. Our research leverages as-if-random assignment to a UN cash transfer program for Syrian refugees with an altitude-based regression-discontinuity design. Our results provide evidence that insurgent mobilization among Syrian refugees in Lebanon is low at baseline – the first empirical estimates of the magnitude of the rate of Syrian refugees returning home to fight. And despite an intervention that increased household income by 66%, we find little evidence that the aid program had a large effect on insurgent mobilization. If anything, our estimates point into the direction of a small decrease in mobilization, likely because aid increases the opportunity cost of participating in insurgency. Our quasi-experimental results stand in contrast to the dominant findings from the largely observational published literature arguing that refugees are prime candidates to join insurgencies, aid delivered in the context of civil war worsens conflict, and humanitarian aid to refugees worsens conflict.

Refugees in particular exit the state because of a direct experience of persecution or political violence and therefore have strong reasons to oppose the regime from which they have fled. Although refugees are of course victims of violence, they are also prime candidates for recruitment involvement in rebel factions. (Salehyan, 2009, pp. 40)

1 Introduction

Are refugees prime candidates for recruitment into insurgencies? Does humanitarian aid delivered to refugees fleeing civil war have the perverse effect of fueling insurgencies? In this article, we examine the Syrian refugee crisis in Lebanon to test predictions from existing theory about refugees, aid, and conflict, leveraging as-if random assignment to a humanitarian aid program and an original dataset that records the migration choices of 1,358 Syrian refugees in Lebanon. In spite of a large literature addressing these questions, this is the first study to offer quasi-experimental evidence for when and why refugees crises and humanitarian aid worsen civil war.

When and why refugees join insurgencies is not simply a question of academic concern. Over the past one hundred years, two world wars, numerous civil wars, ethnic-cleansing, and genocidal violence have forced millions of people to leave their homes and communities in order to escape violence. At the end of 2016, the number of

refugees worldwide totaled 22.5 million. If we include those people forcibly displaced within the borders of their home country, the number of displaced people worldwide rises to 65.6 million. Understanding when refugees fight in insurgencies can shed light on the origins and evolution of conflict. But it can also help in evaluating strategies for responding to refugee crises, a central question of the international community. The United Nations (UN) spends billions of dollars every year on refugees and the trend is increasing. Whether refugees are at significant risk of joining armed groups, and if humanitarian aid will make conflict worse, has important implications for how policy makers should design responses to humanitarian crises.

This article's key contributions are providing empirical evidence based on original survey data and a natural experiment to establish specific causal facts for a well-defined subpopulation, studying whether Syrian refugees returned home to join the insurgency in Syria. This evidence addresses the related questions of whether refugees are prime candidates for recruitment into insurgent groups and whether humanitarian aid to refugees impacts their choice to join insurgent groups. The majority of published literature predicts that refugees exacerbate conflict (Zolberg, Suhrke and Aguayo, 1992; Salehyan and Gleditsch, 2006; Lischer, 2006; Muggah, 2006; Salehyan, 2009), aid to refugees exacerbates conflict (Stedman and Tanner (2004); Salehyan (2009); Choi and Salehyan (2013), and aid during civil war also

exacerbates conflict (Nunn and Qian, 2014; Crost, Felter and Johnston, 2014; Wood and Sullivan, 2015).

Although existing research on refugees and conflict is theoretically rich, offering numerous testable predictions, many of the research designs are ill-suited to provide dispositive conclusions. Much theory building on refugees and conflict is based on case studies of a handful of exceptional and extreme instances of refugee mobilization and may exaggerate the risk of refugee recruitment and militarization (e.g., Lischer, 2006; Muggah, 2006). Leenders (2009) and Onoma (2013) offer similar critiques of the case study literature on so-called refugee warriors, and Leenders (2009) calls such work “a lucid example of conceptual overstretch” (p. 354). Existing theory testing about refugees and conflict is often based on observational analysis of cross-national data on refugees and conflict, and consistently finds a positive correlation between refugees and conflict (e.g., Gleditsch, 2007; Checkel, 2013). This correlational relationship is not surprising as a majority of refugees flee their home countries because of conflict, and despite a battery of control variables, the specter of endogeneity – that is, either conflict causing refugees through reverse causation, or omitted variables causing both conflict and refugees – remains more plausible than the assertion that refugees cause conflict.

In contrast to work on refugees and conflict, the work on aid and conflict (without

focusing specifically on refugees) is often well identified (e.g., Nunn and Qian, 2014; Crost, Felter and Johnston, 2014; Wood and Sullivan, 2015). However, the body of evidence is incomplete as null findings are generally not published and subsequently integrated into theory-building.¹ Furthermore, existing work on aid and conflict does not explore the question of whether aid in refugee crises is at particular risk of worsening conflict, as Choi and Salehyan (2013) argue.

Not only are the predictions about aid to refugees grim in general, they are particularly dire in the case of the Syrian refugee crisis in Lebanon, which exhibits a vast majority of the risk factors for violence in humanitarian and refugee crises presented by published literature. In Lebanon we find many of the country's political and paramilitary groups directly or indirectly involved in Syria's war, often falling along pre-existing politicized ethnic divisions; resentment among the host population about the refugee population; a central state with little capacity to limit mobilization; and armed groups that regularly crossed porous borders between Lebanon and Syria, operated near refugee populations, and could have captured humanitarian aid and

¹More broadly, credible and replicable science requires the publication of important null findings from well-designed research. We know that the published record of political science research is fundamentally biased toward positive findings and many published results are false positives (for example, Bem and Honorton (1994); Ioannidis (2005); Gerber and Malhotra (2008); Simmons, Nelson and Simonsohn (2011); Pashler and Wagenmakers (2012); Gelman and Loken (2013); Monogan (2013); Maniadis, Tufano and List (2014); Franco, Malhotra and Simonovits (2014); Nyhan (2015); Reinhart (2015); Esarey and Wu (2016)).

recruited refugees.

We study a United Nations unconditional cash transfer program for Syrian households in Lebanon, implemented in 2013 and 2014, coinciding with the period of the highest levels of violence in Syria and highest levels of refugee outflows from Syria into Lebanon. Our research leverages original survey data of 1,358 Syrian households in Lebanon and as-if-random assignment of cash transfers to Syrian refugees with an altitude-based regression-discontinuity design. Our research design offers many advantages for inference about the micro-level link between humanitarian aid and conflict. To study whether Syrian refugees are returning home to fight in the insurgency, we offer novel data with a high level of demographic disaggregation on whether Syrian refugees return to Syria. Next, we study whether an exogenous income shock mobilizes refugees to return to their home country to fight. Unlike many articles on the topic that use aggregate national-level data to study the correlates of conflict, our research offers a household-level intervention and household-level outcome measurement. Furthermore, our sample comprises Syrian households that the UN classified as poor, which allows us to study a subpopulation with few economic opportunities, which existing research would predict to be at high risk of insurgent mobilization (Humphreys and Weinstein, 2008).

In what follows, we summarize the literature on so-called ‘refugee warriors’ (and

related work), which contends that refugees are prime candidates to join insurgencies and that aid to refugees has the perverse effect of fueling insurgencies, linking existing theory to the context of the Syrian refugee crisis in Lebanon. We also discuss existing literature that rebuts the refugee warriors literature, although only a handful of studies address when and why refugees do *not* mobilize. In this article, we provide alternative empirical findings about refugee mobilization and the effect of aid on refugee mobilization into insurgent groups. We do not find strong evidence that refugee mobilization occurs at any meaningful rate in the Syrian refugee crisis, or that aid increased the rate of refugee mobilization. Almost all effect estimates are close to zero, and the few effect estimates that are not small point in the direction of a decrease in mobilization, which aligns with predictions that aid increases the opportunity cost of participating in insurgency.

2 Humanitarian Aid, Refugees, and Conflict

A broad literature has found that numerous factors make the provision of humanitarian aid to refugees fraught: Refugee camps, lootable aid, a weak host state, pre-existing ethnic rivalries that map onto the cleavages driving the civil war in the country of origin, transnational insurgent groups operating among refugee populations and camps, and negative attitudes among refugees toward their home gov-

ernments. All of these factors are theorized to increase the risks that refugees will join insurgent groups, aid delivered in the context of civil war will worsen conflict, and humanitarian aid to refugees will worsen conflict. These arguments, however, contradict the intuitive fact that most refugees flee their home country to escape conflict, and may be unlikely to return to fight. Refugee populations generally do not incite conflict because they generally comprise civilian noncombatants. Furthermore, we might predict that aid increases the opportunity cost of fighting, thereby predicting that incentives to rebel grow smaller as household income and economic opportunities from non-rebel activities rise.

2.1 The Correlates of Conflict in Refugee Crises

A wide range of characteristics related to conflict and state weakness are theorized to increase the risk that refugee crises and humanitarian aid exacerbate conflict. The majority of existing studies on refugees, aid, and conflict make ominous predictions: refugees, aid to refugees, and aid during civil war all exacerbate conflict. Although effect sizes are generally not quantified, and no clear set of necessary or sufficient conditions is offered, the literature often makes strong statements about the risks of refugee mobilization and aid to refugees fueling insurgencies.

Much existing work argues that refugees are particularly likely to join insurgencies. Salehyan (2009) writes that “[r]efugees in particular exit the state because of a

direct experience of persecution or political violence and therefore have strong reasons to oppose the regime from which they have fled [...and are] prime candidates for recruitment involvement in rebel factions” (p.40). Zolberg, Suhrke and Aguayo (1992) caution that refugees are not only victims escaping persecution but also political activists who mobilize while in the host country. Salehyan (2007) argues that if rebel groups can use international sanctuaries, it lowers their costs of fighting and they gain bargaining leverage. Lischer (2006) writes that refugee flows facilitate the spread of civil war because they facilitate the transnational spread of arms, combatants, and ideologies conducive to conflict.

Refugee camps are often cited as a leading risk factor. Salehyan and Gleditsch (2006, p. 324) write that refugee camps “often provide sanctuary to rebel organizations, a base of operations, and fertile recruitment grounds.” Camps may facilitate the capture of humanitarian aid by fighters, or the fighters themselves may receive aid by blending into refugee populations. Zolberg, Suhrke and Aguayo (1992) describe refugee camps as potential military bases for ‘refugee-warriors’ to continue opposition activities.

Weak states hosting refugees may be incapable of carrying out effective actions to prevent the mobilization of refugees (Lischer, 2002, 2006; Salehyan, 2007). Loescher (1992) argues that a precarious ethnic balance and pre-existing ethnic rivalries in the

host country increase the risk of mobilization. Whitaker (2003) argues that refugee movements are more likely to produce conflict when ethnic difference is politicized in the host country.

Choi and Salehyan (2013) argue that aid will increase mobilization when it provides armed groups with “opportunities for looting and theft” (p. 57), and that groups are better able to recruit refugees into armed groups when they possess more resources. Refugees may be particularly likely to be drawn to fighting by the dire circumstances of oppression, poverty, and abuse, and as armed groups capture more aid, the expected returns for refugees from joining an insurgent group increase.

The predictions about so-called refugee warriors contradict two intuitive facts about refugees and aid. First, most refugees flee their home country to escape conflict, and may be unlikely to return to fight, suggesting a preference for living as a refugee, rather than fighting as an insurgent. A sparse but growing body of work argues that refugee populations generally do not incite conflict because they generally comprise civilian noncombatants (see, for example, Whitaker (2003); Leenders (2009); Onoma (2013); Shaver and Zhou (2017)).

Second, humanitarian aid to refugees increases the opportunity cost of fighting, and economic theories of crime (Becker, 1968; Ehrlich, 1973) and insurrection (Grossman, 1991; Collier and Hoeffler, 1998) predict that incentives to rebel grow smaller

as household income and economic opportunities from non-rebel activities rise. The theories imply that an increase in the income of the population raises the opportunity cost of participating in conflict, and thus, all else equal, will reduce insurgent mobilization. Despite the widespread use of the opportunity cost framework to consider the economic incentives of participation in insurgencies (Collier and Hoeffler, 1998; Elbadawi and Sambanis, 2002; Fearon and Laitin, 2007; Humphreys and Weinstein, 2008; Chassang and Padro-i Miquel, 2009; Beath, Christia and Enikolopov, 2012), scholars of refugees and conflict rarely use employ the framework in developing predictions about whether aid in refugee crises will exacerbate conflict.

Overall, we find divergent predictions in theoretical and empirical work that focuses specifically on refugees, and in work studying conflict generally. The existing literature on refugees and conflict predicts that refugee crises in the context of a weak state, ethnic divisions, and porous borders, would pose a significant risk of refugee mobilization and of aid to refugees exacerbating mobilization. In contrast, economic theory suggests that few refugees would return home to fight and that aid to refugees would reduce mobilization. To explore these contradictory predictions, in the next section we examine the Syrian refugee crisis in Lebanon through the lens of existing work on aid and refugees. In the subsequent section, we then test the predictions using novel survey data and a natural experiment.

3 The Syrian Refugee Crisis in Lebanon

Alarmingly, the Syrian refugee crisis in Lebanon manifests nearly all the risk factors forwarded by existing work on refugees and conflict. Given the apparent risk of refugee mobilization and aid exacerbating conflict the crisis appears to be what the case-study literature would call a ‘most-likely case.’ Approximately 15%-20% of Syrians in Lebanon live in thousands of camps across the country’s east, near the Syrian border.² Syrian insurgent groups could have found a rearguard for continuing the war in Syria in Lebanon’s Syrian refugee camps and areas of the country with dense refugee populations. Syrian and Lebanese armed groups could have used the areas as recruiting sites for fighting in Syria or for bolstering their strength in Lebanon.

Many Syrian refugees hold negative views about the Assad regime. As Corstange (2018) reports, the majority of Syrian refugees (53%) support a faction of the opposition compared to 39% who sympathize with the government. Furthermore, the government draws its popular support from a base of wealthier Syrians, meaning that the population eligible for humanitarian aid will likely exhibit an even higher

² The UN has not established official refugee camps in Lebanon. People conversant in NGO/UN legalese may be familiar with the term ‘*informal settlement*’ (*IS*) used to describe refugee camps in Lebanon. The term is meant to emphasize the fact that the camps are not run by the UN Refugee Agency. I maintain that the difference is more bureaucratic than useful, and I deliberately use the term ‘camp’ rather than *IS*. There is no reason that the existing literature would predict that the camp being formally run by the UN would be a necessary condition for mobilization.

level of opposition to the government than presented in Corstange's data.

A tremendous amount of humanitarian aid has flowed into Lebanon, which could have been captured in ways that support armed groups. According to the UN Office for the Coordination of Humanitarian Affairs (OCHA), \$6.6 billion in international humanitarian funding has gone to Lebanon as a result of the Syrian refugee crisis.³ Armed groups could have captured aid supplies and used them to support recruitment activities. Syrians in Lebanon have no legal rights, and the legal protections that refugees are supposed to be granted under international law do not apply because Lebanon is not a signatory to international treaties guaranteeing the rights of refugees.

Lebanon is often labeled a 'weak state,' and has a weak central government with limited capacity to police refugee populations. After 15 years of civil war from 1975 to 1990, Lebanon's central government never established strong control of all its territory. Today the central government fails to deliver basic public services like water, electricity, and waste collection to most of the country. Large parts of the country's south and east are controlled by the non-state political and military group Hezbollah. Other parts of the country's northeast lie outside of effective state control, where the Lebanese police and army rarely enter.

³United Nations Office for the Coordination of Humanitarian Affairs. <https://fts.unocha.org/countries/124/summary/2017>. Accessed December 18, 2017.

During our study period of 2013-14, Syrian armed groups operated in Lebanon, traversing the porous border between the two countries. The groups moved people and supplies across the borders, and if refugees had wanted to join a group, they could have done so in Lebanon, and then traveled into Syria.

Armed groups fighting in Syria's war had members living among or near refugee populations and controlled parts of Lebanese territory in the mountainous border region with little state presence.⁴ Throughout 2013 and 2014 fears among Lebanese policy makers rose sharply that insurgents had 'sleeper cells' among the refugee population (Dionigi, 2016, p. 15). ISIS and Jubhat al-Nusra had a presence in Syria in the border region adjacent to the central Biqa'a valley, along Lebanon's eastern border with Syria, and clashed with the Lebanese army and Hizbullah throughout 2014.⁵ Lebanon's mountainous border areas offered a natural pathway for transnational insurgent groups between contested areas of Syria and Lebanon.

In Lebanon, many things move across the borders, including people, materiel, and medical supplies. Unlike the Jordanian government, the Lebanese government did not have the capacity to effectively close its borders with Syria. In addition to

⁴ Declan Walsh. "Hezbollah and Syrian Army Attack Islamists on Lebanon Border." *New York Times*. <https://www.nytimes.com/2017/07/21/world/middleeast/hezbollah-syrian-army-lebanon.html>. Accessed February 25, 2018.

⁵ "Lebanon World Report 2015: Lebanon. Events of 2014." Human Rights Watch. <https://www.hrw.org/world-report/2015/country-chapters/lebanon>. Accessed February 25, 2018.

the challenges of policing a long and mountainous frontier, many border towns have close ties with villages just over the border in Syria, and long histories of unfettered cross-border travel. In April 2015, a member of Lebanon’s Border Control Committee stated that effective Lebanese border management was impossible while the Syrian war continued.⁶ Negotiations between the governments of Syria and Lebanon to further demarcate their porous shared border had already reached an impasse years before the war began. Although official numbers are not available, these two official crossings probably only account for a minority of human movement between the two countries. In October 2014, an article in the Lebanese Arabic-language newspaper, *As-Safir*, argued that the “and” in “Syria and Lebanon” had disappeared.⁷

The cleavages of Syria’s war also drive conflict in Lebanon, where the divide between the country’s two political factions flows from the country’s civil war and the question of Syria’s influence in Lebanon. The March 14 political coalition formed in opposition to the Syrian occupation of Lebanon (1976-2005) and today is largely bound together by its anti-Syrian regime stance. Lebanon’s pro-Syrian regime coalition, March 8, includes Hezbollah as its most powerful member. Hezbollah is actively fighting on the government’s side in Syria’s civil war, and the organization

⁶ The Daily Star, April 29, 2015. <http://www.dailystar.com.lb/News/Lebanon-News/2015/Apr-29/296141-effective-border-management-a-pipedream.ashx>

⁷ www.assafir.com/Article/1/377949, quoting the mid-century Lebanese writer Sayyid Taqi al-Din, the “and” in Syria and Lebanon has become an “unfaithful and,” *waw kāfir*.

receives financial, political, and military assistance from Iran, the Syrian government's foremost patron in the ongoing civil war. The divide between the political and ethno-sectarian groups regularly causes violent conflict in Lebanon. Rafic Hariri, the leader of the anti-Syrian-regime political bloc, was assassinated in 2005, and all indicted parties in the subsequent prosecution are Hezbollah members. His son, Sa'ad Hariri currently leads the anti-Syrian bloc and reported to the media in October 2017 that his life was in danger, presumably threatened by Iran or Hezbollah, before he took temporary shelter in Saudi Arabia. In May 2008, the government of Lebanon attempted to disable Hezbollah's fiberoptic telecommunications network, which provoked three weeks of violence between the March 14 and March 8 camps and left dozens of civilians and soldiers dead. Given the clear divisions in Lebanon, and their close relation to Syria's civil war, the country's ethno-sectarian groups could have recruited and mobilized refugees to form fighting forces.

4 Experimental Design

UNHCR and partners ran an unconditional cash transfer (UCT) program for Syrian refugees in Lebanon giving \$575 (\$993 in PPP terms) over six months via ATM cards to 87,700 families.⁸ \$95 per month is a significant amount for the beneficiary

⁸'Unconditional' refers to requirements on the beneficiaries' actions, like children's school attendance or regular medical checkups. 'Unconditional' does not mean that selection is not conditional

population, equal to about two-thirds of control-group household monthly income (\$149) and about one third of the value of control-group household food consumption, which is largely supplied by humanitarian aid.

Our identification strategy relies on the fact that due to funding restrictions and a desire to target those living in colder climates, the UN delivered cash transfers to refugee households living above 500 meters altitude. In addition to the altitude criterion, the UN used demographic criteria to target ‘poor’ Syrian households, creating a weighted average of the number of men, women, children, elderly, and handicapped in the house, and a cutoff below which a family was classified as ‘poor.’ This variable cannot be used for a regression discontinuity because the outcome is effectively categorical rather than continuous due to the values of the weights that UNHCR used. 57% of Syrian refugee households in Lebanon qualified as poor, suggesting that the refugees in the study are not exceptional in a way that compromises the evidentiary value of the sample.

Beneficiaries were notified by text message whenever the UN transferred funds to the ATM card. ATM cards only operated within Lebanon and money could be withdrawn at any ATM in the country. The first aid was distributed in November 2013. Households received an ATM card with 220,000 Lebanese Pounds (\$147) pre-

on eligibility criteria, like poverty or altitude.

loaded. For the following four distributions, from December 2013 to March 2014, eligible households received a monthly transfer of 160,000 Lebanese Pounds (\$107). The total amount of assistance received between November 2013 and March 2014 was \$575 (\$993 in PPP terms). Since the program was only intended for the winter it ran from November 2013 to March 2014. The value of assistance was calculated to cover the costs of heating fuel. The November payment was higher than later payments to allow beneficiaries to buy a stove in addition to heating fuel. The UN did not impose any restrictions or conditions on beneficiary behavior or spending. A detailed schematic of the program is presented in the appendix.

Refugees received no information about the aid program in advance and no information about targeting criteria. The first notification that beneficiary families received about the program was sent via text message and read, in translation, *Receive financial assistance/vouchers from the village printing shop on Sunday, December 8 at 8:30am. Bring the family (UNCHR) file and identification.* (The pick-up location varied by town.) The head of the household would pick up the card and receive a PIN. After this point, they received monthly text messages when new money was transferred to the account, and anyone could withdraw the money with the ATM card and the corresponding PIN.

4.1 Sharp Regression-Discontinuity Design

We use the 500-meter altitude eligibility cut-off to estimate a (sharp) RD, using household- and community-level data we collected for all refugees classified as ‘poor’ and residing between 450 and 550 meters altitude. We chose this particular bandwidth based on available funding for the survey and bandwidths chosen by the existing literature; +/- 50 meters — that is, +/- 10% of the 500-meter altitude cut-point — is similar to bandwidths used in the existing literature (for example, Ludwig and Miller (2007)). Our bandwidth is small relative to the entire range of the forcing variable, which ranges from sea level to 2,209 meters altitude, the highest altitude at which a registered Syrian refugee lives.

Following Hahn, Todd and der Klaauw (2001) and Imbens and Lemieux (2008), our estimation approach is local regression around the 500-meter altitude eligibility cut-off.

$$Y_i = \alpha + \beta \mathbb{1}(A_i \geq 500) + f(A_i) + \eta X_i + \epsilon_i, \quad \forall 450 \leq A_i \leq 550 \quad (1)$$

where A_i is the altitude of household i at the time of treatment assignment in October 2013, before the program began in November. α is a constant, β measures the causal effect of the intervention on the outcome Y_i at $A_i = 500$, $\mathbb{1}$ is the indicator function, X_i is a vector of covariates (and η is the corresponding vector of coefficient estimates), and ϵ_i is a mean-zero disturbance term. If $(A_i \geq 500)$, the unit received

the treatment and otherwise it did not. We refer to households that lived between 450 and 499 meters altitude at the time of treatment assignment as our control group, and households that lived between 500 and 550 meters altitude as the treatment group. The term $f(A_i)$ is a polynomial function of A_i . Following the suggestion of Lee and Lemieux (2010), we use linear and quadratic functional forms for $f(A_i)$, allowing for different slopes of the regression function on both sides of the cut-off:

$$f(A_i) = \gamma_1(A_i - 500) + \gamma_2\mathbf{1}(A_i \geq 500) \times (A_i - 500) \quad (1a)$$

$$f(A_i) = \theta_1(A_i - 500) + \theta_2\mathbf{1}(A_i \geq 500) \times (A_i - 500) + \theta_3(A_i - 500)^2 + \theta_4\mathbf{1}(A_i \geq 500) \times (A_i - 500)^2 \quad (1b)$$

In the main paper we present results for model 1a, and in the online appendix we present results for model 1b and results without covariates, showing that neither changes the article’s overall conclusions.

4.2 Data Collection

Our population comprises all 1,851 refugee households that, at the time of treatment assignment, resided between 450 and 550 meters altitude and qualified as ‘poor’ (1,000 households between 450 and 499 meters, and 851 between 500 and 550 meters). We surveyed households immediately after the program ended and reached 1,358 of 1,851 households. The main reasons for attrition were that contact information

from the UN was incorrect, people refused to be interviewed, or families had moved back to Syria. Attrition was balanced across treatment and control groups (74.1 vs. 72.7%, Pearson’s chi-squared test p value: 0.52).⁹ To facilitate measurement symmetry between treatment and control groups, we used the same survey technique, instrument, and enumerators to collect data for both treatment and control units, and we collected data for both groups at the same time and under similar conditions. We have UN household baseline data, which we use to conduct randomization checks and to construct estimates of household demographic changes. A full range of descriptive statistics about the population is discussed in the appendix.¹⁰

4.3 Study Population

Our control group consists of Syrian households just below 500 meters altitude and the treatment group consists of households just above 500 meters altitude, all of which the UN classified as ‘vulnerable.’ Eligibility was determined by geographic criteria, to target refugees living at high altitudes exposed to cold weather, as well as demographic criteria, to target poor and vulnerable refugees. The UN determined

⁹ Enumerators informally collected information on reasons for attrition from neighbors and shopkeepers. Beyond demonstrating that attrition rates were balanced across groups, we cannot test for balance in the reasons for attrition.

¹⁰ The survey questions analyzed in the article are in the appendix. The complete survey questionnaire is available at <http://tinyurl.com/pvyub87>.

vulnerability according to a weighted mean of household demographic variables.¹¹ In addition to the vulnerability criteria, only households residing at or above 500 meters altitude were eligible for cash assistance, while those living below 500 meters were not.¹²

The study population is therefore a subset of the broader registered Syrian population according to altitude (only those around 500 meters altitude) and vulnerability (only vulnerable households). 57% of Syrian refugee households in Lebanon qualified as vulnerable, suggesting that the refugees in the study are not exceptional in a way that compromises the evidentiary value of the sample. If there were something about the households the UN classified as poor that keeps them from going across the border to fight we might find no effect among this sub-population, when there could in fact be an effect in the broader population of Syrian refugees. We believe that

¹¹ The demographic criteria calculated a vulnerability score based on a weighted sum of the number of: children ages 0-2, children ages 3-4, children ages 5-12, children ages 13-15, children ages 16-18, able-bodied adult males 18-59, disabled individuals in household, adults 51-61, adult dependents 61-70, adult dependents 71+, adult females 18-22, adult males 18-22, and children at risk of not attending school.

¹² In addition to all poor households living at or above 500 meters altitude, Syrians living in so-called *informal tended settlements* (ITSs) and *informal settlements* (ISs) also received the cash transfer program regardless of altitude. The vast majority of ISs are in the Bīqā'a around 1,000 meters altitudes or at sea level in the north, especially around Tripoli and in Akkar. Only 16 households in our full sample were in ISs at the time of treatment assignment. UNHCR was able to tell us *how many* households in our full sample were in an IS, but not *which ones*, so we cannot state how many of these 16 should, according to the RD, be in the treatment or control group.

this is unlikely given the economic geography of the conflict. Many fighters are from poorer and lower middle-class backgrounds. Early opposition activity, both peaceful and violent, drew people largely from drought-stricken areas of the countryside, less well-to-do cities, urban peripheries, and poorer parts of wealthy cities. If anything, poor and lower-middle-class Syrian households may have a higher baseline prevalence of involvement in the insurgency than the general population of Syrian refugees.

5 Sample Descriptives

Figure 1 shows the distribution of refugees in Lebanon across altitudes. The upper graph plots a histogram of all 2,736 towns and villages in Lebanon. The middle graph shows the altitude distribution of all the 158,129 refugee households who registered with the UN between March 2011 and October 2013. Of those, 89,597 households were classified as ‘poor’ by the UN Refugee Agency, and the lower graph plots the altitude distribution of these poor refugee households. The figure reveals two clusters where refugees mainly settled: first, in Lebanon’s western, coastal, metropolitan area (at sea level); and second, in the eastern mountainous area around 1000 meters altitude, close to the border with Syria. The remainder of the refugees are spread out between sea level and the mountains. When comparing refugees classified as poor vs. non-poor, we see that poor refugees are less likely to live in the metropolitan

area and more likely to live at higher altitudes, where the cost of living is lower.

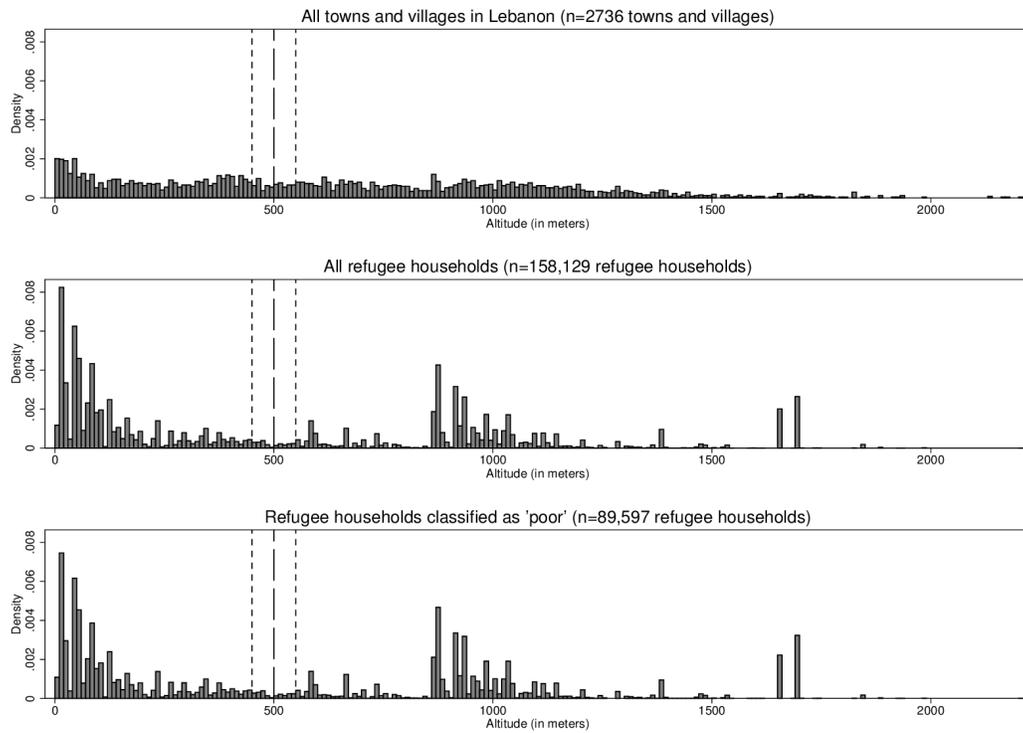


Figure 1: *Top*: Nationwide Population Density; *Middle*: Density of Registered Refugees; *Bottom*: Density of Eligible ‘Poor’ Refugee Households

The geographic distribution of Syrian households registered with UNHCR living between 450 and 550 meters covers nearly the entire country, running from the north in ‘Akkar to the south in Bint Jbeil. Figure 2 illustrates the location of all towns where survey respondents lived at the time of treatment assignment. Respondents who researchers could contact were surveyed wherever they were living at the time

of survey conduct. In November 2013, when the program began, survey respondents lived in 16 of Lebanon's 25 districts (Arabic: *aqdia*).¹³ Visual inspection of treatment and control communities suggests no systematic distribution of treated and untreated towns across the country.

Due to the nature of the research design, we only surveyed households that lived between 450- and 550-meters altitude at the time of treatment assignment in October 2013. This means that most of our respondents were not living in the border region in October 2013. It might be that we would have identified an effect if we had a research design for areas along the border.

A few facts mitigate this concern. First, Figure 2 shows where respondents lived at the time of treatment assignment. We can see that survey sites in the north and the south are close to or directly on the Syrian border. Second, nowhere in Lebanon is far from the border; there are few places in the country that are more than a 90-minute drive to a Syrian border crossing (traffic permitting). The country's average geographic width is 34.7 miles.¹⁴ At its widest point, the country is 55 miles wide from the Mediterranean Sea to the Syrian border, and the country is 20 miles wide

¹³ At the time of treatment assignment, respondents were living in the following districts: El Batroun, Chouf, Kesrwan, El Nabatieh, Marjaayoun, Aley, Akkar, El Minieh-Dennie, Sour, Jbeil, El Koura, El Meten, Bent Jbeil, Baabda, Jezzine, and Hasbaya

¹⁴ <http://countrystudies.us/lebanon/30.htm>

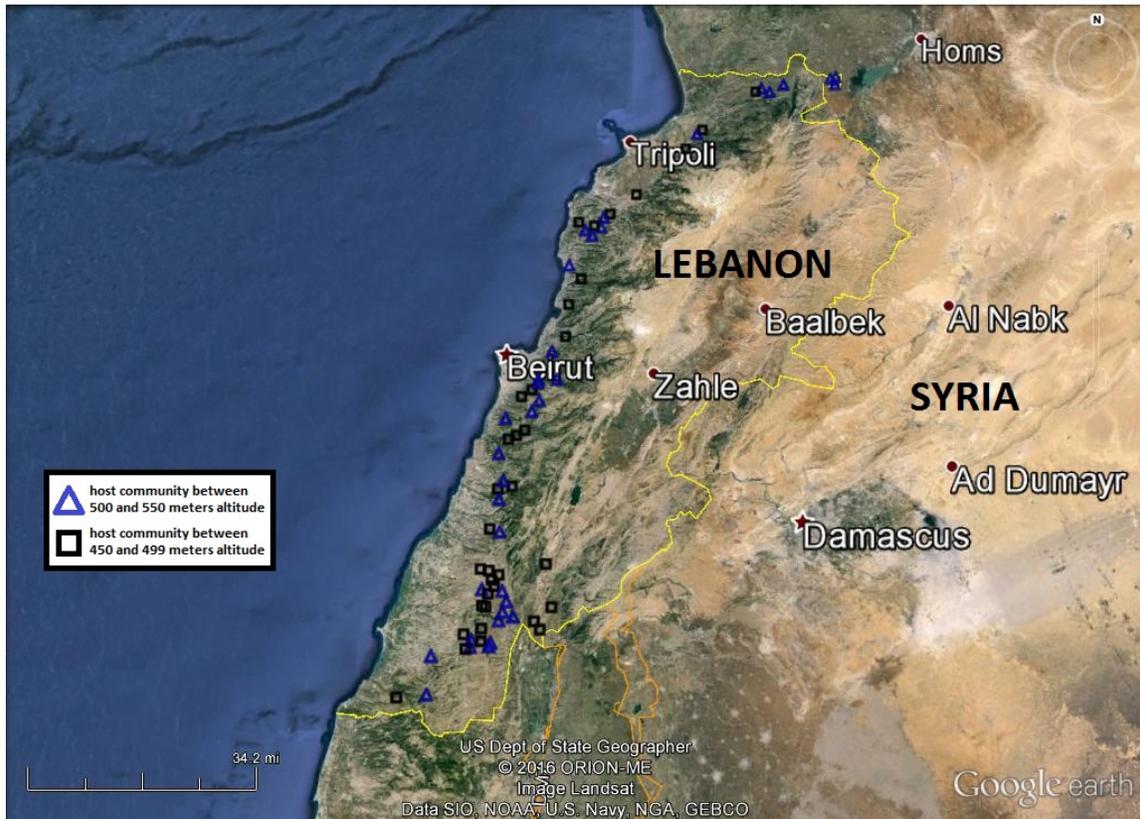


Figure 2: Locations of survey respondents at the time of treatment assignment, treatment communities in blue triangles, and control communities in black squares at its narrowest point.¹⁵

In our sample, the mean household size at baseline is 5.5 individuals, and is balanced between treatment arms (5.53 individuals among control households, and

¹⁵ In theory, we could subset our analysis to respondents who lived within some distance from the border at the time of treatment assignment. In practice, this would dramatically reduce sample size and statistical power, undermining the test's value for testing a null effect.

5.47 among treatment households). We define a household as a group of people who spend most nights under the same roof and share financial activities like income and spending. For instance, two “households” may live under the same roof if they operate independently of each other in financial matters.

29% of our respondents live in a rented house and 59% in a rented apartment. The remainder lives in tents or other improvised shelters. 53% have a fridge and 21% have a freezer. 2% have a car and 4% have a motorbike. 97% of households have a cell phone and 81% have a TV. 34% of household heads have no schooling or have not finished primary school, 30% have completed primary school, 27% have completed middle school, and 9% have completed secondary school or higher. The average age of the household head is 38, and he/she has been in Lebanon for 18 months. 99% of households reported zero savings. The average value of household food consumption is about \$300 per month (which is less than \$2 per day per capita).

Average household labor income of respondents is \$149 per month. This may understate income since some economic activities may not require the exchange of currency, such as work for housing. 77.4% of adult beneficiaries report zero working days during the past four weeks, which shows how scarce employment opportunities are for refugees in Lebanon. On average, an adult beneficiary worked 2.7 days during the past four weeks, but this is dragged down the by large number of individuals who

did not work. Among the 23% of adults who worked at least one day during the past four weeks, the average number of days worked was 11.7 days. Many households take on debt to make ends meet, with the average value of currently outstanding cash loans being about \$570 per household. The average community where our respondents live has 264 Lebanese households and 96 refugee households.

More than 85% of respondents come from five of Syria's 14 governorates. (Homs: 22.4%, Dera'a: 20.9%, Idlib: 18.6%, Aleppo: 15.9%, and Hama: 8.7%.) The remaining respondents came from Syria's other governorates, except Suweida, where no respondents came from.

6 Internal Validity

As with all regression discontinuity designs, our regression estimates reflect the local average treatment effect (LATE) at the cutoff, in our case the difference of the values of the regression functions at the cutoff for each group. This allows us to determine, all else equal, the effect of being assigned to benefit from this humanitarian aid program, by calculating the magnitude of the discontinuity at and close to the theoretical limit. The UN's altitude measure is defined at the town-level, using the highest natural (not man-made) point. Therefore, a household could be located at 400-meters altitude in a town with a hilltop above 500 meters, and receive aid.

Whereas a household located at another town's highest point at 499 meters would not receive aid. Lebanon's stark topography increases the as-if randomness of household altitude. Because altitude changes so suddenly in Lebanon, a distance of just a mile as the crow flies could mean a difference of hundreds of meters. Therefore households near the 500-meter altitude cutoff at the time of treatment assignment are essentially randomly sorted into the experimental groups. Although our estimates are only formally identified at the cut-point, there is some as-if randomness in each household's value of the forcing variable. This suggests that households around the 500-meter cut-point remain as-if randomly assigned to their experimental group for some unknown, but non-zero, bandwidth.

To violate the as-if-random nature of the decision rule, certain types of households would need to self-select into the treatment or control groups, whether deliberately or not, *before* the program began. In order to avoid the possibility of families sorting into the program, the UN determined eligibility using location and vulnerability data from October 2013, a full month before the program began, and we define our treatment and control groups using the same data the UN used. As discussed above, refugees received no information about the aid program in advance and no information about targeting criteria, alleviating the threat that Syrians self-select into the treatment or control groups.

Differential measurement of treatment and control groups is yet another threat to the internal validity of RDs, whereby the the research team can invalidate an otherwise well-identified research design. For example, if the research team scheduled interviews according to the forcing variable, or assigned enumerators according to the forcing variables, then the timing and measurement in the interviews would differ systematically between groups, and time trends could lead to mismeasurement. To avoid differential measurement between treatment and control groups, we used the same survey technique, instrument, and enumerators to collect data for both treatment and control units, and we collected data for both groups at the same time and under similar conditions (Gerber and Green, 2012, chap. 2).

6.1 Randomization Checks

RDDs rely on the assumption that whether an observation was just above or below the cutoff is not correlated with their potential outcomes. Generally, this requires assuming that whether an observation is just above the cutoff or just below was determined by chance. This implies that households just above and below the threshold should be very similar on average but for their receipt of treatment. We evaluate the claim of “as-if randomness” around the altitude threshold by testing whether groups differ on observed pretreatment characteristics.

We compare 61 pre-treatment variables with linear and quadratic specifications.

As is visible in table 1, where we present the share of pre-treatment variables imbalanced in both the linear and quadratic polynomial specifications across significance levels, imbalance is higher than what we would expect given pure chance and random assignment.

Ten of the 61 pre-treatment variables (16%) have values that are significantly different between treatment and control groups at the 0.05 level in both linear and quadratic specifications. Eight of the ten (13%) are imbalanced across both linear and quadratic specifications. In order to increase precision and potentially alleviate confounding due to this pre-treatment imbalance, we present all results both with and without covariates.

For balance variables we used a wide range of household demographic statistics, including the number of people within multiple age ranges, calculated separately sex-aggregated and sex-disaggregated, and calculated separately for both disabled and not disabled households members.

First, we see imbalance in terms of where respondents are from in Syria, but only for 2 of Syria's 14 governorates. There were fewer people from Homs in treatment and more people from Idleb in treatment. The number of respondents from each of the other 12 governorates is similar between the treatment and control groups.

The third imbalanced variable is the year of arrival, with the treatment group

having arrived a few months earlier on average than the control group. Specifically, on average the treatment group arrived between 0.19 and 0.24 of a year earlier than the control group (depending on the specification used to calculate balance). Also, more households in the treatment group arrived in December, although arrivals for the other 11 months are similar between the treatment and control groups.

Across 23 household demographic variables, we see baseline imbalance in 4. Overall family size is larger in the treatment group. When we test for balance in household demographic subgroups, we see fewer disabled men between the ages of 18 and 50, fewer not disabled people between the ages of 60 and 70, and more not disabled men between the ages of 18 and 50. All other demographic subgroups are balanced.

We take two steps to alleviate concerns about confounding due to baseline imbalance. First, since we have both baseline and endline data for the number of men, in the results section we present results for the change in the number of men over time, not the raw count. Estimating the effect of treatment on the change in the number of men avoids bias due to baseline imbalance between treatment and control groups, by adjusting for time-invariant differences between groups. Rather than mistakenly interpreting higher baseline values as an effect of treatment, we can compare groups, even when they have consistently high or low values of the outcome, by studying whether variation around the groups' mean values is correlated with treatment. Sec-

ond, in order to potentially alleviate confounding due to this pre-treatment imbalance we present all results both with and without covariates.

Table 1: Share of pre-treatment variables imbalanced by model and significance level

	Linear	Quadratic
p<0.01	0.07	0.05
p<0.05	0.16	0.16
p<0.10	0.26	0.18

6.2 Manipulation of the Forcing Variable

We find no evidence of sorting or manipulation of the forcing variable, which is consistent with reports by the UN that it did not publicly announce the program or the selection criteria. To violate the as-if random nature of the decision rule certain types of households would need to self-select into the treatment or control groups, whether deliberately or not, *before* the program began. Our definition of treatment and control groups is defined according to vulnerability criteria and altitude from October 2013, immediately before the program began. The UN determined eligibility using location and vulnerability data from a full month before the program began in an effort to avoid sorting. We use the same data that the UN used to define our treatment and control groups. Nevertheless to be cautious, our models include

controls for altitude and altitude interacted with treatment.

Beneficiaries received minimal information about the program in order to avoid the possibility of households sorting into the program. No information was delivered in advance and, during the six months of operation, beneficiaries received only short text messages with basic instructions for accessing funds. Visual inspection of the trends in population density in altitude over time in Figure 3 does not suggest sorting. If anything, figure 3 shows that town population in treatment communities was lower, although the difference is not significant.

McCrary (2008)'s density test rejects the null-hypothesis of a continuous distribution of refugees at the 500 meters altitude cut-point. We do not think this is a cause for concern since the discontinuity is very small — barely visible to the naked eye, as shown in figure 4 — and when we test for placebo discontinuities at every possible altitude, the McCrary density tests find significant discontinuities for almost all of them. Using UNHCR's full pre-intervention registration database, we test for placebo discontinuities at every altitude in Lebanon, and the McCrary density tests finds significant discontinuities in population at 97.89% of the altitudes. This likely emerges because the number of refugees is large but variation in altitude is small due to the small number of populated altitudes (that is, 691,709 refugees at only 764 unique altitudes from 3,089 possible altitudes in Lebanon, creating nu-

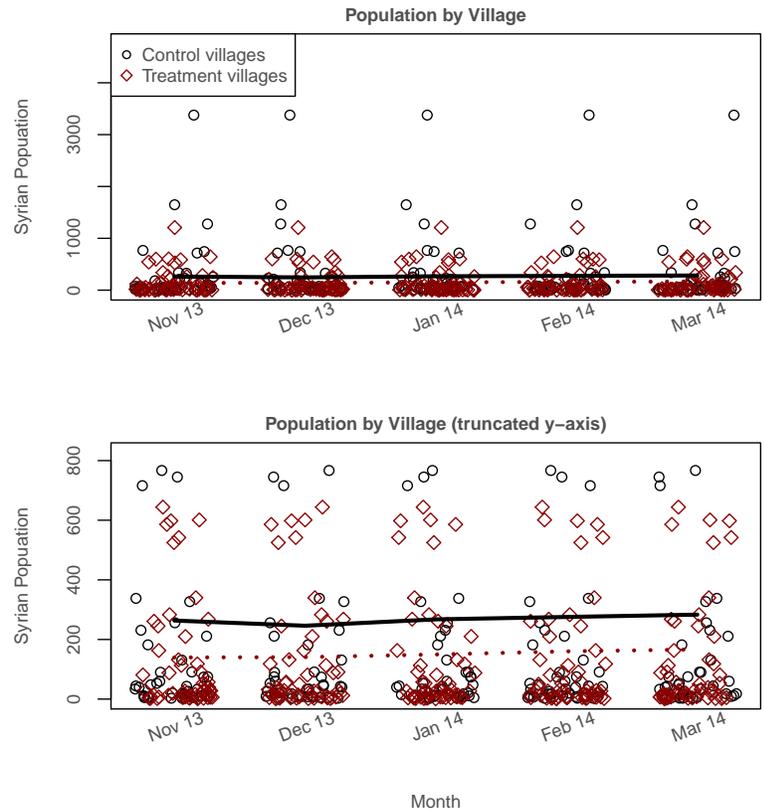
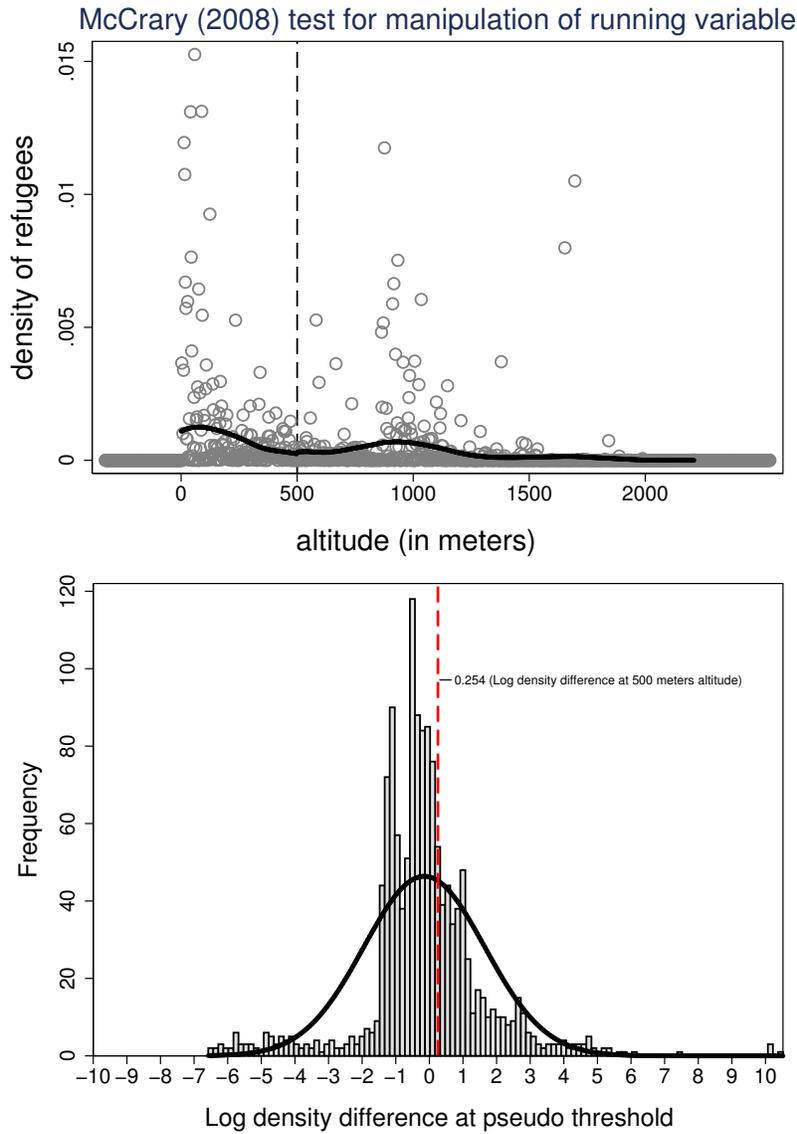


Figure 3: Trends in Village Populations, means shown with solid line (control) and dashed line (treatment), jittered along x-axis

merous discontinuities with respect to altitude). As shown in figure 1, owing to a very mountainous landscape, Lebanese towns are not smoothly distributed across altitude, with population concentrated along the coast and in the agricultural plains between the country's two mountain ranges, at around 1,000 meters altitude.



Notes: In the upper graph, each circle displays the density of refugees in that particular altitude bin. The black line is a non-parametric regression of density of refugees on altitude, separately estimated on both sides of the 500 meter altitude eligibility cut-off. The lower graph shows the result of placebo McCrary tests. We create placebo eligibility cut-offs, starting at 1 meter altitude, then 2 meters, then 3 meters, and so forth, until 2209 meters altitude, yielding a total of 2209 placebo McCrary tests. We calculate the log difference in the density of refugees at each placebo cut-off. The lower graph shows the frequency distribution of these 2209 log differences. The log difference at 500 meters altitude, that is, at the true eligibility cut-off, is 0.254, as indicated by the red dashed line.

Figure 4: McCrary Tests at Treatment Cut-off (top), Psuedo Thresholds (bottom)

6.3 Measuring Insurgent Mobilization

The challenges of survey research on sensitive topics go beyond the threat of measurement error for the sensitive question itself. In some contexts, explicitly asking about a sensitive topic may end an interview, introduce measurement error for all subsequent questions, or put the data collectors or respondents at physical risk. Existing methods for sensitive topics, like randomization techniques and list experiments, require an explicit statement of the sensitive topic, which was infeasible with our population and topic.

To measure insurgent mobilization without explicitly mentioning the topic we measure outcomes that are *consequences* of refugee mobilization. By examining a large number of these outcomes we can test for the existence of evidence of an effect of aid on insurgent mobilization and its direction, although we cannot estimate a point estimate of the magnitude of the effect. We draw conclusions from the set of metrics in aggregate. If we estimate a zero treatment effect for a large number of outcomes that mobilization affects, then the results serve as evidence that the aid intervention did not affect mobilization. Although a zero treatment effect for a single metric could suggest an absence of evidence rather than evidence of a null effect, the larger the number of metrics for which we estimate no treatment effect, the more confident we can be that we demonstrate evidence of the absence of an

effect. We measure 13 outcomes that would be affected by insurgent mobilization. Each outcome captures variation in mobilization and other activities as well, thereby providing respondents with the safety to answer honestly. Measuring consequences of our outcome of interest is similar to existing sensitive topic methodologies in that it adds noise around the true estimate of interest, creating a situation with reduced incentives for systematic misreporting, and we seek to learn something through the noise.

We measure demographic and behavioral metrics of consequences of mobilization. For example, mobilization affects the demographic profile of refugee families, and if treatment caused more men to join the insurgency, we would see fewer men of fighting age in those households compared to control-group households. We test this outcome with multiple age ranges within the range of fifteen-years old to fifty-five years old. Because some fighters in Syria send money to their families living as refugees in neighboring countries we also ask households how much money they received from people not living in the household in general and specifically from individuals living in Syria. We would expect that, all else equal, families with more members fighting would receive more transfers in general and more from Syria. We asked if anyone in the household undertook physically dangerous activities to earn money, which could include fighting, and we would expect that households with more individuals

fighting would more frequently report that a family members is engaged in physically dangerous activities. We asked if anyone in the household moved to earn money, and specifically whether anyone moved to Syria to earn money. We ask how many individuals moved and how many moved to Syria; we also ask about the age and gender of all movers. Families with more individuals who returned from Lebanon to Syria to fight would have more family members who moved and more who moved to Syria. We also ask whether someone in the family was living in parts of Syria that were under siege or where there was fighting, since if family members are actively fighting, there is a reasonable chance that they would be in an embattled or besieged area.

The relevant questions were spread throughout the survey and were always surrounded by other questions on similar topics. Questions about earnings and movement were placed in a broader section about earnings and employment. We asked demographic questions about the age and gender of all household members, instead of focusing on young adult men alone. A question about whether individuals are under siege was asked in a battery of questions about protection issues. The survey's primary purpose was an impact evaluation and the vast majority of questions were clearly intended to evaluate straightforward humanitarian concerns.

We present the results for five key variables below and for eight other outcomes

in the appendix. Our outcomes comprise non-sensitive items that would be affected by a change in insurgent mobilization. The five outcomes that we present below are the change in the number of men ages 18-50 in each household over the course of the program, whether someone in the household returned to Syria during the program, the interaction of the change in the number of men ages 18-50 and whether a household member returned to Syria during the program, whether a family member is currently living in Syria in an active war zone, defined as an area in Syria that is under siege or where there is active fighting, and the interaction of whether a family member is in an active war zone in Syria and whether a household member returned to Syria during the program.

The interacted outcomes maintain the protection of individual responses, while increasing the precision of overall measurements. The interactions focus our analysis on the substantively important variation in the consequence of the outcome. The first interaction (the interaction of the change in the number of men ages 18-50 and whether a household member returned to Syria during the program) forces a 0 value for the metric of the change in the number of men if the household had no one return to Syria. The second interaction (the interaction of whether a family member is in an active war zone in Syria and whether a household member returned to Syria during the program) forces a 0 value for whether someone returned to Syria if no one in

the family is living in a siege zone. Tests on the two interacted outcomes should be interpreted, respectively, as (i) whether there is a treatment effect on the change in the number of men who returned to Syria and (ii) whether there is a treatment effect on whether a household had someone return to Syria and has a family member living under siege.

Although estimating treatment effects for numerous outcomes and interacted outcomes would normally raise concerns about fishing or p-hacking we do not adjust p-values for multiple comparisons because we are arguing that there is *no effect of treatment* on our outcomes, meaning that the unadjusted p-values provide a *conservative* measure. Adjusting the p-values for multiple comparisons would only increase our p-values, thus bolstering our argument.

6.4 Statistical Power and Minimum Detectable Effect Sizes

Following the advice of Hoenig and Heisey (2001) we do not conduct ex-post power analysis. Although there is a large literature advocating that power calculations be made whenever one performs a statistical test of a hypothesis and obtains a statistically nonsignificant result, Hoenig and Heisey argue that we do not in fact learn anything meaningful from such tests, showing that higher observed post-experiment power does not imply stronger evidence for a null hypothesis that is not rejected. Fol-

lowing Hoenig and Heisey's recommendations, we argue that the best evidence that our statistical power was sufficient given our sample size of 1,358 survey respondents distributed among 89 villages is to examine the magnitude of the point estimates in the regressions relative to the size of the standard errors. Hoenig and Heisey call all values within the confidence interval *non-refuted values*. If the non-refuted values for the treatment effect estimate are tightly clustered around zero, then we can be confident that the true value is near zero. If the non-refuted values cover a wide range, then we cannot confidently interpret a non-statistically significant treatment effect estimate as evidence of a zero or near-zero treatment effect. Even if we interpret the confidence interval more traditionally as the range that includes the true value with some fixed level of probability, the width of the confidence interval around zero still tells us the same thing: how confident we can be that the true treatment effect is zero or near-zero. Point estimates close to zero with relatively narrow confidence intervals suggest a meaningful null finding, whereas large point estimates and broad confidence intervals that include zero do not.

7 Results

7.1 Refugees and Mobilization

Based on the dominant intuitions and predictions from the existing literature on refugees and conflict, we would expect that the Syrian refugee crisis in Lebanon, in the context of a weak state, ethnic divisions, and porous borders, would pose a significant risk of refugee mobilization. In contrast we present evidence that the share of refugees who went back to fight is probably very small.

We provide the first evidence of the magnitude of Syrian refugees who returned to Syria to fight in insurgent groups. The evidence suggests that at the time of the study, November 2013 to March 2014, a very small number of Syrian refugees were returning home to fight. To estimate the magnitude of the number of Syrians who returned to Syria to fight, we begin by estimating the total number of Syrian refugees who returned to Syria, regardless of the reason. The survey asked respondents how many members of their household returned to Syria during the study period, which allows us to estimate our sample's baseline rate of return to Syria from our control group. We then extrapolate from our control group to the broader population to obtain an estimate of the number of returnees to Syria in Lebanon's refugee population during the study period. We can multiply the average number of returnees per household in

our control group, denoted r_C , by the total number of Syrian households in Lebanon k . $r_C \times k$ denotes an estimate of the total number of Syrian refugees who returned to Syria during the study period. Next, to estimate the number of Syrian refugees in our sample who returned to Syria to join armed groups, denoted m , we offer a hypothetical value of the share of refugees who returned to Syria to join armed groups, denoted s . Our estimate of the number of Syrians who returned to Syria to join armed groups is calculated as:

$$m = r_C \times k \times s \tag{2}$$

We observe r_C from the survey data, draw k from UNHCR registration data, and input conservative (i.e., high) estimates of s . By calculating m for a range of values for s , for example ranging from 1% to 5%, we can imagine the plausible magnitude of return to join armed groups given the return rate we observe in our control group.

Of the 727 households in the control group, 49 had at least one person return to Syria during the study period, only 6.7% of households, and among those households, the average number of returnees was 1.6 ($r_C = 0.067 \times 1.6$). In October 2013, the point at which program eligibility was determined, 691,709 registered Syrians lived in Lebanon in 158,129 households ($k = 158,129$). If 6.7% of the households had an average of 1.6 people return, then 16,867 Syrians returned to Syria from November 2013 to April 2014. If we were to assume that $s = 0.01$, that is, 1% of Syrians

who returned to Syria went to join the insurgency, our best guess is that a very small number of Syrian refugees returned to fight – roughly 170 people from 691,709 refugees. Even if we were to assume that a staggering 5% of returnees went to join the insurgency, our best guess is that the Syrian refugee population in Lebanon contributed roughly 850 people from 691,709 refugees to the ranks of Syrian armed groups, at a time when there were between 75,000 and 110,000 members in insurgent groups in Syria.¹⁶

Our estimate should be understood as evidence of the *magnitude* of mobilization, and should not be taken as a *point estimate* of the number of Syrian refugees returning to fight in Syria. Understood as evidence of the magnitude of mobilization, the number is clearly substantively small. Although our sample is not representative of the Syrian refugee population in Lebanon, the broader population's behavior would need to be dramatically different from our sample's to suggest the possibility of a meaningful level of refugee mobilization among Syrian refugees in Lebanon. To imply that even one percent of refugees joined insurgent groups, one would need to argue that the rate of mobilization is approximately forty times higher than in our subsample of poor Syrians living around 500-meters altitude. Overall, the evidence

¹⁶ January 29, 2014. Congressional testimony by US Director of National Intelligence James Clapper. Transcript available via *The Washington Post*. http://wapo.st/1mFlmR?tid=ss_tw. Accessed March 17, 2017.

presented here suggests that the Syrian refugee crisis in Lebanon does not justify a claim like Salehyan (2009)'s that refugees are often "prime candidates" for recruitment rebel factions (p. 40).

Although our estimate is almost certainly biased by the fact that our sample is not representative of the broader Syrian refugee population, this is the best data available on the phenomenon since the Lebanese government does not share border-crossing data. Furthermore, the estimates may be upwardly biased because our sample is a poor subpopulation. Due to the economic geography of the conflict, poor and lower-middle-class Syrians may be more likely to fight. And they have few economic opportunities in Lebanon, which existing research would predict to be at high risk of insurgent mobilization (Humphreys and Weinstein, 2008). Therefore, the bias may run counter to our argument that refugee mobilization among Syrians in Lebanon in 2013 and 2014 was low.

7.2 Aid and Mobilization

Although published evidence suggests that aid delivered to refugees often exacerbates conflict, we find no strong evidence that this occurred in the Syrian refugee crisis in Lebanon. The majority of metrics and models in table 2 and the Online Appendix have point estimates that are close to zero and most standardized effect sizes are

small, providing little evidence that the program had a large effect on mobilization. Furthermore, when estimates are not well identified zeros, the signs of point estimates suggest a small decrease in mobilization due to aid rather than an increase. This suggests that if aid had a non-negligible impact on mobilization, it was likely a decrease, not an increase, possibly because aid increases the opportunity costs of participation in insurgency.

Table 2 presents results of the linear model with covariates (equations 1 and 1a). Figure 5 offers a visual presentation of the same results. Linear and quadratic models with and without covariates are presented in the appendix. Because the forcing variable, and thus treatment assignment, was determined at the village-level, we use Eicker-Huber-White robust standard errors, clustered at the village level according to where respondents lived at the time of treatment assignment. Results are in the article are robust to model specification and different outcome variables. The Online Appendix presents results for linear and quadratic models with and without covariates, a number of additional outcomes, including dimension-reduced outcomes calculated using principal component analysis (PCA).

In Table 2, $\hat{\beta}$ denotes the estimated treatment effect at $A_i = 500$. The control-group mean shows the regression model's prediction of the dependent variable for refugees residing at 499 meters altitude. Because the outcomes are measured in

different units, we also derive standardized effect-size estimates by dividing $\hat{\beta}$ by the control-group standard deviation, giving us the estimated treatment effect at $A_i = 500$ in terms of each outcome's standard deviation, which is analogous to Cohen's d effect size. The standardized standard error, presented in square brackets, indicates our uncertainty about the treatment-effect estimate in terms of each outcome's standard deviation. Cohen (1988) offers a rule of thumb that $d = 0.2$ can be considered a small, but not necessarily trivial, effect, and $d = 0.5$ can be considered a medium effect.

The results show a lack of strong evidence of an effect of treatment on mobilization. The evidence points toward a null effect, and if anything a negative effect of aid on mobilization. Across all the metrics, we find essentially no evidence pointing toward an increase in mobilization. The treatment effect estimates for outcomes (1) and (3) are well identified zeros – small treatment-effect estimates with narrow confidence intervals. The treatment effect estimate for outcome (2) is less precise, but also small. These three null results provide evidence that the cash program did not affect Syrians' choices to return to Syria, and did not affect the return patterns of Syrian refugee men aged 18-50.

Looking at outcomes (4) and (5), we see that estimated effect sizes are not small, but the sign of the point estimates suggests suggests that aid decreased mobi-

lization, rather than increasing it. Although we cannot confidently assert that there was no change in whether a family member is living in an active war zone or whether a household member returned to one, the findings at most suggest that humanitarian aid actually decreased the rates of these outcomes. If humanitarian aid had increased refugee mobilization, we would be unlikely to observe the estimated negative relationship between treatment and metrics of living in an active war zone. A decrease in mobilization aligns with what we would expect from the opportunity-cost literature, but contradicts what we would expect from the literature on refugees, humanitarian aid, and conflict.

Looking at the full set of 40 regressions presented in the main body of the article and the Online Appendix, we find the same overall trends: the evidence points toward a null effect, and if anything a negative effect of aid on mobilization. We find essentially no evidence pointing toward an increase in mobilization. Looking at the sign of our point estimates, 23 of 40 treatment effect estimates have a sign pointing toward an decrease in mobilization. Among these 23, 8 estimates have p values less than 0.05, and 10 have standardized coefficient sizes greater than 0.2. 6 treatment effect estimates both have p values less than 0.05 and standardized coefficient sizes greater than 0.2. These 6 estimates are entirely for two outcomes variables: whether a household member undertook dangerous work in the past months, and the interaction

of whether a household member returned and whether a family member is living in a siege zone. Both of these could be interpreted as the program

Among the 17 of 40 treatment effect estimates that have a sign pointing toward an increase in mobilization, almost all are well identified zeros. Among these 17, only one estimate has a p value less than 0.05, and all 17 have standardized coefficient sizes less than 0.2. That is, among the 42.5% of treatment effect estimates where to sign points toward an increase in mobilization, they are all substantively small and only one is statistically distinguishable from zero.

Looking at p values of our point estimates, only 8 of 40 treatment effect estimates have p values less than 0.05, and 6 of these 8 estimates point toward a negative treatment effect on mobilization. That is, among the 20% treatment effect estimates that are statistically distinguishable from zero, all but two point toward a decrease in mobilization.

Looking at standardized coefficient size, 10 of 40 treatment effect estimates have a standardized treatment effect estimate larger than 0.2 (i.e., Cohen (1988)'s cutoff for a 'small' effect size), and among these 10 regressions, all estimates point in the direction of a decrease in mobilization. That is, among the 25% of treatment effect estimates that are not substantively small, all have signs pointing toward a negative effect of aid on mobilization. In addition to the 40 regressions discussed above, the

Online Appendix presents results from 12 PCA regressions, which also provide no strong evidence of a treatment effect on the metrics studied. It is important to note again that while such a large number of regressions would often raise concerns about fishing or p-hacking, it should not in our study because we are arguing that there is *no effect of treatment* on our outcomes.

Table 2: Results: Linear fit with covariates (equations 1 and 1a)

	1. Change in number of men ages 18-50	2. A household member returned to Syria	3. Change in number of men 18-50 when someone returned to Syria	4. Family member in an active war zone	5. Household member returned to an active war zone
$\hat{\beta}$	-0.003 (0.068)	-0.032 (0.034)	-0.001 (0.018)	-0.118 (0.119)	-0.066 (0.028)
Control-group mean	0.173	0.067	0.011	0.238	0.043
p -value	0.969	0.34	0.974	0.319	0.02
Standardized $\hat{\beta}$	-0.005 [0.105]	-0.128 [0.136]	-0.007 [0.122]	-0.277 [0.279]	-0.326 [0.138]

Notes: $n = 1,358$. Because the forcing variable, and thus treatment assignment, was determined at the village-level, we use Eicker-
Huber-White robust standard errors, clustered at the village where respondents lived at the time of treatment assignment, reported in
parentheses. The standardized standard error is presented in square brackets. The bandwidth in all regressions is $h = 50$ meters. The
control-group mean shows the regression model's prediction of the dependent variable for refugees residing at 499 meters altitude. All
variables refer to the time period from November 2013 to April 2014. Covariates include baseline household demographics (number of
children, adults, elderly), education, age, and Syria origin of household head.

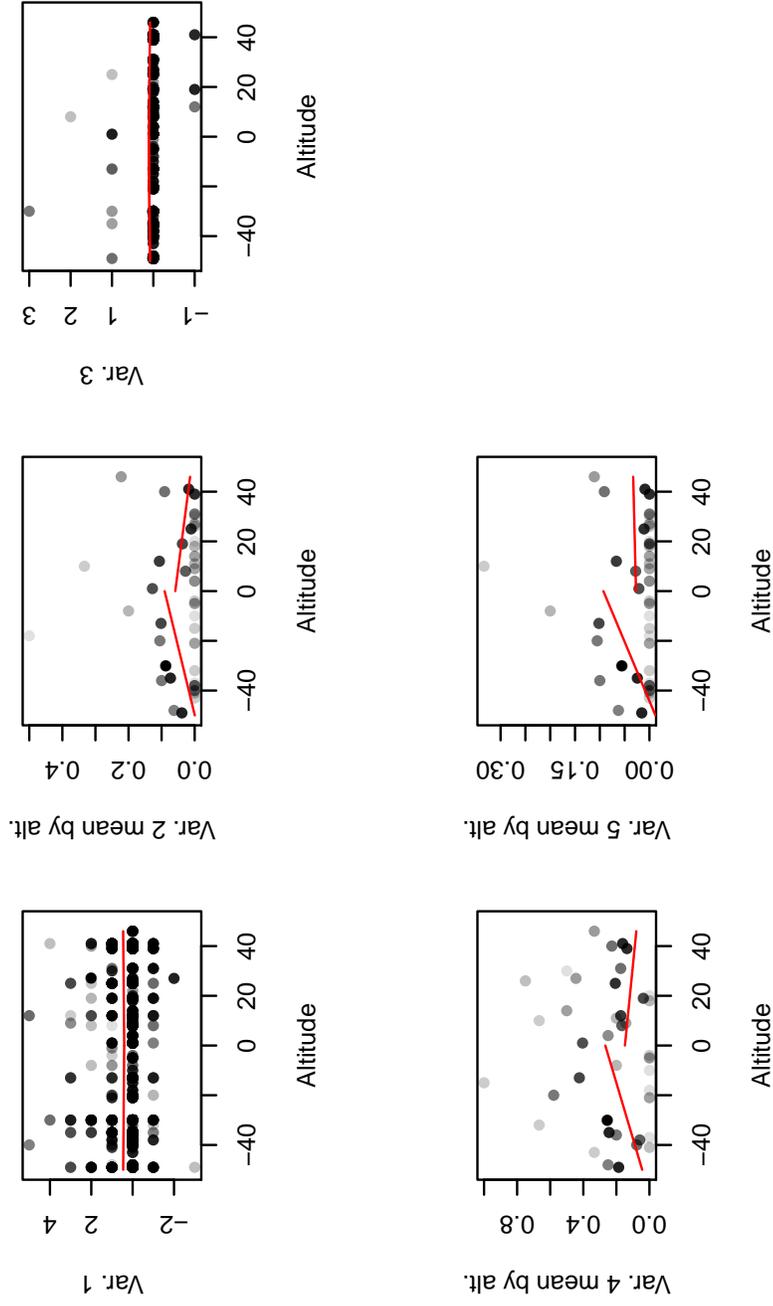


Figure 5: Linear fit with covariates (equations 1 and 1a)

8 Conclusion

Given the prevailing conclusion of existing literature that refugees and aid often exacerbate conflict we might question the wisdom of providing humanitarian aid to refugees. This article is the first to provide direct quasi-experimental evidence to the relevant policy question: when refugee crises occur, what are the impacts of humanitarian aid on conflict? Our main result provides evidence that directly contradicts the dominant findings in the published literature that aid and refugees cause conflict. If anything, our estimates point in the direction of a small decrease in mobilization, which is likely because aid increases the opportunity costs of participation in insurgency.

The empirical evidence that published literature uses for theory building and testing around refugee crises and conflict is largely based on either case-study evidence or analysis of observational data, which may be affected by selection bias. Leenders (2009) and Onoma (2013) point out that the case studies most often used to develop theory about refugees and conflict are exceptional and extreme cases of refugee mobilization, so it is unsurprising that they exaggerate the risks of refugee recruitment and militarization. The positive correlation between refugees and conflict that many studies observe in OLS results is not surprising; worldwide, a majority of refugees flee their home countries because of conflict and regional instability, resulting in complex

and unobservable forms of endogeneity between refugees and conflict that covariate adjustment is unlikely to solve (Gleditsch, 2007; Checkel, 2013).

This study uses a natural experiment and in-depth knowledge of the context to establish specific causal facts for a well-defined subpopulation, rather than a general ‘universe-of-cases’ regression, and this should not be viewed as a limitation (Samii, 2016). As Aronow and Samii (2016) show, the trade-off between internal and external validity is illusory; simply because a regression model includes observations for many countries does not mean it is representative of that population.

Questions about whether some feature of the context may limit external validity are at their core questions about heterogeneous treatment effects, or conditional average causal effects. Whether the effects of humanitarian aid vary based on features of the study context – such as the brutality of the civil war and the aid modality – present opportunities for future research to explore. Future research should explore whether effects of humanitarian aid vary by features of the context, such as how the brutality of a civil war affects refugee mobilization, and whether different types of humanitarian aid may fuel insurgencies differently.

While the majority of published literature would predict refugee mobilization as well as a high risk of aid to refugees worsening conflict in the context under study, we find little evidence of either. The majority of our findings suggest that aid did

not have a large effect on refugee mobilization in this study. Among estimates that have broad confidence intervals, the signs of point estimates suggest a decrease in mobilization due to aid rather than an increase. Our results suggest that existing theories arguing that refugees are prime candidates for mobilization, and that aid will often exacerbate conflict, have limited predictive power. Our estimates provide a baseline from which other studies can progress, to build an evidence base toward the question of whether refugees and aid cause conflict. Our findings also highlight the need for more case-oriented research in the domain of civil war, which may cumulatively shape our understanding of when and why humanitarian aid and refugees will exacerbate, alleviate, or – importantly – have no effect on civil conflict.

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