

Refugees, Mobilization, and Humanitarian Aid

Evidence from the Syrian Refugee Crisis in Lebanon *

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Abstract

This article examines whether refugees are prime candidates for recruitment into armed groups and whether humanitarian aid to refugees impacts their choice to join armed groups. First, our original survey data of 1,358 Syrian households in Lebanon provides evidence that mobilization among the refugee population is low at baseline – the first empirical estimates of the magnitude of the rate of Syrian refugees returning home to fight. Second, leveraging as-if-random assignment around a strict altitude cut-off for a UN cash-transfer program for Syrian refugees, we find little evidence that the aid program had a large effect on mobilization. If anything, our estimates indicate a small decrease in mobilization. Our results stand in contrast to published literature arguing that refugees are prime candidates to join armed groups and humanitarian aid to refugees may support armed groups and fuel recruitment.

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, p. 40

1 Introduction

In this article, we examine the Syrian refugee crisis in Lebanon to test whether Syrian refugees were prime candidates for recruitment into armed groups, and if humanitarian aid delivered to the refugees had the perverse effect of fueling mobilization. The Syrian refugee crisis in Lebanon exhibits a vast majority of the risk factors for refugee mobilization and aid worsening conflict 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 split 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 recruited refugees. Despite these risk factors, we do not find evidence of refugee mobilization or that aid

affected mobilization.

When and why refugees join armed groups 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. As of June 2018, the number of refugees worldwide totaled 25.4 million. If we include people forcibly displaced within the borders of their home country, the number of displaced people worldwide rises to 68.5 million. Understanding when refugees fight in armed groups can shed light on the origins and evolution of conflict. It can also help in evaluating strategies for responding to refugee crises, a central question for the international community. The United Nations (UN) spends billions of dollars every year on responses to refugee crises and the amount is increasing. Whether refugees are at significant risk of joining armed groups, and whether humanitarian aid will make conflict worse, has important implications for how policy makers should design responses to refugee crises.

Existing academic and journalistic work has highlighted cases of refugee crises where refugees joined armed groups and transnational insurgent groups captured aid intended for refugees, which increased the groups' operating budget and capacity to recruit fighters. Furthermore, transnational insurgent groups may see the potential for profit from recruiting more refugees who are well positioned to help the group

secure aid rents. If this is the case, then humanitarian aid that is meant to alleviate the suffering of refugees, may in fact increase refugee mobilization into armed groups, strengthening those groups, prolonging refugee crises, increasing violence in the home country, and increasing the risk of conflict spillover into the refugee hosting country.

This article makes two key contributions, one is descriptive, the other causal. The article's descriptive contribution studies original household level data that records the migration choices of 1,358 Syrian refugee families in Lebanon, and our analysis finds that the refugees in our sample are unlikely to return home to join armed groups in Syria. This result stands in contrast to the dominant findings from published literature (reviewed in the next section) arguing that refugees are prime candidates to join insurgencies.

The article's causal contribution provides quasi-experimental evidence suggesting that humanitarian aid to the Syrian refugees in our sample did not impact mobilization of aid recipients into armed groups. This stands in contrast to a body of literature arguing that aid to refugees exacerbates conflict. The aid program we study is a UN 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 movement from Syria into Lebanon.¹ To identify

¹Source for levels of violence in Syria: Uppsala UCDP Battle-Related Deaths dataset. Source for levels of refugee movement from Syria into Lebanon: UNHCR Data Portal.

the causal effect of the aid program, we exploit the fact that only low-income refugees residing above 500 meters altitude were eligible to receive cash transfers, hence our strategy is to compare migration choices among low-income refugees slightly above and below this arbitrary eligibility cut-off (using a regression-discontinuity design).

We use original household data with a high level of demographic disaggregation and a sample of 1,358 refugee households. We are able to provide causal estimates of the effect of humanitarian aid to refugees because households were ‘as-if’ randomly assigned to aid based on their altitude of residence. We use proxy variables to measure mobilization into armed groups, studying a set of variables that would be affected by armed group mobilization. Each outcome captures variation in mobilization and other activities as well, which provides respondents with the safety to answer honestly, avoiding many of the threats to measurement that arise in survey research on sensitive topics. For example, one metric captures change in the number of adult men in the household. If men returned to Syria to fight, *ceteris paribus*, we would observe a decrease in the number of men in households. Our measurement strategy relies on the fact that a proportion of refugees return, not to fight, but for other reasons such as to earn an income in conventional occupations. Because this strategy, by design, adds noise around our outcome of interest, we draw conclusions

<https://data2.unhcr.org/en/situations/syria/location/71>. Accessed September 19, 2018.

from the set of metrics in aggregate and not any single metric. By examining a large number of outcomes, we can test for the existence and scale of mobilization. We discuss the measurement strategy in detail in section 7.2.

A sizable body of existing work addresses this article’s two research questions of whether refugees are prime candidates for recruitment into armed groups and whether humanitarian aid exacerbates mobilization. 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 for both of the article’s research questions is based on case studies of a small number of exceptional and extreme instances of refugee mobilization and may exaggerate the risk of refugee recruitment and militarization. Existing theory testing is often based on observational analysis of cross-national data on refugees and conflict, and consistently finds a positive correlation between refugees and conflict. 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 (and hence aid to refugees) through reverse causation, or omitted variables causing both conflict and refugees (and hence aid) – remains more plausible than the assertion that refugees (or aid) cause conflict.

One caveat of our quasi-experimental design is external validity. We only surveyed poor refugee families, and of those only the ones living around the elevation cutoff that determines eligibility for the aid program we study (more specifically, poor refugee families residing between 450 and 550 meters altitude), hence it is not clear if our results can be extrapolated to Syrian refugees in Lebanon more broadly. We further discuss external validity in section 6.

2 Theory and Existing Evidence on Humanitarian Aid, Refugees, and Conflict

The majority of existing studies on refugees, aid, and conflict make ominous predictions, arguing that both refugees and aid to refugees exacerbate conflict. Although effect sizes are generally not quantified, no clear set of necessary or sufficient conditions is offered, and counterfactual causal claims are not always clear, the literature often makes strong statements about the risks of refugee mobilization and of aid to refugees fueling insurgencies.

2.1 Are Refugees Prime Candidates to Join Armed Groups?

The existing literature on refugees and conflict argues that refugees may be prime candidates for recruitment into armed groups, and that numerous factors exacerbate

this risk, including 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 government. All of these factors are theorized to increase the probability that refugees will join armed groups.

Refugee crises may facilitate the transnational spread of arms, combatants, and ideologies conducive to conflict (Lischer, 2006; Salehyan, 2007). In such volatile settings, refugees have strong incentives to join rebel groups in order to oppose the regime from which they fled (Salehyan, 2009, p. 40). Refugee populations are not entirely composed of victims escaping persecution but also political activists who mobilize while in the host country (Zolberg, Suhrke and Aguayo, 1992). Furthermore, material hardship and legal marginalization, both widespread experiences for refugees worldwide, make the financial rewards and sense of purpose from fighting in an insurgency relatively more attractive (Salehyan, 2009, pp. 40-41).

Refugee camps are cited as a leading risk factor for the militarization of refugees. Salehyan and Gleditsch (2006, p. 324) write that refugee camps “often provide sanctuary to rebel organizations, a base of operations, and fertile recruitment grounds.” 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, particularly if the state lacks capable and well-equipped security forces (Lischer, 2002, 2006; Salehyan, 2007, 2009). A precarious ethnic balance and pre-existing politicized ethnic rivalries in the host country may also increase the risk of mobilization (Loescher, 1992; Whitaker, 2003).

All these arguments suggest or state that refugees are viable candidates to join armed groups but contradict the intuitive fact that most refugees flee their home country to escape conflict, and may be unlikely to return to fight. A sparse but growing body of work argues that refugee populations are unlikely to incite conflict because they generally comprise civilian noncombatants (e.g., Leenders, 2009; Onoma, 2013; Shaver and Zhou, 2017). If refugees are indeed, in general, prime candidates to join armed groups, then a conducive context may be sufficient to prompt widespread refugee mobilization. If a conducive environment for mobilization, however, is not sufficient, then we will observe refugee crises in weak states with politicized ethnic divisions where refugees do not mobilize on any substantively important scale.

2.2 Does Humanitarian Aid Make Refugees More Likely to Join Armed Groups?

We find divergent predictions in theoretical and empirical work that focuses on how aid to refugees affects conflict. One line of work, focusing specifically on aid to refugees, argues that distributing aid near conflict zones may inadvertently support armed groups and refugee warriors, increasing their capacity and incentives to recruit new fighters from refugee populations. In contrast, more general work on crime and insurgency would predict that aid to refugees would reduce mobilization since it increases the opportunity cost of fighting.

Humanitarian aid to refugees may increase mobilization when it is easily diverted into the hands of armed groups, such as through taxation, looting, and theft. Such capture increases the operating budget of transnational insurgent groups (see e.g., Choi and Salehyan, 2013, p. 57), thereby augmenting groups' capacity to recruit and pay fighters. Armed groups may increase the intensity and frequency of their activities in order to access aid rents, to increase the inflow of aid into an area, or to sustain the conflict and therefore the aid economy that they benefit from. If refugee populations receive aid, then transnational insurgent groups may see the potential for profit from recruiting more refugees who are well positioned to help the group secure aid rents. Aid may be particularly likely to support recruitment efforts if neighbors

are unwilling or unable to police refugee communities and prevent the intrusion of transnational insurgents (Salehyan, 2009).²

Existing work argues that refugee camps are particularly conducive to insurgent recruitment because they create a unique opportunity for fighters to capture humanitarian aid, and for fighters themselves to receive aid by blending into refugee populations (Adelman, 1998). Even beyond camp settings, refugees may be particularly likely to join armed groups due to 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.

Furthermore, aid may affect mobilization through its impact on refugee community economies. If aid is given in the form of in-kind transfers, it decreases recipients' demand for goods and services in their community, which may adversely affect wages and employment, hence refugee incomes, and reduce the opportunity cost of returning home to fight. The opposite reasoning applies if aid is given in the form of cash because it increases recipients' demand for goods and services, hence income in the

²The literature about how aid to conflict zones (i.e., non-refugee populations) affects conflict, specifically on the perverse effects of aid, highlights the effects of aid on the budget constraints of rebel groups and state actors (Besley and Persson, 2011; Nunn and Qian, 2014; Collier and Hoeffler, 2002; De Ree and Nillesen, 2009; Ahmed and Werker, 2015), as well as the potential incentive it provides for actors to increase violence against civilians in expectation of obtaining aid (e.g., Anderson, 1999; De Waal, 1997; Polman, 2010)

refugee economy.

On the other hand, humanitarian aid to refugees increases the opportunity cost of fighting, and economic theories of insurrection predict that incentives to rebel grow smaller as household income and economic opportunities from non-rebel activities rise (Grossman, 1991; Collier and Hoeffler, 1998). 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 refugee mobilization. Despite the widespread use of the opportunity cost framework to consider the economic incentives of participation in insurgencies (Collier and Hoeffler, 1998; Fearon and Laitin, 2007; Humphreys and Weinstein, 2008), scholarship about whether aid in refugee crises will exacerbate conflict rarely employs it in developing predictions.

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 refugee mobilization and on the link between aid to refugees and conflict. Given the presence of many risk factors for refugee mobilization and for aid exacerbating conflict, the crisis appears to be what the case-study literature would call a ‘most-likely case’ for observing refugee mobilization and aid to refugees increasing mobilization. Approximately 15%-20% of Syrians in Lebanon

live in thousands of camps across the country's east, near the Syrian border.³ 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 Syrian government. 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, Corstange shows that 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 level of opposition to the government than in Corstange's representative sample.

Syrian refugees in Lebanon cannot work legally and have few legal rights; because Lebanon has not ratified the 1951 Refugee Convention, Syrian refugees lack many of the rights normally afforded to refugees under the convention. The Lebanese gov-

³ 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. We maintain that the difference is more bureaucratic than useful, and we 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.

ernment crafted its policy response to the refugee crisis to leave Syrians in a position of precarity, forced to either leave the country or accept exploitation, vulnerability, and material hardship (Janmyr, 2016).

Lebanon is often labeled a ‘weak state’ and state institutions have 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. The country’s system of sectarian power sharing hinders the state’s ability to provide universal access to public goods and social services for its citizens, since non-state actors seek to maintain their own communally based institutions, even going as far as blocking the development of effective state responses to social problems (Cammett, 2014). Public service provision is further hindered by widespread corruption at high levels of state bureaucracy (Leenders, 2012). 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.

During our study period of 2013-14, Syrian armed groups operated in Lebanon, traversing the porous border between the two countries. If refugees had wanted to

join a group, they could have done so in Lebanon and then traveled to 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.

The Lebanese military and security forces lack the capacity to adequately police mobilization and transnational insurgent movements. Many things move across the borders, including people, materiel, and medical supplies. *The Military Balance* (International Institute for Strategic Studies, 2014) states that "Lebanon's overstretched and unevenly capable military and security services struggled to deal with the pressure" (p. 299) of refugee flows, cross-border trade, and movement of jihadi fighters

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.

and government agents, and that the security forces are generally “afflicted by severe logistical, operational and political problems” (p. 330). In addition to the general 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.⁶ Although official numbers are not available, formal border crossings probably only account for a minority of human movement between the two countries. In October 2014, an article in the pro-Syrian Lebanese Arabic-language newspaper, *As-Safir*, argued that the “and” in “Syria and Lebanon” had disappeared.⁷ In April 2015, a member of Lebanon’s Border Control Committee stated that effective Lebanese border management was impossible while the Syrian war continued.⁸

The cleavages of Syria’s war in 2013 and 2014 also drove 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 2005 in opposition to the Syrian occupation of Lebanon

⁶For example: Karha and Knaissat Akkar in Wadi Khaled, the village of Deir El Achayer in the south of Beqaa Governorate, and the area along the western Qalamun Mountains near Aarsal. For more discussion see Vignal (2017).

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 kāfira*.

⁸ The Daily Star, April 29, 2015. <http://www.dailystar.com.lb/News/Lebanon-News/2015/Apr-29/296141-effective-border-management-a-pipedream.ashx>

(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 receives financial, political, and military assistance from Iran, a leading patron of the Syrian government in the civil war. Even the Lebanese Armed Forces suffer from politicized sectarian divisions, reflecting rather than transcending the country's internal fault lines (International Institute for Strategic Studies, 2014). 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.

The foremost driver of Lebanon's limited capacity to police the refugee population was the enormous scale of the population influx, which was larger than nearly any country would be able to adequately monitor. Lebanon witnessed a 25% population increase from 2011-2014, arguably one of the largest relative population increases that any modern state has experienced. Despite the limitations on the Lebanese security apparatus, Lebanon (along with other states in the region) increased electronic surveillance and bolstered border security to manage and monitor the movement of goods, refugees, fighters, and government agents. Lebanese security forces also demonstrated capability to disrupt terrorist threats within Lebanon, some related to

the war in Syria.⁹

Finally, one must not ignore that the decision of armed groups to recruit from refugee populations, and refugees' decision to participate, depends on the constraints imposed by the Lebanese government. We view the observed choices of refugees, armed groups, and the Lebanese state as an equilibrium with those three actors making choices to maximize the expected benefits from, respectively for the actors, mobilization, recruitment, and monitoring, given the constraints they each face. Without knowledge of the mobilization rate, the Syrian refugee crisis in Lebanon could have been in a high refugee mobilization equilibrium. Alternatively, the crisis might have been in an equilibrium where refugee mobilization was low and not a meaningful threat (and Lebanese security capacity therefore does not matter very much). Last, the crisis could have been in an equilibrium where refugee mobilization would have been a threat but for the actions and capacity of the Lebanese security apparatus. In the descriptive contribution of this article, we provide evidence that refugee mobilization was low, arguing that the crisis was, at the very least, in one of the equilibria with a low level of realized mobilization, rather than one where refugee

⁹During a spate of terrorist attacks in 2014, a number were carried out prematurely when Lebanese security forces stopped suspects' vehicles at checkpoints, which security services often put up around the country. Lebanese intelligence was actively trying to prevent terrorist attacks. On June 20, 2014, for example, when 102 people were arrested in Hamra hotels, and 17 were detained as terrorist suspects at the ISF Intelligence Bureau.

mobilization was high. Our research design is not able to provide evidence as to what would have happened if Lebanon’s security capacity had been higher or lower.

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.¹⁰ \$95 per month is a significant amount for the beneficiary population, equal to about two-thirds of control-group household monthly income (\$149, source: our survey) and about one third of the value of control-group household food consumption, which is largely supplied by humanitarian aid.¹¹ 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

¹⁰‘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 on eligibility criteria, like poverty or altitude. Although UNHCR determined eligibility for the program, managed the funding and the partnership with the bank that managed the ATM cards, and scheduled distributions and electronic transfers to the ATM cards, Lebanese and international NGOs with local capacity in certain parts of the country were contracted to implement the physical distribution of ATM cards at the beginning of the program. UNHCR distributed more than half of the cards. Other organizations that managed local card distribution efforts included ACTED, AMURT, AVSI, CARE, Caritas, CISP, DRC, Handicap International, Humedica, IOCC, IOM, Makhzoumi, MEDAIR, Mercy Corps, NRC, Oxfam, Save the Children, SHEILD, SIF, Solidar Suisse, and World Vision.

¹¹Both income and household consumption statistics come from our survey data.

living above 500 meters altitude. For a more detailed discussion of the cash-grant program, refer to the online appendix.

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 (e.g., 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.

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

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:

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

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

$$+ \theta_4\mathbb{1}(A_i \geq 500) \times (A_i - 500)^2$$

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 Study Population

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). The 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 vulnerability according to a weighted mean of household demographic variables.¹² 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.¹³ We discuss how this population differs from the broader Syrian refugee population in the external validity section.

¹² 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.

¹³ In addition to all poor households living at or above 500 meters altitude, Syrians living in so-called *informal tended settlements* (ITs) and *informal settlements* (ISs) also received the cash transfer program regardless of altitude. The vast majority of ISs are in the Biqa’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.

4.3 Data Collection

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).¹⁴ 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.¹⁵

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

¹⁴ 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.

¹⁵ The survey questions analyzed in the article are in the appendix. The complete survey questionnaire is available at <http://tinyurl.com/pvyub87>.

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 1,000 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.

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*).¹⁶ Visual inspection of treatment

¹⁶ At the time of treatment assignment, respondents were living in the following districts: El Batroun, Chouf, Kesrwane, El Nabatieh, Marjaayoun, Aley, Akkar, El Minieh-Dennie, Sour, Jbeil, El Koura, El Meten, Bent Jbeil, Baabda, Jezzine, and Hasbaya

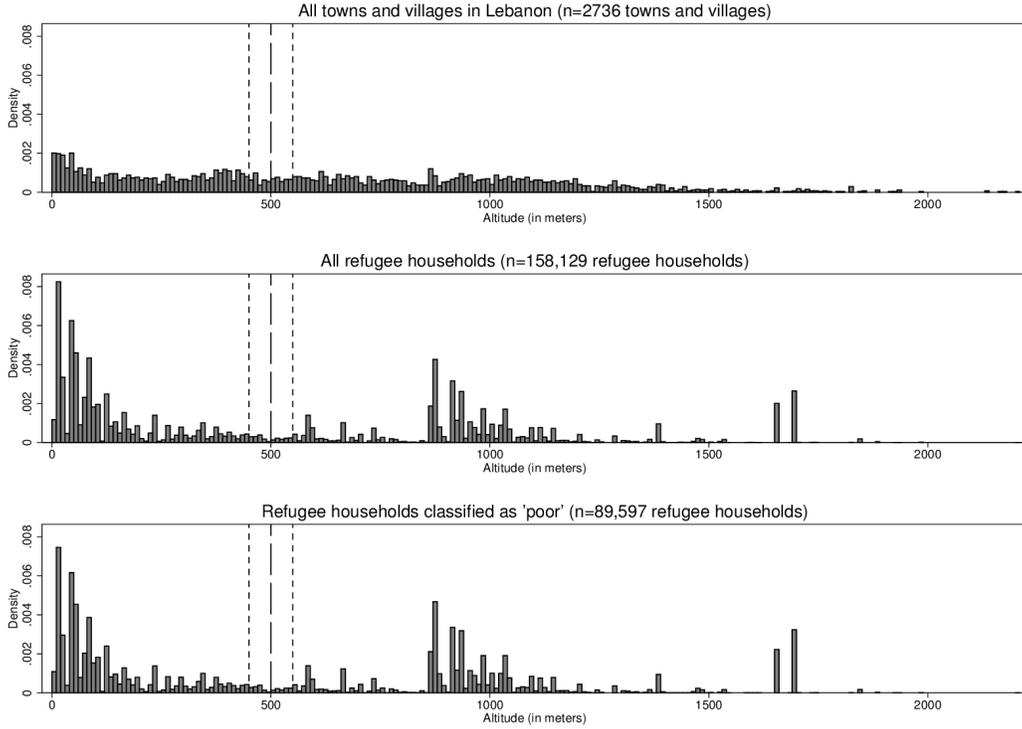


Figure 1: *Top*: Nationwide Population Density; *Middle*: Density of Registered Refugees; *Bottom*: Density of Eligible ‘Poor’ Refugee Households

and control communities suggests no systematic distribution of treated and untreated towns across the country.

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 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

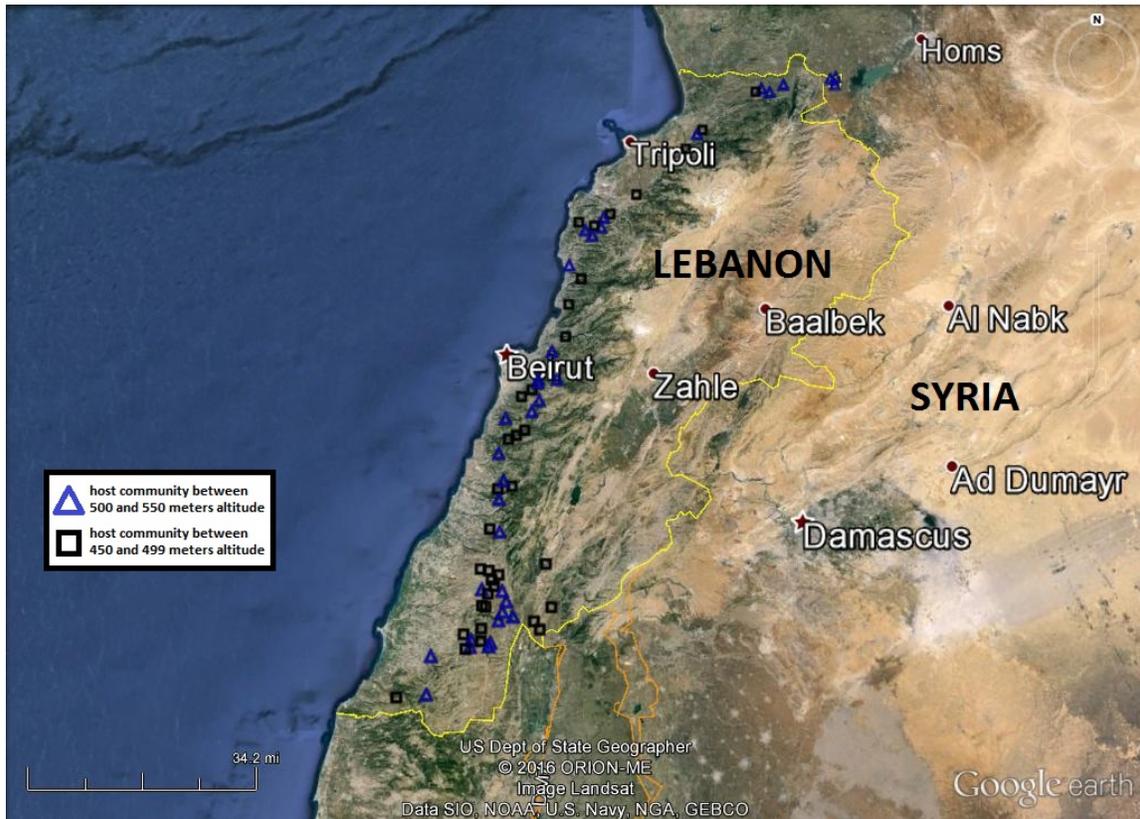


Figure 2: Locations of survey respondents at the time of treatment assignment, treatment communities in blue triangles, and control communities in black squares spending.

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. 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. 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. 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 External Validity

The study population is a subset of the broader registered Syrian population, sampled according to altitude (only those around 500 meters altitude) and vulnerability (only vulnerable households). We believe that neither of these sampling choices undermines our ability to speak to our research questions with our data. First, 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 observe little mobilization and find no effect of aid on mobilization among this sub-population, when there could in fact be an effect in the broader population of Syrian refugees. We believe that this is unlikely given the economic geography of the conflict. Many fighters in the opposition and the army were 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 in Lebanon may have a higher baseline prevalence of involvement in armed groups than the general population of Syrian refugees.

The second concern about external validity is whether households living around

500-meters altitude are unique in ways that limit our ability to draw conclusions about mobilization rates and the effects of aid on mobilization. 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 directly along the border in October 2013. It might be that we would have observed higher rates of mobilization or identified an effect of aid on mobilization if we had a research design that included more 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.¹⁷ 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 at its narrowest point.¹⁸

¹⁷ <http://countrystudies.us/lebanon/30.htm>, accessed July 28, 2015

¹⁸ 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.

7 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. 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. We show in the Online Appendix that 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.

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.

7.1 As-if Randomness Checks

RDDs rely on the assumption that whether an observation was just above or below the cutoff is not correlated with its 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 68 pretreatment variables with linear and quadratic specifications. As shown in Table 1, where we present the share of pretreatment 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. For balance variables we use 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. To test for balance in Lebanese demographics in the communities where Syrians live, we use (Mourad, N.d.)’s data on

Lebanese community majority sect.

We see that 15% of the 68 pretreatment variables exhibit a detectable jump at the cutpoint at the 0.05 level in a linear specification, and 16% in a quadratic specification. We see 8 of the 68 variables (12%) are imbalanced across both linear and quadratic specifications. First, we see imbalance in terms of where respondents are from in Syria for 2 of Syria's 14 governorates. Households that come from the Homs governorate appear to be less likely to receive treatment, and households from Idlib more likely to receive treatment. We observe balance for respondents from each of the other 12 governorates.

We find imbalance in the year of arrival, with the treated units at the cutoff having arrived a few months earlier on average than control households at the cutoff. Specifically, the observed difference in arrival at the cutoff is between 0.19 and 0.24 of a year earlier for treated units (depending on the specification used to calculate balance). We also find imbalance in arrival month, where households that arrived in December are more likely to be treated, although arrivals for the other 11 months are balanced.

Across 23 household demographic variables, we see baseline imbalance in 4. Overall, we find imbalance in family size, which is larger for treated units at the cutoff. When we test for balance in household demographic subgroups, we see that treat-

ment is associated with 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. Second, in order to increase precision and potentially alleviate confounding due to this pretreatment imbalance, we present all results both with and without covariates.

Table 1: Share of pretreatment variables imbalanced by model and significance level

	Linear	Quadratic
p<0.01	0.06	0.04
p<0.05	0.15	0.16
p<0.10	0.24	0.19

7.2 Measuring Refugee 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.

Our outcome of interest is whether refugees join armed groups, and we use a set of proxy variables as evidence for that empirical question. To measure refugee 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 mobilization and its direction, although we cannot estimate a point estimate of the magnitude of the effect. We measure 13 outcomes that would be affected by refugee 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 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 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 return home to fight, we would see fewer adult men in those households compared to control-group households. We ask how many individuals moved and how many moved to Syria, and we ask about the age and gender of all movers. Families with more individuals who returned from Lebanon to Syria to fight would have, all else equal, more family members who moved and more who moved to Syria. We also ask whether someone in the family is living in parts of Syria that are under siege or where there is fighting, since if family members are actively fighting, there is a reasonable chance that they would be in an embattled or

besieged area. We ask if anyone in the household moved to earn money, and specifically whether anyone moved to Syria to earn money. 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 ask 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 member is engaged in physically dangerous activities.

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. 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.

7.3 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. Following 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.

8 Results

8.1 Refugees and Mobilization

Based on the dominant predictions from the existing literature, 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. That is, at the time of the study, November 2013 to March 2014, a very small number of Syrian refugees returned home to fight.

We begin by estimating the magnitude of the 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 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 . As we discuss more below, our sample is not representative of the full Syrian population, and we therefore cautiously interpret our estimates as evidence of the magnitude of return, but not as point estimates of the return rate. The magnitude of $r_C \times k$ denotes an estimate of the magnitude of the number of Syrian refugees who returned during the study period. Next, to estimate the magnitude of return to join armed groups, we estimate the number of respondents in our sample who returned to Syria to join armed groups, denoted m , by offering 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 of 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.

To be clear, this simulation does not provide evidence of the mobilization rate among refugee returnees, and we do not suggest that s could be as high as 5%. We use $s=5\%$ to illustrate that even if one assumes a high mobilization rate, the overall magnitude of mobilization would be small given the low return rates in our data. 5% would be a very high mobilization rate if applied to the general population given the collective action problem of rebellion (see Lichbach 1995's 5% rule).

We find evidence that the magnitude of refugee mobilization was likely substantively small, suggesting that Syrian refugees in Lebanon were not prime candidates for recruitment into armed groups in Syria's civil war. 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 armed groups, 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 armed groups, 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 and approximately 178,000 soldiers in the Syrian Army.¹⁹

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. When the estimate is interpreted 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

¹⁹ Insurgent Groups: January 29, 2014. Congressional testimony by US Director of National Intelligence James Clapper. Transcript available via *The Washington Post*. http://wapo.st/1mFmR?tid=ss_tw. Accessed March 17, 2017. Syrian Army: Aram Nerguizian. 2015. "The Military Balance in a Shattered Levant." *Center for Strategic and International Studies*. Page 22.

possibility of a meaningful level of refugee mobilization among Syrian refugees in Lebanon. To imply that even one percent of refugees joined armed groups, one would need to argue that the rate of mobilization was approximately forty times higher than in our subsample of poor Syrians living around 500-meters altitude. Overall, the evidence presented here suggests that the Syrian refugee crisis in Lebanon does not justify a claim like Salehyan (2009)'s that refugees are prime candidates for recruitment into rebel factions.

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 put them at higher 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.

Syrians may return from Lebanon to fight in the Syrian Army rather than in insurgent groups, and the evidence above pertains to both forms of mobilization.

Many Syrians fled violence perpetrated by insurgent groups, rather than the government, and Salehyan (2009)'s grievance-based argument about why refugees return to fight could be understood to explain why Syrians would want to join the national army. We know from literature on recruitment (e.g., Arjona and Kalyvas, 2012) that fighters often join the group that is present or strongest in the area in which they live. Our data indicates that the magnitude of return in 2013-2014 was very small, and the magnitude of return to fight in armed groups, including insurgent groups and the army, must therefore have been small.

8.2 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 3 offers a visual presentation of the same results. Because the forcing variable, and thus treatment assignment, was determined at the village-level, we use Eicker-White robust standard errors, clustered at the village level according to where respondents lived at the time of treatment assignment. Results 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 mobilization, 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 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 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 decrease in mobilization due to aid. We find essentially no evidence pointing toward an increase in mobilization. Looking at the sign of our point estimates, 24 of 40 treatment effect estimates have a sign pointing toward a decrease in mobilization. Among these 24, 5 estimates have p values less than 0.05, and 10 have standardized coefficient sizes greater than 0.2. We see that 5 treatment effect estimates both have p values less than 0.05 and standardized coefficient sizes greater than 0.2. These 5 estimates are entirely for two outcome variables: whether a household member undertook dangerous work in the past six months, and the interaction of whether a household member returned and whether a family member is living in a siege zone.

Among the 16 of 40 treatment effect estimates that have a sign pointing toward an increase in mobilization, almost all are well identified zeros. Among these 16, only 2 estimates have a p value less than 0.05, and all 16 have standardized coefficient sizes less than 0.2. That is, among the 40% 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.

household demographic variables Looking at p values of our point estimates, only 7 of 40 treatment effect estimates have p values less than 0.05, and 5 of these 7 estimates point toward a negative treatment effect on mobilization. That is, among the 17.5% of 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 decrease in mobilization due to aid. 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.

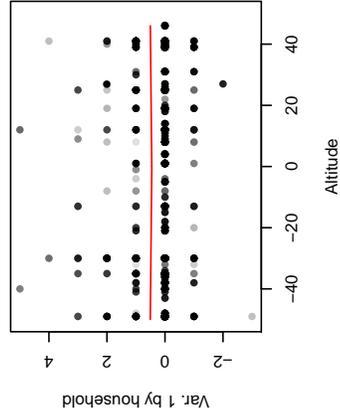
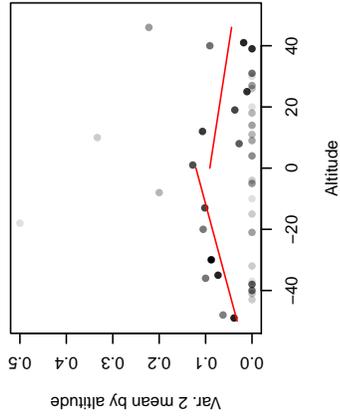
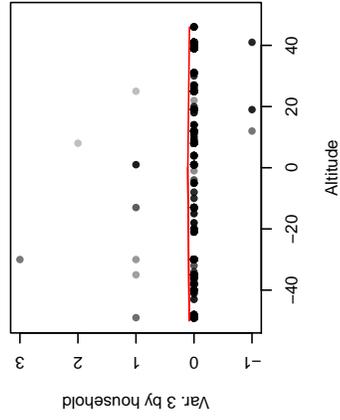
Similar to the results from the descriptive analysis of the magnitude of Syrian refugee return, if aid did affect Syrian refugees' choices about whether to return

to Syria to join an armed group, we would not be able to distinguish joining an insurgent group from joining the national army. Nonetheless, the extremely low levels of return, and the lack of evidence of any effect of aid on return rates, stands as a lack of evidence of return to join the insurgency or to join the national army.

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
$\hat{\beta}$	0.002 (0.069)	-0.03 (0.033)	-0.001 (0.018)	-0.119 (0.119)	-0.064 (0.028)
Control-group mean	0.173	0.067	0.011	0.238	0.043
p -value	0.978	0.366	0.946	0.32	0.021
Standardized $\hat{\beta}$	0.003 [0.107]	-0.12 [0.132]	-0.007 [0.122]	-0.279 [0.279]	-0.317 [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.



- Var. 1: Change in number of men ages 18–50
- Var. 2: A household member returned to Syria (0/1)
- Var. 3: Change in number of men 18–50 when someone returned to Syria
- Var. 4: A family member in an active war zone (0/1)
- Var. 5: Household member returned to an active war zone (0/1)

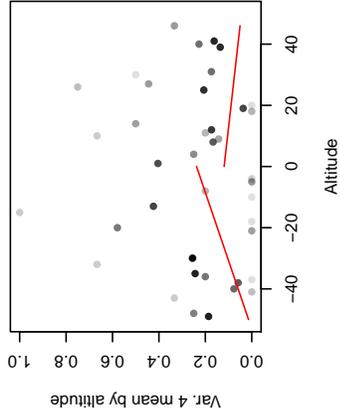
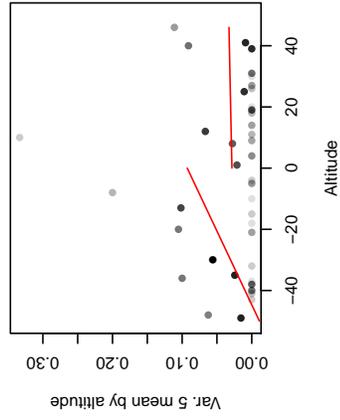


Figure 3: Linear fit with covariates (equations 1 and 1a)

9 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 questions: when refugee crises occur, are refugees likely candidates to join armed groups, and what are the impacts of humanitarian aid on conflict? Our main results provide evidence that contradicts the dominant findings in the published literature that refugees and aid cause conflict. First, we present evidence that Syrian refugees were not prime candidates for recruitment into armed groups. Second, we show 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, likely because aid increases the opportunity costs of participation in fighting.

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. 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 (as pointed out by Leenders, 2009; Onoma, 2013). 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).

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. Future research should explore whether effects of humanitarian aid vary by features of the context, such as the brutality of the civil war and whether different types of humanitarian aid may fuel armed groups 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. 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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