Humanitarian Aid and Civil War

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

Insurgent Mobilization *

Daniel T. R. Masterson & M. Christian Lehmann

September 1, 2017

Abstract

Understanding when humanitarian aid affects political violence is critical for both theory and policy regarding the world's many displacement crises. The paper provides a rare null finding in a contentious literature about when refugees and humanitarian aid to refugees exacerbate conflict. We examine whether a household-level income shock impacts the choice of refugees to return home to join an insurgency, leveraging as-if-random assignment to a UN cash-transfer program for Syrian refugees and original survey data of 1,358 Syrian households in Lebanon. First, we show that insurgent mobilization among Syrian refugees in Lebanon is low at baseline. Second, despite an intervention that increased household income by 66%, we find little evidence that aid had a meaningful effect on insurgent mobilization.

This paper provides new empirical evidence toward the question of whether humanitarian aid to refugees impacts insurgent mobilization. The majority of published literature predicts that refugees, aid to refugees, and aid during civil war all exacerbate conflict. Not only are the predictions about aid to refugees grim in general, they would be particularly so if we applied them to the case of Syrian refugees in Lebanon. Syrian refugees arrived in Lebanon in the context of politicized ethnic divisions, pre-existing ethnic rivalries, resentment among the host population about the refugee population, and a central state with little capacity to limit mobilization, where armed groups can capture humanitarian aid and recruit refugees.

We study a United Nations unconditional cash transfer program for Syrian households in Lebanon, implemented at a time when members of armed groups regularly crossed porous borders between Lebanon and Syria. Our research leverages as-if-random of cash transfers to Syrian refugees with an altitude-based regression-discontinuity design and original survey data of 1,358 Syrian households in Lebanon. Our research design offers many advantages for inference about the micro-level link between income shocks and conflict. To our knowledge, this is the first paper that studies whether an exogenous income shock mobilizes refugees to return to their home country to fight. Unlike many papers on the topic that use aggregate national-level data to study income shocks and 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).

This note provides alternative empirical findings in the literature about the impacts of refugees, aid to refugees, and aid during civil war on conflict processes. Rather than offering

evidence against specific mechanisms from previous work or developing new theory, this research note provides a rare null finding in a contentious literature, and contributes to the body of evidence for case-oriented theory-building about aid, refugees, and conflict.

Humanitarian Aid, Refugees, and Conflict   A majority of existing work argues that refugees, aid to refugees, and aid during civil war all exacerbate conflict. Although the problem's scale is generally not quantified, the literature often makes strong statements about the risk of refugee mobilization and emphasizes refugees' role in both provoking and prolonging civil wars (e.g., Adelman, 1998; Stedman and Tanner, 2004). Salehyan (2009) argues that refugees sustain insurgencies and prolong ongoing conflicts because armed groups recruit from refugee populations and capture humanitarian aid resources intended for refugees, particularly when neighbors are unable or unwilling to police refugee populations (p.37). Salehyan (2009) writes that refugees are "prime candidates for recruitment [. . . into] rebel factions" (p.40). Numerous scholars find that aid extends wars and increases subnational violence because armed groups capture humanitarian supplies — including food, medical supplies, and vehicles — (Nunn and Qian, 2014; Crost, Felter and Johnston, 2014; Wood and Sullivan, 2015), and that aid to refugees in particular will exacerbate conflict (Barber, 1997; Lischer, 2006; Salehyan, 2008; Choi and Salehyan, 2013).

A significant number of existing studies would thus predict that Syrian refugees in Lebanon are at high risk of refugee militarization and that humanitarian aid would exacerbate conflict. Lischer (2006) and Salehyan (2007) argue that refugee militarization is more likely in weak host states, such as Lebanon, that have little capacity to limit mobilization, based on the claim that effective host government action can prevent the militarization

of refugees.[1] Loescher (1992) argues that a precarious ethnic balance, pre-existing ethnic rivalries, and resentment among the host population about the refugees will increase the risk of mobilization, all of which are present in Lebanon. Whitaker (2003) argues that refugee movements are more likely to produce conflict when the host country's regime lacks political legitimacy and when ethnic difference is politicized in the host country, as is the case in Lebanon.

On the other hand, the predictions above contradict the intuitive fact that most refugees flee their home country to escape conflict, and may be unlikely to return to fight. Whitaker (2003) and Onoma (2013) argue that refugee populations generally do not incite conflict because they generally comprise civilian noncombatants. Furthermore, aid increases the opportunity cost of fighting. Economic theories of crime (Becker, 1968; Ehrlich, 1973) and formal theories of 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. Lischer (2006) argues that without a 'state-in-exile,' where the displaced population is integrated into the strategy of an exiled political organization, the refugee population may lack the organized structure to systematically capture, loot, and exploit humanitarian resources. Although the Syrian refugee crises in Lebanon demonstrates almost all of the characteristics that studies argue will exacerbate refugee mobilization, it does not serve as a refugee state-in-exile. Last, much theory building on refugees and conflict is based on a handful case studies of refugee mobilization and conclusions 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

---

[1] Lebanon is often grouped with Yemen and Sudan as one of the Middle East's chronically "weak" states.

mobilization, so it is not surprising that they exaggerate the risk of refugee recruitment and militarization.

To sum up, existing work on refugees, aid, and conflict makes predictions that point in both directions. However, the majority of work that focuses specifically on refugees, aid, and conflict predicts that both refugees and aid to refugees will exacerbate conflict, and would be especially likely to do so in the Syrian refugee crisis in Lebanon.

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. \$95 per month is a significant amount for the beneficiary 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-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.

Sharp RDD 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.

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

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:

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

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

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

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 paper's overall conclusions.

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).[2] 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.[3]

Internal Validity   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

---

[2]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.

[3]The survey questions analyzed in the paper are in the appendix. The complete survey questionnaire is available at http://tinyurl.com/pvyub87.

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. Details about the minimal information that the UN provided to beneficiaries are discussed in the online appendix.

The geographic distribution of Syrian households registered with UNHCR living between 450 and 550 meters covers nearly the entire country. A map in the online appendix illustrates the location of all towns where survey respondents lived at the time of treatment assignment, and visual inspection of treatment and control communities suggests no systematic distribution of treated and untreated towns across the country.

Nine of the 60 pre-treatment variables (15%) have values that are significantly different between treatment and control groups at the 0.05 level in 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.

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 draw inferences about the effect of aid on insurgent mobilization. We draw conclusions from the set of metrics in aggregate. If we estimate a zero treatment effect

8

for a large number of outcomes that mobilization affects, then the results serve as evidence that the aid intervention did not affect mobilization. We measure 13 outcomes that would be affected by insurgent mobilization and present the full set of results in the appendix. 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 "risky 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 embattled or besieged areas.

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.

In the main paper we present the results for five key variables and present eight others in the appendix. Our outcomes comprise non-sensitive items that would be affected by a change in insurgent mobilization. The five outcomes presented in the main paper 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 variation within the CO that we are interested in. The first interaction forces a 0 value for the metric of the number of men if the household had no one return to Syria. The second interaction forces a 0 value for whether someone returned to Syria if no one in the family is living in a siege zone. 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.

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

11

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.

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. Of the 727 households in the control group, 49 had at least one person who moved back to Syria, only 6.7% of households, and among those households, the average number of returnees was 1.6.

To estimate the overall number of Syrian refugees who returned to Syria, we generalize from control group's return rate. Although this 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 with few economic opportunities, which existing research would predict to be at high risk of insurgent mobilization (Humphreys and Weinstein, 2008). Therefore, the bias runs counter to our argument that refugee mobilization among Syrians in Lebanon in 2013 and 2014 was low.

In October 2013, the point at which program eligibility was determined, 691,709 reg-

istered Syrians lived in Lebanon in 158,129 households. 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. Even if we assume a high percentage of returnees went to join the insurgency, even a staggering 5%, our best guess is that a very small number of Syrian refugees returned to fight – roughly 850 people from 691,709 refugees, at a time when there were between 75,000 and 110,000 members in insurgent groups in Syria.[4] Even though this number is biased because we do not have a representative sample, the share of returnees in the broader population would need to be dramatically different from our sample statistic to approach a meaningful level of refugee mobilization.

Aid and Mobilization Published evidence suggests that aid delivered to refugees often exacerbates conflict. Although our results are not definitive due to broad confidence intervals for some outcomes, point estimates are close to zero and most standardized effect sizes are small, and the majority of metrics and models in table 1 and the appendix provide little evidence that the program had a large effect on mobilization. Table 1 presents results of the linear model with covariates (equations 1 and 1a), and linear and quadratic models with and without covariates are presented in the appendix. We discuss other aspects of empirical analysis in the online appendix, including alternative specifications, multiple comparisons adjustments, and other robustness tests.

In Table 1, $\hat{\beta}$ 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

---

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

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 general 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. Although the standardized effect sizes for most variables in the main paper and the appendix are less than or close to 0.2, we can see that some of the confidence intervals around our effect estimates are wide, constraining our ability to generally rule out large effects.

Table 1: 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	-0.032	-0.001	-0.118	-0.066
	(0.068)	(0.034)	(0.018)	(0.119)	(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	-0.128	-0.007	-0.277	-0.326
	[0.105]	[0.136]	[0.122]	[0.279]	[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.

Conclusion   Given the common conclusion of the existing literature that refugees and aid often exacerbate conflict, we might question the wisdom of providing humanitarian aid to refugees. This paper 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? While the majority of published literature would predict refugee mobilization and a high risk of aid to refugees worsening conflict in the context under study, we find little evidence of either. Although we cannot generally statistically rule out large effects, our estimates provide a baseline from which other studies can progress. Our findings highlight the need for more case-oriented research in the domain of civil war, which may cumulatively shape our understanding of when humanitarian aid and refugees will exacerbate, alleviate, or – importantly – have no effect on conflict.

References

Adelman, Howard. 1998. "Why refugee warriors are threats." *Journal of Conflict Studies* 18(1).

Barber, Ben. 1997. "Feeding Refugees, or War? The Dilemma of Humanitarian Aid." *Foreign Affairs* 76:8–14.

Becker, Gary S. 1968. Crime and punishment: An economic approach. In *The Economic Dimensions of Crime.* Springer pp. 13–68.

Choi, Seung-Whan and Idean Salehyan. 2013. "No good deed goes unpunished: refugees, humanitarian aid, and terrorism." *Conflict Management and Peace Science* 30(1):53–75.

Cohen, Jacob. 1988. "Statistical power analysis for the behavioral sciences." *Lawrence Earlbaum Associates: Hillsdale, NJ* .

Collier, P and A Hoeffler. 1998. "On economic causes of civil war." *Oxford economic papers* .

Crost, Benjamin, Joseph Felter and Patrick Johnston. 2014. "Aid Under Fire: Development Projects and Civil Conflict." *American Economic Review* 104(6):1833–1856.

Ehrlich, Isaac. 1973. "Participation in illegitimate activities: A theoretical and empirical investigation." *Journal of political Economy* 81(3):521–565.

Grossman, Herschell I. 1991. "A general equilibrium model of insurrections." *The American Economic Review* pp. 912–921.

Hahn, Jinyong, Petra Todd and Wilbert der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69(1):201–209.

Hoenig, John M. and Dennis M. Heisey. 2001. "The Abuse of Power: The Pervasive Fallacy of Power Calculations for Data Analysis." *The American Statistician* 55(1):19–24.

Humphreys, Macartan and Jeremy M Weinstein. 2008. "Who fights? The determinants of participation in civil war." *American Journal of Political Science* 52(2):436–455.

Imbens, Guido W and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142(2):615–635.

Lee, David S and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48(2):281–355.

Leenders, Reinoud. 2009. "Refugee Warriors or War Refugees? Iraqi Refugees' Predicament in Syria, Jordan and Lebanon." *Mediterranean Politics* 14(3):343–363.

Lischer, Sarah Kenyon. 2006. *Dangerous Sanctuaries: Refugee Camps, Civil War, and the Dilemmas of Humanitarian Aid.* Cornell University Press.

Loescher, Gil. 1992. *Refugee movements and international security.* Number 268 Brassey's for the International Institute for Strategic Studies.

Ludwig, Jens and Douglas L Miller. 2007. "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design." *The Quarterly Journal of Economics* 122(1):159–208.

Nunn, Nathan and Nancy Qian. 2014. "US Food Aid and Civil Conflict." *American Economic Review* 104(6):1630–1666.

Onoma, Ato Kwamena. 2013. *Anti-refugee violence and African politics.* Cambridge University Press.

Salehyan, Idean. 2007. "Transnational rebels: Neighboring states as sanctuary for rebel groups." *World Politics* 59(02):217–242.

Salehyan, Idean. 2008. "The externalities of civil strife: Refugees as a source of international conflict." *American Journal of Political Science* 52(4):787–801.

Salehyan, Idean. 2009. *Rebels without borders: transnational insurgencies in world politics.* Cornell University Press Ithaca, NY.

Stedman, Stephen John and Fred Tanner. 2004. *Refugee manipulation: war, politics, and the abuse of human suffering.* Brookings Institution Press.

Whitaker, Beth Elise. 2003. "Refugees and the spread of conflict: Contrasting cases in Central Africa." *Journal of Asian and African Studies* 38(2-3):211–231.

Wood, Reed M and Christopher Sullivan. 2015. "Doing harm by doing good? The negative externalities of humanitarian aid provision during civil conflict." *The Journal of Politics* 77(3):736–748.