

Refugee Return and Conflict: Evidence from a Natural Experiment*

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Abstract

We estimate the causal effect of a large-scale cash assistance program for refugee returnees on conflict in Afghanistan. The program led to a significant increase in repatriation. Leveraging historical returnee settlement patterns and previously unreleased combat records, we find that policy-induced refugee return had cross-cutting effects, reducing insurgent violence, but increasing social conflict. The program's cash benefits were substantial and may have raised reservation wages in communities where returnees repatriated. Consistent with this hypothesis, policy-induced return had heterogeneous effects on insurgent violence, decreasing use of labor-intensive combat, increasing the lethality of capital-intensive insurgent attacks, and reducing the effectiveness of counterinsurgent bomb neutralization missions. Additionally, social capital and local institutions for dispute resolution significantly offset the risks of refugee return for communal violence. Our study provides the first causal evidence demonstrating the link between aid-induced refugee return and sociopolitical conflict. These results are economically significant, highlighting unintended consequences of repatriation assistance programs, and clarifying the conditions under which refugee return affects conflict. Supporting social capital and legitimate, local institutions are key antecedents for safe refugee repatriation.

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1 Introduction

Today, more than 45 million people are forcibly displaced across international borders by war and chronic insecurity. These refugees and asylum-seekers often find themselves in protracted displacement situations, facing acute vulnerabilities stemming from poverty, political exclusion, poor access to services, and xenophobic hostility. Ensuring forcibly displaced people have access to durable, safe, and dignified solutions is essential for making progress on conflict resolution, economic development, and minority empowerment, and for upholding normative and legal commitments on refugee protection. As such, addressing displacement programmatically is a key priority of international policymakers.

Three pathways define the global solutions architecture for forcibly displaced people: resettlement in Global North countries; permanent integration in Global South host countries; or repatriation to origin countries. Despite concerted humanitarian effort, exceedingly few displaced people can access these solutions.¹ The collapse of the international solutions architecture represents a profound economic and political challenge. Resettlement is hampered by discriminatory, anti-migrant backlash across the world’s high-income democracies (Mayda, 2006; Hangartner et al., 2019). Opportunities for naturalization in Global South host states are similarly constrained. One barrier is institutional—73% of refugees reside in developing countries with low bureaucratic capacity. Another barrier is political—though host countries increasingly encourage refugee self-reliance through integrative policies, citizenship remains contentious (Blair, Grossman and Weinstein, 2022a,b).

Given these challenges, the international community regards repatriation as the preferred solution for forcibly displaced people (Harrell-Bond, 1989; Zieck, 1997).² Notwithstanding significant humanitarian efforts to encourage voluntary return, however, repatriation has declined over time (Figure A-1). In the ideal setting, displaced people return when conflict abates in their home countries. Declining repatriation is the direct result of the growing protraction of wars and sociopolitical crises in refugee-sending states (Fearon and Shaver, 2020). Insecurity is the main deterrent to return (Camarena and Hägerdal, 2020; Alrababa’h et al., 2023), so as conflicts in origin countries become longer and more complex, fewer opportunities exist for refugees to safely and voluntarily repatriate.

¹Since 2013, fewer than 3.8% of the global stock of refugees and asylum-seekers has been resettled, naturalized, or returned home (Figure A-1).

²For host country politicians, repatriation is attractive because it reduces the fiscal burden of refugee-hosting (Chimni, 2002). For refugees themselves, returning home is fundamental to realizing rights of citizenship and belonging (Bradley, 2013; Long, 2013).

To promote refugee repatriation, international policymakers have innovated a number of programs that directly incentivize return through cash and in-kind transfers. These material inducements are intended to subsidize refugees’ journeys home and ease their reintegration into origin communities upon return.³ More than 30 countries, from Australia and Germany to Ghana and Tanzania, operate repatriation assistance programs (Table A-1). In April 2025, the U.S. became the most recent country to pursue incentivized return, when officials introduced a \$1,000 credit for migrant “self-deportation” (Sanchez and Elba, 2025). International humanitarian organizations like the International Organization for Migration (IOM) and the UN Refugee Agency (UNHCR) also administer repatriation assistance programs. In 2024, IOM staff gave material inducements to more than 81,000 returnees across 121 host countries (Figure A-2). The overwhelming share of refugee beneficiaries of repatriation assistance programs return to fragile, low-income origin countries like Afghanistan, Iraq, Somalia, Mali, and Nigeria (IOM, 2025). Ongoing conflicts and institutional weakness render these returnee-receiving states highly vulnerable to economic shocks and regional instability.

Despite billions of dollars spent to incentivize refugee repatriation, cash-for-return programs have received little systematic evaluation. This represents an important gap given the stark ethical questions surrounding these programs, and the potential for significant, unintended consequences of large-scale refugee return to fragile and conflict-affected origin countries. Proponents of assisted repatriation argue that these programs are critical for enabling destitute refugees to realize their return intentions and safely reintegrate into origin communities (Ruben, van Houte and Davids, 2009). Others hail assisted return as a cheaper and less coercive tool for migration control (Kalicki, 2020), or a “humane alternative to deportation” (IOM, 2011). Opponents counter that incentivized return is unjust if refugees’ only accept compensated repatriation to avoid punitive alternatives like immigration detention (Scalettaris and Gubert, 2019). The crux of the issue is that incentives can degrade the voluntariness of refugees’ choices to repatriate. Further normative issues arise if refugees are not informed of the risks of returning, if material inducements are insufficient to guarantee program-induced returnees’ safe reintegration, or if host states coerce refugees to accept incentivized return offers (Gerver, 2017).⁴

At the center of normative and policy debates over repatriation assistance is the question of how incentivized returns affect security and peacebuilding in returnee-receiving communi-

³Host country policymakers may hold other motivations when implementing incentivized repatriation policies, like xenophobic desires to reduce migrant-hosting (Sanchez and Elba, 2025).

⁴We give a more comprehensive survey of normative debates over incentivized repatriation in section A.3.

ties. Does large-scale, assisted refugee repatriation enflame or dampen conflict and promote or degrade social cohesion?⁵ To answer this question, we consider a repatriation cash assistance program for Afghan refugees in Pakistan, administered by the UNHCR in 2016.⁶ Motivated by [Rozo and Vargas \(2021\)](#), we identify the causal effect of encashed returns by exploiting the program’s quasi-random timing and historical returnee settlement patterns in a difference-in-differences framework. Our findings represent the first plausibly causal evidence on the consequences of incentivized repatriation for conflict in returnee-receiving communities. In addition to examining the effects of assisted returns on violence, we offer novel insights into economic incentives for combatant mobilization and the ways informal institutions shape refugee reintegration.

UNHCR officials announced the sudden, unanticipated doubling of cash grants for Afghan refugees repatriating from Pakistan in late June 2016. The magnitude of the grant was large, representing several months of rent or wages. Using a combination of observational and survey-based measures, we demonstrate that the introduction of the cash-for-repatriation policy led to a surge in refugee returns from Pakistan to Afghanistan. The scale of repatriation following the intervention was unparalleled: more than 363,000 refugees returned under the program, representing 66% of documented repatriates worldwide in 2016. The timing of the cash transfer—during Afghanistan’s annual fighting season—offers a unique opportunity to explore how assisted returns affected violence. Every summer during the war in Afghanistan, Taliban commanders mobilized a large force of temporary fighters. These part-time recruits were chiefly income-motivated, and supported professional militant cadres in major offensives ([Malkasian, 2021](#)). Impoverished Afghan returnees from Pakistan historically formed an important source of these recruits ([Harpviken and Lischer, 2013](#); [Lakhani and Amiri, 2020](#)). Mass repatriation during the fighting season therefore creates ripe conditions for accelerating insurgent violence. However, since each member of repatriating households received cash under the repatriation assistance program, the return of each family represented

⁵Disentangling the effects of refugee return on conflict is a thorny empirical challenge, and must confront two inferential hurdles. First, data on repatriation varies starkly in quality across contexts. During complex crises, field staff face logistical challenges in tracking migrants across peripheral regions. Complicating measurement further, returnees may conceal their identities due to safety concerns. Second, when and where returnees flee is directly linked to security conditions in prospective destination communities ([Ghosn et al., 2021](#)). Because returnees often eschew return as a result of ongoing ([Alrababa’h et al., 2023](#)) or past violence ([Camarena and Hägerdal, 2020](#)), few quantitative studies have been able to credibly estimate the effects of repatriation on subsequent conflict.

⁶Rolled out amidst the world’s most enduring displacement crisis, this program remains the largest incentivized repatriation policy ever implemented.

a meaningful stimulus to the local economy in returnee-receiving communities. In line with recent evidence on the pacifying impacts of refugee aid (Taylor et al., 2016; Lehmann and Masterson, 2020) and integrative policies (Couttenier et al., 2019), and broader evidence on the violence-reducing effects of development programming (Rohner, 2024; Beath, Christia and Enikolopov, 2025), we find that encashed returns reduced militancy.

We probe several mechanisms underpinning this result. First, inter-household, intra-communal economic spillovers from returnees to non-migrants (i.e., stayees) in impacted areas may have raised reservation wages, increasing the premium on part-time rebel mobilization. Consistent with this mechanism, we find that insurgents substituted from labor-intensive combat into capital-intensive attacks (which require fewer recruits) during the program. This composition shift in violence was also associated with improving insurgent lethality, implying a stark, negative welfare consequence. In addition, the community-level economic boon from encashed returns may have constrained counterinsurgent tip-buying by raising the price of information. We provide suggestive evidence of this dynamic. Program exposure reduced the rate of counterinsurgent bomb neutralizations, which are highly sensitive to civilian informing (Sonin and Wright, 2022).

We also use the policy intervention to study consequences of refugee return for social conflict. Existing research highlights property disputes as a crucial source of tension between returnees and hosts (Schwartz, 2019; Ruiz and Vargas-Silva, 2022). In Afghanistan, repatriation has sparked honor feuds and clashes over land rights (Murtazashvili, 2016). Exposure to this violence has important welfare implications, increasing risk-taking and discounting (Voors et al., 2012; Callen et al., 2014), undermining trust (Ruiz and Vargas-Silva, 2025), and exacerbating poverty and trauma (Joireman, 2018). Using unique survey and event data on relations between returnees and stayees, we find that social conflict increased in communities exposed to policy-induced return. We also explore how social capital and informal dispute resolution mechanisms help offset risks of communal violence. We develop novel measures of proximity to kin and the accessibility of village shuras in returnee-receiving communities. Social networks and strong, local institutions dampen communal conflict following repatriation.

Our study makes three important contributions. First, we provide new evidence on the risks associated with refugee return induced by a large-scale economic intervention. As governments and international organizations scale repatriation assistance policies, it is critical to examine the downstream economic, political, and social impacts of these policies. Doing so is essential for understanding the humanitarian stakes, normative contours, practical ef-

fects, and unintended consequences of cash-for-return programs, and for informing debates over whether and how policymakers should facilitate refugee repatriation to fragile origin countries. While ours is not a full program evaluation, we provide evidence on one key aspect of incentivized return—how it shapes violence in returnee-receiving communities.⁷ Understanding this aspect of the broader policy marks an important first step in evaluating the program’s larger welfare implications.

Although we find that encashed returns are associated with a decline in insurgent violence overall, our results are not particularly sanguine. The incentivized repatriation program caused a composition shift in militant attacks, leading rebels to substitute labor-intensive gunfights for capital-intensive bombings. Increasing reliance on explosive tactics drove an overall increase in insurgent lethality, implying the program boosted Taliban combat effectiveness. We also document a reduction in counterinsurgent effectiveness attributable to the repatriation cash program. The success rate of government bomb clearance missions declined in returnee-receiving communities, likely because encashment constrained government tip-buying. Improvised explosive attacks were the deadliest form of rebel violence for civilians in Afghanistan, so declining success of bomb neutralizations represents a stark negative consequence of the encashment program. Beyond militancy, we also find that incentivized repatriation heightened tensions between returnees and stayees, exacerbating communal violence and property disputes. Together, these results underscore substantial practical and ethical questions about the appropriateness of repatriation cash assistance programs. Incentivizing large-scale return to fragile origin countries risks undermining the voluntariness of return decisionmaking and exposing repatriates and their non-migrant neighbors to conflict. The international community has a strategic interest, a legal obligation, and a moral imperative to ensure return only occurs when conditions are safe.

Our second contribution is to the growing literature on return migration (e.g., [Bucheli, Fontenla and Waddell, 2019](#); [Ruiz and Vargas-Silva, 2021, 2022, 2025](#); [Bahar et al., 2024](#); [Godlonton and Theoharides, 2025](#)), and wider scholarship on the economics of displacement (e.g., [Boustan, 2010](#); [Couttenier et al., 2019](#); [Brell, Dustmann and Preston, 2020](#); [Bahar, Ibáñez and Roza, 2021](#); [Ruiz and Vargas-Silva, 2022](#)). While this work carefully interro-

⁷Our analysis abstracts from potential direct effects on household income, asset accumulation, investment decisions, or consumption, since we lack data (e.g., on wages) to test these outcomes. Our objective is to identify whether and how a financial transfer accompanying refugee return shaped local equilibrium dynamics related to security and social stability, not to benchmark overall program performance or cost-effectiveness. Future work using alternative data and research designs should examine the direct economic effects of repatriation cash assistance on household welfare, as well as other downstream impacts on health, education, political participation, and women’s empowerment.

gates the ways migration and return shape security and development in migrants’ origin and destination communities, the extant empirical record is mixed. Reconciling existing findings requires disentangling diverse theoretical mechanisms and probing the ways displacement interacts with economic, institutional, and policy factors. We begin to fill this gap by studying an important and costly economic intervention intended to incentivize refugee repatriation, and by leveraging a quasi-experimental design. In so doing, we offer a quantitative micro-foundation for future studies of repatriation policy. Credibly identifying the downstream consequences of repatriation is crucial given the prevailing view that return is the best available solution to forced displacement.

Our third contribution is to scholarship on the political economy of violence. Although canonical work recognizes a diversity of events that occur within substate conflicts (e.g., [Berman, Shapiro and Felter, 2011](#); [Berman et al., 2017](#); [Fetzer et al., 2021](#); [Bazzi et al., 2022](#)), ranging from anti-government attacks and state repression to warlordism and social conflict, these outcomes tend to be studied in isolation. The prevailing focus on specific forms of violence limits our ability to craft generalized theories of conflict. Our empirical investigation explicitly considers how varieties of violence relate to mass repatriation. By distinguishing conflict tactics, we gain a window into wartime civilian agency, as well as economic incentives for combatant mobilization, and the pacifying effects of social and political institutions. We document divergent effects of refugee return on different forms of conflict, highlighting the unique and nuanced ways displacement shocks, resources, social capital, and local institutions interact to shape wartime violence in its various forms. This paper deepens our understanding of these dynamics in a highly relevant context, and provides actionable insights about how to improve future efforts to help displaced people return home.

2 The Consequences of Return Migration

Our inquiry extends a growing literature on the consequences of return migration for security and social cohesion in returnee-receiving communities. Findings across this literature are mixed, and generalizing from prior work is difficult given the diversity of contexts, heterogeneous composition of returnees, and variation in policy environments. In some settings, repatriation has fueled economic prosperity ([Bucheli and Fontenla, 2025](#)), productivity and employment growth ([Hausmann and Nedelkoska, 2018](#); [Bahar et al., 2024](#)), institutional rejuvenation ([Chauvet and Mercier, 2014](#); [Müller-Funk and Fransen, 2023](#)), and infrastructural investment ([Kibreab, 2002](#); [Davies et al., 2011](#))—effects that can dampen conflict and

foster social accord between returnees and their non-migrant neighbors (Bucheli, Fontenla and Waddell, 2019). In other cases, returns have worsened violence by intensifying competition over land, jobs, and housing (Allen and Morsink, 1994; Schwartz, 2019; Ruiz and Vargas-Silva, 2021, 2022, 2025), entrenching intergroup divisions (Joireman, 2017), straining fragile state institutions (Scalettari and Gubert, 2019; Ambrosius and Meseguer, 2020), and bolstering rebel recruitment (Lischer, 2011; Schultz, 2011; Haer and Hecker, 2018). Countervailing economic, institutional, structural, and individual mechanisms shape these processes and condition the effects of repatriation on different forms of conflict.⁸

Considering the impact of cash assistance layered atop these underlying dynamics complicates the picture further. Repatriation assistance may stimulate local economies and ease returnee reintegration, mitigating conflict and fostering social cohesion (Ruben, van Houte and Davids, 2009; Kamminga and Zaki, 2018). If positive externalities of repatriation assistance are sufficiently large that they spillover from returnees to stayees, rising community-wide living standards can dampen militant mobilization and communal tensions (Kibreab, 2002; Harild, Christensen and Zetter, 2015). Yet, encashment can also act as an accelerant of violence if armed groups extort assisted returnees to finance their operations, or if stayees, resentful of repatriates’ access to aid, engage in anti-returnee discrimination (van Houte, 2014; Schwartz, 2019). Given these offsetting channels, it is not possible to a priori predict which mechanisms will dominate. The question of how cash-for-return policies affect conflict is therefore, ultimately, an empirical one. For exposition, we outline key theoretical channels that inform our empirical investigation in appendix section A.4.

3 Background

Afghanistan’s modern history is defined by cycles of displacement and return. Since 1979, no country has produced more refugees and asylum-seekers. Pakistan has been their primary host. The population of Afghan refugees in Pakistan has been among the world’s six largest refugee populations every year since 1980. For most of the past four decades, Afghans in Pakistan have formed the world’s single largest displaced population. In appendix figure A-3, we plot displacement and return flows between Afghanistan and Pakistan. Intuitively, migration patterns track major conflict trends. Mass outflows of Afghan refugees to Pakistan followed

⁸Tables A-2 – A-3 offer a more detailed mapping of pathways identified in prior scholarship. As Ruiz and Vargas-Silva (2022, p. 696) note, “there is not a single mechanism related to refugee return that can cover all aspects of a complex concept such as [conflict and] social cohesion.”

the 1979 Soviet invasion, the 2001 U.S. invasion, and the 2006 Taliban resurgence. Brief moments of stability, such as the 1992 collapse of the Soviet-backed communist government and the 2001 toppling of the Taliban, have precipitated returns from Pakistan.

We study a sudden, exogenous increase in returns from Pakistan to Afghanistan in 2016, following the unanticipated expansion of UNHCR’s repatriation cash assistance program. Since 2002, UNHCR officials have offered small, one-time grants to Afghan returnees to facilitate repatriation.⁹ Prior to the implementation of the expanded cash assistance policy we study, returnees received \$200—an amount UNHCR staff deemed just sufficient to cover the costs of transportation and essentials during returnees’ journeys home. On June 23, 2016, while visiting Pakistan, UNHCR Commissioner Filippo Grandi announced that repatriation cash assistance for Afghan returnees from Pakistan would be doubled—with grants increasing from \$200 to \$400 per person. Six days later, UNHCR staff began disbursing payments at this higher rate. At the time the program was announced, nearly 1.6 million Afghan refugees resided in Pakistan.¹⁰ This entire population was eligible for repatriation assistance—any Afghan refugee who registered with UNHCR prior to returning from Pakistan could receive the grant.¹¹ The encashment program operated from June 29 – December 7, 2016, when UNHCR declared a winter pause in assisted returns. UNHCR officials initially planned to continue the program in 2017, but budget constraints prevented renewal. When assisted returns resumed on April 3, 2017, grants reverted to the pre-program level.

Process Assisted returns followed a standardized, closely-monitored sequence, which we describe in detail in appendix section A.8. This process and the administrative infrastructure that enabled encashment were well established before 2016. First, Afghan refugees wishing to return had to approach a UNHCR Voluntary Repatriation Centre (VRC) in Pakistan. Two VRCs operated in Baleli (Quetta) and Chamkani (Peshawar); a third opened at Azakhel (Peshawar) in September 2016 to help process the mass influx of intended returnees during the encashment program. At VRCs, refugees were deregistered from the Pakistani refugee management system, and were issued Voluntary Repatriation Forms (VRFs). All returning

⁹We plot the evolution of UNHCR repatriation cash assistance grants for Afghan returnees in Figure A-5. The size of the 2016 grant expansion was unparalleled.

¹⁰This represents the number of Afghan refugees holding Proof of Registration (PoR) cards issued by the Pakistani government.

¹¹Refugees who first registered with UNHCR are called “documented” or “assisted.” Refugees who returned without UNHCR facilitation are considered “undocumented” or “spontaneous.” Spontaneous returnees could not receive UNHCR cash assistance, though a small number received supplemental assistance (\$20-50) from IOM staff at border crossings.

family members had to be present at a VRC to declare their return, and a biometric scan was conducted to prevent duplicate claims. Once deregistered, returnees lost legal status in Pakistan, but were permitted to travel under UNHCR supervision to designated border crossings at Torkham, Chaman, or Kharlachi. Upon entry into Afghanistan, all documented returnees were directed to report to one of three Encashment Centres (EC) in Kabul, Kandahar, or Nangarhar. To ensure timely return, UNHCR required that repatriates arrive at an EC within seven days of having been issued a VRF in Pakistan.¹² At ECs, UNHCR verified each VRF, conducted brief interviews, and delivered cash payments. Following encashment, returnees were free to leave the EC and repatriate to their intended destination communities.

Economic Significance and Behavioral Response The size of UNHCR’s expanded repatriation assistance package was economically meaningful (Table A-4). Under the revised policy, each documented returnee received \$400—equivalent to 79% of Afghan GDP per capita, 1.3 months of wages as a Taliban recruit, and at least 2.5 months of licit-sector wages. For the median returnee household, this amounted to an average cash grant of \$3,600.¹³ With three working- or recruitment-age males in the typical returnee household, this level of cumulative cash assistance was equivalent to combined income from 8–16 months of employment in the licit economy, or an entire fighting season of potential income from temporary Taliban mobilization.¹⁴ Measured against local housing costs, the median household’s grant was sufficient to cover about 15 months of rent.

Figure 1 shows that refugee returns from Pakistan to Afghanistan increased sharply in response to the encashment program. In the first half of 2016, Afghanistan received 40,769 returnees from Pakistan, including 6,875 assisted and 33,894 spontaneous returnees. In the second half of 2016, when grants were doubled, 577,387 Afghans repatriated from Pakistan, including 363,227 assisted and 214,160 spontaneous returnees.¹⁵ Historically, spontaneous returns have exceeded documented (i.e., assisted) returns in Afghanistan because the process of

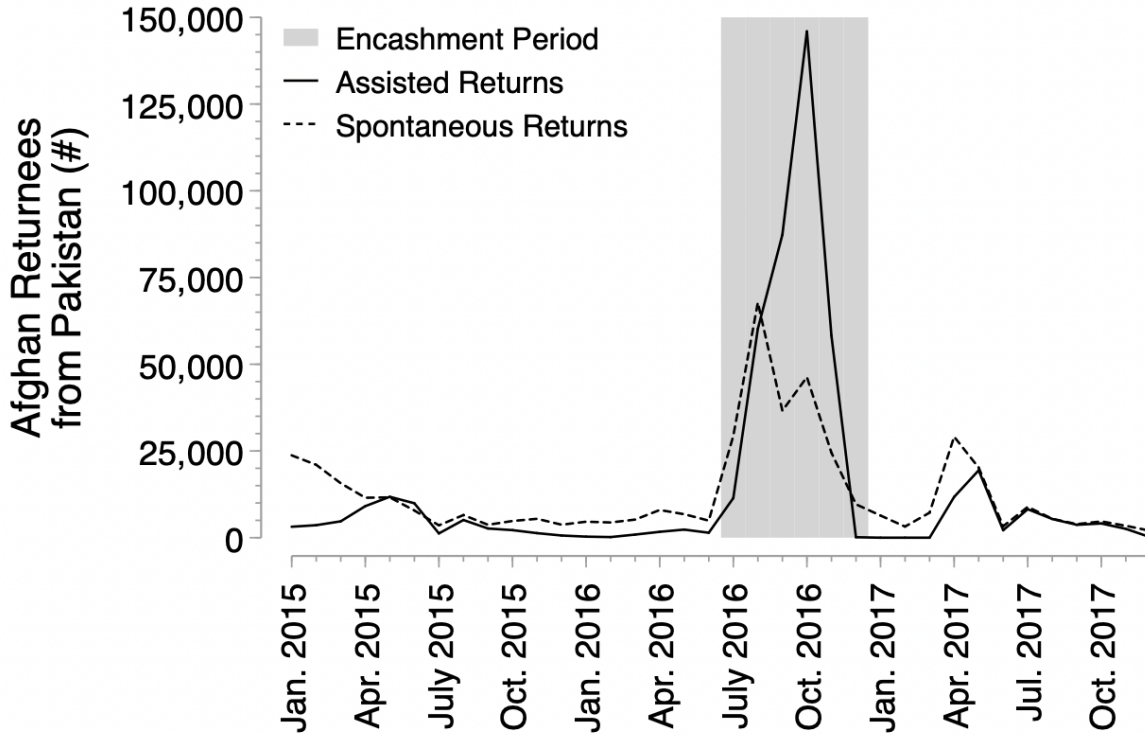
¹²Returnees who failed to arrive at an EC in the requisite seven day period would not receive cash assistance, though exceptions were made for groups who experienced death or serious illness of a family member between the time of VRF issuance and EC arrival. No cash assistance was ever provided to returnees arriving more than 15 days after VRF issuance.

¹³The median size of each returnee household in 2016 was 9. Observed household sizes ranged from 1–643. Most returnee households consisted of immediate family members, but occasionally extended families or entire communities relocated. 70% of returnees were women (any age) or male children (under 18).

¹⁴Working- and recruitment-age males are those aged 12–64 years old. Returnee women had very low labor force participation ($\leq 13\%$) (Haroon et al., 2019). Table A-4 benchmarks average licit and Taliban wages.

¹⁵Put differently, assisted returns increased 5183% during the program, relative to the pre-program period.

Figure 1: The Encashment Program Triggered Mass Refugee Return



Note: The solid line shows assisted (documented) refugee returns from Pakistan to Afghanistan. The dashed line shows spontaneous (undocumented) refugee returns from Pakistan to Afghanistan. Assisted returnees received cash, while spontaneous returnees did not. Gray bars denote the encashment period. *Sources:* UNHCR provides comprehensive information on documented returns. We use reports filed by IOM officials to reconstruct country-wide, monthly flows of undocumented returnees into Afghanistan.

assisted return is slower and more bureaucratically complex (Harild, Christensen and Zetter, 2015, p. 66).¹⁶ This trend reversed under the encashment program, suggesting a deliberate effort of returnees to repatriate through the formal channel in order to take advantage of cash assistance. We use data from a survey of Afghan returnees, described in greater detail below, to further validate that the encashment program spurred refugee return. We cannot directly observe which survey respondents received the incentivized repatriation grant in 2016, but use information on respondents' countries of asylum, return timing, and registration status to identify likely program beneficiaries. Encashment-eligible respondents were significantly more likely to report financing repatriation through UNHCR cash assistance (Table A-5).

¹⁶During the encashment program, UNHCR rapidly scaled logistical capacity to reduce cumbersome administrative processes and expedite return facilitation (UNHCR, 2016b).

The spike in spontaneous returns during the first two months of the encashment program mainly reflects information scarcity.¹⁷ Refugees in Pakistan lived in an information-poor environment: media penetration, literacy, and schooling were low (Haroon et al., 2019). Word-of-mouth was the main way Afghans learned about repatriation opportunities: before the 2016 cash-for-return program, UNHCR found that 84% of Afghan refugees either had no knowledge of the voluntary return process, or learned about it through rumor networks (UNHCR, 2016a). Thus, though the program was announced suddenly on June 23, 2016, information about eligibility and procedures spread slowly, reaching many prospective returnee households only weeks later. In August 2016, UNHCR launched “mass and targeted information campaigns” to publicize how returnees could access assistance (UNHCR, 2016b).¹⁸ Qualitative UNHCR surveys suggest these efforts worked: the share of refugees learning about repatriation from media or official channels rose from 5–24% during the program, helping explain why undocumented returns declined from September 2016 (UNHCR, 2017). Accounting for these spontaneous returns does not impact our results.

Motivation Understanding the genesis of the 2016 encashment program is important for evaluating its exogeneity and broader ethical appropriateness. Officially, UNHCR framed the 2016 policy as a humanitarian enhancement. However, the implementation decision ultimately resulted from more than a year of secret negotiations between UNHCR leadership and the Pakistani government. As described in greater detail in appendix section A.11, the policy’s proximate cause was Pakistani political pressure to accelerate refugee repatriation and thereby shrink Pakistan’s refugee-hosting burden. Ultimately, the roots of this pressure lie in Pakistani domestic politics: a school massacre perpetrated by Tehrik-i-Taliban Pakistan (TTP) militants in December 2014 spurred a surge in popular, anti-Afghan xenophobia.¹⁹ In response, Afghan refugees in Pakistan reported retributive evictions, job losses, and police harassment (Bjelica, 2016). Pakistani politicians issued public calls for forced repatriation, and repeatedly threatened not to renew refugees’ Proof-of-Registration (PoR) cards, which governed access to work, mobile phones, and other basic services. Rising anti-refugee sentiment gained particular salience during Pakistan’s 2015 local elections.

¹⁷Section A.9 gives a more detailed explanation.

¹⁸The main campaign, “Khpal Watan, Gul Watan” (“My country, my beautiful country”), advertised in refugee-populated areas of Pakistan.

¹⁹The TTP is also known as the Pakistani Taliban. Their relationship with the Afghan Taliban is fraught, and the latter often condemn TTP violence against Pakistani targets. We refer to the Pakistani Taliban as TTP and the Afghan Taliban as the Taliban.

For UNHCR, securing continued legal status for Afghan refugees in Pakistan was a top priority. Amidst Pakistani threats, UNHCR initiated high-level, secret negotiations with the Pakistani government in 2015.²⁰ Importantly, these negotiations were conducted at the diplomatic level, and bore no relationship to conflict trends inside Afghanistan. As negotiations stalled, Pakistani officials briefly allowed PoR cards to lapse in early 2016. After emergency UNHCR intercession, PoR cards were quickly renewed, but the episode lent new urgency to negotiations. Critically, Pakistan’s renewal of PoR cards in January 2016 bore a firm expiration deadline—June 30, 2016. In the face of Pakistani intransigence, and with this looming deadline, UNHCR agreed to expand repatriation cash assistance to incentivize returns and satisfy Pakistani government calls for a reduction in Afghan refugee-hosting. The repatriation assistance program was publicly revealed on June 23, 2016, one week before the slated expiration. In exchange, Pakistan renewed PoR cards through 2016.

Throughout the pre-program negotiating period, Pakistani officials engaged in anti-refugee coercion as a lever for pressuring UNHCR. This targeted coercion continued at lower levels after the encashment program began.²¹ Afghan refugees in Pakistan faced extortion, rent hikes, and arbitrary increases in fees for access to documents and services throughout summer 2016 ([Human Rights Watch, 2017](#), p. 18-27). This abuse was economic in nature, and likely calibrated to encourage refugee uptake of the repatriation assistance program, as well as to capitalize on the unexpected windfall in returnee support. Concurrent coercion in Pakistan raises natural concerns about identification. To this end, we provide multiple pieces of evidence. First, we formalize our design with a directed graph, which clarifies key parameters of interest (Figure A-8). Second, in Tables A-6 – A-7 we present survey evidence on returnees’ self-reported reasons for return. We find no evidence that program beneficiaries were more likely to report repatriating because of repressive conditions in Pakistan.²² Third, in section A.12 we highlight qualitative evidence that underscores the importance of cash assistance in enabling repatriation ([Human Rights Watch, 2017](#)).

²⁰The Pakistani negotiating position was normatively problematic: incentivized repatriation was offered purely as an alternative to mass deportation. From UNHCR’s perspective, the policy could be ethically defensible if organizational leadership, having exhausted all channels for opposing anti-refugee restrictions, opted instead to mitigate harm through repatriation cash assistance ([Gerver, 2017](#), p. 639). See also section A.3.

²¹Figure A-7 plots the number of Afghans deported by Pakistan each month, a proxy for broader anti-Afghan coercion ([Human Rights Watch, 2017](#); [Seefar, 2019](#)). The level of deportations was lower during the encashment program than in the 18 months preceding the program.

²²The survey did not ask directly about encashment as a pull factor. However, we also find that program beneficiaries were more likely to report repatriating because of improved economic conditions in Afghanistan (Table A-7), a fact likely attributable to encashment.

4 Data

In this section we describe our microdata. Summary statistics for all variables are described in Tables B-8 and B-9. Variable definitions and sources are discussed in appendix C. Details on data collection and related ethical considerations are described in appendix D.

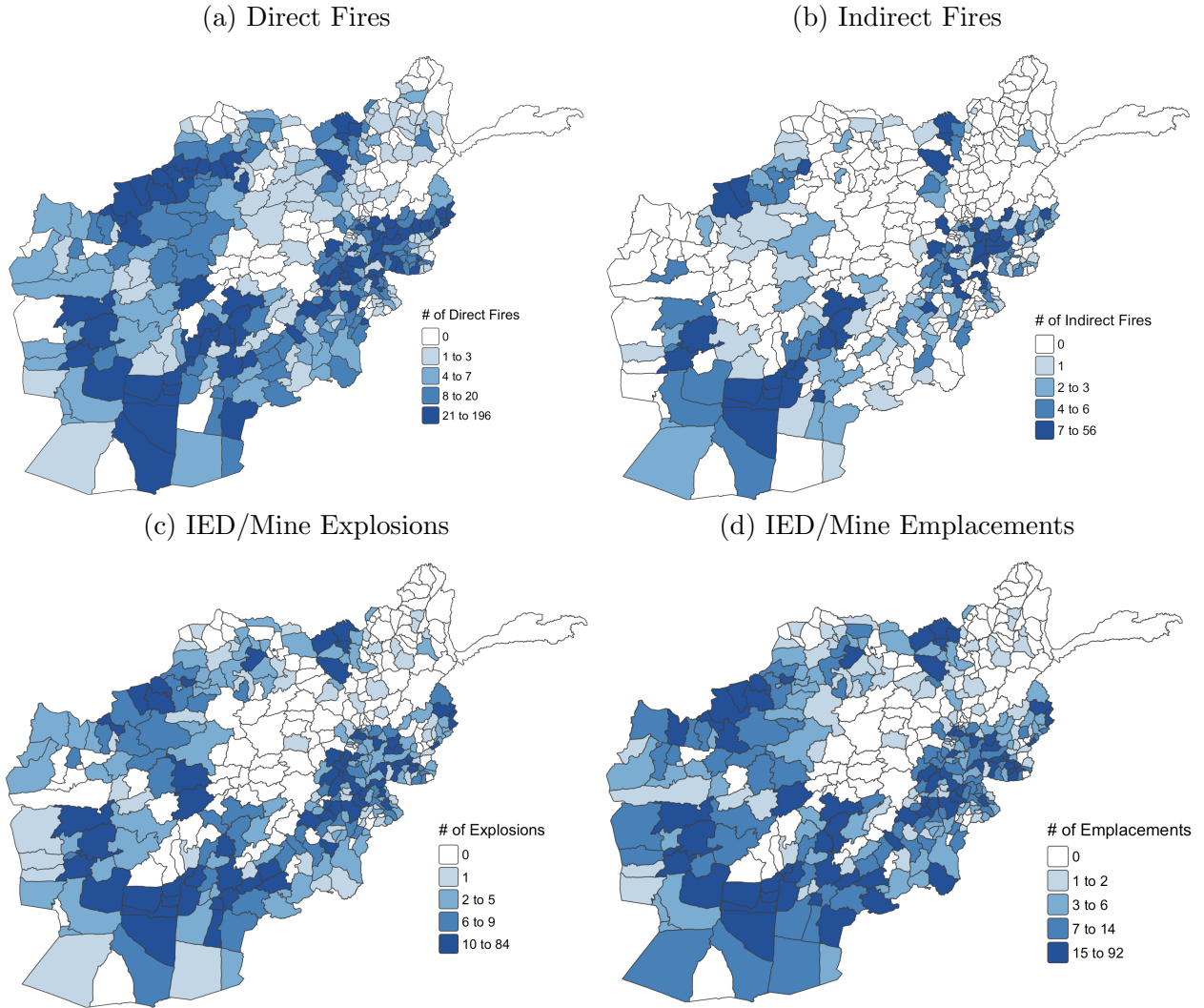
Combat Records Our investigation exploits newly-released, sensitive but unclassified military records from the International Distributed Unified Reporting Environment (INDURE), which catalog insurgent and counterinsurgent combat engagements in Afghanistan between 2015–2017. The INDURE platform was populated using detailed significant activity (SIGACT) reports logged by Afghan security forces and NATO troops, and offers extremely high resolution. Data are time-stamped down to the minute, geo-referenced down to the meter, and include additional details about the event type, units involved, and casualties. Section D.2 offers additional details about the data collection process. These data are most directly comparable to data described in Condra et al. (2018) and Fetzer et al. (2021). However, whereas extant combat records from Afghanistan cover the period from 2006–2014, our records cover the period from 2015–2017. The records we draw on offer a substantial improvement in coverage and precision over media-based collection efforts, representing the virtual universe of anti-government violence.

To understand how refugee return shapes conflict, we focus on insurgent-initiated attacks against Afghan security forces or their NATO partners. The detailed nature of our conflict microdata allows us to track several types of insurgent activity, including direct fire, indirect fire, and explosive hazards. Direct fire attacks are line-of-sight, close combat operations, like frontal assaults on convoys or patrolling troops. Indirect fires consist of rocket and mortar attacks perpetrated at long-range. Explosive hazards include improvised explosive devices (IEDs) and landmines.²³ We depict the spatial distribution of these attacks in Figure 2.

Survey of Returnees Our military records offer a comprehensive portrait of combat between insurgent and counterinsurgent forces. However, these data do not track social conflicts occurring between civilians, tribes, or other groups. We draw on rich survey data to understand communal violence and returnee relations with their non-migrant neighbors. Specifically, we leverage individual-level data from the Survey of Afghan Returnees, fielded

²³We focus on detonated rather than emplaced hazards in the main specifications because explosives can only achieve intended effects when they are successfully detonated. Results are substantively similar when we study emplaced hazards.

Figure 2: Insurgent Violence During the Encashment Program



Note: Panels shade districts by the intensive margin of insurgent violence during the encashment program (July–December 2016). Legend bins reflect a zero category and quartiles of the nonzero distribution.

in two waves in 2018-2019 by the Asia Foundation. The survey was contracted by the United States Agency for International Development (USAID) and implemented by the Afghan Center for Socio-Economic and Opinion Research (ACSOR), a subsidiary of the international firm D3. ACSOR hired and trained local enumerators in household and respondent selection, including lessons on how to correctly record answers to questions, culturally-sensitive interview methods, and secure storage of contact information. Face-to-face interviews were conducted by gender-matched enumerators in 65 districts in Kandahar, Nangarhar, Kabul,

Balkh, and Herat Provinces. Section D.1 offers additional details about the data collection process. Together, sampled districts received 57% of all assisted refugee returnees during the encashment program. We use information on respondents’ country of asylum, date of repatriation, and registration status to triangulate likely program beneficiaries.

To measure community relations between returnees and their non-migrant neighbors, we construct a multi-item index using inverse covariance-weighting (Anderson, 2008).²⁴ This index combines responses to six questions about returnees’ local relations since their return to Afghanistan, including: experiences of (1) disputes and (2) discrimination; whether neighbors (3) invite returnees to community events; and perceptions that neighbors are (4) helpful, (5) respectful, and (6) friendly. Together these items capture multiple dimensions of returnees’ community ties, ranging from overt experiences of local violence to subjective feelings of social exclusion (Ruiz and Vargas-Silva, 2022).²⁵ This approach has limitations—namely, our survey-based measure of communal conflict is from respondent self-reports captured two years after the encashment program. To build confidence in our findings, we therefore also develop a measure of communal violence based on conflict event data. Using incident files from the Afghan National Police Command Center and embassy-contracted security firms, we sum reported disputes and tribal feuds across district-months from 2016–2017.²⁶ We confirm the robustness of our survey-based results using this supplemental measure.

We consider a range of additional survey measures that allow us to probe heterogeneity or otherwise bolster our empirical strategy. Using items related to social capital and returnees’ neighborhood composition, we build an index reflecting whether returnees resided in proximity to their familial kin or other repatriates. Scholarship on returnee reintegration underscores the important role kinship and migrant social networks play in easing communal tensions (Schwartz, 2019), and this measure allows us to test the moderating role of cohesion on strife in returnee-receiving communities. Other survey-based outcomes enable us to validate key program details. For instance, we document that program beneficiaries were more likely: (1) to report financing return through UNHCR assistance (Table A-5); and (2) to report returning because they perceived improving economic conditions in Afghanistan (Table A-7), an effect likely attributable to encashment.

²⁴Results are substantively similar using principal component analysis.

²⁵We plot mean returnee-stayee relations across districts in Figure B-9.

²⁶We describe this measure in greater detail in section B.3.

Refugee Returns We supplement our conflict measures with granular data on assisted refugee returns provided by the UNHCR-Afghanistan office. For each group of returnees repatriating with UNHCR facilitation, we observe the date of return, country of asylum (Pakistan, Iran, or elsewhere), group size and gender composition, and geographic details, including province and district of origin, as well as intended province and district of return. This data was gathered by UNHCR staff during interviews with assisted returnees at Encashment Centres in Afghanistan. Comprehensive data on assisted returns are available for 2013-2014 and 2016-2019. We focus on returns around the cash program (2016), but leverage geographic settlement patterns of earlier returnees (2013) to predict the destinations of these later returnees, as we detail below. While granular data on assisted returns are not available for 2015, we use UNHCR reports to reconstruct the country-wide, monthly flow of returnees from each asylum country in 2015.²⁷ Figure 3 depicts the destination districts of assisted returnees from Pakistan to Afghanistan during the encashment program. There was strong gravitation toward origin communities: 58% of program-induced repatriates returned to their origin district, and 70% returned to their origin province (Figure B-11).

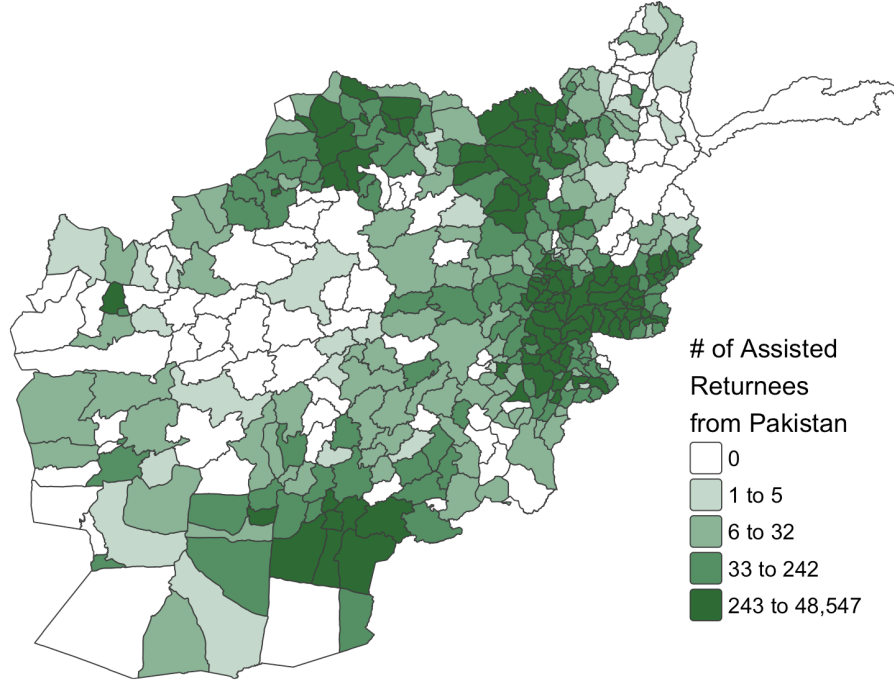
5 Empirical Strategy

To identify the causal effect of incentivized repatriation on conflict, we combine historical returnee settlement patterns and the unexpected timing of the 2016 UNHCR repatriation assistance program. This approach resembles the identification strategy used in [Camarena and Hägerdal \(2020\)](#) and [Rozo and Vargas \(2021\)](#), who also leverage temporal shocks to estimate the effects of migration. Our measure of exposure to repatriation during the encashment program is analogous to a Bartik or *shift-share* instrument, where cross-sectional variation is interacted with an otherwise exogenous time-series shift ([Goldsmith-Pinkham, Sorkin and Swift, 2020](#)). In our setting, the *shares* are district-level proportions of returnees from Pakistan in 2013, while the *shift* is the repatriation cash assistance program implemented from July–December 2016. Because there is a single national shift applied to one cross-section of historical shares, our design yields a single shift–share instrument, and is therefore exactly identified.²⁸

²⁷The absence of disaggregated data on 2015 returns is due to clerical error. Links to the 2015 data shared by UNHCR redirect to the 2019 returnee file.

²⁸We note that [Jaeger, Ruist and Stuhler \(2018\)](#) identify a potential concern with shift-share designs in the immigration literature: when the same baseline shares predict repeated waves of immigrant inflows over time, contemporaneous instruments can capture lingering adjustment to past shocks, confounding short- and long-

Figure 3: Observed Repatriation During the Encashment Program



Note: The map shows destination districts of encashed returnees. Districts are shaded by the total number of assisted (i.e., encashed) returnees from Pakistan who repatriated to the district during the encashment period. Legend bins reflect a zero category and quartiles of the nonzero distribution.

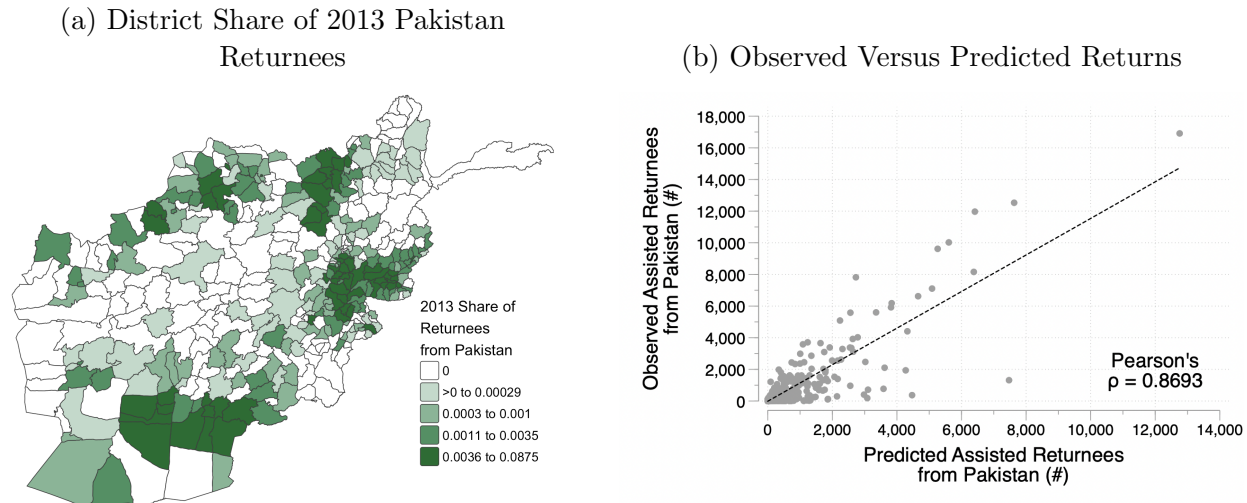
Constructing Exposure to the Repatriation Shock To measure historical resettlement patterns, we rely on the earliest available district-level returnee data provided by UNHCR—in 2013. Formally, we define each district’s share as:

$$S_d = \frac{\text{District Returns}_{2013}}{\text{Total Returns}_{2013}}$$

We depict these 2013 shares in the left panel of Figure 4. Multiplying s_d by the total, national-level monthly inflow of assisted returnees gives a measure of predicted repatriation at the district-month. As reflected in the right panel of Figure 4, predicted and observed returns are highly correlated (Pearson’s $\rho = 0.869$), suggesting that 2013 returnee settlement

run effects. This critique does not apply to our setting because we analyze a single, discrete policy shock rather than a sequence of correlated shocks. The 2016 encashment program was a one-time intervention, and pre-2016 returnee flows occurred without enhanced cash assistance under different circumstances. Therefore, there are no overlapping adjustment dynamics from prior encashment waves that could bias our estimates. See also [Rozo and Vargas \(2021\)](#).

Figure 4: Predicting Repatriation Using Past Returnee Settlement Patterns



Note: Panel (a) depicts each district's share of the total number of assisted Afghan returnees from Pakistan in 2013. Darker shades indicate a greater share. Legend bins reflect a zero category and quartiles of the nonzero distribution. Panel (b) depicts the correlation between predicted and observed returns from Pakistan at the district-month.

patterns strongly predict 2016 returnee destinations.²⁹ By contrast, in Table B-10 we show that the baseline shares are not significantly correlated with most other baseline district characteristics in 2013, including insurgent violence. In our main specification, we estimate the reduced-form effects of returnee exposure on conflict. This approach is equivalent to an intent-to-treat design, mitigating concerns that actual returnee settlement patterns are endogenous to security conditions (Camarena and Hägerdal, 2020; Alrababa'h et al., 2023).

Baseline Specification

We start from a generalized difference-in-differences framework. As noted above, district-level shares of 2013 returnee inflows from Pakistan define cross-sectional exposure to 2016 encashed returns. Temporally, the UNHCR encashment program provides a time-series shock to repatriation from Pakistan. We identify the causal effect of refugee return on conflict by combining these features in the following reduced-form least squares equation:³⁰

²⁹This correlation is stronger than Roza and Vargas (2021)'s benchmark correlation (Pearson's $\rho = 0.741$) between observed and predicted Venezuelan migration to Colombia. Figure B-10 plots observed versus predicted returns from other host countries to Afghanistan in 2016.

³⁰Naive and instrumental variables estimates are substantively similar (Table 2). To mitigate additional concerns that past settlement patterns are themselves endogenous, we explore additional sources of cross-

$$Y_{d,t} = \delta(S_d \times P_t) + \alpha_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{d,t} \quad (1)$$

where d indexes districts, t indexes year-specific months, and u indexes UNHCR areas of operation.³¹ $Y_{d,t}$ are conflict-related dependent variables. S_d is the share of Afghan refugees returning from Pakistan to district d in 2013, relative to all returnees from Pakistan to Afghanistan in that year. We z -standardize 2013 shares for interpretability. P_t is an indicator for months during the encashment program (July–December 2016). δ is the coefficient of interest and captures whether the repatriation cash assistance program induced a differential shift in violence in districts more heavily exposed to encashed returns. Given the discrete temporal boundedness of the encashment policy, we focus on variation in 2016—the year the policy was implemented and completed.³² α_d , β_t , and $\gamma_{u \times t}$ are respectively district, year-specific month, and UNHCR area by year-specific month fixed effects. Together, these absorb time-invariant differences across districts, common time shocks affecting all districts, and time-varying shocks common to all districts within a UNHCR zone. X_d is a vector of pre-shock, district-level controls, which we interact with year-specific month fixed effects to account flexibly for pre-treatment heterogeneity in observables across districts. $\varepsilon_{d,t}$ are robust, district-clustered standard errors. Estimates are scaled using population weights.³³

Reduced-Form Estimand The coefficient δ measures the reduced-form, or intent-to-treat, effect of exposure to policy-induced returns. It captures the average change in conflict for more-exposed relative to less-exposed districts during the encashment period, and reflects how outcomes change *per unit increase* in predetermined exposure $s_d \times P_t$ (Callaway, Goodman-Bacon and Sant’Anna, 2024). Under a parallel-trends assumption for districts with different exposures, δ admits the standard difference-in-differences interpretation as an average level treatment effect on the treated, scaled by exposure intensity.

sectional variation in returnee exposure in Table B-19.

³¹UNHCR divides Afghanistan into eight areas of operation (Figure A-6).

³²We explore longer-run effects in Table B-17 and Figure B-13. We find some evidence for persistence, though effects are largest during the period of active cash distribution in 2016. Given effect persistence, including periods after completion of the cash grant program (e.g., in 2017) in our estimation sample will render our estimates conservative (Goodman-Bacon, 2021).

³³Two reasons motivate our decision to incorporate population weights. First, these address heterogeneous effects of treatment with respect to population. It is a stylized fact that refugee returnees flock to urban areas of their origin countries. In Afghanistan, roughly 25% of encashed returnees repatriated to Kabul. Population weighting captures this pattern. Second, population weights account for heteroskedasticity with respect to population (Solon, Haider and Wooldridge, 2015), improving precision of our estimates relative to unweighted results (Table B-26).

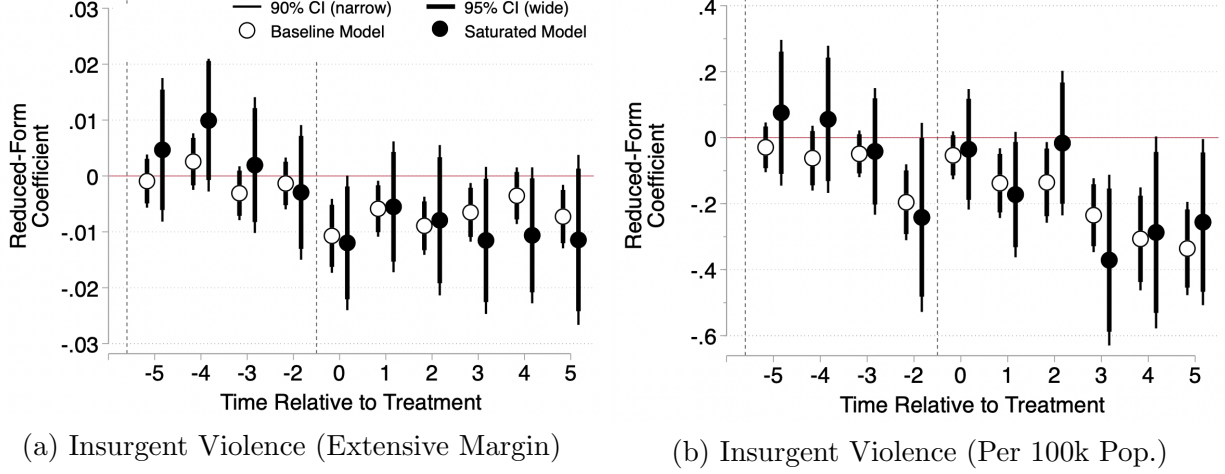
Indeed, the key identifying assumption for both the reduced-form and the instrumental-variables (IV) estimand below, is that, in the absence of the 2016 encashment program, districts hosting larger shares of returnees would have experienced common trends in violence. Differential trends could be driven by a number of factors, including anticipation of the policy. We expect that anticipation is unlikely, since the announcement of the grant increase in late June 2016 was sudden and unexpected, triggering a sharp, discontinuous increase in repatriation. Nonetheless, we follow the suggestion of [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#) and provide graphical evidence of parallel pre-trends in Figure 5. We illustrate these pre-program trends using the event-study method introduced in [Sun and Abraham \(2021\)](#), excluding two pre-policy periods. This design helps account for secular and potentially non-linear pre-trends ([Borusyak, Jaravel and Spiess, 2024](#)). Violence is consistently parallel in the pre-treatment period, except for one placebo estimate on per capita insurgent violence at time $t - 2$ in our parsimonious specification. This slight negative effect is likely estimation noise rather than evidence of a pre-trend, as the estimate at $t - 0$ reverts to near zero. To further assess pre-trends, in Figure B-12 we provide month-by-month estimates of the coefficient on the Bartik instrument centered by the pre-treatment mean, as in [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#). With this approach, none of the pre-treatment coefficients are significantly different from zero. We find significant treatment effects—with effect magnitudes consistent with our main results—in these more demanding dynamic specifications, even though the precision of the estimates is mechanically lower due to fewer observations per parameter when estimating month-by-month effects. These results build confidence in our strategy.

Instrumental Variables Estimand Because actual inflows $R_{d,t}$ may respond endogenously to local security conditions, we also estimate a two-stage least-squares specification that instruments observed returns with the interaction $S_d \times P_t$:

$$\begin{aligned} R_{d,t} &= \pi(S_d \times P_t) + \alpha_d + \beta_t + \gamma_{u \times t} + \nu_{d,t}, \\ Y_{d,t} &= \beta_{IV} \hat{R}_{d,t} + \alpha_d + \beta_t + \gamma_{u \times t} + \eta_{d,t}. \end{aligned} \tag{2}$$

Here $S_d \times P_t$ serves as a *shift-share instrument* for encashed returns. The first stage exploits the fact that districts with larger historical shares are more exposed to the policy-induced returnee inflows. The second-stage coefficient β_{IV} therefore has the standard IV/LATE

Figure 5: Dynamic Event Study Estimates of the Effect of Encashed Returns



Notes: Bars are 90 and 95% confidence intervals. Plots depict trends in insurgent violence, estimated with a two-way fixed effects event study specification. Outcomes are: (a) an indicator for whether any insurgent-initiated violence occurred in a given district-month; and (b) the total number of such events per 100,000 population within the district-month. White circles are estimates from a baseline model with district and year-specific month fixed effects. Black circles are estimates from a saturated model with parameters described in Table 1. Two pre-policy periods (January and June 2016), denoted by vertical gray dashed lines, are omitted. The x-axis indicates time relative to July 2016, when the program began. The horizontal red line denotes 0.

interpretation:

$$\beta_{IV} = \frac{\mathbb{E}[Y_{d,t}(1) - Y_{d,t}(0) \mid R_{d,t}(1) > R_{d,t}(0)]}{\mathbb{E}[R_{d,t}(1) - R_{d,t}(0) \mid R_{d,t}(1) > R_{d,t}(0)]},$$

that is, the *average causal effect of one additional encashment-induced returnee* on conflict among “complier” districts whose inflows rose because of the program. In the [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#) framework, Bartik IVs identify weighted averages of shock-specific causal effects; with a single aggregate shift as in our setting, this collapses to a single-shock effect.

Identification In the canonical [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#) framework, identification in shift-share designs hinges on the *exogeneity of the shares with respect to the shock-induced change in outcomes*. In our single-shift setting, this means that, conditional on fixed effects and pretreatment controls, historical settlement shares S_d are uncorrelated with the district-specific differential response to the encashment shock (the shock-specific component of the error term). The encashment program was abrupt and uniform in timing,

which strengthens this condition, since it is equivalent to a parallel trends assumption across different treatment intensities (Goldsmith-Pinkham, Sorkin and Swift, 2020, p. 2598). With a single national shift, the design is exactly identified, and β_{IV} can be interpreted as the causal effect of the single aggregate shock as above. We give formal intuition in section B.8.

Effects on Survey-Based Outcomes As noted above, we also complement our district-level analyses with data from a survey of Afghan returnees. Using this individual-level survey data, we estimate a series of least-squares equations of the following form:

$$Y_{i,d,t} = \delta(\text{Encashment Beneficiary}_{i,d,t}) + \alpha_d + \beta_t + \mu(X_{i,d,t}) + \epsilon \quad (3)$$

where i indexes respondents, d indexes districts, and t indexes survey waves. $Y_{i,d,t}$ are dependent variables, like returnees' experiences of communal disputes. $\text{Encashment Beneficiary}_{i,d,t}$ is an indicator for respondents likely to have returned under the encashment program. We triangulate this measure using information on respondents' country of asylum, month of repatriation, and registration status while displaced. Under the UNHCR cash scheme, registered Afghan refugees in Pakistan were eligible for the expanded grant if they returned between July–December 2016. δ is the coefficient of interest, and captures whether, relative to other Afghan returnees, encashment beneficiaries experienced differential relations with their non-migrant neighbors. α_d and β_t are district and survey wave fixed effects. $X_{i,d,t}$ is a vector of individual-level controls including respondents' countries of asylum, months of return, and registration status, as well as demographic traits. ϵ are robust, district-clustered standard errors. All estimates are scaled using sampling weights.

Selection Before proceeding to the main results, we conduct a series of tests to probe selection into treatment. In Table B-11 we present differences-in-means comparing encashment beneficiaries with non-beneficiary returnees, and find few significant differences. Similar findings emerge when we focus the comparison on encashment beneficiaries versus spontaneous returnees from Pakistan during the encashment program (Table B-12). Finally, in Table B-13 we estimate a regression discontinuity design with our UNHCR microdata, using assisted returnees' dates of repatriation from Pakistan, relative to the expansion of the cash grant, as the running variable. This further isolates any potential selection into the encashment program from underlying temporal dynamics in returnee composition that may not have been driven by the program itself. In this specification we find no evidence for selection on returnees' origin or destination provinces, or on the female share or size of returnee groups.

6 Main Results

Insurgent Violence Many existing accounts highlight significant dangers of refugee return during conflict. Large-scale repatriation can afford militants opportunities to move combat matériel and recruit vulnerable returnees and their non-migrant neighbors (Lischer, 2011; Harpviken and Lischer, 2013; Haer and Hecker, 2018). At the same time, significant returns can strain fragile state institutions, exacerbating risks of violence and hampering government responses (Ambrosius and Meseguer, 2020). The situation during the 2016 encashment program we study was particularly risky—expanded repatriation assistance was implemented at the height of the summer fighting season, when Taliban forces escalated recruitment efforts in preparation for major offensives. In previous fighting seasons, Taliban commanders explicitly targeted refugee returnees from Pakistan for mobilization (Schultz, 2011).

In Table 1 we estimate the effects of encashed returns on insurgent violence in a reduced-form specification. Columns 1–2 evaluate the extensive margin of combat, and columns 3–4 evaluate violence per 100,000 district residents. Columns 1 and 3 represent our most basic difference-in-differences specifications, while columns 2 and 4 add a battery of controls. We find that returns under the encashment program caused a modest, distinguishable *reduction* in insurgent violence.³⁴ Estimated coefficients are precise, stable, and negative across specifications. In substantive terms, a one standard-deviation increase in exposure to encashed returns was associated with a roughly 2–3% decline in per-capita levels of insurgent-initiated violence relative to the pre-treatment mean.

While these baseline models define district-level exposure to refugee returns using 2013 returnee shares. An alternative is to define program exposure using the observed inflow of refugee returnees. To do so, we estimate a series of additional OLS and two-stage least squares (2SLS) models in Table 2. In the naive specifications, we calculate district shares of returnees from Pakistan for each month. We z-standardize this measure for interpretability, and regress the focal violence outcomes on it. Results of these models are presented in columns 3, 5, 7, and 9 of Table 2.

Because returnees’ choices over when and where to repatriate are endogenous, naive estimates are likely biased. To overcome this issue, we turn to an instrumental variables strategy. As in Boustan (2010), we instrument for refugee return using the reduced-form policy shock from the main specification in Table 1. First-stage estimates in columns 1–2 of Table 2 establish that 2013 returnee shares are highly correlated with actual returnee

³⁴In Table B-14 we show that encashed returns also reduced combat in raw levels.

Table 1: Reduced-Form Estimates of Refugee Return and Insurgent Conflict

	Insurgent-Initiated Attacks			
	Extensive Margin (0/1)		Per 100k Pop.	
	(1)	(2)	(3)	(4)
2013 Returnee Share x Grant Increase	-0.006*** (0.001)	-0.016*** (0.005)	-0.123*** (0.042)	-0.160** (0.071)
Observations	4378	4378	4378	4378
Clusters	398	398	398	398
Pre-Treatment Mean	0.717	0.717	4.955	4.955
Pre-Treatment SD	0.451	0.451	11.851	11.851
PARAMETERS				
District FE	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓
UNHCR Area x Month		✓		✓
Demographic Controls x Month		✓		✓
Development Controls x Month		✓		✓
Agricultural Controls x Month		✓		✓
Reconstruction Controls x Month		✓		✓
Lagged DV		✓		✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

shares.³⁵ Turning to columns 4, 6, 8, and 10, we estimate 2SLS models. Kleibergen-Paap F statistics suggest our instrument is strong and relevant. Instrumental variables estimates match our baseline and naive estimates in substantive terms, and are larger in magnitude. A one standard deviation increase in the district-month share of encashed returns reduced the probability of insurgent violence by 6.1 percentage points (column 6) and reduced insurgent violence per 100,000 residents by 0.627 attacks (column 10). Relative to the pre-treatment mean, these effects represent reductions in violence of roughly 9% and 13% respectively.

Together, these results suggest that encashed repatriation had a modest violence-suppressing

³⁵We observe district-month returns for 2014 and 2016-2017. Column 1 studies the correlation between shares in this full span, and column 2 studies the correlation in the focal period (2016), before and during the cash program.

Table 2: Naive and 2SLS Estimates of Refugee Return and Insurgent Conflict

	Insurgent-Initiated Attacks									
	Share of Returnees from Pakistan		Extensive Margin (0/1)				Per 100K Pop.			
	(1) 1 st Stage OLS	(2) 1 st Stage OLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS	(9) OLS	(10) 2SLS
2013 Returnee Share x Grant Increase	0.533*** (0.048)	0.257*** (0.016)								
Share of Returnees from Pakistan			-0.002 (0.004)	-0.045*** (0.010)	-0.002 (0.004)	-0.061*** (0.020)	-0.165 (0.105)	-0.964*** (0.247)	-0.246** (0.095)	-0.627** (0.278)
Observations	19104	4378	4378	4378	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398	398	398	398	398
Pre-Treatment Mean			0.717	0.717	0.717	0.717	4.955	4.955	4.955	4.955
Pre-Treatment SD			0.451	0.451	0.451	0.451	11.851	11.851	11.851	11.851
Kleibergen-Paap F Statistic				89.852		269.135		89.852		275.584
PARAMETERS	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓			✓	✓			✓	✓
Demographic Controls x Month	✓	✓			✓	✓			✓	✓
Development Controls x Month	✓	✓			✓	✓			✓	✓
Agricultural Controls x Month	✓	✓			✓	✓			✓	✓
Reconstruction Controls x Month					✓	✓			✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July through December 2016, when the UNHCR offered an increased repatriation grant. Share of returnees from Pakistan is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in a given year-specific month. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

effect. This finding is notable given widespread fears that mass returns during the summer fighting season would significantly exacerbate combat in returnee-receiving communities. Indeed, as the repatriation assistance program was implemented, Afghan officials were publicly warning that “returnees [were] vulnerable to Taliban messaging” and recruitment, and that conflict could consequently spike as a result of the program (Lakhani and Amiri, 2020).

Communal Violence Apart from insurgent conflict, refugee return may also affect social conflict. In general, communal strife in the wake of repatriation stems from animosity and resentment between repatriates and stayees (Schwartz, 2019). Several specific dynamics are relevant. Mass repatriation can spur resource and labor market competition (Harild, Christensen and Zetter, 2015), alter local ethnic balances (Joireman, 2017), and exacerbate criminality (Seefar, 2019; Nguyen and Nguyen, 2025). Property disputes are an especially common cause of tension between returnees and their neighbors (Allen and Morsink, 1994; Ruiz and Vargas-Silva, 2021, 2025). In the context of aid, stayees may also react violently

when programming is perceived as disproportionately benefiting returnees (van Houte, 2014).

Table 3: Refugee Return and Social Conflict

	Returnee–Stayee Relations Index (Z-Standardized)			
	ICW		PCF	
	(1)	(2)	(3)	(4)
Encashment Beneficiary	-0.133** (0.056)	-0.233* (0.126)	-0.178** (0.073)	-0.199* (0.113)
Observations	7391	7391	7283	7283
Clusters	65	65	65	65
Control Group Mean	0.035	0.035	0.033	0.033
Control Group SD	0.978	0.978	0.967	0.967
PARAMETERS				
District FE	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓
Urbanicity		✓		✓
Gender		✓		✓
Age		✓		✓
Ethnicity		✓		✓
Education		✓		✓
Income		✓		✓
Dwelling		✓		✓
Marital Status		✓		✓
Tazkira		✓		✓
Country of Asylum		✓		✓
Registration Status x Month of Return		✓		✓
Social Desirability		✓		✓
Interview Order		✓		✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights. ICW indicates an index constructed using inverse covariance-weighting. PCF indicates an index constructed using principal component factor analysis.

In Table 3 we explore these dynamics, focusing on how the encashment program impacted returnees’ communal relations. We specifically assess encashed returnees’ self-reports about their relations with their non-migrant (i.e., “stayee”) neighbors. The dependent variable combines information on the incidence of disputes and discrimination, as well as perceptions of social isolation (Ruiz and Vargas-Silva, 2022) Items are scaled so that higher values indicate better relations between returnees and stayees. All estimates are large, negative, and

precise, suggesting program beneficiaries suffered *worse* communal relations. In substantive terms, encashment beneficiaries’ relations with stayees were 0.13 – 0.23 standard deviations worse on average. In Tables B-15 – B-16 we corroborate these survey-based estimates using observational data we assembled on the incidence of tribal feuds and communal strife in Afghanistan.

Although typically less severe than insurgent attacks, social conflicts are nevertheless economically important. In Afghanistan, these disputes often involve whole villages or tribes, and are prone to protraction as they metastasize into multi-generational blood feuds (Mur-tazashvili, 2016; Joireman, 2018). Communal clashes have also contributed to mine contamination, destruction of agricultural infrastructure, and harvest disruptions. Our findings hence bear key welfare implications, and reinforce prominent accounts about how tensions between returnees and hosts spur social conflict (Schwartz, 2019; Ruiz and Vargas-Silva, 2025). More broadly, these results highlight important cross-cutting effects of UNHCR repatriation cash assistance on violence. While the grant increase attenuated insurgent violence in returnee-receiving communities, it worsened social strife.

6.1 Robustness

We conduct a number of additional tests to probe the robustness of these findings.

Temporal Dynamism Our main specifications focus narrowly on the effect of encashed returns on insurgent violence in 2016. Yet, the size of the cash transfer to returnees was large, suggesting the program could have persistent effects on conflict. We estimate six supplementary specifications to probe the durability of the main effects and examine how violence evolved after the program. These specifications, described in detail in section B.13, allow us to decompose the overall post-program period (from July 2016 onward) into two phases: the period of active cash distribution (July–December 2016), and the subsequent interval after cash assistance reverted to the pre-program level (January–December 2017). Across these additional analyses we confirm that insurgent-initiated attacks declined sharply and significantly during the encashment period, and this reduction persisted with modest attenuation through 2017. Table B-17 specifically reveals that effects in 2017 were roughly 29% as large as effects during the encashment period for the extensive margin of violence, and 82% as large as effects during the encashment period for per capita levels of violence. Event study estimates (Figure B-13) show conflict reductions were strongest in the first six months after program launch, mostly persisted through July 2017, and then dissipated in the last five

months of 2017. Taken together, these findings suggest substantial, economically meaningful persistence of program impacts. The concentration of effects during active cash distribution, combined with gradual decay rather than immediate disappearance, is consistent with cash assistance operating through both immediate economic channels and longer-lasting spillovers that dissipate over time.

Table 4: Accounting for Spontaneous Returns

	Insurgent-Initiated Attacks		Communal Violence	
	(1) Extensive Margin	(2) Per 100k Pop.	(3) Extensive Margin	(4) Per 100k Pop.
2013 Returnee Share x Grant Increase	-0.016*** (0.005)	-0.164** (0.072)	0.016*** (0.003)	0.009*** (0.003)
2012-2015 Spontaneous Returnee Share x Grant Increase	0.006 (0.010)	0.060 (0.208)	-0.018 (0.011)	-0.016 (0.012)
Observations	4367	4367	4367	4367
Clusters	397	397	397	397
Pre-Treatment Mean	0.718	4.963	0.098	0.048
Pre-Treatment SD	0.450	11.858	0.297	0.302
Difference: Assisted (2013) - Spontaneous (2012–2015)	-0.022** (0.011)	-0.223 (0.220)	0.034*** (0.012)	0.026** (0.012)
PARAMETERS				
District FE	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓
Pashtun Share x Month	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Undocumented returnee share is each district's standardized share of the sum of undocumented, refugee returnees from Pakistan and Iran to Afghanistan in 2012-2015. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016. One district, Hisa-I-Awal Panjsher, is excluded because IOM has no estimate of 2012–2015 undocumented returns.

Spontaneous Returnees Our main analyses focus on the effect of encashed returns (i.e., assisted returns from Pakistan during the encashment program). However, as described above, a considerable number of Afghan refugees also repatriated from Pakistan in 2016 without UNHCR facilitation. These spontaneous returnees were not eligible for repatriation

cash assistance. Testing for differences in the effects of assisted versus spontaneous returns is important for demonstrating that this concurrent surge in spontaneous returns does not confound our results. Comparing the effects of assisted returnees (who received the cash transfer) versus spontaneous returnees (who did not receive the cash transfer) also helps illuminate the unique effect of encashment on our violence outcomes. In Table 4, we exploit IOM estimates of each district’s historical exposure to spontaneous returns.³⁶ In columns 1-2 we study our main insurgent violence outcomes, and in columns 3-4 we consider our observational measure of communal violence. We find that the main effects are specific to encashed returnees. Coefficients on the term for exposure to spontaneous returns are consistently null, and in three of the four models, the effect of exposure to encashed returns is distinguishably different from the effect of exposure to spontaneous returns. Using our survey data, we take an analogous approach and define an indicator for spontaneous returnees from Pakistan during the encashment program (Table B-18). As with our panel evidence, survey results suggest that encashed returnees—but not comparable spontaneous returnees—suffered distinguishably worse relations with their non-migrant neighbors.

Alternative Measure of Returnee Exposure We also consider an additional source of cross-sectional variation in district-level exposure to encashed returns. In 2016, all assisted returnees from Pakistan passed through one of three Encashment Centres in Kabul, Jalalabad, or Kandahar (Figure A-6), where they were interviewed and provided cash assistance. To reduce travel costs, most returnees resided closer to the facilities where they received their repatriation cash (Haroon et al., 2019). In Table B-19 we define exposure to encashed returns using a cross-sectional measure of proximity to encashment centers and find substantively similar results.

Spatial Spillovers A potential concern with our empirical strategy is that spillover effects across district boundaries could bias our estimates. Two types of spatial spillovers merit consideration. First, conflict in neighboring districts could confound our results if nearby violence affects local outcomes independently of local returnee shares. Second, secondary migration could cause spillovers if encashed returnees systematically moved to other areas after repatriating, accepting cash, and then settling in their initial locations.³⁷ We address

³⁶Full details on this specification are provided in section B.14.

³⁷We are sanguine because a 2018 World Bank survey of 2016 encashed returnees found that more than 90% resided in their initial destination community and 91% indicated holding no plans to move (Haroon et al., 2019).

both concerns empirically using a combination of spatial econometric techniques and direct survey evidence on migration intentions. In Tables B-20 – B-21 we confirm that the main findings on insurgent and communal violence are robust to incorporating spatial lags of the outcome. In Table B-22 we show that encashment beneficiaries do not systematically differ from non-beneficiary returnees in their migration intentions.

Additional Confounders and Modeling Choices We take a number of additional steps to address potential threats to identification in the online supplement. First, our main specifications control flexibly for a range of theoretically-relevant district characteristics related to population size, ethnicity, economic and agricultural conditions, and reconstruction spending. In Tables B-23 – B-24 we confirm that the main results hold while incorporating a host of additional controls including levels of historical violence and crime and pre-treatment measures of territorial control, internal displacement, and ethnic fractionalization and polarization. Our survey-based estimates are similarly robust to controlling for additional factors that could shape returnee-stayee relations, like land ownership and returnees reported reasons for repatriating (Table B-25). Second, the main estimates on insurgent violence are scaled by analytic population weights, which help address heteroskedasticity and identify heterogeneous effects by district population size. Weighting more populous districts also helps address returnee gravitation toward urban centers. Table B-26 reveals that, as expected, weighting improves precision of the core estimates. Finally, we consider whether the results are heterogeneous in relative exposure (i.e., how large the encashed returnee population is relative to pre-treatment district population) or population density. In Table B-27 we show that the focal effects on insurgent and communal violence are modestly but not statistically significantly larger in more relatively-exposed and population-dense districts.³⁸ This finding reinforces our main shift-share specification, and suggests the effects we estimate are driven more by the concentration of the shock rather than its relative size (see also Taylor et al., 2016; Bahar et al., 2024).

³⁸With one exception: encashed returns had a distinguishably greater positive effect on the extensive margin of communal violence in districts where the inflow of encashed returnees relative to the pre-treatment district population was high versus low.

7 Mechanisms

In this section, we describe and test several potential mechanisms underpinning our results. We begin with a focus on insurgent violence, where prior work suggests participation is linked to economic incentives.³⁹ We then investigate the roles of social capital and local institutions for resolving communal disputes. Here, we explicitly consider and test how social networks and other immaterial factors may help facilitate returnee reintegration.

7.1 Economic Spillovers and Reservation Wages

Participation in organized violence is linked to local economic conditions. When licit, outside options are limited, the opportunity costs of violence are diminished (Collier and Hoeffler, 2004; Bueno de Mesquita, 2013). Negative shocks in the formal economy increase rebel mobilization (Dube and Vargas, 2013; Blair, Christensen and Rudkin, 2021) and fighting capacity (Wright, 2020), particularly where rebels hold lootable resources (Vanden Eynde, 2018). In response, governments have implemented a range of developmental policies to offset the returns to militancy. These interventions include humanitarian aid, community driven development, cash-for-guns schemes, and conditional cash transfers (Croft, Felter and Johnston, 2014; Sexton, 2016; Beath, Christia and Enikolopov, 2025). Employment schemes that provide financial support also reduce risks of mobilization (Blattman and Annan, 2016). The broader aim of these policies is to stimulate the licit economy. By raising wages, positive economic shocks reduce militant recruitment and capacity (Bueno de Mesquita, 2013; Wright, 2020). If reservation wages increase without any corresponding increase in the returns to conflict (i.e., increasing compensation for mobilization), participation in violence will decline.

We extend these insights by considering inter-household economic spillovers owing to the returnee cash program. If a household-level transfer is of a sufficient scale, it may generate positive intra-communal externalities.⁴⁰ As local economic conditions improve, reservation

³⁹We recognize the importance of immaterial incentives for mobilization. We focus on political economy accounts of conflict for two reasons. First, because we leverage an *encashment* shock it is natural to explore the impact of economic inducements. Second, our analyses are limited by the fact that we cannot observe details about returnee’s politically-salient experiences before and during displacement. For instance, we lack individual-level data on prior experiences of violence in Afghanistan, where refugees were hosted in Pakistan, or what returnees think about Afghan politics. One ripe avenue for future work is to examine how immaterial incentives intersect with economic motivations for mobilization in Afghanistan and elsewhere.

⁴⁰Maystadt and Verwimp (2014), Taylor et al. (2016), and Zhou and Shaver (2021) highlight community-level spillovers from refugee settlements. Lehmann (2020) and Lehmann and Masterson (2020) highlight positive spillovers of refugee aid programming.

wages for household members (who benefit from the direct transfer) and their neighbors (who benefit indirectly) increase (Bloemen and Stancanelli, 2001; Taylor et al., 2016). We lack granular data on wages and employment in Afghanistan in the period we study, so we cannot directly observe and test these positive economic externalities of the repatriation cash program. However, the political economy theories of conflict detailed above suggest a number of observable downstream implications of this positive economic spillover channel for violence.

Effects on Tactics and Lethality Above all, if reservation wages increase enough to diminish the mobilization capacity of incumbent insurgent groups, militant violence will decline. Mobilization-strapped insurgents should respond to rising reservation wages by reducing labor-intensive attacks, such as frontal assaults on government outposts, which require significant numbers of fighters to perpetrate. To offset this reduction in personnel-heavy fighting, insurgents may also reallocate into capital-intensive tactics, such as roadside bombings, which can be carried out by few—or even lone—fighters.⁴¹ We explore these theory-informed, general equilibrium effects of incentivized repatriation on conflict to assess the plausibility of intra-communal economic spillovers from the returnee encashment policy.⁴²

To investigate rebels’ tactical adaptations to the encashment program, we leverage supplemental details in our conflict microdata. First, we separately estimate the effects of refugee return on direct fire, indirect fire, and explosive events.⁴³ If the encashment program raised reservation wages in impacted communities, we should observe a reduction in insurgent-initiated direct and indirect fires, and increasing use of explosive attacks. These

⁴¹The overall reduction in insurgent violence is consistent with another mechanism: aid as prize (Nunn and Qian, 2014). This argument is developed for the refugee case in Lehmann (2020), where insurgents may reduce combat activity to maximize the number of returnees that can later be taxed. In the Afghan case, this would be analogous to Fetzer et al. (2021), who find that insurgents reduced violence strategically to facilitate the withdrawal of international troops. If insurgents calibrated their use of violence to maximize civilian welfare while continuing to engage in anti-government combat, we anticipate this would have resulted in greater direct and indirect fire attacks and fewer explosive hazards, since the primary cause of civilian casualties during the Afghan conflict was IEDs. As we describe below, this is the inverse of the disaggregated pattern we observe.

⁴²Critically, these downstream effects are consistent with—but not directly evidence of—positive externalities from repatriation assistance. Future work should prioritize quantifying direct economic effects of cash-for-return programs.

⁴³Direct fires are attacks using projectile-fired weapons and perpetrated within the line-of-sight of the target. Small arms fire is an example of a direct fire. Indirect fires are also projectile-fired attacks, but unlike direct fires, these attacks are perpetrated beyond the line-of-sight, aimed at targets far from the firing platform. Rocket and mortar attacks are examples of indirect fires. Explosive attacks are bombings using improvised explosive devices (IEDs) or mines.

expectations directly follow from insights on Taliban force employment (Sonin and Wright, 2022, 2024). Direct fires are most labor-intensive because these require coordinated teams of insurgents directing close-range fire. Indirect fires are also relatively more labor-intensive than IEDs, requiring coordination between insurgents to set-up and launch heavy weapons against distant counterinsurgent positions (Blair, 2024). In contrast, explosive attacks are typically perpetrated in the form of roadside bombings, with one or a few insurgents planting IEDs along convoy routes. In our case, these stylized facts suggest the labor-intensivity of violence is inversely related to capital-intensivity. Direct and indirect fires are least capital-intensive, employing readily-available arms. Explosives require a higher degree of technical expertise to produce and emplace; during the conflict, insurgents also invested significant resources to defeat evolving counter-IED capabilities (Trebbe et al., 2020).

Table 5: Reduced-Form Estimates of Refugee Return and Tactical Diversity

	Tactical Diversity							
	Extensive Margin (0/1)				Per 100k Pop.			
	(1) Insurgent-Initiated Attacks	(2) Direct Fires	(3) Indirect Fires	(4) Explosions	(5) Insurgent-Initiated Attacks	(6) Direct Fires	(7) Indirect Fires	(8) Explosions
2013 Returnee Share x Grant Increase	-0.016*** (0.005)	-0.031*** (0.005)	-0.016*** (0.005)	0.018*** (0.005)	-0.160** (0.071)	-0.092* (0.054)	-0.025* (0.015)	-0.033 (0.032)
Observations	4378	4378	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398	398	398
Pre-Treatment Mean	0.717	0.639	0.215	0.504	4.955	3.125	0.340	1.490
Pre-Treatment SD	0.451	0.480	0.411	0.500	11.851	8.499	1.965	4.123
PARAMETERS								
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓	✓	✓	✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Results in Table 5 provide initial support. For reference, in columns 1 and 5 we provide our benchmark estimate of the effect of encashed return on insurgent violence based on columns 2 and 4 of Table 1. In columns 2-4, we separately estimate the focal specification for the extensive margin of direct fires, indirect fires, and explosive hazards. Columns 6-8 report comparable results for tactics per capita. Encashed returns significantly reduced direct and indirect fires, with most of the overall reduction in insurgent violence stemming from a reduction in the most labor-intensive tactic—direct fires. In contrast, encashment increased

the probability of capital-intensive explosive violence by 1.8 percentage points (column 4). Estimates on explosions per capita are small in magnitude and indistinguishable. Together, these results are consistent with the reservation wage mechanism we posit.

Yet, changes in insurgent tactical portfolios are merely suggestive. To provide further evidence, we study lethality, using casualty records documenting the number of counterinsurgent forces killed or wounded in each attack.⁴⁴ We specifically consider the average lethality of insurgent attacks, as well as tactic-specific lethality (i.e., the average number of counterinsurgents killed in attacks of each tactic). These measures are expressed as $\frac{\# \text{ Counterinsurgent Casualties from Violence}_{d,t}}{\# \text{ of Violent Events}_{d,t}}$, where violence variously indicates all insurgent-initiated attacks, direct fires, indirect fires, or explosive attacks.⁴⁵ Lethality rates offer a clearer window into reservation wages because the lethality of violence is a function of its input intensity. If the encashment program raised reservation wages, constraining insurgent recruitment, it should cause militants to deploy smaller attack teams less capable of inflicting harm on counterinsurgents. In other words, rising reservation wages imply declining effectiveness of direct and indirect fires. At the same time, insurgents may adapt by allocating more resources into capital-intensive attacks, such as larger or more sophisticated IEDs, which can be emplaced in spite of high reservation wages, and which enable insurgents to maintain lethality under reduced mobilization. In other words, rising reservation wages imply increasing lethality of explosives if fighting capacity is substituted from labor to capital.

In Table 6 we provide evidence that encashment induced these shifts in insurgent lethality. Repeating our saturated specification, we study the average number of counterinsurgent casualties per attack (column 1) and per tactic (columns 2-4). Program-induced returns are associated with a large reduction in the number of counterinsurgent casualties per direct fire attack, and a modest but imprecise reduction in the lethality of indirect fires. A one standard deviation increase in exposure to encashed returns reduced the lethality of direct fires by 20% relative to the pre-treatment mean. However, these effects are compensated by increasing lethality of explosive attacks in impacted districts. A one standard deviation increase in encashed returns was associated with a 50% increase in explosive lethality compared to the pre-program mean. Indeed, rising effectiveness of explosive violence drove an aggregate increase in the lethality of overall insurgent violence. Total lethality increased 36%, relative

⁴⁴We do not observe civilian casualties, but in general these are highest in explosive attacks, which are least discriminate (Sonin and Wright, 2024).

⁴⁵Because the lethality rate is undefined when no insurgent violence occurs, we fill these observations with zeroes.

Table 6: Reduced-Form Estimates of Refugee Return and Insurgent Lethality

	Lethality Rate (# Casualties/# of Attacks)	Tactic-Specific Lethality Rate (# Casualties/# of Attacks)		
	(1) Insurgent-Initiated SIGACTs	(2) Direct Fires	(3) Indirect Fires	(4) Explosions
2013 Returnee Share x Grant Increase	0.200*** (0.036)	-0.066*** (0.010)	-0.009 (0.007)	0.306*** (0.044)
Observations	4378	4378	4378	4378
Clusters	398	398	398	398
Pre-Treatment Mean	0.556	0.328	0.052	0.612
Pre-Treatment SD	1.137	0.836	0.442	1.345
PARAMETERS				
District FE	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓
Violence (=1)	✓	✓	✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Violence(=1) is an indicator for whether any attacks of the respective type occurred. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

the pre-encashment mean, for a one standard deviation increase in returnee exposure.

One limitation of these analyses is that we anticipate composition shifts, but our dependent variables cannot reveal relative substitution; that is, rising employment or lethality of capital-intensive violence as a proportion of total violence. The share-based outcomes we study in Table B-28 are better-suited to test these effects. We specifically probe two outcomes: the share of all insurgent attacks that are explosive, and the share of all counterinsurgent casualties from explosive attacks. Precise positive coefficients for both outcomes indicate the repatriation assistance program caused insurgent substitution from labor-intensive into capital-intensive violence, which also grew more lethal. Together, these results are indicative of positive externalities—namely, rising reservation wages—from returnee cash assistance in impacted communities. More generally, these findings comport with evidence that aid to refugees may benefit hosts by spilling over to non-beneficiary, non-migrant neighbors (Mason and Lehmann, 2020; Taylor et al., 2016), raising community-wide wages.

Effects on Civilian Informing Information is a paramount constraint on insurgent and counterinsurgent violence. With information, belligerents can selectively target adversaries, improving battlefield success (Sonin and Wright, 2024). Local economic conditions shape tip flows from civilians, in addition to the opportunity costs of mobilization. Counterinsurgents exchange outreach and development to “buy” tips, cultivating collaboration through service provision (Berman, Shapiro and Felter, 2011). In a market for tips, negative economic shocks reduce the cost of information, increasing civilian collaboration with the government (Vanden Eynde, 2018). Conversely, positive economic shocks raise the price at which potential collaborators are willing to provide information to counterinsurgents.

Extending these insights, we explore how repatriation cash assistance shaped civilian informing. The program we study was unique. Cash for returnees was granted by an independent third-party, the UNHCR. As such, it is unlikely that civilians viewed the aid as an anti-insurgent investment, and rewarded counterinsurgents for issuing it.⁴⁶ If the encashment program induced inter-household spillovers from returnees to non-migrant hosts, improved welfare would increase the price of tips and reduce information flows to counterinsurgents.⁴⁷ That is, positive intra-communal spillovers from repatriation assistance may both reduce rebel mobilization through an opportunity cost channel, and reduce counterinsurgent effectiveness through an information channel.

We do not observe civilian tips in the combat microdata we use. Instead, we investigate how repatriation affected counterinsurgent bomb neutralizations, an outcome highly sensitive to civilian informing (Berman, Felter and Shapiro, 2018; Sonin and Wright, 2024). When civilians provide more information, government forces are better able to interdict explosive hazards before detonation. In Table B-29 we study the share of emplaced explosives detected and cleared by government forces prior to detonation. This measure is defined as $\frac{\# \text{ of Explosives Found and Cleared}_{d,t}}{\# \text{ of Explosives Emplaced}_{d,t}}$. Results suggest the repatriation program reduced government success in neutralizing insurgent bombs. The magnitudes are operationally meaningful. The pre-treatment clearance rate averaged 34.8% when explosives were emplaced. A one standard deviation increase in exposure to encashed returns reduced clearance rates by 2 percentage points (column 2), a 6% decline relative to baseline. In tandem with the evidence on tactical heterogeneity, these findings suggest that insurgents strategically reallocated fighting effort

⁴⁶Encashed returnees correctly attributed UNHCR as the source of aid (Table A-5).

⁴⁷US/NATO forces in Afghanistan had vast fiscal resources, and hence were unconstrained by small-scale increases in the cost of information. However, Afghan forces had primary responsibility for counterinsurgency in the period we study. These forces were highly budget constrained, meaning small increases in the price of tips had large effects on information flows.

in response to mobilization constraints, and capitalized on reduced civilian collaboration with counterinsurgents. These results also highlight a salient cost of repatriation assistance during conflicts—declining counterinsurgent effectiveness resulting from reduced informing.

7.2 Communal Conflict, Social Capital, and Informal Institutions

Refugee return may fuel communal violence through several channels. An influx of returnees could disrupt the balance of power between local tribes or ethnic groups (Joireman, 2017), trigger status anxiety and fears over labor market competition (Kamminga and Zaki, 2018), or challenge informal property rights (Schwartz, 2019; Ruiz and Vargas-Silva, 2022). These mechanisms are particularly relevant in Afghanistan, where clan ties, tribal affiliations, and ethnic identities play an outsized role in village-level politics (Murtazashvili, 2016).

Social Capital We anticipate that social capital, namely ties between returnees and stayees, are central to the link between refugee return and communal violence. As Harild, Christensen and Zetter (2015, p. 10) explain, “social networks play an important role for the ability of returnees to reintegrate and reestablish livelihoods.” Where returnees repatriate to areas they originated from, they hold preexisting community ties, which ease reintegration and mitigate backlash.⁴⁸ By contrast, conflict should be more severe in areas where returnees lack ties and are more socially distant from their neighbors. In these receiving communities, returnees struggle to access land, jobs, and public services, compounding protection deficits.

To study the moderating role of social capital, we leverage unique information about returnees’ neighborhood settlement patterns from the Survey of Afghan Returnees. In particular, we draw on two survey questions about the social composition of returnees’ neighborhoods in destination communities. The first question we study captures self-reports about whether returnees selected their destination communities in order to be near family, co-ethnics, or other returnees. The second question we study captures returnees’ self-reports that they reside near familial kin or other repatriates. We combine responses to these items to produce an inverse covariance-weighted social capital index reflective of the density of returnees’ social connections.⁴⁹

⁴⁸It is possible, as Schwartz (2019) points out, that pre-existing family or kinship ties may exacerbate conflict upon return. For instance, land disputes may emerge between those family members that stayed and those who left and subsequently returned. In the Afghan context, family networks and kinship ties are central to informal governance. Hence, we anticipate and find evidence suggesting familial proximity and connectedness reduce the potential for communal conflict.

⁴⁹The results are robust to alternative measures of social capital (Table B-30).

Table 7: Social Capital Dampens Return-Induced Social Conflict

	Returnee–Stayee Relations Index (Z-Standardized)					
	ICW			PCF		
	(1) Baseline	(2)	(3)	(4) Baseline	(5)	(6)
Encashment Beneficiary	-0.233* (0.126)	-0.142** (0.057)	-0.235* (0.126)	-0.199* (0.113)	-0.186** (0.076)	-0.201* (0.112)
Encashment Beneficiary x Social Capital		0.196*** (0.054)	0.191*** (0.054)		0.182** (0.076)	0.176** (0.075)
Social Capital		0.063*** (0.020)	0.055*** (0.019)		0.067*** (0.018)	0.056*** (0.018)
Observations	7391	7391	7391	7283	7283	7283
Clusters	65	65	65	65	65	65
Control Group Mean	0.035	0.035	0.035	0.033	0.033	0.033
Control Group SD	0.978	0.978	0.978	0.967	0.967	0.967
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓	✓	✓
Urbanicity	✓		✓	✓		✓
Gender	✓		✓	✓		✓
Age	✓		✓	✓		✓
Ethnicity	✓		✓	✓		✓
Education	✓		✓	✓		✓
Income	✓		✓	✓		✓
Dwelling	✓		✓	✓		✓
Marital Status	✓		✓	✓		✓
Tazkira	✓		✓	✓		✓
Country of Asylum	✓		✓	✓		✓
Registration Status x Month of Return	✓		✓	✓		✓
Social Desirability	✓		✓	✓		✓
Interview Order	✓		✓	✓		✓

Note: * p <.10, ** p <.05, *** p <.01. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Social capital is an ICW index comprising measures of returnees’ proximity to kin and other repatriates. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights. ICW indicates an index constructed using inverse covariance-weighting. PCF indicates an index constructed using principal component factor analysis.

Results in Table 7 confirm the essential role of social capital in mitigating communal conflict after repatriation. Columns 1 and 4 provide our benchmark estimates from columns 2 and 4 of Table 3 for reference. Columns 2-3 and 5-6 of Table 7 study the moderating effects of social capital as proxied by neighborhood-level settlement patterns, and specifically, proximity to kin. Taking the estimate from column 3, we find that encashment beneficiaries

low in social capital report 0.24 standard deviations worse social relations with stayees. This effect is essentially fully offset for encashment beneficiaries one standard deviation above the mean in social capital. Together these results highlight the significant mitigatory role of social capital for tensions between repatriates and stayees.

In Table B-31 we find parallel conflict-dampening effects of social capital while studying our event data on communal feuds. To evaluate the role of social capital in these panel specifications, we use granular data linking returnees' settlement patterns with their regions of origin. Specifically, UNHCR records describe the district and province of origin for each group of documented returnees (see also Figure B-11). We interact the reduced-form, 2013 Returnee Share $\times$ Grant Increase, with two continuous measures capturing the 2013 share of returnees to: (1) a district in the same province from which they originated; and (2) the same district from which they originated. Consistent results emerge, suggesting familial kinship networks in destination communities are central to blunting return-induced social conflict.

Informal Institutions As with social capital, institutional mechanisms for resolving interpersonal and intra-communal conflicts may blunt tensions between returnees and stayees. In particular, strong local institutions can facilitate bargaining, monitoring, and enforcement, and thereby stop disputes from escalating to violence (Blattman, Hartman and Blair, 2014). These institutions are particularly relevant in Afghanistan where, after centuries of weak national governance, conflict resolution has been largely devolved to informal, local institutions (Murtazashvili, 2016). Shuras, for example, use customary law and traditional norms to litigate disputes between villagers. The Taliban similarly engage in governance, deploying judges and providing enforcement in rural communities. Reliance on these informal or rebel-led systems for dispute resolution has been reinforced by two decades of failed judicial reform and rampant corruption among government bureaucrats. We anticipate that where these informal institutions for dispute resolution are strong, communal violence in the wake of the encashment shock may be offset. We investigate the potential conflict-dampening role of local institutions for dispute resolution in the appendix.

Unfortunately, the Survey of Afghan Returnees, from which we draw our main communal relations measure, does not include relevant information on local institutional quality. Instead, to assess whether the strength of informal institutions moderates the effect of refugee return on communal conflict, we rely on our supplementary event data on social strife. We develop four pre-treatment, cross-sectional measures of local institutional quality (Table B-32). First, we use data from the NATO-funded Afghanistan Quarterly Nationwide Assessment

Research (ANQAR) survey. With ANQAR data from 2013, we measure district average responses on: the preference for dispute resolution via local shuras, the preference for solving problems via elders, and whether individuals who had experienced disputes resolved them via a local shura. Second, we leverage novel data from ACSOR, the firm that administers ANQAR and the Survey of Afghan Returnees, on the location of districts under Taliban influence. These data identify areas where insurgents are able to intercede in local governance and provide enforcement. We measure influence using the 2013 base period. We then interact these measures of local institutional strength with our main reduced-form measure of exposure to encashed returns (2013 Returnee Share x Grant Increase).

Table B-33 shows the repatriation assistance program primarily increased communal violence where local institutions were weak. In districts with strong, informal dispute resolution mechanisms, return is negatively associated with communal conflict. It is reasonable to wonder whether districts with strong, local institutions are less violent in the first place. Promisingly, we find consistent results when controlling for historical violence (Table B-34). An implication of these findings is that international actors should prioritize building legitimate local institutions before seeking to facilitate refugee return. The most effective repatriation efforts will engage informal dispute resolution mechanisms in the peacebuilding process as a means of mitigating risks of social conflict.

8 Conclusion

Refugee return during and after conflict remains one of the most complex challenges for postwar recovery and peacebuilding. While prior research has deepened our understanding of why refugees choose to return, little systematic evidence exists on how incentive-based repatriation programs shape conflict dynamics in fragile returnee-receiving countries. We address this gap by studying a large-scale, unexpected increase in repatriation assistance for Afghan refugees in Pakistan in 2016. In response to the policy, more than 363,000 refugees repatriated to Afghanistan. By linking administrative and survey data, we identify the effects of incentivized return on multiple forms of violence, and explore the mechanisms that mediate these outcomes. Our findings shed new light on how economic inducements can alter both the pace of repatriation and the stability of communities that receive returnees.

We find that the cash-for-return program reduced insurgent violence overall, but simultaneously heightened communal conflict. We also document a key composition shift in insurgent attacks. Declining rebel violence was driven by reductions in labor-intensive tactics,

like direct and indirect fires. Correspondingly, insurgents shifted effort into the production of capital-intensive explosive attacks in returnee-receiving communities. Growing reliance on explosive tactics drove up the lethality of insurgent-initiated violence overall. We interpret these results through the lens of canonical political economy models of conflict. The cash transfer that induced repatriation may have stimulated local economic activity in areas where returnees settled. Our results are thus consistent with a long-standing explanation for violent mobilization, namely opportunity costs. As reservation wages rise and rebel capacity remains fixed, production of (personnel-intensive) violence becomes more costly. We also find evidence consistent with a theoretical mechanism proposed by [Berman, Shapiro and Felter \(2011\)](#)—that the market for collaboration responds to economic shocks as well. Following the cash transfer program, we observe a decline in government bomb neutralizations, likely driven by a decline in civilian information-sharing about the location of explosive hazards. Together, these findings underscore how shifts in local economic conditions can reshape the organization and intensity of violence in the aftermath of refugee return.

We also illuminate the importance of social capital and strong, informal institutions for dispute resolution in mitigating intra-communal conflict associated with mass repatriation. Preexisting kinship ties and dispute resolution institutions attenuate the effect of refugee repatriation on social strife. Recent work has shed light on optimal settlement strategies when refugees aim to rebuild their lives in host countries ([Bansak et al., 2018](#)). Our results clarify how a similar intervention could be used to evaluate when, where, and with whom returning refugees should be located. Although the decision to repatriate and where to settle remains the right of returnees, it may be possible to encourage targeted return in a manner that reduces the potential for local clashes and improves protection and development outcomes overall ([Gerver, 2017](#)). At the same time, returnee settlement patterns could be used to strategically deploy additional governance resources to help preempt conflicts before they emerge or resolving disputes after once they have begun. We anticipate that local support for conflict resolution through informal institutions could also be tied to preexisting risk factors, including customary land tenure, livestock grazing patterns, vulnerability of irrigation networks, and heterogeneous ethnic settlement patterns.

The impacts of refugee repatriation are nuanced. So too are the ethical considerations relevant to programmatic interventions aimed at facilitating return. These challenges are exacerbated when international organizations and policymakers consider how best to enable return during an active conflict ([Long, 2013](#)). If repatriation assistance is deployed to appease asylum countries eager to reduce their refugee-hosting burdens, it risks inadvertently

incentivizing coercive tactics and degrading the voluntariness of repatriation (Gerver, 2017). Eroding the voluntariness of return and the free choice of refugees undermines fundamental principles of international law and standards of refugee protection. Safe, voluntary return must remain the foundation of any repatriation effort. Our evidence underscores why: the encashment program, though successful in inducing return, also intensified communal violence and increased the lethality of insurgent attacks. These outcomes are both normatively troubling and practically destabilizing. Designing ethically-sound repatriation policies therefore demands a holistic approach that weighs the human rights of returnees, anticipates how armed actors might exploit their return, strengthens local institutions to manage social frictions, and affirms the responsibilities of host states and international organizations to protect displaced populations.

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APPENDIX [FOR ONLINE PUBLICATION ONLY]

Christopher W. Blair, David Van Dijcke, and Austin L. Wright

List of Figures and Tables

Table/figure numbers are noted in left column. Page numbers are noted in the right column.

Supplemental Figures

A-1	Access to Durable Solutions for Refugees and Asylum-Seekers	SI-3
A-2	Beneficiaries of IOM-Assisted Return Programs	SI-5
A-3	Afghan Refugee Flight to and Return from Pakistan	SI-12
A-4	Wave Dynamics in Patterns of Global Refugee Return	SI-13
A-5	UNHCR Repatriation Cash Assistance for Afghan Returnees	SI-14
A-6	Mapping the Process of Assisted Return from Pakistan	SI-15
A-7	Anti-Refugee Coercion in Pakistan	SI-21
A-8	Directed Graph on Encashment, Coercion, and Refugee Return	SI-23
B-9	Survey-Based and Observational Measures of Communal Violence	SI-30
B-10	Observed Versus Predicted Returns During the Cash Program	SI-31
B-11	Returnee Gravitation Toward Origin Districts	SI-32
B-12	Goldsmith-Pinkham, Sorkin, and Swift (2020) Shift-Share Pre-Trends Test	SI-35
B-13	Month-by-Month Event Study	SI-47
B-14	Historical Settlement Patterns: Assisted vs. Spontaneous Returns	SI-49

Supplemental Tables

A-1	Prominent Repatriation Assistance and Cash-for-Return Programs	SI-4
A-2	Negative Pathways Linking Return With Conflict	SI-9
A-3	Positive Pathways Linking Return With Conflict	SI-11
A-4	Returnee Cash Assistance Was Economically Meaningful	SI-17
A-5	Encashment Beneficiaries Financed Return with UNHCR Aid	SI-19
A-6	Encashment Beneficiaries' Self-Reported Push Factors	SI-25
A-7	Encashment Beneficiaries' Self-Reported Pull Factors	SI-26
B-8	Summary Statistics: District Analyses	SI-27
B-9	Summary Statistics: Survey Analyses	SI-28
B-10	Baseline Correlations: 2013 Returnee Share and District Characteristics .	SI-33
B-11	Covariate Balance Between Encashed and All Other Returnees	SI-37
B-12	Covariate Balance Between Encashed and Spontaneous Returnees	SI-38

B-13	RDD Balance Check: Assisted Returnees from Pakistan	SI-39
B-14	Refugee Return and Violence in Levels	SI-40
B-15	Communal Violence Increased Under the Encashment Program	SI-41
B-16	Naive and 2SLS Estimates of Communal Violence	SI-42
B-17	Temporal Specifications: Effect Persistence Tests	SI-45
B-18	Spontaneous Returnees' Community Relations	SI-50
B-19	Cross-Sectional Variation from Encashment Centre Proximity	SI-52
B-20	Insurgent Violence: Controlling for Spatial Lags	SI-54
B-21	Communal Violence: Controlling for Spatial Lags	SI-55
B-22	Migration Intentions of Encashment Beneficiaries	SI-56
B-23	Insurgent Violence Results Robust to Additional Parameters	SI-58
B-24	Communal Violence Results Robust to Additional Parameters	SI-59
B-25	Survey-Based Results Robust to Additional Parameters	SI-60
B-26	Robustness to Excluding Population Weights	SI-61
B-27	Heterogeneity in the Effect of Encashed Returns by Relative Exposure and Population Density	SI-62
B-28	Substitution Into Explosive Violence	SI-64
B-29	Counterinsurgent Bomb Neutralizations	SI-65
B-30	Robustness with an Alternative Measure of Social Capital	SI-66
B-31	Social Capital Mitigates Observed Communal Conflict	SI-67
B-32	Measuring Local Institutional Quality in Afghanistan	SI-68
B-33	Strong, Local Institutions Dampen Return-Induced Communal Violence . .	SI-69
B-34	Communal Violence, Local Institutions, and Historical Violence	SI-70

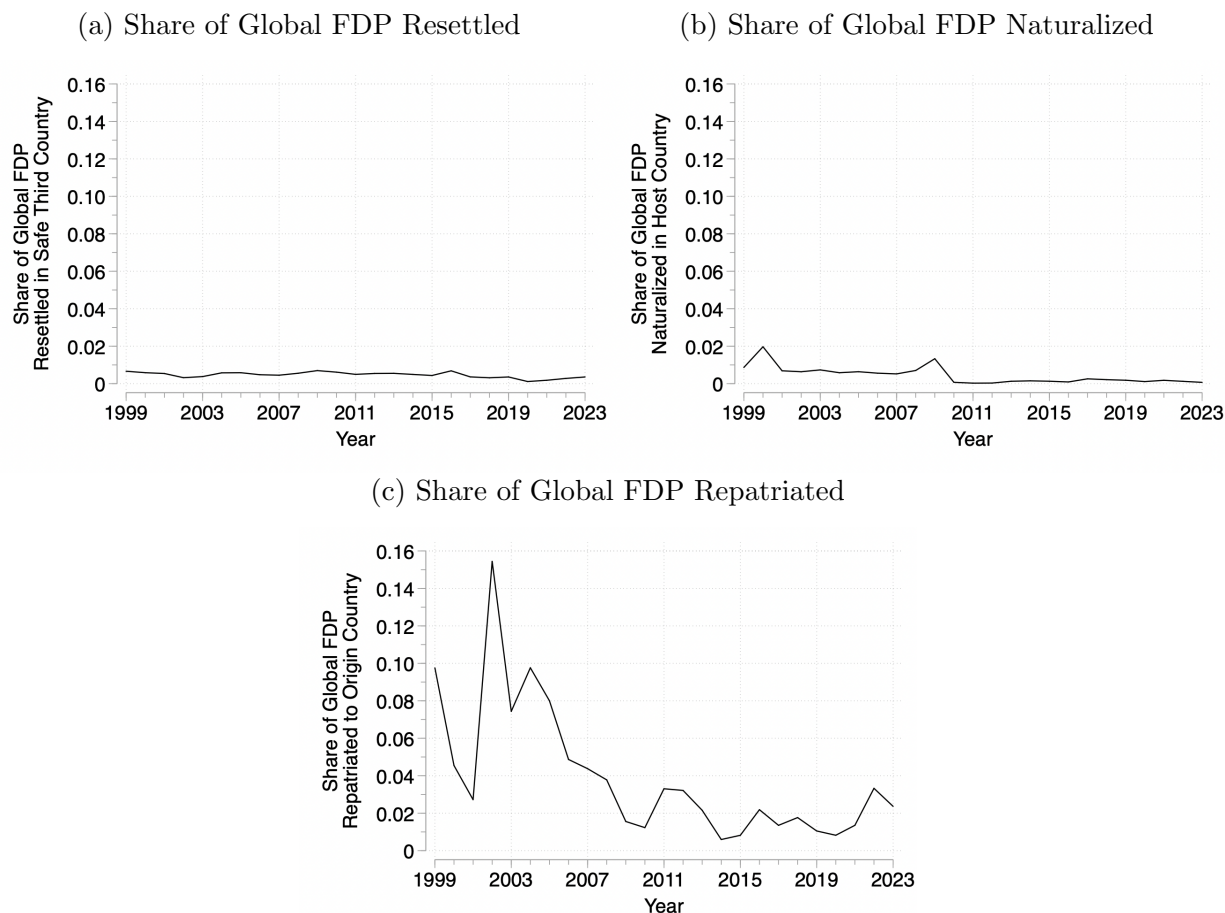
A Context Appendix

In this brief appendix, we elaborate key contextual details of the intervention we study.

A.1 Collapse of the Durable Solutions Architecture

The international community defines three “durable solutions” for refugees: resettlement in safe third countries, naturalization in first countries of asylum, or repatriation to origin countries. Over the past 25 years, no more than 16% of refugees and asylum-seekers (i.e., forcibly displaced people, FDP) have been able to access any durable solution. The collapse of the international solutions architecture is particularly apparent over the past decade; since 2013, fewer than 3.8% of global FDP have been resettled, naturalized, or returned home.

Figure A-1: Access to Durable Solutions for Refugees and Asylum-Seekers



Note: Panels (a), (b), and (c) respectively plot annual shares of the world’s forcibly displaced people (FDP) who were resettled to a safe third country, permanently naturalized into a host country, or repatriated to an origin country. *Source:* UNHCR Refugee Population Statistics database.

A.2 Incentivized and Assisted Return Programs Worldwide

A growing number of refugee-receiving countries and international organizations now offer assistance and incentives for repatriation to origin countries. These programs typically pay cash and in-kind benefits. Table A-1 offers an illustrative but non-exhaustive list of these assisted repatriation programs.

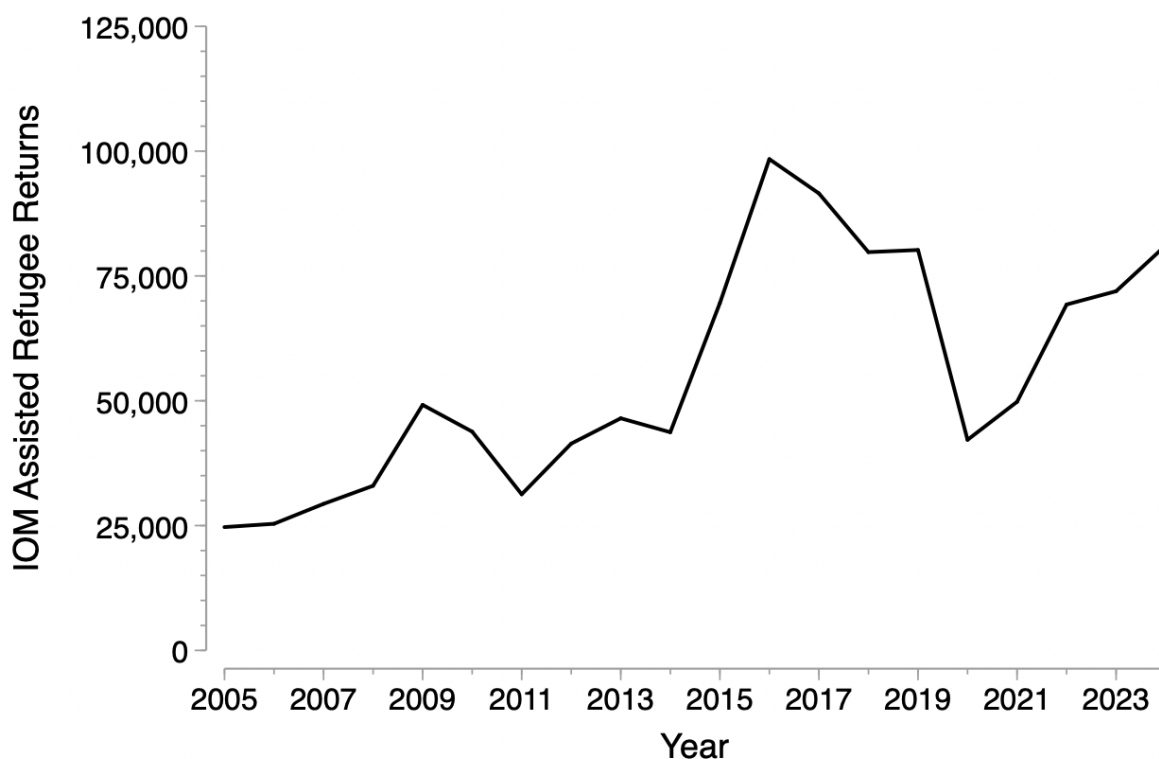
Table A-1: Prominent Repatriation Assistance and Cash-for-Return Programs

Host Country	Amount	Program Name	Source
Australia	—	RRAP	Kalicki (2020)
Austria	—	—	Kalicki (2020)
Belgium	\$2,000	REAB	Kalicki (2020)
Cambodia	—	—	UNHCR
Canada	—	AVRR	Kalicki (2020)
Czechia	\$1,000	—	Kalicki (2020)
Denmark	\$5,700 - \$18,700	—	Gerver (2017)
France	\$1,700	Aide au Retour Volontaire	Kalicki (2020)
Germany	\$1,400 - \$6,200	REAG, GARP, StarthilfePlus, DLDZJ	Kalicki (2020)
Ghana	\$100	—	Gerver (2017)
Greece	—	—	IOM
Iraq	—	—	IOM
Ireland	\$1,400 - \$2,900	—	IOM
Israel	\$3,500	—	Gerver (2017)
Italy	—	—	IOM
Japan	\$3,000	Nikkeijin	Kalicki (2020)
Libya	—	—	IOM
Myanmar	—	—	UNHCR
Netherlands	—	REMPLOD, REAN	Kalicki (2020)
Norway	—	—	IOM
Pakistan	\$200 - \$400	—	Esper, Krishnan and Wieser (2022)
Rwanda	—	—	UNHCR
Somalia	\$400 - \$800	—	UNHCR
Spain	\$9,400	APRE	Kalicki (2020)
South Sudan	—	—	UNHCR
Sri Lanka	—	—	UNHCR
Sweden	\$1,600 - \$7,200	—	Gerver (2017); Kalicki (2020)
Syria	—	—	UNHCR
Switzerland	—	—	IOM
Tanzania	In-Kind	—	Schwartz (2019)
United Kingdom	\$3,500	VARR, AVRFC	Gerver (2017); Kalicki (2020)
United States	\$1,000	CBP One	Sanchez and Elba (2025)

Note: We document a non-exhaustive list of 32 refugee-hosting countries that have implemented, independently or in collaboration with international organizational partners, repatriation assistance programs. Under these programs, countries and implementing organizational partners offer cash and/or in-kind assistance to migrants to induce migrants to return to their origin countries. Not all programs listed are currently active. Amounts of assistance paid are expressed in U.S. dollars, and represent the reported amount the average returnee is offered. In the source column, UNHCR and IOM indicate that program information was sourced from myriad reports published by one or both of those organizations.

We assemble data from the International Organization for Migration (IOM) to characterize the rise in assisted repatriation programming over time. In 2024, more than 81,108 refugees participated in IOM-implemented incentivized repatriation programs, operating across 121 host countries and 143 origin countries (IOM, 2025). Data on repatriation assistance programs implemented directly by host countries or by the UNHCR are not systematically available, so true numbers of incentivized returns are higher.

Figure A-2: Beneficiaries of IOM-Assisted Return Programs



Note: The solid line shows the number of refugee beneficiaries returning with IOM cash and in-kind assistance. Afghan returnees from Pakistan in 2016 are not included in these figures, as they returned under a UNHCR-administered program. *Source:* IOM (2025).

A.3 Is it Morally Permissible to Incentivize Refugee Return?

A small but growing literature in political philosophy and law examines whether it can ever be morally permissible to incentivize refugee repatriation. We survey debates in this literature below.

Under international law, the right to return is recognized as a fundamental right enshrined in statutory instruments and customary human rights law. Similarly, displaced people also enjoy the right to non-refoulement—freedom from being forcibly returned “to the frontiers of territories where [their] life or freedom would be threatened on account of [their] race, religion, nationality, membership of a particular social group or political opinion.” Together, the rights of return and non-refoulement define the legal backbone of refugee repatriation. Legal scholars emphasize that refugees’ enjoyment of these rights hinges critically on the voluntariness of their choices. Refugee repatriation must be free from coercion or manipulation in order to be considered truly voluntary (Bradley, 2013). A range of factors that shape refugees’ choices about whether and when to return are relevant when evaluating the voluntariness of their repatriation decisions. For instance, Long (2013) argues that refugee protection standards are violated where refugees decide to return as a result of poor information about conditions in origin countries, where hostile policies in hosting countries drive refugees to repatriate, or where refugees feel compelled to accept return offers because of power asymmetries between themselves and host state or international organizational officials. In all of these contexts, the consensual nature of repatriation is compromised. Legal and normative theorists generally agree that robust safeguards are needed to protect the voluntariness of repatriation decisions, including access to legal counsel, transparency about risks in origin countries, and independent monitoring of repatriation processes.

Building from these insights, political philosophers debate the moral permissibility of material inducements given to refugees to incentivize return. As we elaborate below, the consensus view is that paying refugees to repatriate is only morally permissible under a narrow set of circumstances that rarely materialize in actuality. Particularly important is that incentives used in repatriation programs must not transform voluntary return into a disguised form of refoulement or expulsion, which host countries and international organizations use to evade asylum obligations and coerce displaced people back to unsafe origin countries (Long, 2013; Schwartz, 2025). This requires that financial incentives for repatriation must be coupled with guarantees of free choice and genuine consent, and that refugees who refuse incentivized repatriation offers will not face worsening or coercive conditions in host countries (Gerver, 2017, 2018b). Critically, there must be at least one available option other than assisted repatriation that still ensures an acceptable standard of welfare.

Proponents Advocates of repatriation assistance programs make a range of primarily utilitarian arguments in support of incentivized return. For instance, Kalicki (2020) suggests that assisted return is consistent with basic liberal principles because it allows refugee-hosting countries to exercise their rights to migration control through a cheaper and (allegedly) more

humane mechanism than punitive border and immigration policing tactics states would otherwise have to resort to to reduce asylum inflows. Officials at the IOM extend this argument, suggesting that incentivized repatriation is “more cost effective and administratively expedient than forced return” (IOM, 2011). Indeed, this rationale is one of the explicit motivations for the U.S.’s recent decision to pay migrants \$1,000 to “self-deport.” Officials at the Department of Homeland Security argue that traditional enforcement and deportation operations cost taxpayers \$17,000 per removed migrant, making \$1,000 payments a significant cost-savings (Lowenkron and Caldwell, 2025).

Another argument put forth by advocates of incentivized repatriation focuses on foreign policy advantages of these programs. In particular, repatriation assistance can promote warmer bilateral relations between host and origin countries. Two factors drive this effect. First, origin countries often desire to encourage repatriation because return is a benchmark indicator of stability and progress. Second, if host countries are intent on reducing their populations of displaced people and are considering coercive means like deportation, incentivized repatriation represents a “politically more palatable and less sensitive [option] than the return of émigrés ‘in shackles’” (IOM, 2011). In other words, returnee-receiving countries may find it less galling to receive returnees repatriating with material inducements than to receive returnees simply forced to repatriate by coercive enforcement tactics perpetrated by host country authorities.

A final set of arguments in favor of cash-for-repatriation programs highlight purported benefits of repatriation assistance for displaced people themselves. For one, proponents of assisted repatriation hail the promise of aid in facilitating peaceful and productive reintegration of refugee returnees. Sustainable repatriation requires repatriates to reintegrate into economic and social networks in their origin countries. Ruben, van Houte and Davids (2009) argue return assistance is particularly helpful for facilitating reintegration, for instance by enabling repatriated refugees to find jobs and housing. Second, incentivize programs may also help refugees realize their return intentions. For displaced people who intend to return but cannot afford to do so, assistance programs that relax credit constraints and subsidize return journeys may help refugees actualize their desires to return (IOM, 2011).

Critics Critics of incentivized repatriation programs offer a range of arguments against these policies, and have made significant progress in clarifying the normative contours of the debate. First, detractors have established that voluntariness is the most important evaluative criterion for judging assisted repatriation programs. Incentives to repatriate are not morally permissible if they compromise the free, consensual nature of refugees decisions to return (Long, 2013; Gerver, 2018a). This idea underscores why it is inappropriate to frame repatriation assistance as a “humane alternative to deportation”—as IOM (2011) staff have argued. Refugees confronted with an incentivized return offer “must have at least one [alternative] option that ensures an acceptable level of welfare” if they reject assisted repatriation (Gerver, 2017, p. 635). Consider a host country that offers a refugee a choice between

accepting \$1,000 to return home, or the promise of detention and deportation at the hands of host state immigration authorities. In this setting, a refugees' choice to accept assisted repatriation is involuntary, since no alternative to assisted return provides an acceptable modicum of welfare. Put differently, to be voluntary, refugees must perceive the decision to take up assisted return as a positive opportunity, and not merely a means to avoid worse alternatives (Scalettaris and Gubert, 2019).

Several other conditions are also relevant for evaluating the morality of incentivized repatriation. One important requirement is that refugees who accept repatriation assistance must be fully informed about risks they may face during the return journey and as they attempt to reintegrate into their origin countries (Gerver, 2018b). Second, refugees who accept material inducements in exchange for repatriating must be allowed to re-enter host countries if conditions deteriorate and they are required to flee again (Gerver, 2017). Third, cash or in-kind transfers must be sufficient to ensure refugees' post-return safety and reintegration. Finally, the implementation of incentivized return policies must not reinforce host countries' deportation efforts or encourage host countries to expand the use of anti-migrant coercion Gerver (2018a). In practice, these conditions are rarely met. Feneberg (2025) and Schwartz (2025) show that repatriation assistance programs often encourage states to expand coercive migrant deportations, restrict access to asylum, and shirk international legal responsibilities on refugee protection.

A.4 How Return Migration Shapes Conflict

Existing scholarship on the consequences of return migration highlights a variety of negative and positive pathways operative in fragile repatriation settings. Economic, structural, institutional, and individual channels are relevant for shaping how return affects conflict. We classify mechanisms developed in extant work in Tables A-2 – A-3 below, before surveying the broader literature in detail.

A.4.1 Negative Effects of Return

Militancy Refugee repatriation is generally viewed as an indicator of peace and stability in post-conflict societies, but large-scale return can also ignite conflict. For instance, if militants have infiltrated refugee populations abroad, mass return can give fighters cover to re-enter origin countries for attacks (Harpviken and Lischer, 2013). In Afghanistan and Rwanda, rebel cadres have embedded among returnee populations to move combat matériel, disseminate their ideologies, and conscript vulnerable youths (Lischer, 2011; Schultz, 2011). When refugees return to contested or insurgent-held communities, governments may engage in preemptive repression, fueling grievances and militant mobilization (van Houte, 2017). Where returnees support government forces, violence may increase as insurgents launch retributive attacks to deter collaboration (van Houte, 2014). Conditions in origin countries can also drive returnees to support militants. Impoverished repatriates are often forced into itinerant or illicit jobs (Fransen, Ruiz and Vargas-Silva, 2017; Verwimp and Muñoz-

Table A-2: Negative Pathways Linking Return With Conflict

Category	Mechanism	Characteristic Example	Anticipated Effect on Conflict	Source(s)
Economic	Skill Deterioration and Human Capital Loss	Refugees facing restrictions on labor market participation in host countries suffer skill and human capital losses. When they return, they hold fewer economic assets and report lower subjective well-being.	Worsens Insurgent and Communal Violence	Fransen, Ruiz and Vargas-Silva (2017); Verwimp and Muñoz-Mora (2018)
	Acquisition of Criminal Capital	Refugees exposed to economic informality in host countries develop expertise in smuggling, fraud, and other illicit enterprises.	Worsens Insurgent and Communal Violence	van Houtte (2014); Nguyen and Nguyen (2025)
	Resource Scarcity and Competition	Returnees compete with stayees over land, water, employment opportunities, housing, and access to schools and healthcare.	Worsens Communal Violence	Allen and Morsink (1994) Joireman (2018)
	Price Shocks	Sudden, large-scale returns generates demand shocks, causing rapidly increasing prices (e.g., of food, housing).	Worsens Insurgent and Communal Violence	Camarena (2016)
	Remittance Shocks	Returnees who were sending remittances from abroad cease to do so upon repatriation.	Worsens Insurgent and Communal Violence	Scalettaris and Gubert (2019); Blair, Krick and Wright (2025)
Structural	Acquisition of Arms	Returnees displaced in regional conflicts repatriate with arms acquired abroad.	Worsens Insurgent and Communal Violence	Hoogeveen, Rossi and Sansone (2019); Akoi and Pendle (2020)
	Foreign Radicalization and Training	Refugees are recruited and militarized by armed groups in host countries. Upon return, they engage in militancy.	Worsens Insurgent Violence	Lischer (2011); Harpviken and Lischer (2013); Byman (2015); Haer and Hecker (2018)
	Preemptive Repression	Government forces engage in preemptive repression to deter insurgent infiltration of returnee populations.	Worsens Insurgent Violence	Stein and Cuny (1994)
	Foreign Subversion	Adversarial powers exploit return shocks to covertly surge support to proxy forces in a returnee-receiving country.	Worsens Insurgent Violence	Blair, Krick and Wright (2025)
	Identity-Based Division	Returnees entrench sectarian divides and reignite ethnopolitical mobilization.	Worsens Insurgent and Communal Violence	van Houtte (2014); Joireman (2017)
Institutional	Policy-Induced Divisions	Returnee-receiving states implement policies that privilege returnees or stayees. Unprivileged groups mobilize in response.	Worsens Insurgent and Communal Violence	Schwartz (2019)
	Unmet Expectations	Poorly-managed reintegration programs create crises of unmet expectations, as hopeful returnees learn about low state capacity, or returnee-receiving states.	Worsens Insurgent and Communal Violence	Kindersley (2017); Ambrosius and Meseguer (2020)
	Institutional Strain	Fragile state institutions are overwhelmed by large-scale returns, and returnees' demands for service provision.	Worsens Insurgent and Communal Violence	Camarena (2016 <i>b</i>) Scalettaris and Gubert (2019)
Individual	Negative Selection	Individuals who decide to return are more risk-tolerant, and consequently more inclined to participate in conflict.	Worsens Insurgent and Communal Violence	Jaeger et al. (2010); Ghosh et al. (2021)
	Ideological Motivation	Exposure to violence forces individuals to become refugees. Upon return, these repatriates are motivated to exact revenge on initial perpetrators.	Worsens Insurgent and Communal Violence	Pérez-Armendáriz and Duquette-Rury (2021); Krick (2025)
	Alienation and Trauma	Returnees feel marginalized and socially-isolated, and lack developed emotional or cognitive coping strategies.	Worsens Insurgent and Communal Violence	Siriwardhana and Stewart (2013); Fennig (2021) Dünçer and Şahin-Mencitek (2025)

Note: The table describes major mechanisms linking return migration with conflict in extant scholarship. We group mechanisms by category, focusing on economic, structural, institutional, and individual-level explanations. For each mechanism we offer a characteristic example and classify the predicted effect on conflict. The last column highlights key sources that develop the respective mechanism.

Mora, 2018), making them ripe targets for rebel recruitment (Haer and Hecker, 2018). If returnees have acquired arms and criminal capital while displaced (Nguyen and Nguyen, 2025), if they are negatively selected for risk tolerance (Ghosn et al., 2021), if they hold enduring ideological and identity-based motivations to fight (Joireman, 2017), or if they lack psychosocial support (Siriwardhana and Stewart, 2013) they may be highly attractive to insurgent recruiters. Other economic and institutional effects of return are also relevant. Where returnees were sending remittances from abroad, repatriation implies a drop in remittance income in receiving communities, lowering the opportunity costs of mobilization (Blair, Krick and Wright, 2025). This situation may be further exacerbated by return-induced demand shocks putting upward pressure on food prices (Camarena, 2016). States institutions in fragile origin countries are often ill-equipped to alleviate these tensions. (Scalettaris and Gubert, 2019), increasing dissatisfaction with the state (Ambrosius and Meseguer, 2020; Lakhani and Amiri, 2020).

Social Conflict In addition to militancy, refugee return may affect social conflict. As Schwartz (2019, p. 110) notes, “conflict between returning and non-migrant populations after civil war is a nearly ubiquitous issue for societies recovering from such wars.” Return-induced competition over jobs, housing, and land may spur criminality and communal strife (Allen and Morsink, 1994; Harild, Christensen and Zetter, 2015). In countries like Afghanistan, where livelihoods are tied to agriculture, property disputes are a particularly common source of grievance. In these settings, violent land clashes have erupted between returnees and host community members (Kamminga and Zaki, 2018; Ruiz and Vargas-Silva, 2022). These clashes have significant welfare implications. Economically, they destroy the productivity of land by increasing contamination with mines and damaging irrigation infrastructure (Seefar, 2019). Socially, land conflicts are likely to metastasize into broader tribal disputes or honor feuds, which can spur retaliatory killings (Murtazashvili, 2016; Joireman, 2018). Property disputes can also exacerbate insurgent violence, especially where returnees or stayees ally with militants to combat alleged usurpers (Lakhani and Amiri, 2020). More broadly, intergroup tensions can precipitate social violence. If returnee inflows alter sensitive demographic balances (Joireman, 2017; Camarena and Hägerdal, 2020), repatriation can spur ethnic conflict. Migration status (i.e., returnee or stayee) may itself take on identity salience if policies attach privileges to those collective categories. For instance, when government regulations were perceived as benefiting returnees in Burundi, violent cleavages erupted between returnees and non-migrant community members (Schwartz, 2019). This dynamic is especially likely to unfold over humanitarian assistance. Aggrieved hosts frequently clash with returnees who they believe hold disproportionate access to aid (Kibreab, 2002).

A.4.2 Positive Effects of Return

Other work emphasizes the contributions of returnees to security and stability. Above all, this perspective views returnees as a source of human capital, and hence an engine for peace-building and development. When returnees repatriate with knowledge, training, and expertise acquired through employment while displaced, they may transmit new skills into origin

Table A-3: Positive Pathways Linking Return With Conflict

Category	Mechanism	Characteristic Example	Anticipated Effect on Conflict	Source(s)
Economic	Skill Acquisition and Human Capital Gain	Refugees participate in labor markets in host countries, gaining knowledge, training, and technology. When they return, they transmit skills and productivity gains.	Reduces Insurgent and Communal Violence	Hausmann and Nedelkoska (2018) Bahar et al. (2024)
	Productive Social Norms	Refugees gain exposure to productive social norms favoring education, healthcare, and political engagement in host countries. Upon return, they transmit these norms.	Reduces Insurgent and Communal Violence	Chauvet and Mercier (2014) Bucheli and Fontenla (2025) Godlonton and Theoharides (2025)
	Infrastructural Investment	Governments and humanitarian organizations invest in service provision and public goods in returnee-receiving communities.	Reduces Insurgent and Communal Violence	Kibreab (2002); Davies et al. (2011)
	Aid Spillovers	Aid channeled toward returnee reintegration spillovers to stayees, boosting community-wide living standards and cohesion.	Reduces Insurgent and Communal Violence	Kibreab (2002) Harild, Christensen and Zetter (2015)
Individual	Positive Selection	Individuals who decide to return are positively selected for emotional attachments to home and interest in peacebuilding	Reduces Insurgent and Communal Violence	Blitz, Sales and Marzano (2005); Müller-Funk and Frausen (2023)
	Ideological Motivation	Exposure to violence forces individuals to become refugees. Upon return, these repatriates are motivated to prevent conflict.	Reduces Insurgent and Communal Violence	van Houtte (2014)
	Post-Traumatic Growth	Returnees traumatic experiences of displacement motivate strong commitments to peacebuilding.	Reduces Insurgent and Communal Violence	Lakhani and Amiri (2020)

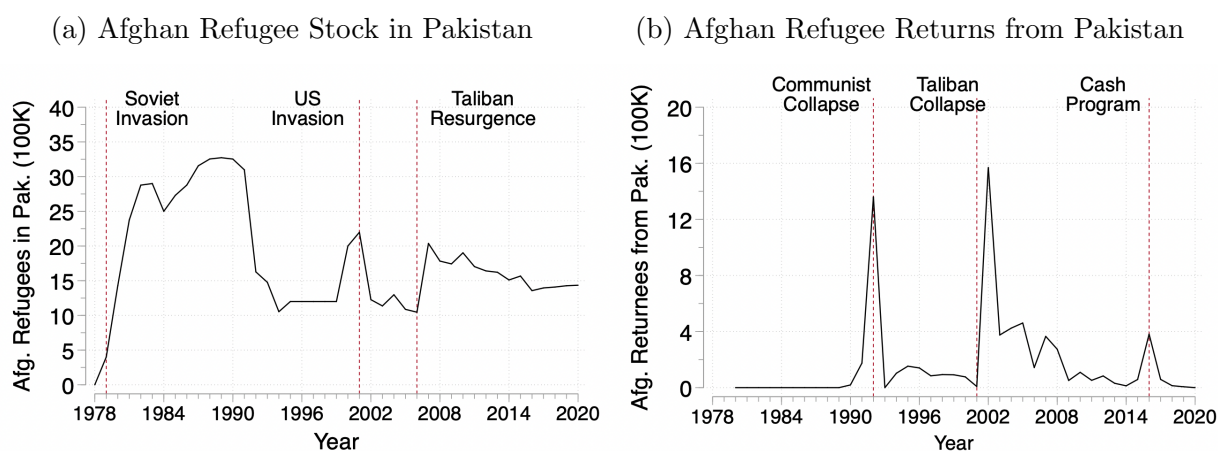
Note: The table describes major mechanisms linking return migration with conflict in extant scholarship. We group mechanisms by category focusing on economic, structural, institutional, and individual-level explanations. For each mechanism we offer a characteristic example and classify the predicted effect on conflict. The last column highlights key sources that develop the respective mechanism.

economies, fueling productivity and wage growth (Hausmann and Nedelkoska, 2018; Bahar et al., 2024). These effects reduce rebel and social conflict in returnee-receiving communities by increasing the attractiveness of licit employment versus violent mobilization (Bucheli, Fontenla and Waddell, 2019). New social norms that returnees acquire while displaced and impart to their non-migrant neighbors can also fuel entrepreneurship, educational attainment, health, and political participation (Chauvet and Mercier, 2014; Bucheli and Fontenla, 2025; Godlonton and Theoharides, 2025). At the individual level, returnees’ past experiences of violence foster emotional attachments to home (Blitz, Sales and Marzano, 2005), as well as self-efficacy and commitment to peacebuilding (Müller-Funk and Fransen, 2023). These factors make returnees a crucial asset for post-conflict reconciliation. Returnees’ familiarity with hardships of war may also lead them to oppose future violence as a result of post-traumatic growth (Lakhani and Amiri, 2020). More instrumentally, mass returns may attract aid and infrastructural investment, as governments and humanitarian organizations direct development programming and public goods provision to returnee-receiving communities (Kibreab, 2002; Davies et al., 2011). Positive externalities of these investments can benefit returnees to stayees, increasing market exchange and fostering cohesion (Harild, Christensen and Zetter, 2015).

A.5 Afghan Refugee Flight to and Return from Pakistan

From 1979–2020, the Afghan refugee population has ranged between 500,000 and 3.5 million. In the same period, the number of annual documented returns from Pakistan to Afghanistan has ranged from 0 to 1.6 million. In the median year over this period, Pakistan hosted 1.6 million Afghans, of whom 58,000 returned.

Figure A-3: Afghan Refugee Flight to and Return from Pakistan

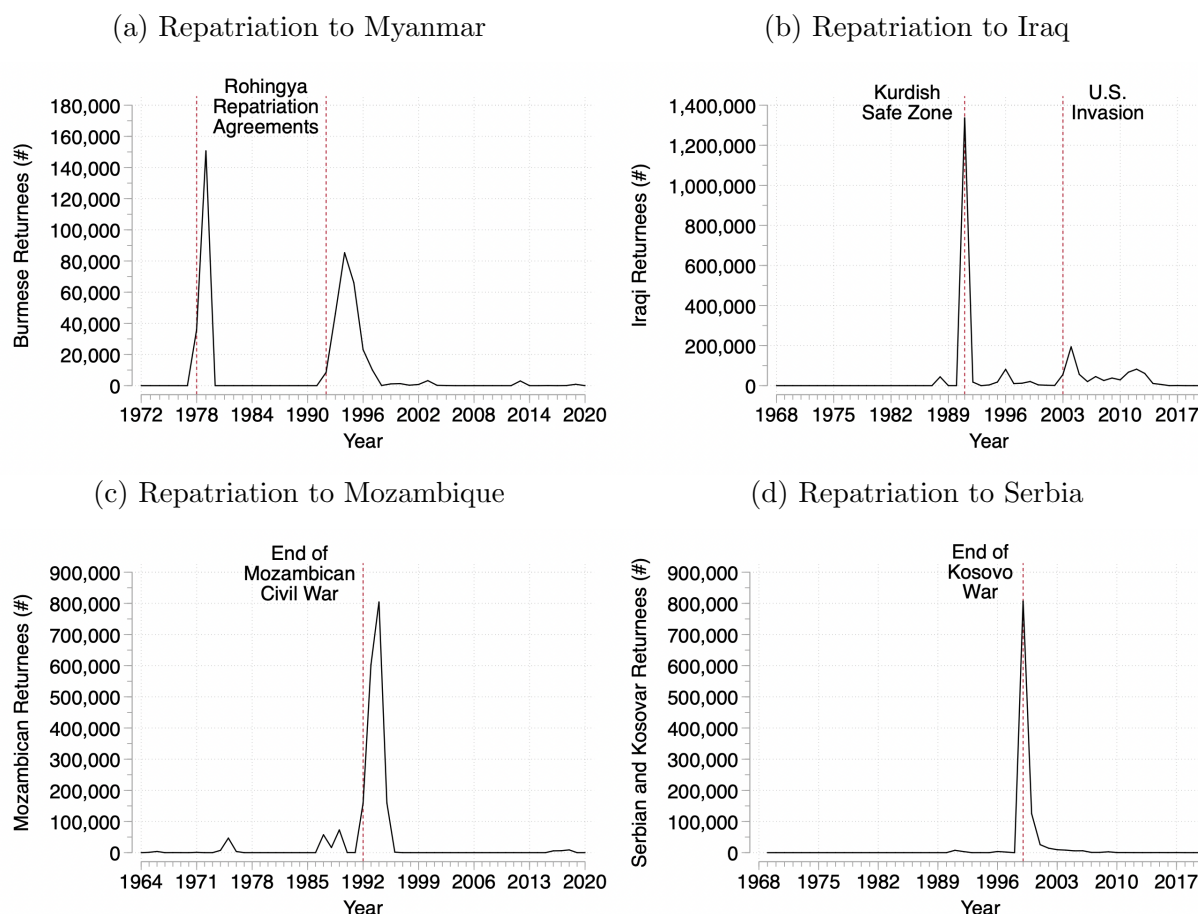


Note: Panel (a) depicts the stock of Afghan refugees in Pakistan over time. Panel (b) depicts documented Afghan refugee returns from Pakistan over time. *Source:* UNHCR Refugee Population Statistics database.

A.6 Wave Dynamics in Global Refugee Return

Forced displacement and refugee repatriation often occurs in waves (Holland and Peters, 2020). Repatriation from Pakistan to Afghanistan spiked in 1992 upon collapse of the communist PDPA government, in 2002 after the Taliban collapse, and in 2016 after the UNHCR encashment program. We study the 2016 return shock (panel (b) of Figure A-3). To build confidence in the generality of the repatriation shock we evaluate, we show that comparable wave-like dynamics emerge in patterns of refugee return across numerous other global contexts.

Figure A-4: Wave Dynamics in Patterns of Global Refugee Return

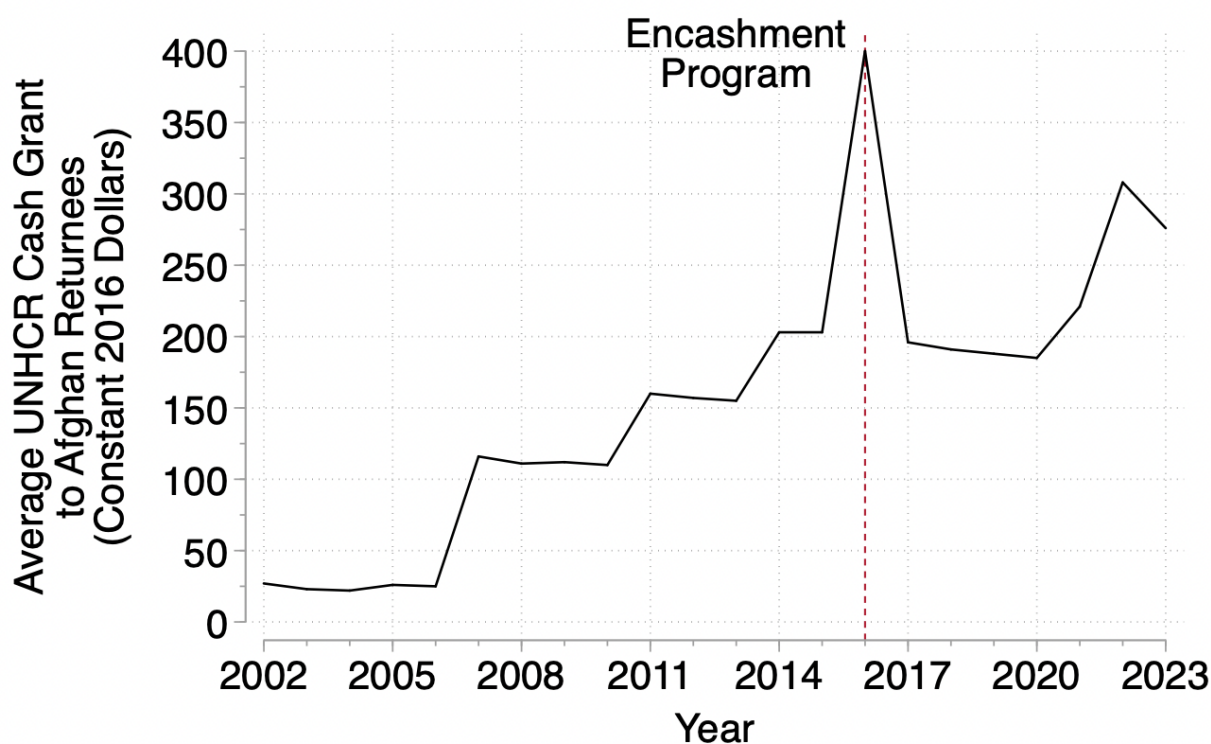


Note: Panels (a) – (d) respectively depict refugee returns to Myanmar, Iraq, Mozambique, and Serbia over time. *Source:* UNHCR Refugee Population Statistics database.

A.7 UNHCR Repatriation Cash Assistance for Afghan Returnees

Since 2002, the UNHCR has offered small, one-time repatriation cash assistance grants to Afghan returnees from Pakistan. Historically, these grants were calibrated to be just sufficient to cover the immediate transportation, logistical, and food expenses that returnees incurred on their journeys home. While UNHCR staff periodically increased cash assistance (e.g., in 2007, 2011, 2014), the adjustments were merely intended to keep pace with “rising commodities and transportation costs in Afghanistan” (UNHCR Afghanistan, 2014). Surveys fielded by UNHCR in Afghanistan in 2015, prior to the encashment program we study, validate these notions. For instance, 74% of surveyed returnees reported that they mainly spent their repatriation assistance grants on “pa[ying] transportation costs” or “b[uying] food and other commodities” (UNHCR Afghanistan, 2015). The 2016 encashment program was unique because it marked the first time that UNHCR officials raised repatriation cash allowances far beyond the costs of transportation and other essentials. As shown in Figure A-5, the size of the grant issued during the program was unparalleled—at no subsequent point have grants reached the same level.

Figure A-5: UNHCR Repatriation Cash Assistance for Afghan Returnees



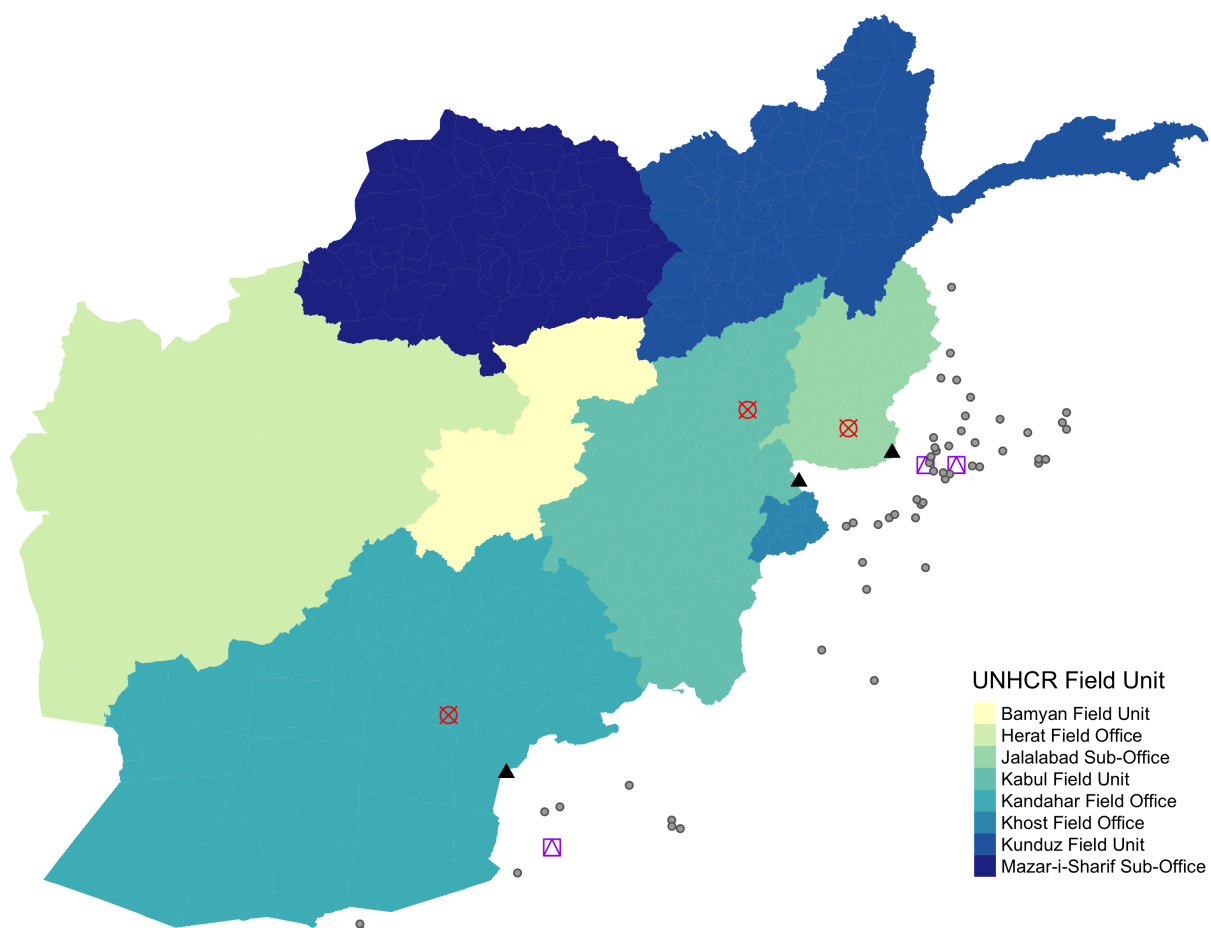
Note: The solid line shows the inflation-adjusted value (in 2016 dollars) of average UNHCR cash grants to Afghan refugee returnees. The dashed red line marks the year of the encashment program we study.

Source: Various UNHCR–Afghanistan Voluntary Repatriation reports.

A.8 The UNHCR Encashment Process

Under the 2016 UNHCR program, all registered Afghan refugees residing in Pakistan and holding a valid Proof of Registration (PoR) card were eligible to repatriate with UNHCR assistance. Individuals with no PoR card, and those who had returned and received assistance in previous years, were not eligible for the expanded repatriation grant. The procedures of encashed return were as follows.

Figure A-6: Mapping the Process of Assisted Return from Pakistan



Note: Gray dots mark Afghan refugee settlements in Pakistan. Purple tents mark Voluntary Repatriation Centres (VRCs) in Pakistan. Black triangles mark border crossings through which returnees re-entered Afghanistan after processing at VRCs. Red reticles (crosshairs) mark Encashment Centres (ECs) where returnees collected cash payments, and from which they returned to destination districts. Districts are shaded according to the UNHCR office responsible for a given area. *Source:* UNHCR Geoservices.

First, registered Afghan refugees wishing to return would approach the nearest UNHCR Voluntary Repatriation Centre (VRC). Two long-standing VRCs were operated in Baleli,

Quetta, Pakistan and Chamkani, Peshawar, Pakistan. A third VRC was opened at Azakhel, Peshawar, Pakistan in September 2016 to help process the mass flow of intended returnees during the program. At the VRC, refugees were de-registered from the Pakistani system, had their PoR cards invalidated, and were issued with Voluntary Repatriation Forms (VRFs). All returning family members had to be present at the VRC to declare their return, and an iris scan or thumb impression was conducted on all returnees over five years of age to ensure that no one received assistance twice.

Following PoR invalidation, refugees no longer held legal status in Pakistan. However, VRF forms allowed safe travel under UNHCR supervision between the VRC and the nearest border crossing. Designated routes were specified for returnees to travel from VRCs to border crossings, and returns were processed through one of three border crossings: Torkham, Chaman, or Kharlachi. No customs or fees were assessed on household items and goods brought by returning refugees.

Once across the border into Afghanistan, all registered returnees had to travel to one of three Encashment Centres (ECs). The three ECs processing returnees from Pakistan were Jamail Mayna in Kandahar, Samar Khail in Nangarhar, and the Returnee Centre in Kabul. To ensure timely return, UNHCR required that returnees must approach an EC within seven days of having been issued a VRF in Pakistan. Returnees who failed to arrive at an EC in the requisite seven day period would not receive cash assistance, though exceptions were made for groups who experienced death or serious illness of a family member between the time of VRF issuance and EC arrival. No cash assistance was ever provided to returnees arriving more than 15 days after VRF issuance.

At ECs in Afghanistan, returning refugees were given several important trainings. In particular, returnees were briefed on mine awareness and access to education in Afghanistan. In addition, basic health services were provided, and all children five years of age or younger were vaccinated for measles and polio. Interviews were also conducted with returnees at ECs. During interviews, returnees could report instances of bribery and extortion during the journey from VRCs.

The most important procedure at ECs was cash distribution. On average, each returnee during the 2016 program received \$400 in cash, up from an average grant of \$200 given before (and after) the program. The average \$400 grant consisted of \$350 for short-term integration and \$30-70 for transportation costs. The transportation grant varied according to the travel distance from a refugee returnee's area of residence in Pakistan to their area of origin in Afghanistan. After encashment, all returnees were free to leave the EC and repatriate to their intended destination community.

The size of the 2016 cash grant was large relative to key economic benchmarks. In [Table A-4](#), we compare the amount of the grant paid to each individual returnee against GDP per

capita and average rents and wages in Afghanistan at the time of the program. Returnees received at least several months of rent or income.

Table A-4: Returnee Cash Assistance Was Economically Meaningful

	Annual Afghan GDP/Capita (\$509)	Monthly National Average Rent (\$250)	Monthly National Average Wage (\$160)
Returnee Cash Assistance (\$400)	79%	160%	250%
	Monthly Returnee Wage-Low (\$73)	Monthly Returnee Wage-High (\$146)	Monthly Part-Time Fighter Wage (\$300)
	548%	274%	133%

Note: All cells depict returnee cash assistance as a percentage of the top row indicator. 2016 Afghan GDP/capita comes from the World Bank. Mean monthly rent comes from [Majale \(2017\)](#). Mean monthly wages nationwide come from a representative survey fielded by the Asia Foundation. [Haroon et al. \(2019\)](#) report mean monthly wages of the “bread-winner” in each family of Afghan returnees from Pakistan, with low and high estimates. [Landinfo \(2017\)](#) report mean monthly wages of part-time Taliban cadres during the fighting season. Amounts in US dollars are converted from Afghanis by June 29, 2016 exchange rates.

A.9 Spontaneous Returns During the Encashment Program

As Figure 1 shows, flows of both assisted (i.e., documented) and spontaneous (i.e., undocumented) Afghan returnees from Pakistan increased during the 2016 encashment program. Spontaneous returns were particularly large in the first two months of the program (July and August 2016), before gradually tapering. Our primary focus is on assisted returnees, who received UNHCR cash assistance upon repatriation, rather than spontaneous returnees, who did not receive cash assistance. Still, it is important to clarify why relatively large numbers of spontaneous returnees repatriated during the program period, despite the absence of a cash incentive. Three factors are relevant.

First, the chief reason levels of undocumented return also spiked, especially in the first two months of encashment program, was information scarcity. Prospective refugee returnees residing in Pakistan are predominately illiterate and lowly educated ([Haroon et al., 2019](#)). Media penetration of refugee settlements in Pakistan is also weak. Consequently, word-of-mouth is the primary means by which refugees learn about voluntary repatriation opportunities. Just prior to the implementation of the 2016 encashment program we study, UNHCR found that 84% of Afghan refugees in Pakistan either had no information about the voluntary repatriation process, or only knew about this process through rumor networks within the Afghan displaced population ([UNHCR, 2016a](#)). The earliest returnees could have become aware of the incentivized repatriation program was June 23, 2016, when it was first announced. However, with news spreading by word-of-mouth, information about the program and the process required to access repatriation cash took several weeks to disseminate. In August 2016, UNHCR moved to rectify refugee confusion about the specific

procedures returnees had to follow to repatriate with UNHCR assistance. Specifically, UNHCR launched “mass and targeted information campaigns” to publicize the program and procedures (UNHCR, 2016b). The largest such campaign was “Khpal Watan, Gul Watan” (“My country, my beautiful country”), a joint effort of UNHCR and the Afghan government. Through this program, radio broadcasts and billboard advertisements were posted in Afghan refugee-populated areas of Pakistan. These publicity efforts seemed to work. Qualitative UNHCR surveys show that during the encashment program, the share of Afghan refugees who reported hearing about the voluntary repatriation process from media or UNHCR communications increased from 5%–24% (UNHCR, 2017).

Bureaucratic overload is the second factor explaining why spontaneous returns also spiked during the early phases of the encashment program. Following the sudden rollout of the expanded cash intervention, the numbers of Afghan refugees seeking repatriation surged dramatically. Within eight weeks of implementation, more Afghans had returned with UNHCR facilitation than had returned in 2014 and 2015 combined. Thousands of refugees arrived at Voluntary Repatriation Centres (VRCs) in Pakistan every day, overwhelming UNHCR staff responsible for processing repatriation requests. Although UNHCR quickly scaled logistical capacity, increasing staffing and equipment and reducing cumbersome administrative processes to expedite return, many prospective Afghan returnees faced long queues and extended wait times (UNHCR, 2016b). This was especially true early in the encashment period, when only two VRCs were operating in Pakistan. Facing a prolonged wait for case processing, some refugees who intended to repatriate through formal UNHCR channel simply opted to cross into Afghanistan on their own, wrongly assuming they could still claim cash assistance upon arrival in Afghanistan. These individuals spontaneously returned because of bureaucratic overload at UNHCR facilities, and their own mistaken beliefs about the compensation process. In September 2016, UNHCR completed a third VRC at Azakhel in Peshawar, Pakistan, alleviating most prevailing administrative and processing delays (UNHCR, 2017). This also helps explain the reduction in spontaneous returns from September–December 2016.

Adverse hosting conditions in Pakistan are the third reason some returnees spontaneously repatriated during the encashment period. In particular, some Afghan refugees reported escalating hostility from Pakistani officials and host community members in summer 2016 (Human Rights Watch, 2017). Facing acute crises like discriminatory job losses, evictions, and police harassment, these Afghan refugees may have chosen to flee back to Afghanistan immediately, rather than waiting for UNHCR assistance. We discuss these forced returns in greater detail in section A.11. Available evidence suggests that this factor explains a small but important share of overall spontaneous returns during the encashment program.

A.10 Financing Return

In Table A-5 we study data from the Survey of Afghan Returnees. We specifically examine a response to the question “How did you finance your trip back to Afghanistan?” Our focal outcome, reflected in columns 1–2, is “support from UNHCR.” In columns 3–7 we study placebo outcomes based on other responses to the financing question, like savings, gifts/loans, and “support from IOM.” By definition, encashment beneficiaries repatriated through UNHCR-administered rather than IOM-administered. The distinguishable negative effect of beneficiary status on financing through IOM aid is therefore natural.

Table A-5: Encashment Beneficiaries Financed Return with UNHCR Aid

	Financed Return Through: (=1)						
	(1) UNHCR Aid	(2) UNHCR Aid	(3) Savings	(4) Gifts or Loans	(5) Selling Property	(6) IOM Aid	(7) MoRR Aid
Encashment Beneficiary	0.153*** (0.030)	0.100** (0.046)	-0.068 (0.067)	0.020 (0.044)	0.039 (0.033)	-0.102*** (0.035)	-0.001 (0.001)
Observations	7345	7345	7345	7345	7345	7345	7345
Clusters	65	65	65	65	65	65	65
Control Group Mean	0.074	0.074	0.539	0.259	0.067	0.043	0.000
Control Group SD	0.262	0.262	0.498	0.438	0.250	0.202	0.019
PARAMETERS							
District FE		✓	✓	✓	✓	✓	✓
Wave FE		✓	✓	✓	✓	✓	✓
Urbanicity		✓	✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓	✓
Age		✓	✓	✓	✓	✓	✓
Ethnicity		✓	✓	✓	✓	✓	✓
Education		✓	✓	✓	✓	✓	✓
Income		✓	✓	✓	✓	✓	✓
Dwelling		✓	✓	✓	✓	✓	✓
Marital Status		✓	✓	✓	✓	✓	✓
Tazkira		✓	✓	✓	✓	✓	✓
Country of Asylum		✓	✓	✓	✓	✓	✓
Registration Status x Month of Return		✓	✓	✓	✓	✓	✓
Social Desirability		✓	✓	✓	✓	✓	✓
Interview Order		✓	✓	✓	✓	✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights.

A.11 Motivations for Implementing The Encashment Program

As described in the main text, the encashment program resulted from more than a year of secret negotiations between UNHCR and the government of Pakistan. Throughout the period in the lead-up to the program, and for several months after the program began, Pakistan engaged in a parallel campaign to coerce Afghan returns. To contextualize these developments, and clarify the motivations of Pakistani and UNHCR officials, we offer brief background below. Interested readers may also consult [Bjelica \(2016\)](#) and [Human Rights Watch \(2017\)](#).

The repatriation cash assistance scheme we study was implemented by UNHCR in response to Pakistani efforts to reduce the Afghan refugee population hosted in Pakistan's tribal areas. What explains intensifying Pakistani pressure to reduce the refugee-hosting burden? Evidence suggests that Pakistan's insistence on reducing its hosting burden grew out of two major issues in Pakistani domestic politics and Afghanistan-Pakistan foreign relations: (1) the 2014 Peshawar school attack; and (2) growing economic ties between Afghanistan and India, Pakistan's major geopolitical rival ([Bjelica, 2016](#)). Both of these issues contributed to worsening Pakistani treatment of Afghan refugees in 2015, and pressure for incentivized returns in 2016.

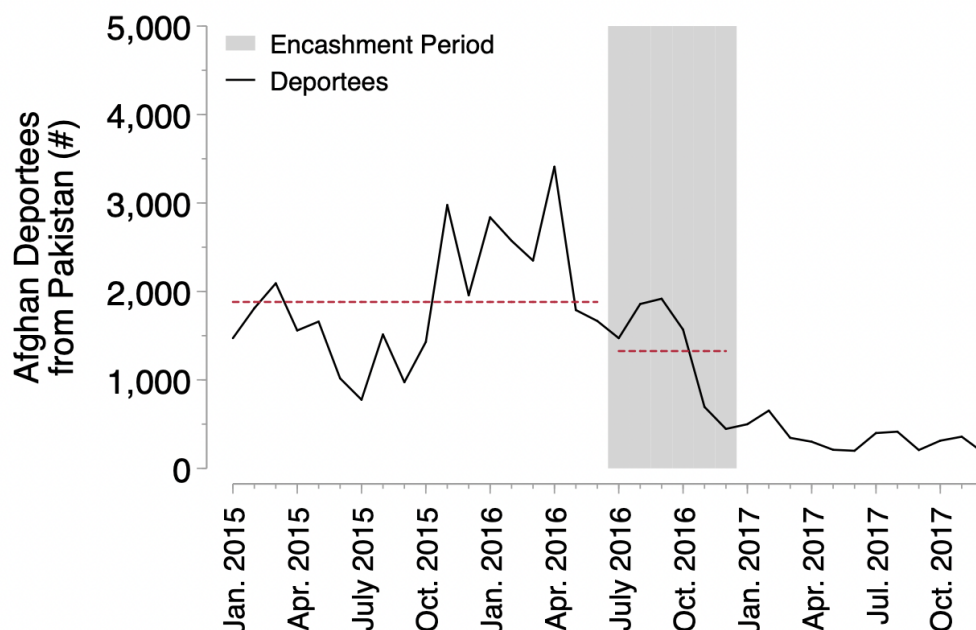
The immediate catalyst for Pakistani politicians' desire to reduce the Afghan refugee-hosting burden was a school massacre perpetrated by Tehrik-i-Taliban Pakistan (TTP) militants in December 2014. Specifically, on December 16, 2014, six TTP militants killed 149 people in an attack on a school in Peshawar, Pakistan. Although no Afghan refugees were involved in the violence, the attack spurred a wave of anti-Afghan xenophobia. The Pakistani government contributed to this xenophobic backlash by claiming the attack was planned in Afghanistan, and by identifying two of the six perpetrators as Afghan nationals. In the immediate aftermath of the attack, Afghan refugees in Pakistan reported rising anti-Afghan sentiment, retributive evictions and job losses, extensive police harassment, and an increasingly coercive environment throughout 2015 ([Bjelica, 2016](#); [Human Rights Watch, 2017](#)). Calls from Pakistani politicians advocating deportations and forced returns grew louder at this time, particularly during Pakistan's 2015 local elections.

Pakistan's growing concern over Afghanistan's warming ties with India provided the second key motivation for Pakistani desires to restrict Afghan refugees. Specifically, in response to worsening relations with Pakistan in 2014–2015, Afghanistan began deeper pursuit of trade ties with India. The result was a series of bilateral agreements, the largest of which was the India-Afghanistan Friendship Dam announced in December 2015 ([Bjelica, 2016](#)). This and other Indo-Afghan infrastructure agreements were particularly concerning for Pakistan, since these projects aimed at diversifying Afghanistan's trade and energy infrastructure away from reliance on Pakistan ([Human Rights Watch, 2017](#), p. 16-17). Afghan cooperation with India exacerbated prevailing anti-Afghan sentiment in Pakistan, and further reinforced Pakistan's intransigent negotiating position in secret talks with UNHCR.

A.12 Anti-Refugee Coercion in Pakistan

Reporting by [Human Rights Watch](#) (2017, p. 18-27) suggests that in summer 2016, after the cash program had begun, Pakistani police and landlords continued to pursue anti-refugee harassment, including bribe-taking, rent hikes, and arbitrary increases in fees for access to documents and services. These specific forms of coercion are all consistent with predation following the unexpected windfall in returnee support, and were likely used to stimulate refugee uptake of the encashment program itself. In short, the program likely motivated these forms of economic coercion. Figure A-8 clarifies this point.

Figure A-7: Anti-Refugee Coercion in Pakistan



Note: The solid line shows deportations of Afghans from Pakistan. Dashed red lines denote mean levels of deportation before and during the repatriation cash program. Gray bars denote the period of the repatriation cash program. *Source:* IOM reports.

To better understand anti-refugee coercion in Pakistan, we plot deportations of Afghans from Pakistan over time in Figure A-7. Deportations are highly correlated with broader, anti-refugee repression ([Human Rights Watch, 2017](#); [Seefar, 2019](#)), and so can help illuminate relevant trends in harassment and abuse. Levels of deportations were high and increasing throughout much of the pre-program period. The start of the program coincided with a modest uptick in coercion that then sharply decreased after September 2016. Overall, the

average number of deportations per month was slightly lower during the encashment program (1326) than before (1881).

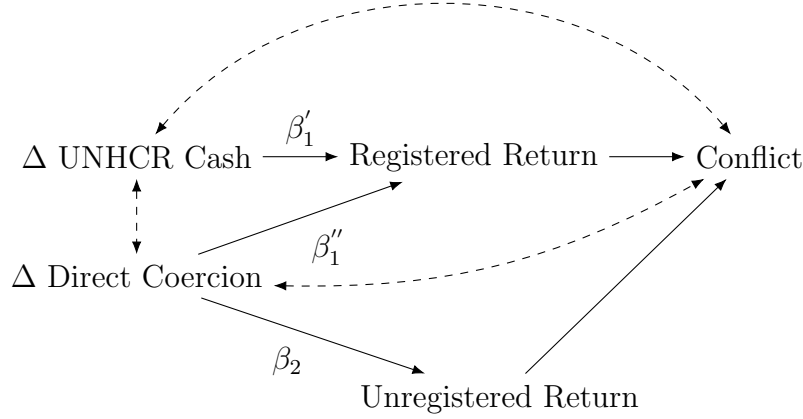
Of course, anti-refugee coercion in Pakistan before and during the cash program raises natural identification questions. In order to bias our estimations, coercion in Pakistan would have had to escalate at the same time the program was implemented. Yet, as reporting by Bjelica (2016) and Human Rights Watch (2017) makes clear, coercion was an enduring feature of the period from December 2014 through September 2016—a period spanning 18 months before the cash program and the first 4 months of the program itself. Figure A-7 makes this explicit—deportations were actually greater before than during the cash program. If coercion was primarily driving the returns we study, we would expect to see large increases in return in the pre-program period, as Afghan refugees fled Pakistani crackdowns. We do not observe any such pattern (Figure 1). Instead, returns spiked shortly after the encashment program began and subsided directly as it ended. Moreover, while spontaneous, undocumented returns historically exceeded documented, assisted returns in Afghanistan, this trend reversed during the encashment program (Figure 1). This pattern is highly suggestive of returnees making conscious efforts to repatriate through formal channels to obtain the cash grant.

To further assuage concerns, we provide multiple additional pieces of evidence. First, we formalize our design with a directed graph, which clarifies key parameters of interest (Figure A-8). Second, in section A.12 we highlight qualitative evidence from interviews conducted by Human Rights Watch (2017), which underscores the importance of cash assistance in enabling repatriation. Third, in Tables A-6 – A-7 we present survey evidence on returnees’ self-reported reasons for return. We find no evidence that program beneficiaries were more likely to report repatriating because of repressive conditions in Pakistan. Fourth, we show that our focal effects are unique to encashed returns. In contrast, spontaneous, undocumented returnees—who did not receive cash assistance—had no distinguishable effects on conflict (Table 4). By demonstrating that non-beneficiary returnees who repatriated during the program period did not impact the conflict, we offer strong suggestive evidence that cash assistance is the core mechanism.

Directed Graph Some subset of the variation in return may have been purely motivated by the cash assistance program, even in the absence of coercion, while another subset was motivated by the combination of these factors. We lay out this causal chain as a directed graph in Figure A-8. Note that the direct effect of policy-induced return on conflict is captured in β'_1 , while the indirect effect of the policy on return, mediated by coercion, is captured by β''_1 . What we estimate in the main specification is the reduced form combination of these pathways. We can call this parameter β_1 . We cannot separately estimate β'_1 and β''_1 . Yet, we provide multiple points of evidence for the importance of the cash mechanism.

Leveraging the notation in Figure A-8, we can account for a secondary pathway through

Figure A-8: Directed Graph on Encashment, Coercion, and Refugee Return



Note: Directed graph clarifying mapping between conceptual mechanisms and estimated quantities in research design. Parameters are intended to capture the reduced form effect of each parameter through the corresponding, total pathway. Dashed lines indicate unlikely direct causal pathways.

which coercion that escalated after the program’s announcement impacted violence in Afghanistan: β_2 . Unregistered returns during the period of the encashment program—the type of returns most likely to be caused by any escalating coercion (Human Rights Watch, 2017; Seefar, 2019)—did not have the same effect on conflict in Afghanistan as encashed returns. We provide evidence of this point in Table 4, where we consider the impact of spontaneous, unregistered returns. These returnees may have been influenced by coercion during the program period, but were not eligible for cash assistance. While we cannot recover β_1'' , we can estimate β_2 . If β_1 remains robust while accounting for the channel captured in β_2 , this suggests the reduced form evidence in the main specification is not contaminated by the other pathway through which coercion influenced return (i.e., return without cash aid). Furthermore, if β_1 is distinct from β_2 in magnitude and/or sign, this suggests the dominant driver of return-related shifts in conflict is due to the presence of financial assistance and not just the escalation of coercion. On the other hand, we should see similar effects of both registered (i.e., encashed) return and unregistered (i.e., spontaneous, undocumented, un-encashed) return on conflict in Afghanistan if escalating coercion and not cash assistance is the main mechanism. Using data on undocumented returns from the IOM Displacement Tracking Matrix, we re-estimate our focal specifications while adding a term for the undocumented returnee share interacted with an indicator for the grant period (Table 4). These results show that the main effects are robust, and unique to assisted returnees who received repatriation cash. In contrast, effects are null or opposite for undocumented returnees, who did not receive cash assistance. We provide an additional discussion of these tests in section B.14.

Qualitative Evidence on Encashment and Coercion As described in [Human Rights Watch \(2017\)](#), Human Rights Watch (HRW) officials interviewed 92 Afghan refugee returnees to Kabul between October 26 and November 1, 2016. Quotes from these interviews are revealing, and suggest that the UNHCR cash program we study played a central role in returnees’ decisionmaking. Indeed, as early as 2014—before the expanded cash program we study—when average assistance was just \$200, UNHCR had found that “more than 30% of Afghan returnees ... cited UNHCR assistance packages as a pull factor to return” ([Human Rights Watch, 2017](#), p. 41). In HRW’s subsequent interviews, although many returnees also cited the role of Pakistani coercion as a push factor, they noted that cash assistance was generally what made return feasible. In other words, their return would likely not have happened in the absence of the cash assistance program we study. Excerpts below underscore these points.

- “Fearing destitution in Afghanistan, the doubling of the UNHCR cash grant to returnees was a critical factor in their decision to leave.” ([Human Rights Watch, 2017](#), p. 25)
- “Numerous returning Afghan refugees told Human Rights Watch they would have been too poor to leave without UNHCR’s money.” ([Human Rights Watch, 2017](#), p. 26)
- “[A 33-year-old Afghan refugee in Mardan, Pakistan] finally decided to leave because he was afraid that if he didn’t, he risked deportation alone and with nothing, instead of leaving with his family and almost \$3,000.” ([Human Rights Watch, 2017](#), p. 26)
- “Others fearing destitution in Afghanistan said that UNHCR money was their only hope of surviving after leaving Pakistan; and that without the money they would have remained in Pakistan and hoped the government would treat them better in 2017.” ([Human Rights Watch, 2017](#), p. 26)
- “Many returning Afghan refugees told Human Rights Watch that UNHCR’s cash hand-out was an important factor in their decision to return after Pakistani police abuses started.” ([Human Rights Watch, 2017](#), p. 40)
- “Many Afghan refugees interviewed by Human Rights Watch said that UNHCR’s cash grant was key to taking the decision to return to Afghanistan. UNHCR told Human Rights Watch that many of the returnees it interviewed in Kabul and Jalalabad said the same.” ([Human Rights Watch, 2017](#), p. 49)

A.13 Reasons for Return

In Tables A-6 and A-7 we study data from the Survey of Afghan Returnees. Specifically, we examine responses to the question “Why did you return?” Open-ended responses were collected and categorized with pre-codes determined by the survey administration team. In Table A-6 we assess six outcomes related to negative experiences in the host country: (1) “Deported/forcibly removed from host country”; (2) “People of the host country were unwelcoming”; (3) “Could not get visa/permanent residency in host country”; (4) “Poor security conditions in the host country”; (5) “Poor economic conditions in the host country”; and (6) “Unemployment in host country.”

Table A-6: Encashment Beneficiaries’ Self-Reported Push Factors

	Reason for Return: Push Factors (=1)					
	(1) Deported/ Forcibly Removed	(2) Unwelcoming Conditions	(3) Lacked Visa/ Permanent Residency	(4) Poor Security	(5) Poor Economy	(6) Unemployment
Encashment Beneficiary	-0.043 (0.051)	-0.006 (0.022)	0.015 (0.036)	0.005 (0.031)	-0.001 (0.039)	0.003 (0.043)
Observations	7382	7382	7382	7382	7382	7382
Clusters	65	65	65	65	65	65
Control Group Mean	0.212	0.056	0.081	0.097	0.120	0.220
Control Group SD	0.409	0.231	0.272	0.295	0.325	0.414
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓	✓	✓
Urbanicity	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓
Ethnicity	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓
Income	✓	✓	✓	✓	✓	✓
Dwelling	✓	✓	✓	✓	✓	✓
Marital Status	✓	✓	✓	✓	✓	✓
Tazkira	✓	✓	✓	✓	✓	✓
Country of Asylum	✓	✓	✓	✓	✓	✓
Registration Status x Month of Return	✓	✓	✓	✓	✓	✓
Social Desirability	✓	✓	✓	✓	✓	✓
Interview Order	✓	✓	✓	✓	✓	✓

Note: * p <.10, ** p <.05, *** p <.01. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights.

In Table A-7 we assess six additional outcomes related to positive expectations about conditions in Afghanistan: (1) “Economic situation improved in Afghanistan”; (2) “Security situation improved in Afghanistan”; (3) “Family reunification”; (4) “People are being treated well in Afghanistan”; (5) “Education situation improved in Afghanistan”; and (6) “Patriotism.” Although the survey did not ask directly about cash assistance as an inducement to return, we believe that the role of cash assistance is most likely to be captured by perceptions that the “economic situation [had] improved in Afghanistan.” Strikingly, this is the only factor for which we find a distinguishable positive effect. Encashment beneficiaries are 1.6 percentage points more likely to self-report returning because of improving economic conditions in Afghanistan.

Table A-7: Encashment Beneficiaries’ Self-Reported Pull Factors

	Reason for Return: Pull Factors (=1)					
	(1) Improving Economy	(2) Improving Security	(3) Family Reunification	(4) Welcoming Conditions	(5) Improving Education	(6) Patriotism
Encashment Beneficiary	0.016* (0.009)	0.013 (0.022)	0.018 (0.041)	-0.009 (0.013)	0.003 (0.003)	0.003 (0.020)
Observations	7382	7382	7382	7382	7382	7382
Clusters	65	65	65	65	65	65
Control Group Mean	0.010	0.022	0.122	0.011	0.006	0.028
Control Group SD	0.098	0.147	0.327	0.102	0.079	0.165
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓	✓	✓
Urbanicity	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓
Ethnicity	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓
Income	✓	✓	✓	✓	✓	✓
Dwelling	✓	✓	✓	✓	✓	✓
Marital Status	✓	✓	✓	✓	✓	✓
Tazkira	✓	✓	✓	✓	✓	✓
Country of Asylum	✓	✓	✓	✓	✓	✓
Registration Status x Month of Return	✓	✓	✓	✓	✓	✓
Social Desirability	✓	✓	✓	✓	✓	✓
Interview Order	✓	✓	✓	✓	✓	✓

Note: * p <.10, ** p <.05, *** p <.01. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights.

B Empirical Appendix

In this brief empirical appendix, we introduce supplemental results.

B.1 Summary Statistics: District-Level Analyses

Descriptive statistics for variables from our main, district-level analyses are provided here.

Table B-8: Summary Statistics: District Analyses

	Observations	Mean	Std. Dev.	Min	Max
DEPENDENT VARIABLES					
Incidence of Insurgent-Initiated SIGACTs (=1)	4378	0.553	0.497	0.000	1.000
Insurgent-Initiated SIGACTs/100k Pop.	4378	7.002	20.736	0.000	464.178
Incidence of Communal Violence (=1)	4378	0.022	0.147	0.000	1.000
Communal Violence/100k Pop.	4378	0.040	0.358	0.000	12.533
INDEPENDENT VARIABLES					
Share of 2013 Documented Returnees from Pakistan	4378	0.003	0.007	0.000	0.087
Grant Increase	4378	0.545	0.498	0.000	1.000
Share Returning to Origin Province	4378	0.480	0.454	0.000	1.000
Share Returning to Origin District	4378	0.402	0.426	0.000	1.000
Prefer Shura to State Court	3817	0.481	0.143	0.123	0.942
Prefer Elders to Gov't Officials	3817	0.403	0.154	0.000	0.906
Took Dispute to Shura	3795	0.206	0.154	0.000	1.000
Taliban Control	4378	0.538	0.499	0.000	1.000
CONTROL VARIABLES					
Share of Population-Speaking Pashtun	4378	0.456	0.426	0.000	1.000
Population	4378	53904.812	37210.014	12467.000	151674.000
Distance to International Border	4378	0.858	0.649	0.024	2.840
Travel Time to Provincial Center	4378	97.166	140.276	0.000	1477.900
Provincial Unemployment	4378	12.782	9.551	1.900	42.300
Provincial GINI Coefficient	4378	25.997	4.777	18.800	36.100
Nightlights/Population	4378	2.852	5.336	0.000	56.201
Ruggedness	4378	361.469	236.501	4.933	1030.219
Share of Land Under Cultivation	4378	20.070	23.927	0.000	100.000
Share of Cultivated Land Under Irrigation	4378	69.855	37.065	0.000	100.000
Hectares of Opium Cultivation	4378	1.957	3.081	0.000	10.552
Hectares of Opium Eradication	4378	0.686	1.684	0.000	7.936
Governance Spending/Population	4378	183190.471	632141.061	0.000	4687742.159
Infrastructure Spending/Population	4378	76669.563	301558.564	0.000	2140158.119
Health Spending/Population	4378	752798.881	3375408.841	0.000	39326316.661
Conflict Stabilization Spending/Population	4378	598783.819	4777412.345	0.000	52504168.908
Refugees Spending/Population	4378	3201.398	20403.410	0.000	301105.179

Note: Observations are district-months used in the main estimating sample (2016). Population is winsorized at the top and bottom 5 percentiles. Hectares of opium cultivation and eradication are transformed by the inverse hyperbolic sine.

B.2 Summary Statistics: Survey Analyses

Descriptive statistics for variables from our survey analyses are provided here.

Table B-9: Summary Statistics: Survey Analyses

	Observations	Mean	Std. Dev.	Min	Max
DEPENDENT VARIABLES					
Returnee-Stayee Relations Index (ICW)	7391	0.028	0.981	-4.523	1.624
Returnee-Stayee Relations Index (PCF)	7283	0.027	0.971	-4.970	1.219
Experienced Communal Violence (=1)	7364	0.123	0.329	0.000	1.000
Experienced Linguistic Discrimination (=1)	7353	0.281	0.449	0.000	1.000
Financed Return With UNHCR Aid (=1)	7345	0.080	0.272	0.000	1.000
Reason for Return: Deported/Forcibly Returned (=1)	7382	0.215	0.411	0.000	1.000
Reason for Return: Unwelcoming Host Community (=1)	7382	0.056	0.230	0.000	1.000
Reason for Return: Lacked Visa/Permanent Residency in Host (=1)	7382	0.079	0.270	0.000	1.000
Reason for Return: Poor Security Conditions in Host (=1)	7382	0.097	0.296	0.000	1.000
Reason for Return: Poor Economic Conditions in Host (=1)	7382	0.118	0.322	0.000	1.000
Reason for Return: Unemployment in Host (=1)	7382	0.219	0.413	0.000	1.000
Reason for Return: Improving Economy in Afghanistan (=1)	7382	0.010	0.100	0.000	1.000
Reason for Return: Improving Security in Afghanistan (=1)	7382	0.023	0.151	0.000	1.000
Reason for Return: Family Reunification (=1)	7382	0.121	0.326	0.000	1.000
Reason for Return: Welcoming Conditions in Afghanistan (=1)	7382	0.010	0.101	0.000	1.000
Reason for Return: Improving Education in Afghanistan (=1)	7382	0.006	0.077	0.000	1.000
Reason for Return: Patriotism (=1)	7382	0.029	0.167	0.000	1.000
INDEPENDENT VARIABLES					
Encashment Beneficiary	7391	0.042	0.199	0.000	1.000
Social Capital Index (ICW)	7391	0.021	0.989	-5.387	1.264
Resides Near Out-Group	7295	0.585	0.493	0.000	1.000
CONTROL VARIABLES					
Host Country: Pakistan	7391	0.559	0.496	0.000	1.000
Host Country: Iran	7391	0.355	0.478	0.000	1.000
Host Country: Elsewhere	7391	0.086	0.280	0.000	1.000
Registered Return	7391	0.333	0.471	0.000	1.000
Urban	7391	0.258	0.437	0.000	1.000
Female	7391	0.469	0.499	0.000	1.000
Age	7391	2.350	1.130	1.000	5.000
Education	7391	0.982	1.330	0.000	5.000
Income	7391	2.401	1.854	0.000	9.000
Ethnicity: Pashtun	7391	0.536	0.499	0.000	1.000
Ethnicity: Tajik	7391	0.290	0.454	0.000	1.000
Ethnicity: Uzbek	7391	0.030	0.171	0.000	1.000
Ethnicity: Hazara	7391	0.105	0.307	0.000	1.000
Ethnicity: Other	7391	0.039	0.195	0.000	1.000
Tazkira-Holder	7391	0.850	0.357	0.000	1.000
Dwelling: Single Family House	7391	0.748	0.434	0.000	1.000
Dwelling: Shared House	7391	0.229	0.420	0.000	1.000
Dwelling: Single Family Apartment	7391	0.015	0.120	0.000	1.000
Dwelling: Shared Apartment	7391	0.007	0.085	0.000	1.000
Dwelling: Tent	7391	0.002	0.042	0.000	1.000
Relationship Status: Single	7391	0.143	0.350	0.000	1.000
Relationship Status: Married	7391	0.835	0.371	0.000	1.000
Relationship Status: Widow/Widower	7391	0.022	0.147	0.000	1.000
Relationship Status: Divorced/Separated	7391	0.000	0.016	0.000	1.000
Comfort During Interview	7391	1.278	0.484	1.000	4.000
Interview # Within Sampling Point	7391	2.987	1.411	1.000	5.000

Note: Observations are individual responses used in the main estimating sample. ICW denotes an index constructed by inverse covariance-weighting. PCF denotes an index constructed by principal component factor analysis.

B.3 Measuring Communal Violence

Our military records offer a comprehensive portrait of combat between insurgent and counterinsurgent forces. However, these data do not track social conflicts occurring between civilians, tribes, or other groups. We draw on rich survey data to understand communal violence and returnee relations with their non-migrant neighbors in the main analyses. In supplementary analyses we consider incidents of communal violence recorded in a U.S. government-sponsored conflict tracker, which covers 2016–2017 and combines incidents reported by the Afghan National Police Command Center, open-source media, and embassy-contracted risk analysis firms. For each event in the tracker we observe geographic coordinates, a timestamp, and an incident description. While most events refer to (counter)insurgent violence, a subset of the records cover social strife. We plot these measures in Figure B-9.

Communal violence characterizes incidents of extra-governmental conflict between informally-organized groups in which neither state nor rebel forces are a primary target or perpetrator. This excludes conflicts involving pro-government militias. Government or rebel forces often respond to communal violence, but are not its primary targets. As Murtazashvili (2016) describes, this form of violence includes land and property disputes, honor feuds, and tribal or familial clashes. We use supervised keyword text analysis to search and code events on the basis of incident descriptions. This exercise benefits from standard terminological conventions in each narrative. For instance, most incidents involving insurgent attacks begin “INS conducted [tactic].” Sexton (2016) offers a similar approach. Most narratives are general, and do not allow us to distinguish types of social conflicts, or to identify specific disputes involving returnees. To illustrate, we describe two incidents below:

Date: 12:00, 24 March 2016

Location: Qadis District, Badghis Province

Narrative: “Due to private enmity between two tribes in Qadis village of the mentioned district fought to each other and as result (1) got killed from one side and the other (1) wounded from other side.”

Date: 11:30, 17 December 2016

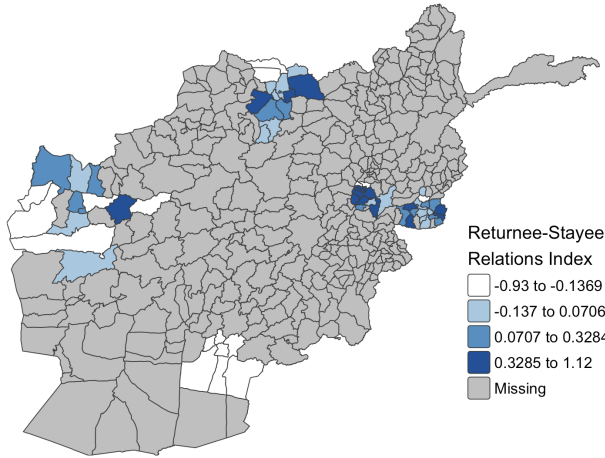
Location: Hazrati Sultan District, Samangan Province

Narrative: “As a result of conflict between two tribes in Ghazni Gak village of the mentioned District, due to land issues, Consequently (05) LNs [local nationals] injured, ANP [Afghan National Police] assigned to the area.”

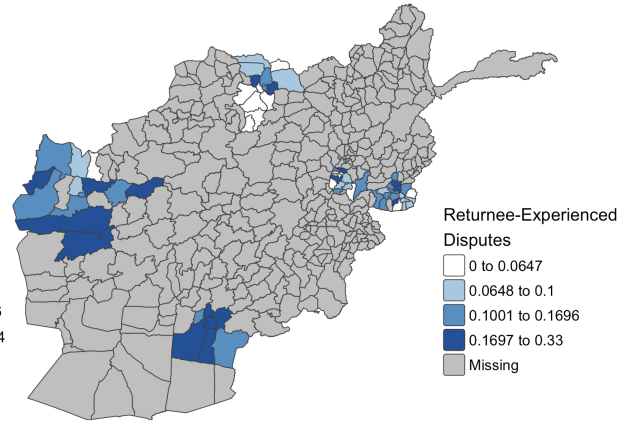
In total, we identify 122 incidents of communal violence across Afghanistan in 2016, including 48 events during the encashment program. Although we believe our data represent the most complete accounting of social conflict in Afghanistan during this period, it is likely that this violence is under-reported. Measurement error in these data would increase the variance of our estimates, reducing precision of our results. To further mitigate possible under-reporting, we study the extensive margin of communal violence in our primary estimations using this alternative measure. Results are substantively similar when we assess communal violence in levels or per capita.

Figure B-9: Survey-Based and Observational Measures of Communal Violence

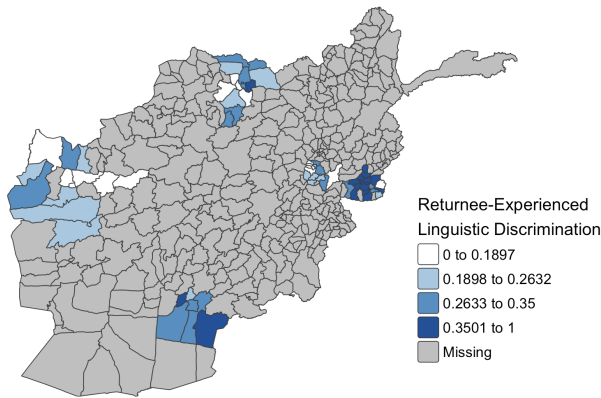
(a) Survey Data: Returnee-Stayee Relations



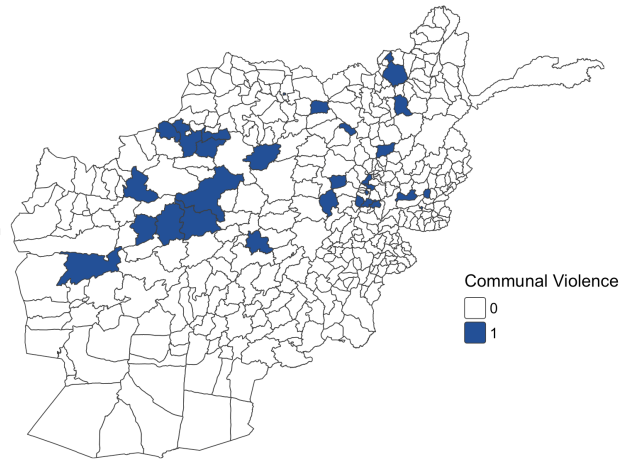
(b) Survey Data: Disputes



(c) Survey Data: Linguistic Discrimination



(d) Event Data: Communal Violence

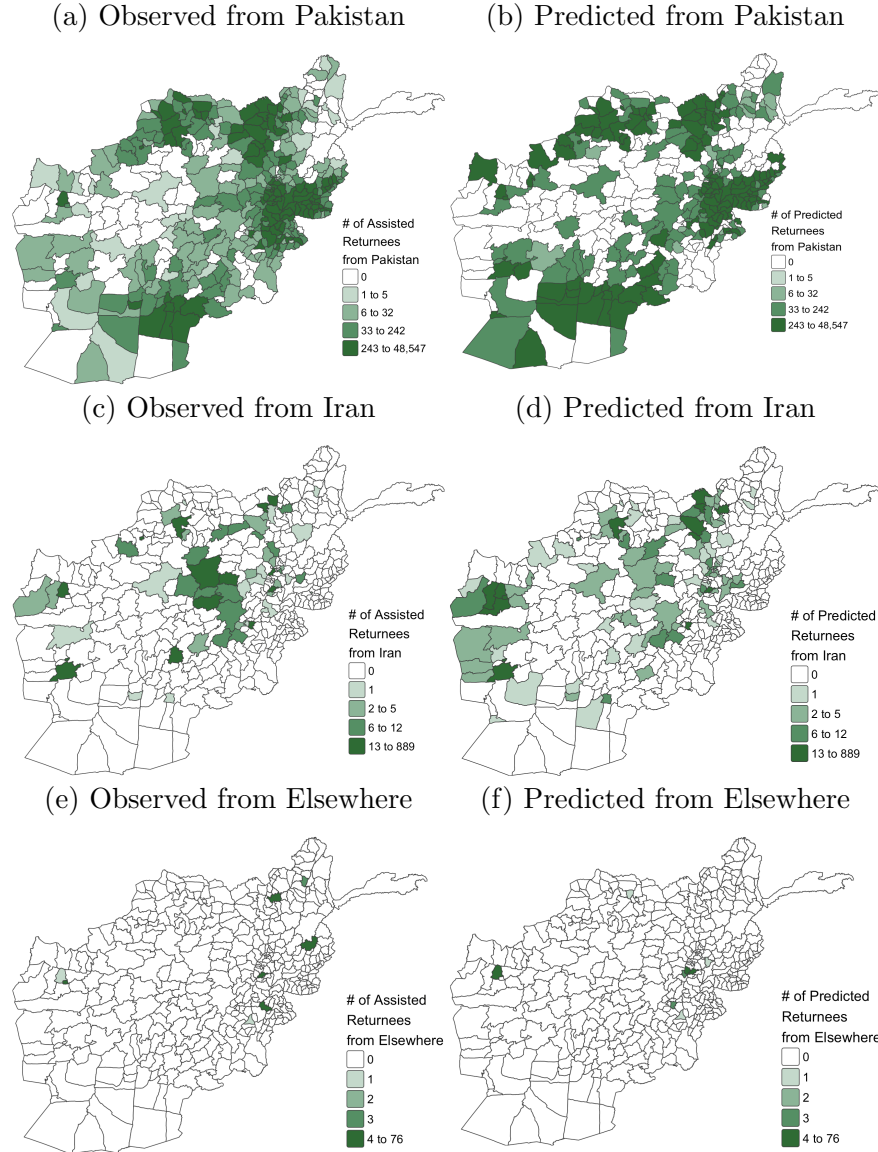


Note: Panel (a) represents average responses on a survey-based, multi-item index measuring relations between returnees and their non-migrant neighbors (i.e., “stayees”). In this case, darker shades indicate more positive relations. Panel (b) represents average responses to a survey item measuring returnees’ self-reported experiences of communal violence. Panel (c) represents average responses to a survey item measuring returnees’ self-reported experiences of linguistic discrimination. Panel (d) represents the extensive margin of communal violence based on observational event data we assembled. In panels (a) – (c), legend bins reflect quartiles of the distribution.

B.4 Observed and Predicted Returns from All Host Countries

As described in the main text, we predict returns during the encashment program by multiplying 2013 (pre-shock) cross-sectional district shares with the total source-specific returnee flow. All predicted returns are highly correlated with observed returns (Pearson's $\rho > 0.865$).

Figure B-10: Observed Versus Predicted Returns During the Cash Program



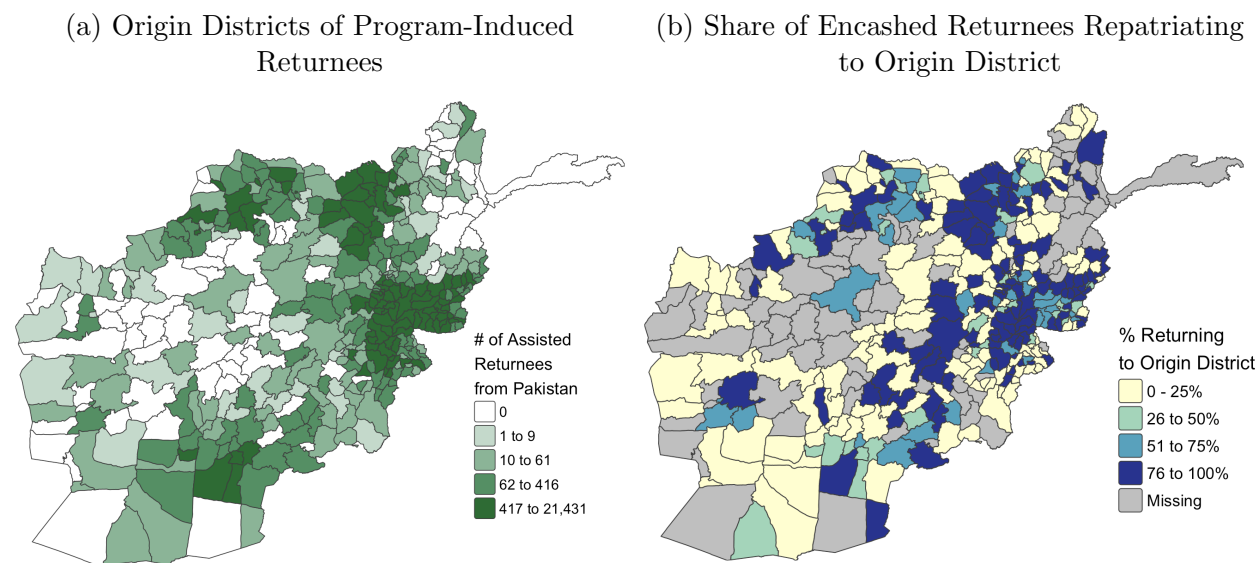
Note: The maps show observed and predicted destination districts of assisted returnees from Pakistan (panels (a) and (b)), Iran (panels (c) and (d)), and all other host countries (panels (e) and (f)). Districts are shaded by the total number of assisted returnees who repatriated to the district during the encashment period. Legend bins reflect a zero category and quartiles of the nonzero distribution

B.5 Origin Districts of Encashed Returnees

Refugee returnees are highly likely to repatriate to the origin provinces and districts they lived in prior to experiencing displacement (Camarena and Hägerdal, 2020; Alrababa’h et al., 2023). During the 2016 encashment program, 58% of all program-induced returnees from Pakistan returned to their district of origin, and 70% of all program-induced returnees from Pakistan returned to their province of origin. In these areas, returnees often owned land or houses, and had pre-existing familial kinship ties.

In Figure B-11 we plot two related measures. First, in panel (a) we depict the origin districts of returnees who repatriated under the 2016 repatriation assistance program. The total number of program-induced returnees originating in a given district is highly correlated with the total number of program-induced returnees repatriating to a given district (Pearson’s $\rho = 0.736$). Second, in panel (b) we plot the fraction of assisted returnees who resettled in their home district during the encashment program. This measure is undefined for districts receiving no program-induced returnees.

Figure B-11: Returnee Gravitation Toward Origin Districts



Note: Panel (a) depict the origin districts of returnees who repatriated under the 2016 repatriation assistance program, shading districts by the total number of program-induced returnees originating in a given district. Legend bins reflect a zero category and quartiles of the nonzero distribution. Panel (b) depicts the percentage of assisted returnees from Pakistan during the 2016 encashment program who returned to their district of origin. Blue shades (50-100%) indicate districts where returnees predominantly returned to their origin district. Yellow-green shades (0-49%) indicate districts where returnees predominantly came from other origin districts. This measure is undefined for districts receiving no program-induced returnees, and these districts are shaded gray.

B.6 Baseline Correlations: 2013 Returnee Share and District Characteristics

To assess balance and potential confounding, Table B-10 presents correlations between districts' 2013 shares of assisted returnees from Pakistan (the key source of identifying variation in our main analysis) and baseline district characteristics measured in 2013. The strongest correlates are population-related variables (correlation of 0.437 with total population and 0.586 with population density, both significant at 1%), which is unsurprising given that return flows naturally scale with district size (Petrin, 2002). Importantly, baseline violence measures show near-zero correlations with returnee shares (range: -0.015 to 0.006), suggesting that returnee shares are not endogenous to conflict intensity. Our main specifications include district fixed effects (which absorb all time-invariant characteristics including 2013 population) and month fixed effects interacted with baseline covariates, thus controlling for these correlations. Evidence in Figure 5 further supports our identification strategy by showing that treatment and control districts followed similar pre-treatment trends.

Table B-10: Baseline Correlations: 2013 Returnee Share and District Characteristics

	Correlation with Returnee Share	Observations
2013 VIOLENCE (PER 100K POP.)		
All SIGACTs	-0.011	398
Insurgent SIGACTs	-0.007	398
Counterinsurgent SIGACTs	-0.015	398
Criminal SIGACTs	0.006	398
GEOGRAPHY		
Distance to Border (km)	-0.090	398
Ruggedness	-0.173	398
DEMOGRAPHICS		
Total Population	0.437***	398
Pashtun Share of Population	0.161***	398
Population Density	0.586***	398
Ethnic Fractionalization Index	-0.071	398
Ethnic Polarization Index	-0.063	398
DEVELOPMENT		
Provincial Unemployment Rate	-0.122	398
Provincial Gini Coefficient	-0.077**	398
Population-Normalized Nightlights	0.053	398
Travel Time to Provincial Center	-0.080	398
AGRICULTURE		
Share of Land Cultivated (%)	0.082	398
Share Irrigated Cultivation (%)	0.103	398
Opium Cultivation (IHS)	-0.044	398
Opium Eradication (IHS)	0.010	398
RECONSTRUCTION AID (PER 100K POP.)		
Government & Civilian Aid	0.015	398
Social Infrastructure Aid	0.023	398
Health Aid	-0.007	398
Conflict Mitigation Aid	0.093	398
Refugee Aid	0.019	398

Note: * p<0.10, ** p<0.05, *** p<0.01. This table presents correlations between districts' 2013 shares of assisted returnees from Pakistan (the key source of identifying variation in the main analysis) and baseline district characteristics measured in 2013. All variables are measured at the district level. The 2013 returnee share variable is the same one used to construct the interaction term (2013 Returnee Share $\times$ Grant Increase) in the main analyses.

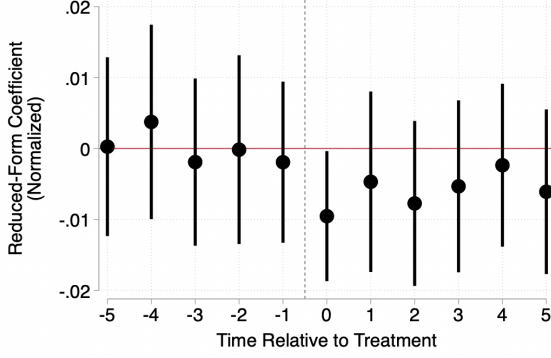
B.7 Shift-Share Pre-Trends Test

To further assess the parallel trends assumption underlying our shift-share identification strategy, we implement the [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#) shift-share pre-trends test. This test examines whether the time-invariant share variable (2013 documented returnee shares) predicts violence levels in the pre-treatment period. Following [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#), we run period-by-period cross-sectional regressions of violence outcomes on the 2013 returnee share for each month from five periods before through five periods after treatment ($t=-5$ to $t=+5$). Under parallel trends, pre-period coefficients should exhibit no systematic trend.

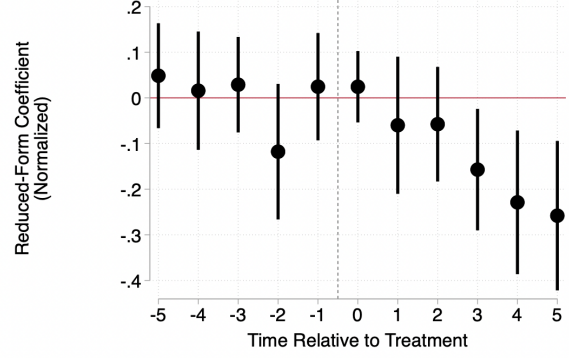
Figure [B-12](#) presents results from this test for both the extensive margin and per capita level of insurgent violence, with baseline (no controls) and saturated (time-invariant district controls) specification. Coefficients are normalized by subtracting the mean of pre-period coefficients ($t=-5$ to $t=-1$) to center the pre-treatment period at zero for clearer visualization. Across all specifications, pre-period coefficients are flat and exhibit no systematic trend (linear trend test p-values range from 0.22 to 0.44), while post-period coefficients show clear treatment effects relative to the pre-period baseline. These results provide additional support for the parallel trends assumption.

Figure B-12: Goldsmith-Pinkham, Sorkin, and Swift (2020) Shift-Share Pre-Trends Test

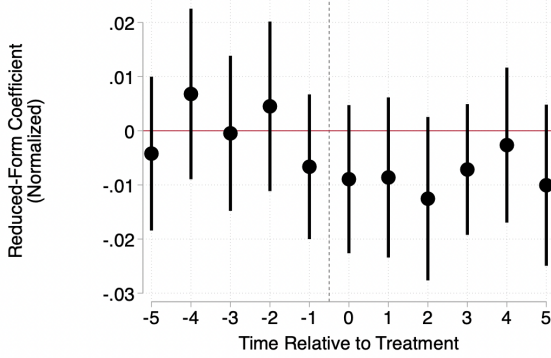
(a) Extensive Margin of Insurgent Violence
(Baseline)



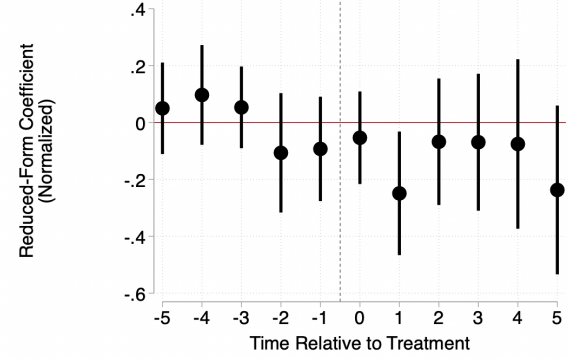
(b) Per Capita Insurgent Violence (Baseline)



(c) Extensive Margin of Insurgent Violence
(Saturated)



(d) Per Capita Insurgent Violence
(Saturated)



Note: Each point represents a coefficient from a separate cross-sectional regression of the insurgent violence outcome on the standardized 2013 returnee share for that month. Coefficients are normalized by subtracting the mean of $t=-5$ to $t=-1$ coefficients to center the pre-period at zero. Panels (a) and (b) show results without controls; panels (c) and (d) include time-invariant district controls from our main specifications. Error bars show 95% confidence intervals with district-clustered standard errors. The vertical dashed line marks the start of treatment in July 2016.

B.8 Equivalence of Difference-in-Differences and Shift-Share in Single-Shift Setting

Consider districts $d = 1, \dots, D$ observed in two periods $t \in \{0, 1\}$, where $t = 0$ denotes the pre-period and $t = 1$ the post-period. Let S_d denote a predetermined share—in our case, the 2013 share of returnees in district d —and let $g_t \in \{0, 1\}$ represent a single national shock that turns on in the post period ($g_0 = 0, g_1 = 1$). The endogenous exposure variable can be written as

$$x_{dt} = \pi S_d g_t + \nu_{dt},$$

where π captures the strength of the relationship between baseline shares and realized exposure, and ν_{dt} is an idiosyncratic component. The outcome equation, controlling for district and period fixed effects, is

$$y_{dt} = \alpha_d + \delta_t + \beta x_{dt} + u_{dt}.$$

Differencing across periods removes the fixed effects:

$$\Delta y_d = \beta \Delta x_d + \varepsilon_d, \quad \text{where } \varepsilon_d = u_{d1} - u_{d0}.$$

Because $g_1 - g_0 = 1$, the Bartik-style instrument simplifies to

$$\Delta z_d = s_d(g_1 - g_0) = S_d.$$

The model can thus be estimated by IV, using S_d as an instrument for Δx_d .

Identification requires that the instrument be exogenous, i.e. that historical shares are uncorrelated with the change in the unobserved component of outcomes:

$$\mathbb{E}[\varepsilon_d \mid S_d] = 0.$$

This is precisely the *share exogeneity* or *exclusion restriction* in the shift-share framework.

Now consider the corresponding difference-in-differences formulation. Let $y_{dt}(0) = \alpha_d + \delta_t + u_{dt}$ denote the potential outcome in the absence of the policy shock. The standard DiD (strong) *parallel-trends* assumption in the continuous treatment setting requires that, in the absence of the program, expected outcome trends would not differ systematically across districts with different baseline shares:

$$\mathbb{E}[y_{d1}(0) - y_{d0}(0) \mid S_d] = \mathbb{E}[y_{d1}(0) - y_{d0}(0)].$$

Subtracting the fixed effects, this can be written as

$$\mathbb{E}[u_{d1} - u_{d0} \mid S_d] = 0,$$

or equivalently,

$$\mathbb{E}[\varepsilon_d \mid S_d] = 0.$$

Thus, the difference-in-differences parallel-trends condition and the shift-share exclusion restriction coincide algebraically and conceptually: both state that differences in unobserved outcome trends are uncorrelated with pretreatment shares.

Intuitively, when the aggregate shock is a single national event that turns on uniformly across time, the instrument collapses to the predetermined shares themselves. The identifying assumption that these shares are uncorrelated with shock-specific residuals is therefore equivalent to requiring parallel outcome trends across districts with different baseline exposure intensities. In this special case, share exogeneity and difference-in-differences parallel trends are simply two ways of expressing the same restriction. With additional time-varying controls, the arguments are analogous but everything is conditional on the controls.

B.9 Covariate Balance

In Table B-11 we estimate a series of difference-in-means using data from the Survey of Afghan Returnees. Comparing encashment beneficiaries to other returnees reveals few demographic differences. Encashed returnee respondents repatriated in larger groups, and were more likely to be Pashtun, male, and to reside in rural areas and in single family homes.

Table B-11: Covariate Balance Between Encashed and All Other Returnees

	All Other Returnees	Encashed Returnees	Difference-in-Means (All Others Returnees - Encashed Returnees)
# of Returnees in Household	5.259 (4.233)	6.771 (5.220)	-1.522* * *
Urban	0.261 (0.439)	0.180 (0.385)	0.087**
Female	0.473 (0.499)	0.372 (0.484)	0.100* * *
Age: 18-25	0.257 (0.437)	0.259 (0.439)	0.001
Age: 26-35	0.351 (0.477)	0.365 (0.482)	-0.014
Age: 36-45	0.228 (0.419)	0.207 (0.406)	0.016
Age: 46-55	0.113 (0.317)	0.104 (0.306)	0.010
Age: 55+	0.051 (0.220)	0.064 (0.245)	-0.014
Ethnicity: Pashtun	0.520 (0.500)	0.889 (0.315)	-0.368* * *
Ethnicity: Tajik	0.300 (0.458)	0.053 (0.225)	0.246* * *
Ethnicity: Uzbek	0.031 (0.174)	0.008 (0.088)	0.022*
Ethnicity: Hazara	0.109 (0.312)	0.010 (0.099)	0.103* * *
Ethnicity: Other	0.039 (0.194)	0.040 (0.197)	-0.003
Monthly Income	2.395 (1.854)	2.531 (1.854)	-0.131
Education	0.984 (1.330)	0.939 (1.328)	0.054
Dwelling: Single Family Home	0.745 (0.436)	0.802 (0.400)	-0.057*
Marital Status: Single	0.144 (0.351)	0.123 (0.329)	0.018
Marital Status: Married	0.834 (0.372)	0.854 (0.354)	-0.017
Marital Status: Widowed	0.022 (0.147)	0.023 (0.151)	-0.000
Marital Status: Separated	0.000 (0.017)	0.000 (0.000)	0.000
Tazkira Holder	0.849 (0.358)	0.867 (0.340)	-0.018
Social Capital Index (ICW)	0.021 (0.993)	0.033 (0.888)	-0.018
Resides Near Out-Group	0.584 (0.493)	0.607 (0.489)	-0.025

Note: * p < .10, ** p < .05, *** p < .01. Column 1 presents means and standard deviations (in parentheses) for survey respondents who were not beneficiaries of the cash program. Column 2 presents means and standard deviations (in parentheses) for survey respondents who were likely beneficiaries of the cash program. Column 3 presents difference-in-means from a series of t-tests.

In Table B-12 we estimate an additional series of difference-in-means using data from the Survey of Afghan Returnees. Comparing encashment beneficiaries to other spontaneous (unassisted) returnees from Pakistan during the encashment program reveals few demographic differences. Encashed returnee respondents were more likely to be Pashtun, male, and better educated.

Table B-12: Covariate Balance Between Encashed and Spontaneous Returnees

	Spontaneous Returnees from Pakistan During Encashment	Encashed Returnees	Difference-in-Means (Spontaneous Returnees - Encashed Returnees)
# of Returnees in Household	7.018 (4.856)	6.771 (5.220)	-0.183
Urban	0.219 (0.414)	0.180 (0.385)	-0.042
Female	0.539 (0.499)	0.372 (0.484)	-0.164* * *
Age: 18-25	0.259 (0.439)	0.259 (0.439)	-0.003
Age: 26-35	0.364 (0.482)	0.365 (0.482)	0.004
Age: 36-45	0.231 (0.422)	0.207 (0.406)	-0.023
Age: 46-55	0.096 (0.295)	0.104 (0.306)	0.006
Age: 55+	0.049 (0.217)	0.064 (0.245)	0.016
Ethnicity: Pashtun	0.797 (0.403)	0.889 (0.315)	0.095* * *
Ethnicity: Tajik	0.139 (0.347)	0.053 (0.225)	-0.089* * *
Ethnicity: Uzbek	0.014 (0.118)	0.008 (0.088)	-0.006
Ethnicity: Hazara	0.015 (0.123)	0.010 (0.099)	-0.008
Ethnicity: Other	0.034 (0.183)	0.040 (0.197)	0.009
Monthly Income	2.245 (1.800)	2.531 (1.854)	0.275*
Education	0.606 (1.084)	0.939 (1.328)	0.327* * *
Dwelling: Single Family Home	0.780 (0.415)	0.802 (0.400)	0.025
Marital Status: Single	0.144 (0.352)	0.123 (0.329)	-0.020
Marital Status: Married	0.830 (0.376)	0.854 (0.354)	0.023
Marital Status: Widowed	0.026 (0.158)	0.023 (0.151)	-0.003
Marital Status: Separated	0.000 (0.000)	0.000 (0.000)	0.000
Tazkira Holder	0.813 (0.390)	0.867 (0.340)	0.055
Social Capital Index (ICW)	-0.057 (1.048)	0.033 (0.888)	0.098
Resides Near Out-Group	0.623 (0.485)	0.607 (0.489)	-0.008

Note: * p <.10, ** p <.05, *** p <.01. Column 1 presents means and standard deviations (in parentheses) for survey respondents who were returned spontaneously from Pakistan during the encashment program. Column 2 presents means and standard deviations (in parentheses) for survey respondents who were likely beneficiaries of the cash program. Column 3 presents difference-in-means from a series of t-tests.

B.10 RDD Balance Checks Around Program Onset

We test for balance in the composition of returnees around the June 29, 2016 program start date using a regression discontinuity design (RDD). If returnee characteristics changed discontinuously at the program cutoff, this could suggest selection effects that might bias our main difference-in-differences estimates. We specifically examine 22 household-level outcomes, including group size, gender composition, and origin and destination province. Table B-13 presents results using `rdrobust` with MSE-optimal bandwidth selection and HC0 heteroskedasticity-robust standard errors. The results show no evidence of systematic compositional changes at the program cutoff, with only one characteristic significant at the 10% level (4.5% of total tests), consistent with random chance rather than systematic selection. These results provide reassurance that returnee composition did not change discontinuously at the program start, supporting the validity of our main, difference-in-differences identification strategy.

Table B-13: RDD Balance Check: Assisted Returnees from Pakistan

Characteristic	RDD Estimate at June 29, 2016 Cutoff			
	Coefficient	95% CI	Bandwidth (days)	N (Households)
Group Characteristics:				
Group Size (Number of Individuals)	-1.674	[-6.651, 3.303]	16	153
Proportion Female in Group	-0.011	[-0.085, 0.062]	43	2622
Origin Province:				
Origin: Nangarhar	0.060	[-0.106, 0.227]	50	3415
Origin: Kabul	0.074	[-0.081, 0.229]	38	2002
Origin: Paktya	-0.125*	[-0.269, 0.019]	48	3256
Origin: Logar	-0.088	[-0.323, 0.148]	41	2449
Origin: Kunduz	-0.070	[-0.284, 0.144]	53	3663
Origin: Laghman	0.012	[-0.014, 0.039]	23	507
Origin: Baghlan	-0.031	[-0.157, 0.096]	51	3600
Origin: Kunar	0.019	[-0.034, 0.072]	33	1374
Origin: Kandahar	-0.008	[-0.022, 0.007]	20	241
Origin: Jawzjan	0.006	[-0.037, 0.050]	38	2002
Destination Province:				
Destination: Nangarhar	0.089	[-0.094, 0.273]	53	3663
Destination: Kabul	0.078	[-0.115, 0.271]	50	3509
Destination: Kunduz	-0.092	[-0.244, 0.061]	69	6344
Destination: Baghlan	-0.040	[-0.148, 0.068]	47	3182
Destination: Laghman	-0.019	[-0.120, 0.081]	36	1727
Destination: Paktya	0.019	[-0.006, 0.044]	29	942
Destination: Logar	-0.032	[-0.243, 0.179]	47	3182
Destination: Balkh	0.011	[-0.090, 0.111]	43	2622
Destination: Kunar	0.004	[-0.010, 0.018]	19	229
Destination: Jawzjan	-0.000	[-0.033, 0.033]	34	1417

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table tests for discontinuities in characteristics of assisted returnee households repatriating from Pakistan. We study the June 29, 2016 program cutoff, when the expanded cash assistance program began. Each row shows an RDD estimate of the jump in the respective characteristic for groups returning just after versus just before the cutoff. Estimates use `rdrobust` with MSE-optimal bandwidth selection, robust bias-corrected inference, triangular kernel, and group size weights. Bandwidth are reported in days. N shows the effective number of returnee households within the bandwidth. Under random sampling, we expect approximately 10%, 5%, and 1% of tests to be significant at those respective levels.

B.11 Intensive Margin of Insurgent Conflict

Our core estimates in Tables 1 – center on the extensive margin of insurgent violence and insurgent violence per 100,000 district residents. In Table B-14, we examine how program-induced returns affected the intensive margin of rebel violence. Because this is a count outcome, we use a Poisson estimator, which is both more suitable for counts and helps verify robustness of our results to alternative estimators. Consistent with our main finding, we see that returnee exposure reduced insurgent attacks in levels.

Table B-14: Refugee Return and Violence in Levels

	Insurgent Violence (#)	
	(1)	(2)
2013 Returnee Share x Grant Increase	-0.013* (0.007)	-0.045*** (0.015)
Observations	3971	3971
Clusters	361	361
Pre-Treatment Mean	6.082	6.082
Pre-Treatment SD	8.756	8.756
PARAMETERS		
District FE	✓	✓
Year-Specific Month FE	✓	✓
UNHCR Area x Month		✓
Demographic Controls x Month		✓
Development Controls x Month		✓
Agricultural Controls x Month		✓
Lagged DV		✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Whereas our OLS models use demeaning to account for fixed effects, in Poisson models the fixed effects are eliminated by conditioning on the sum of the dependent variable over time (Correia, Guimarães and Zylkin, 2020). In the latter case, panels where the dependent variable is constant are not informative, and hence dropped. Given that many panels are singletons or separated by fixed effects, some loss of observations occurs. To facilitate convergence, we therefore omit reconstruction controls.

B.12 Encashed Returns and the Incidence of Communal Violence

Our core estimates on social conflict in Table 3 center on an index of returnees' relations with their non-migrant neighbors. Given the difficulty of measuring communal disputes systematically, we believe this offers the most reasonable approach to understanding how the encashment program shaped social strife. As described in section B.3, we also assembled observational data on social conflict. We study this data in Table B-15, following the reduced-form specification we use in Table 1.

Table B-15: Communal Violence Increased Under the Encashment Program

	Communal Violence			
	Extensive Margin (0/1)		Per 100k Pop.	
	(1)	(2)	(3)	(4)
2013 Returnee Share x Grant Increase	0.009*** (0.001)	0.015*** (0.003)	0.003** (0.001)	0.009*** (0.003)
Observations	4378	4378	4378	4378
Clusters	398	398	398	398
Pre-Treatment Mean	0.097	0.097	0.047	0.047
Pre-Treatment SD	0.297	0.297	0.302	0.302
PARAMETERS				
District FE	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓
UNHCR Area x Month		✓		✓
Demographic Controls x Month		✓		✓
Development Controls x Month		✓		✓
Agricultural Controls x Month		✓		✓
Reconstruction Controls x Month		✓		✓
Lagged DV		✓		✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

In Table 2 we estimate naive and two-stage least squares regressions, taking observed district shares of returnees from Pakistan for each year-specific month. We repeat these specifications while studying communal violence in Table B-16. A one standard deviation increase in the district-month share of encashed returns increased the probability of com-

munal violence by 5.8 percentage points (column 6) and increased communal violence per 100,000 residents by 0.034 attacks (column 10). Relative to the pre-treatment mean, these effects represent increases in violence of roughly 60% and 72% respectively.

Table B-16: Naive and 2SLS Estimates of Communal Violence

	Communal Violence									
	Share of Returnees from Pakistan		Extensive Margin (0/1)				Per 100K Pop.			
	(1) 1 st Stage OLS	(2) 1 st Stage OLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS	(9) OLS	(10) 2SLS
2013 Returnee Share x Grant Increase	0.533*** (0.048)	0.257*** (0.016)								
Share of Returnees from Pakistan			0.020*** (0.007)	0.046*** (0.007)	0.021*** (0.006)	0.058*** (0.011)	0.004 (0.003)	0.021** (0.009)	0.007* (0.004)	0.034*** (0.012)
Observations	19104	4378	4378	4378	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398	398	398	398	398
Pre-Treatment Mean			0.097	0.097	0.097	0.097	0.047	0.047	0.047	0.047
Pre-Treatment SD			0.297	0.297	0.297	0.297	0.302	0.302	0.302	0.302
Kleibergen-Paap F Statistic				89.852		266.512		89.852		268.536
PARAMETERS	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓			✓	✓			✓	✓
Demographic Controls x Month	✓	✓			✓	✓			✓	✓
Development Controls x Month	✓	✓			✓	✓			✓	✓
Agricultural Controls x Month	✓	✓			✓	✓			✓	✓
Reconstruction Controls x Month					✓	✓			✓	✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July through December 2016, when the UNHCR offered an increased repatriation grant. Share of returnees from Pakistan is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in a given year-specific month. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

B.13 Temporal Dynamics and Effect Persistence

In our main analyses, we study effects of the repatriation cash assistance program in 2016, during the period of enhanced cash distribution to refugee returnees from Pakistan. This decision is motivated by the fact that the treatment “turns off” in 2017—UNHCR repatriation cash assistance grants reverted to the pre-program level in that year, and returnee flows from Pakistan to Afghanistan substantially subsided in correspondence. [Goodman-Bacon \(2021, supplemental appendix D\)](#) shows that specifications including the period after treatment reversion are likely to be conservative if treatment effects may persist once treatment stops. We think this is likely to be the case in our setting because the size of the cash transfer during the treatment period was large, allowing returnee beneficiaries to potentially build savings, make productive investments, and smooth consumption over time.

It is substantively interesting to examine whether the effects persist after the UNHCR cash program paused in December 2016. We are also interested in understanding whether effects reversed in 2017. We implement six complementary temporal specifications to test effect persistence across different time horizons and baseline periods.

Specification 1: Post-June 2016 Effects First, we estimate a parsimonious specification using a January 2016 - July 2017 sample, where we interact the pre-treatment (2013) assisted returnee share with a single indicator for the post-June 2016 period:

$$Y_{dt} = \beta_1(S_{d,2013} \times \text{Post-June2016}_t) + \delta_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{dt} \quad (\text{B1})$$

where Post-June2016_t equals 1 for July 2016 - July 2017, and 0 for January-June 2016. This specification tests whether the overall post-treatment effect differs from zero when pooling the grant period and post-pause period together, relative to the pre-treatment baseline.

Specification 2: Post-June 2016 Effects Second, we estimate a parsimonious specification using a January 2016 - December 2017 sample, where we interact the pre-treatment (2013) assisted returnee share with a single indicator for the post-June 2016 period:

$$Y_{dt} = \beta_2(S_{d,2013} \times \text{Post-June2016}_t) + \delta_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{dt} \quad (\text{B2})$$

where Post-June2016_t equals 1 for July 2016 - December 2017, and 0 for January-June 2016. This specification tests whether the overall post-treatment effect differs from zero when pooling the grant period and post-pause period together, relative to the pre-treatment baseline.

Specification 3: Post-Pause vs. Grant Period Third, we focus on a narrower time window spanning July 2016 through July 2017, and compare the post-pause period to the grant period:

$$Y_{dt} = \beta_3(S_{d,2013} \times \text{Post-Pause}_t) + \delta_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{dt} \quad (\text{B3})$$

where Post-Pause_t equals 1 for January-July 2017 and 0 for July-December 2016. This specification directly tests whether effects differ between the period when cash was actively distributed versus the period immediately following the program's pause.

Specification 4: Post-Pause vs. Grant Period Fourth, we focus on a time window spanning July 2016 through December 2017, and compare the post-pause period to the grant period:

$$Y_{dt} = \beta_4(S_{d,2013} \times \text{Post-Pause}_t) + \delta_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{dt} \quad (\text{B4})$$

where Post-Pause_t equals 1 for January-December 2017 and 0 for July-December 2016. This specification directly tests whether effects differ between the period when cash was actively distributed versus the year following the program's pause.

Specification 5: Both Grant and Post-Pause Dummies Fifth, we estimate a specification that includes separate dummies for both the grant period and the post-pause period, using the full January 2016 - July 2017 sample:

$$Y_{dt} = \beta_5(S_{d,2013} \times \text{Grant}_t) + \beta_6(S_{d,2013} \times \text{Post-Pause}_t) + \delta_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{dt} \quad (\text{B5})$$

where Grant_t equals 1 for July-December 2016 and Post-Pause_t equals 1 for January-July 2017, with the pre-treatment period (January-June 2016) as the omitted baseline. This specification allows us to estimate both period effects relative to the same pre-treatment baseline.

Specification 6: Both Grant and Post-Pause Dummies Sixth, we estimate a specification that includes separate dummies for both the grant period and the post-pause period, using the full January 2016 - December 2017 sample:

$$Y_{dt} = \beta_7(S_{d,2013} \times \text{Grant}_t) + \beta_8(S_{d,2013} \times \text{Post-Pause}_t) + \delta_d + \beta_t + \gamma_{u \times t} + \mu(X_d \times \beta_t) + \varepsilon_{dt} \quad (\text{B6})$$

where Grant_t equals 1 for July-December 2016 and Post-Pause_t equals 1 for January-December 2017, with the pre-treatment period (January-June 2016) as the omitted baseline. This specification allows us to estimate both period effects relative to the same pre-treatment baseline, and extends the estimation sample through the 2017 fighting season.

All six specifications use the same controls and fixed effects as our main specification: district fixed effects (δ_d), year-specific month fixed effects (δ_t), UNHCR area $\times$ month interactions ($\gamma_{u \times t}$), the standard set of baseline controls for demographics, development, agriculture, reconstruction aid interacted with month fixed effects ($\mu(X_d \times \beta_t)$), and lagged dependent variables. Standard errors are clustered at the district level, and regressions are weighted by district population.

Results Table B-17 presents results for all six specifications, focusing on the insurgent violence outcome. For the extensive margin of insurgent violence (columns 1–6), Specifications 1 and 2 show a marginally insignificant negative effect when pooling all post-June 2016 months ($\hat{\beta}_1 = -0.008$, $p = 0.11$; $\hat{\beta}_2 = -0.007$, $p = 0.16$), indicating that assisted returns reduced violence over the full year following treatment. Specification 5 decomposes this pooled effect and reveals important temporal heterogeneity. With the pre-treatment period as baseline, the grant period shows a large, statistically significant negative effect ($\hat{\beta}_5 = -0.015$, $p < 0.01$), while the post-pause coefficient is smaller in magnitude and statistically insignificant ($\hat{\beta}_6 = -0.003$, $p = 0.61$). Specification 6 also follows Specification 5, but extends the Post-Pause period through 2017. Again, with the pre-treatment period as baseline, the grant period shows a large, statistically significant negative effect ($\hat{\beta}_7 = -0.014$, $p < 0.01$), while the post-pause coefficient is smaller in magnitude and statistically insignificant ($\hat{\beta}_8 = -0.004$, $p = 0.51$). These patterns indicate that the violence-reducing effect of

encashed returns were strongest during the period of active cash distribution.

Table B-17: Temporal Specifications: Effect Persistence Tests

	Insurgent-Initiated Attacks											
	Extensive Margin (0/1)						Per 100k Pop.					
	Spec. #1	Spec. #2	Spec. #3	Spec. #4	Spec. #5	Spec. #6	Spec. #1	Spec. #2	Spec. #3	Spec. #4	Spec. #5	Spec. #6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2013 Returnee Share × Post-June 2016	-0.008 (0.005)	-0.007 (0.005)					-0.152** (0.074)	-0.112 (0.069)				
2013 Returnee Share × Post-Pause			0.013*** (0.004)	0.011*** (0.004)					-0.044 (0.092)	0.035 (0.066)		
2013 Returnee Share × Grant Period					-0.015*** (0.005)	-0.014*** (0.005)					-0.123** (0.059)	-0.127** (0.060)
2013 Returnee Share × Post-Pause					-0.003 (0.006)	-0.004 (0.006)					-0.176* (0.103)	-0.104 (0.081)
Observations	7164	9154	4776	6766	7164	9154	7164	9154	4776	6766	7164	9154
Clusters	398	398	398	398	398	398	398	398	398	398	398	398
Pre-Treatment Mean	0.717	0.717	0.717	0.717	0.717	0.717	4.955	4.955	4.955	4.955	4.955	4.955
Pre-Treatment SD	0.451	0.451	0.451	0.451	0.451	0.451	11.851	11.851	11.851	11.851	11.851	11.851
Estimation Sample:	Jan. 2016 – July 2017	Jan. 2016 – Dec. 2017	July 2016 – July 2017	July 2016 – Dec. 2017	Jan. 2016 – July 2017	Jan. 2016 – Dec. 2017	Jan. 2016 – July 2017	Jan. 2016 – Dec. 2017	July 2016 – July 2017	July 2016 – Dec. 2017	Jan. 2016 – July 2017	Jan. 2016 – Dec. 2017
PARAMETERS												
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Post-June 2016 is an indicator for all months after June 2016. Post-pause is an indicator for all months after December 2016. Grant period is an indicator for all months from July–December 2016. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Specifications 3 and 4 corroborate this interpretation by directly comparing the two post-treatment periods. The positive coefficients in column 3 and 4 ($\hat{\beta}_3 = 0.013$, $p < 0.01$; $\hat{\beta}_4 = 0.013$, $p < 0.01$) indicate that the extensive margin of insurgent violence was significantly higher during the post-pause period relative to the grant period baseline, consistent with attenuation of the effects after cash distribution ended. Importantly, however, Specifications 1 and 2 show that even when pooling both periods together, the overall effect remains negative and economically meaningful, at roughly 50% of the grant period magnitude.

In columns 7–12 we examine effects on insurgent violence per 100,000 district residents. The results follow a similar pattern. Specification 1 shows a significant negative effect ($\hat{\beta}_1 = -0.152$, $p < 0.05$) over the full post-treatment period when the estimation sample covers the year after program onset. Specification 2 reveals a marginally imprecise negative effect ($\hat{\beta}_2 = -0.112$, $p = 0.11$) over the full post-treatment period when the estimation sample is extended to include the entirety of 2017. Specification 5 shows a highly significant negative effect during the grant period ($\hat{\beta}_5 = -0.123$, $p < 0.05$) and a negative but modestly less precise effect in the first seven months of 2017 ($\hat{\beta}_6 = -0.176$, $p < 0.1$). The post-pause effect is comparably large in magnitude to the grant period coefficient, suggesting substantial persistence into 2017. Specification 6 examines persistence over the entirety of 2017, again showing a highly significant negative effect during the grant period ($\hat{\beta}_7 = -0.127$, $p < 0.05$)

and a modest attenuation in the post-pause period ($\hat{\beta}_8 = -0.104$, $p = 0.20$). The post-pause effect is approximately 82% of the grant period magnitude in Specification 6, underscoring persistence despite loss of statistical significance.

Specifications 3 and 4 corroborate this interpretation. In column 9 we find no distinguishable difference in the effect of encashed returns in the first seven months of 2017, relative to the grant period ($\hat{\beta}_3 = -0.044$, $p = 0.63$). In column 10 we find that per capita insurgent violence was marginally but not significantly higher during the full post-pause period relative to the grant period baseline ($\hat{\beta}_4 = 0.035$, $p = 0.60$). This is consistent with some attenuation of effects after cash distribution ended. Importantly, however, Specifications 1 and 2 show that even when pooling these periods together, the overall effect remains negative and economically meaningful.

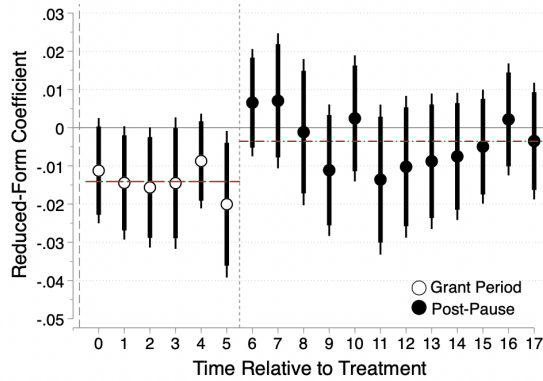
To further visualize temporal dynamics, Figure B-13 presents an alternative event study specification. Note that we do not include pre-treatment dummies here to reflect the true data-generating process and hence improve the accuracy of the treatment estimates (Borusyak, Jaravel and Spiess, 2024). This specification uses the January 2016 – December 2017 sample from Specifications 2, 4, and 6 above, and includes month-by-month post-treatment dummies ($t=0$ to $t=17$) interacted with pre-treatment (2013) returnee shares, but no pre-treatment dummies. The pre-treatment period (January-June 2016) is absorbed by district and year-specific month fixed effects. Panel (a) plots coefficients on the extensive margin of insurgent violence, while panel (b) plots coefficients on the insurgent violence in levels per capita.

Several patterns emerge from Figure B-13. First, during the grant period (months 0-5), coefficients are consistently negative and mostly statistically significant at at least the 10% level, with a grant period mean of -0.014 for the extensive margin and -0.127 for levels per capita. This corroborates the main results showing that encashed returns reduced insurgent violence during the program period. Second, during the post-pause period (months 6-17), coefficients are noisier and much closer to zero, or even positive, with a post-pause mean of -0.006 for the extensive margin and -0.104 for levels per capita. The contrast between period means is visually striking, particularly for the extensive margin. Third, the last five months of the observation window (August–December 2017) show coefficients near or above zero, suggesting effects largely dissipated within one year of program onset.

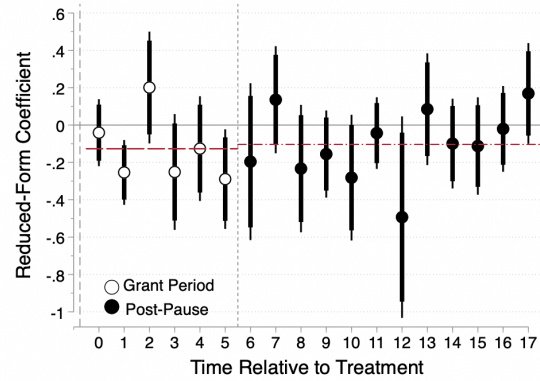
Interpretation Taken together, these results provide strong evidence that assisted returns during the encashment program reduced insurgent violence, with effects driven primarily by the grant period, but exhibiting meaningful persistence. Table B-17 shows three key findings. First, Specification 1 demonstrates a large and significant negative effect when pooling the entire post-treatment period, indicating violence reductions persisted throughout the year following program launch. Specification 2 demonstrates a similarly sized but modestly less precise negative effect when pooling the entire post-treatment period through December

Figure B-13: Month-by-Month Event Study

(a) Extensive Margin of Insurgent Violence



(b) Insurgent Violence Per 100k Pop.



Note: Panels plot coefficients from an event study specification with month-by-month post-treatment dummies (July 2016 – December 2017) interacted with pre-treatment (2013) returnee shares. No pre-treatment dummies are included; the pre-treatment period (January–June 2016) is absorbed by district and month-year fixed effects. Hollow circles indicate the grant period (July–December 2016, under encashment); filled circles indicate the post-pause period (January–June 2017, after cash assistance reverted to the pre-program level). Thin error bars represent 90% confidence intervals; thick error bars represent 95% confidence intervals. Red dashed horizontal lines show period means. Vertical dashed line marks treatment start (July 2016); vertical dotted line marks program pause (December 2016/January 2017 boundary). Specifications include all controls and fixed effects from main analyses. Standard errors clustered at district level; regressions weighted by population.

2017. Second, Specifications 5 and 6 decompose this into grant-period and post-pause components, revealing that effects are strongest during active cash distribution (grant period), but remain economically meaningful in the post-pause period. In the full estimation sample in specification 6 (January 2016–December 2017), estimated effects are roughly 29% of the grant-period magnitude for the extensive margin and 82% of the grant-period magnitude for levels per capita. Third, Specifications 3 and 4 confirm some attenuation by showing a marginal increase in violence on the extensive margin during the post-pause period relative to the grant period. Figure B-13 visualizes this dynamic with month-by-month estimates, showing consistently negative effects during the grant period and smaller but still negative average effects during the post-pause months.

This pattern of strong effects during active cash distribution with partial persistence afterward is consistent with the cash assistance mechanism operating through multiple channels. The immediate effect during the grant period likely reflects direct purchasing power and economic stabilization. The partial persistence suggests spillover effects—such as improved economic conditions, reduced grievances, or strengthened social ties—that decay gradually rather than disappearing immediately when cash stops. Moreover, this temporal pattern

provides additional evidence against alternative explanations based solely on undocumented returns or coercion in Pakistan. Critically, Figure 1 shows that undocumented returns surge again around April 2017 to near-program levels, while documented returns remain low. If undocumented returns, especially those induced by Pakistani coercion, were the primary drivers of our results in the encashment period, we would expect violence-reducing effects to strengthen when undocumented returns resurge in April 2017. Instead, effects attenuate modestly after cash distribution ended in December 2016. The concentration of effects during the grant period, combined with meaningful persistence and the lack of response to the April 2017 undocumented return surge, supports the interpretation that repatriation cash assistance was the key mechanism reducing insurgent violence.

B.14 Effects of Assisted Versus Spontaneous Returnees

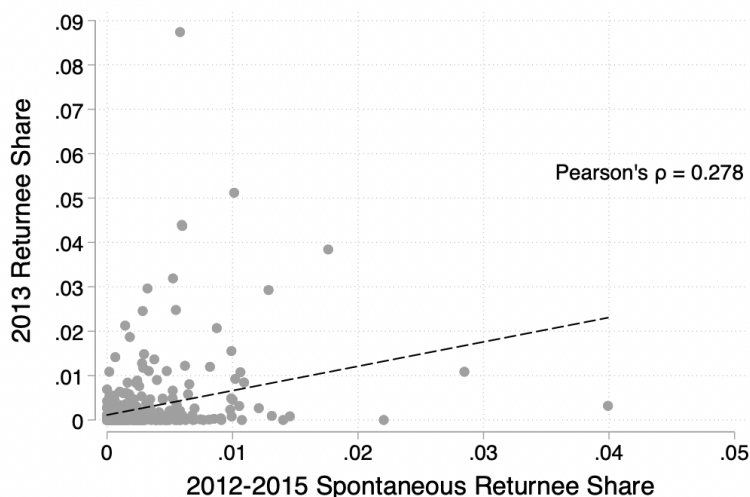
Our main analyses define exposure to assisted returnees from Pakistan to Afghanistan using 2013 shares. During the encashment program, all and only these returnees from Pakistan received the UNHCR cash grant, so by capturing assisted Pakistan returns, our reduced-form measure implicitly captures exposure to encashment. However, it is natural to wonder how spontaneous returnees, who did not receive cash, impacted violence.

Our UNHCR data represent the universe of assisted returns, but no similar data exists to measure spontaneous returns during the program period. We reconstruct the nationwide (but not district-specific) flow of spontaneous returns (Figure 1) using IOM reports. To test for any impact of spontaneous returns in a regression setting, we use supplemental data from the IOM Displacement Tracking Matrix (DTM), which provides a retrospective estimate of spontaneous returns at the district-level for the period from 2012–2015. This source provides the estimated number of spontaneous (i.e. undocumented) returnees from Pakistan and Iran to a district over the period from 2012–2015. Public-facing IOM data do not allow us to distinguish spontaneous returnees from Pakistan versus those from Iran. We calculate the district share of total spontaneous returnees as $\frac{\text{District Undocumented Returnees}}{\text{Total Undocumented Returnees}}$, standardize this share, and interact the measure with an indicator for the grant period. This yields a reduced-form specification using pre-treatment spontaneous shares as a proxy for districts' propensity to receive spontaneous returnees. We believe this approach is the best possible measurement strategy given data constraints, but we hesitate to draw strong inferences because the data on spontaneous returnees pools returns from Pakistan and Iran, covers a different base period (2012–2015 rather than 2013), and is based on retrospective estimates rather than direct observations during the program period.

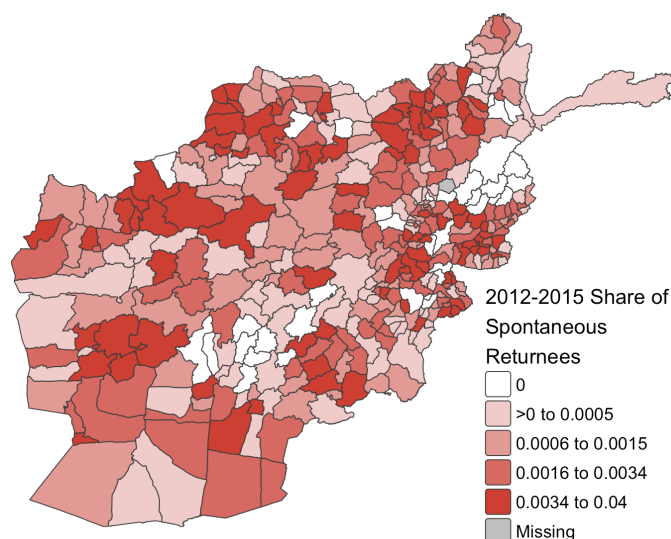
Geographic Overlap Between Documented and Undocumented Returns A natural question is whether assisted and spontaneous returnees historically go to similar districts. Figure B-14 provides evidence on this question using pre-treatment settlement patterns. Panel (a) shows the correlation between our main measure of 2013 (assisted) return shares and 2012–2015 spontaneous return shares. The correlation coefficient is $\rho = 0.278$, indicating moderate positive geographic overlap. This correlation is essential for our identification

Figure B-14: Historical Settlement Patterns: Assisted vs. Spontaneous Returns

(a) Correlation Between Assisted and Spontaneous Returnee Shares



(b) Mapping Spontaneous Returnee Shares



Note: Panel (a) depicts the correlation between district-level shares of spontaneous refugee returnees from Pakistan and Iran in 2012–2015 and district-level shares of assisted refugee returnees from Pakistan in 2013. Panel (b) depicts each district's share of the total number of spontaneous Afghan returnees from Pakistan and Iran in 2012–2015. Darker shades indicate a greater share. Legend bins reflect a zero category and quartiles of the nonzero distribution.

strategy—it provides the variation needed to test whether spontaneous returns confound our estimated effects of assisted returns. Panel (b) maps the distribution of spontaneous return

shares over Afghanistan’s districts. Approximately 74% of districts have historically received as many or more spontaneous than assisted returnees (median ratio = 0.16), consistent with the fact that spontaneous returns have historically exceeded assisted returns in Afghanistan. The geographic overlap combined with this variation in relative exposure allows us to test whether the effects of encashed returns affect violence systematically differently in districts with high versus low historical presence of spontaneous returnees.

Table B-18: Spontaneous Returnees’ Community Relations

	Returnee-Stayee Relations Index (Z-Standardized)			
	ICW		PCF	
	(1)	(2)	(3)	(4)
Encashment Beneficiary	-0.129** (0.057)	-0.128** (0.052)	-0.178** (0.074)	-0.176** (0.068)
Spontaneous Pakistan Returnee During Encashment Program	0.054 (0.048)	0.074 (0.050)	0.009 (0.045)	0.042 (0.045)
Observations	7391	7391	7283	7283
Clusters	65	65	65	65
Control Group Mean	0.035	0.035	0.033	0.033
Control Group SD	0.967	0.967	0.967	0.967
Difference: Encashment Beneficiary - Spontaneous	-0.182** (0.074)	-.203*** (0.072)	-0.186** (0.087)	-0.218*** (0.081)
PARAMETERS				
District FE	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓
Urbanicity		✓		✓
Gender		✓		✓
Age		✓		✓
Ethnicity		✓		✓
Education		✓		✓
Income		✓		✓
Dwelling		✓		✓
Marital Status		✓		✓
Tazkira		✓		✓
Country of Asylum		✓		✓
Social Desirability		✓		✓
Interview Order		✓		✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Note that we do not include the controls for registration status by month of return in these specifications, since they absorb most of the variation in the covariates of interest. Estimates are scaled using sampling weights. ICW denotes an index constructed by inverse covariance-weighting. PCF denotes an index constructed by principal component factor analysis.

Results In Table 4 of the main text, we verify that our core estimates are robust when controlling for the interaction of historical spontaneous returnee shares (2012-2015) and an indicator for the encashment period. While exposure to encashed returns (2013 shares) is

associated with distinguishable reductions in insurgent violence and increases in communal violence during the cash grant program, there are null or weakly opposite effects in districts exposed to spontaneous returns. We conduct an analogous test using our survey data. In particular, we define an indicator for returnees from Pakistan who repatriated during the encashment program but were not registered with UNHCR at the time of their return, and hence were ineligible for cash assistance. Results in Table B-18 suggest that the observed negative effect of returnees on communal relations is specific to encashed (i.e., assisted) returnees who repatriated under the program we study. Coefficients on the term for spontaneous returnees from Pakistan during the program are smaller in magnitude and statistically indistinguishable from zero. Moreover, in all specifications, the coefficients on the encashed vs. undocumented indicators are significantly different at the 5% level.

B.15 Alternative Source of Cross-Sectional Variation

Endogeneity in the demand-pull component of return migration is the main threat to inference in our empirical strategy (Boustan, 2010). That is, initial district characteristics may be correlated with 2013 returnee settlement patterns. If districts with higher shares of earlier returnees are systematically safer, wealthier, or otherwise more conducive to returnee integration, our estimates will be biased. We study correlations between baseline shares and initial district characteristics in Table B-10. We also present evidence that districts with greater exposure to returnees from Pakistan were not on differential trajectories of violence prior to the encashment program (Figure 5). In addition, we include an array of pre-shock controls to account for relevant district characteristics. Still, it remains possible that unobserved factors affect historical returnee settlement choices and violence.

As an alternative, we define cross-sectional exposure to refugee return using the geographic (straight-line) proximity between each Afghan district centroid and a UNHCR Encashment Centre (EC). In 2016, all assisted returnees from Pakistan passed through one of three ECs in Kabul, Jalalabad, or Kandahar (Figure A-6), where they were interviewed and provided cash assistance. To reduce travel costs, most returnees resided closer to the facilities where they received their repatriation cash (Haroon et al., 2019). Distance is the chief influence on migrants' destination choice *ceteris paribus*. Unlike road distance, straight-line distance is unrelated to contemporary conditions in sending or receiving communities (Boustan, Fishback and Kantor, 2010).

In Table B-19 we re-estimate our core specifications from Table 1, replacing our cross-sectional measure of 2013 returnee shares with a cross-sectional measure of EC proximity. A natural concern is that UNHCR strategically places ECs in central locales. To address this, we control for travel time to the provincial center in columns 2, 4, 6, and 8. Estimates are less precise for the extensive margin of insurgent violence, but still generally corroborate our main results.

Table B-19: Cross-Sectional Variation from Encashment Centre Proximity

	Insurgent-Initiated Attacks				Communal Violence			
	Extensive Margin		Per 100k Pop.		Extensive Margin		Per 100k Pop.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proximity to EC x Grant Increase	-0.020 (0.027)	-0.042 (0.030)	-2.153*** (0.630)	-1.785*** (0.599)	0.041** (0.016)	0.046*** (0.015)	0.043*** (0.015)	0.051*** (0.017)
Observations	4378	4378	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398	398	398
Pre-Treatment Mean	0.717	0.717	4.955	4.955	0.097	0.097	0.047	0.047
Pre-Treatment SD	0.451	0.451	11.851	11.851	0.297	0.297	0.302	0.302
PARAMETERS								
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month		✓		✓		✓		✓
Pashtun Share x Month		✓		✓		✓		✓
Development Controls x Month		✓		✓		✓		✓
Agricultural Controls x Month		✓		✓		✓		✓
Reconstruction Controls x Month		✓		✓		✓		✓
Lagged DV		✓		✓		✓		✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. Proximity to EC is each district's standardized, straight-line proximity to a UNHCR Encashment Centre for Afghan returnees from Pakistan. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

B.16 Robustness to Spatial Spillovers

A potential concern with our empirical strategy is that spillover effects across district boundaries could bias our estimates. Specifically, two types of spatial spillovers merit consideration. First, *violence spillovers* from neighboring districts could confound our results if nearby violence affects local outcomes independently of local returnee shares. Second, *secondary migration spillovers* could occur if grant-recipient returnees systematically move to neighboring districts after initial settlement, creating measurement error in our treatment assignment. We address both concerns empirically using a combination of spatial econometric techniques controlling for neighboring districts' violence, and direct survey evidence on migration intentions.

Violence Spillovers We first assess whether violence in neighboring districts confounds our main results using spatial lag specifications. Following standard practice in spatial econometrics, we construct a contiguity-based spatial weights matrix where districts sharing borders are considered neighbors. Using the Afghanistan district shapefile, we identify 2,196 neighbor relationships among the 398 districts (average of 5.5 neighbors per district, ranging from 1 to 12). We row-normalize the weights matrix and calculate spatial lags as weighted averages of neighboring districts' violence outcomes: $W \times Y_t$, where W is the spatial weights

matrix and Y_t is the vector of violence outcomes at time t .

We re-estimate our main specifications for insurgent and communal violence including these spatial lags as additional controls. If violence spillovers from neighboring districts were driving our results, we would expect the spatial lag coefficients to be significant and the main treatment effects to attenuate substantially. Table B-20 presents results for our main insurgent violence analyses. Table B-21 presents results for analyses using our observational measure of communal violence.

The main treatment effects remain highly stable after controlling for spatial lags. For the extensive margin of insurgent violence, the baseline reduced-form estimate of -0.006 is essentially unchanged when including the spatial lag (columns 1–2). With full controls, this estimate shifts marginally from -0.013 to -0.012 when adding the spatial lag (columns 3–4). The insurgent violence per capita specifications show similar stability: the baseline estimate of -0.123 shifts to -0.108 with the spatial lag (columns 5–6), and the saturated estimate remains virtually unchanged at -0.137 versus -0.136 (columns 7–8). The spatial lag coefficients themselves are generally small and inconsistent in sign across specifications, suggesting neighboring districts’ violence does not systematically confound our estimates.

Similar conclusions emerge in the communal violence specifications. For the extensive margin of communal violence, the baseline reduced-form estimate of shifts marginally from 0.009 to 0.008 when including the spatial lag (columns 1–2). With full controls, this estimate shifts from 0.015 to 0.013 when adding the spatial lag (columns 3–4). The communal violence per capita specifications show similar stability: the baseline estimate of 0.003 shifts to 0.002 with the spatial lag (columns 5–6), and the saturated estimate is unchanged at 0.009 (columns 7–8). The spatial lag coefficients themselves are small and inconsistent in sign across specifications.

Together, these spatial lag specifications provide strong evidence that violence spillovers from neighboring districts do not bias our main estimates. The stability of treatment effects after controlling for spatial lags, combined with the weak and inconsistent spatial lag coefficients, indicates that our district-level analysis correctly attributes violence changes to local returnee exposure rather than to spillovers from nearby areas.

Secondary Migration We next assess whether encashment beneficiaries were more likely to engage in secondary migration. This is important because returnees to fragile origin countries are disproportionately likely to suffer repeat displacement (van Houtte, 2017). To examine potential secondary migration, we use data from the Survey of Afghan Returnees. The survey asked: “Over the next year, do you plan to settle here in your current district or city, or do you want to move somewhere else?” This question offers direct evidence on returnees’ intentions to remain in their initial settlement locations versus relocate elsewhere.

Table B-20: Insurgent Violence: Controlling for Spatial Lags

	Insurgent-Initiated Attacks							
	Extensive Margin (0/1)				Per 100k Pop.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2013 Returnee Share x Grant Increase	-0.006*** (0.001)	-0.006*** (0.001)	-0.016*** (0.005)	-0.012*** (0.004)	-0.123*** (0.042)	-0.108*** (0.039)	-0.160** (0.071)	-0.136** (0.060)
Spatial Lag of Outcome		0.057* (0.034)		-0.077** (0.036)		0.126** (0.060)		0.006 (0.053)
Observations	4378	4378	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398	398	398
Pre-Treatment Mean	0.717	0.717	0.717	0.717	4.955	4.955	4.955	4.955
Pre-Treatment SD	0.451	0.451	0.451	0.451	11.851	11.851	11.851	11.851
PARAMETERS								
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month			✓	✓			✓	✓
Demographic Controls x Month			✓	✓			✓	✓
Development Controls x Month			✓	✓			✓	✓
Agricultural Controls x Month			✓	✓			✓	✓
Reconstruction Controls x Month			✓	✓			✓	✓
Lagged DV			✓	✓			✓	✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of documented, refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July through December 2016, when the UNHCR offered an increased repatriation grant. Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. Additional parameters are described above in section B.17. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

In Table B-22 we use this survey data to test whether encashed returnees differ systematically from non-beneficiary returnees in their migration intentions. Across all specifications, we find no significant differences. In our preferred specification (column 2), the coefficient on encashment beneficiary status is 0.003 (SE = 0.027, $p = 0.903$). Together, our findings strongly suggest secondary migration is not a significant concern. Overall, 92.8% of returnees plan to settle in their current district, with only 6% planning to move (Table B-22). This finding closely aligns with evidence from a 2018 World Bank phone survey of Afghan returnees, which found that “mobility of returnees is low; 91 percent of returnee households indicate that they do not have plans to move and 90 percent live in the province that they first settled in upon returning to Afghanistan” (Haroon et al., 2019, p. 18).

Among the small fraction of returnees planning to move ($N = 439$), we further analyze intended destinations to assess whether these moves would generate spillovers affecting our district-level analysis. We code responses into four categories: international migration (Iran, Pakistan, Turkey, Europe), moves to major cities (Kabul, Herat, Mazar-e-Sharif, Jalalabad,

Table B-21: Communal Violence: Controlling for Spatial Lags

	Communal Violence							
	Extensive Margin (0/1)				Per 100k Pop.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2013 Returnee Share x Grant Increase	0.009*** (0.001)	0.008*** (0.002)	0.015*** (0.003)	0.013*** (0.003)	0.003** (0.001)	0.002* (0.001)	0.009*** (0.003)	0.009*** (0.003)
Spatial Lag of Outcome		0.157* (0.094)		-0.000 (0.090)		0.035 (0.041)		-0.052 (0.050)
Observations	4378	4378	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398	398	398
Pre-Treatment Mean	0.097	0.097	0.097	0.097	0.047	0.047	0.047	0.047
Pre-Treatment SD	0.297	0.297	0.297	0.297	0.302	0.302	0.302	0.302
PARAMETERS								
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month			✓	✓			✓	✓
Demographic Controls x Month			✓	✓			✓	✓
Development Controls x Month			✓	✓			✓	✓
Agricultural Controls x Month			✓	✓			✓	✓
Reconstruction Controls x Month			✓	✓			✓	✓
Lagged DV			✓	✓			✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of documented, refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July through December 2016, when the UNHCR offered an increased repatriation grant. Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. Additional parameters are described above in section B.17. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Kandahar), local moves within the same province, and other domestic moves (Table B-22). Only 3.9% of all returnees plan local moves within their current province. The majority of planned moves are to major Afghan cities (39.4%) or other domestic locations (43.4%), with the remainder planning international migration (13.7%). Even if some “other domestic” moves involve neighboring districts outside the home province, the small overall fraction of returnees planning any move (6%), combined with the low rate of local moves, suggests minimal spillover risk. Encashment beneficiary status does not significantly predict destination choice in any category (all p > 0.35), indicating that even among the small group planning to move, grant recipients are not systematically choosing different destination types.

Table B-22: Migration Intentions of Encashment Beneficiaries

	Intend to Move (0/1)		If Planning to Move: Where? (0/1)			
	(1)	(2)	(3) Internationally	(4) Major City	(5) Locally	(6) Other Area in Country
Encashment Beneficiary	0.012 (0.018)	0.003 (0.027)	0.117 (0.151)	0.046 (0.188)	0.086 (0.089)	-0.248 (0.190)
Observations	7298	7298	439	439	439	439
Clusters	65	65	55	55	55	55
Mean of Outcome	0.060	0.060	0.139	0.408	0.039	0.415
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓	✓	✓
Urbanicity		✓	✓	✓	✓	✓
Gender		✓	✓	✓	✓	✓
Age		✓	✓	✓	✓	✓
Ethnicity		✓	✓	✓	✓	✓
Education		✓	✓	✓	✓	✓
Income		✓	✓	✓	✓	✓
Dwelling		✓	✓	✓	✓	✓
Marital Status		✓	✓	✓	✓	✓
Tazkira		✓	✓	✓	✓	✓
Country of Asylum		✓	✓	✓	✓	✓
Registration Status x Month of Return		✓	✓	✓	✓	✓
Social Desirability		✓	✓	✓	✓	✓
Interview Order		✓	✓	✓	✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights.

B.17 Robustness to Incorporation of Additional Parameters

Earlier returnees may have a persistent effect on conflict dynamics in areas where they settle. Similarly, belligerents may anticipate future repatriation by observing historical returnee settlement patterns, and strategically allocate fighting resources in response. To more directly address these possibilities, Tables B-23 – B-24 confirms our core results are robust to controlling for three measures of historical conflict: insurgent-initiated SIGACTs, counterinsurgent-initiated SIGACTs (e.g., air support), and criminal-initiated SIGACTs (e.g., robbery). Using the record of SIGACTs for 2013 (Condra et al., 2018; Fetzer et al., 2021), we sum these events by district, normalize by population, and interact these measures with year-specific month fixed effects. To further mitigate concerns about trends in violence differing across areas contested by insurgents, we incorporate month-specific effects of territorial control as assessed in 2013.

Further, van Houte (2017) shows that refugee returnees in Afghanistan are at risk of repeat displacement, and especially internal displacement. Hence, districts more impacted by

repatriation may also host larger populations of internally-displaced people (IDPs). Competition between returnees and IDPs over scarce jobs and housing has exacerbated strife (Haroon et al., 2019). Our core results are robust to controlling flexibly for each district’s share of the pre-program IDP population. Additionally, violence may be more severe in areas where returnees disrupt fragile demographic balances (Joireman, 2017). We calculate ethnic fractionalization and polarization in 2013, and incorporate these controls, again by interacting the measures with month fixed effects. Our core results are precise and consistent in magnitude across these additional specifications. Column 1 of Table B-23 contains our baseline estimates from Table 1 for comparison. Column 1 of Table B-24 contains our baseline estimates from Table B-15 for comparison.

Finally, in Table B-25 we assess additional confounders in relation to our survey-based results. In particular, we assess robustness to additional controls pertaining to the size of returnee households, land ownership, the ethno-demographic composition of returnees’ neighborhoods, and respondents’ self-reported reasons for return. To measure returnee household size we study responses to the question “how many people in this household have returned to Afghanistan?”. To measure land ownership we study responses to the question “does your household currently own jeribs of land?” To measure the ethno-demographic composition of returnees’ neighborhoods we construct an inverse covariance-weighted index composed of responses to two questions. First, we define an indicator for respondents who answered “to be around people of the same ethnicity,” “to be around people who speak the same language,” or “staying/living with family” when asked “why did you decide to move to the place you are living now?” Second, we define an indicator for respondents who answered that “immediate family,” “extended family,” “returnees from [their] ethnic group,” or “returnees from other ethnic groups” when asked whether “the following types of people live in [their] neighborhood.” We combine these items using an index that captures neighborhood social capital provided by networks of kin and other returnees. To measure reason for return we study responses to the question “why did you return?” The core finding—that returnees report worse relations with their non-migrant neighbors—is confirmed in each case. Columns 1 and 6 of Table B-25 contain our baseline estimates from Table 3 for comparison.

Table B-23: Insurgent Violence Results Robust to Additional Parameters

	Insurgent-Initiated Attacks							
	Extensive Margin (0/1)							
	(1) Baseline	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2013 Returnee Share x Grant Increase	-0.016*** (0.005)	-0.017*** (0.005)	-0.016*** (0.005)	-0.016*** (0.005)	-0.015*** (0.005)	-0.014*** (0.005)	-0.016*** (0.005)	-0.016*** (0.005)
Observations	4378	4378	4378	4378	4378	4367	4378	4378
Clusters	398	398	398	398	398	397	398	398
Pre-Treatment Mean	0.717	0.717	0.717	0.717	0.717	0.717	0.717	0.717
Pre-Treatment SD	0.451	0.451	0.451	0.451	0.451	0.451	0.451	0.451
	Per 100k Pop.							
	(1) Baseline	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(1) Baseline	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2013 Returnee Share x Grant Increase	-0.160** (0.071)	-0.228*** (0.077)	-0.163** (0.072)	-0.170** (0.072)	-0.154** (0.073)	-0.190** (0.074)	-0.170** (0.073)	-0.169** (0.074)
Observations	4378	4378	4378	4378	4378	4367	4378	4378
Clusters	398	398	398	398	398	397	398	398
Pre-Treatment Mean	4.955	4.955	4.955	4.955	4.955	4.955	4.955	4.955
Pre-Treatment SD	11.851	11.851	11.851	11.851	11.851	11.851	11.851	11.851
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Insurgent SIGACTs x Month		✓						
Counterinsurgent SIGACTs x Month			✓					
Criminal SIGACTs x Month				✓				
Territorial Control x Month					✓			
IDPs x Month						✓		
Ethnic Fractionalization x Month							✓	
Ethnic Polarization x Month								✓

Note: * p <.10, ** p <.05, *** p <.01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of documented, refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July through December 2016, when the UNHCR offered an increased repatriation grant. Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. Additional parameters are described above in section B.17. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Table B-24: Communal Violence Results Robust to Additional Parameters

	Communal Violence							
	Extensive Margin (0/1)							
	(1) Baseline	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2013 Returnee Share x Grant Increase	0.015*** (0.003)	0.014*** (0.002)	0.015*** (0.003)	0.015*** (0.002)	0.015*** (0.002)	0.015*** (0.003)	0.014*** (0.003)	0.015*** (0.003)
Observations	4378	4378	4378	4378	4378	4367	4378	4378
Clusters	398	398	398	398	398	397	398	398
Pre-Treatment Mean	0.097	0.097	0.097	0.097	0.097	0.097	0.097	0.097
Pre-Treatment SD	0.297	0.297	0.297	0.297	0.297	0.297	0.297	0.297
	Per 100k Pop.							
	(1) Baseline	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(1) Baseline	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2013 Returnee Share x Grant Increase	0.009*** (0.003)	0.008*** (0.003)	0.009*** (0.003)	0.009*** (0.003)	0.009*** (0.003)	0.009*** (0.003)	0.008** (0.003)	0.008** (0.003)
Observations	4378	4378	4378	4378	4378	4367	4378	4378
Clusters	398	398	398	398	398	397	398	398
Pre-Treatment Mean	0.047	0.047	0.047	0.047	0.047	0.047	0.047	0.047
Pre-Treatment SD	0.302	0.302	0.302	0.302	0.302	0.302	0.302	0.302
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Insurgent SIGACTs x Month		✓						
Counterinsurgent SIGACTs x Month			✓					
Criminal SIGACTs x Month				✓				
Territorial Control x Month					✓			
IDPs x Month						✓		
Ethnic Fractionalization x Month							✓	
Ethnic Polarization x Month								✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of documented, refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July through December 2016, when the UNHCR offered an increased repatriation grant. Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. Additional parameters are described above in section B.17. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Table B-25: Survey-Based Results Robust to Additional Parameters

	Returnee–Stayee Relations Index (Z-Standardized)									
	ICW					PCF				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Encashment Beneficiary	-0.233* (0.126)	-0.231* (0.126)	-0.233* (0.126)	-0.228* (0.125)	-0.244* (0.127)	-0.199* (0.113)	-0.197* (0.113)	-0.199* (0.113)	-0.195* (0.112)	-0.212* (0.115)
Observations	7391	7391	7391	7391	7391	7283	7283	7283	7283	7283
Clusters	65	65	65	65	65	65	65	65	65	65
Control Group Mean	0.035	0.035	0.035	0.035	0.035	0.033	0.033	0.033	0.033	0.033
Control Group SD	0.978	0.978	0.978	0.978	0.978	0.967	0.967	0.967	0.967	0.967
PARAMETERS										
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Urbanicity	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Income	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Dwelling	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Marital Status	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Tazkira	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country of Asylum	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Registration Status x Month of Return	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Social Desirability	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Interview Order	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
# of Returnees in Household		✓					✓			
Landowner			✓					✓		
Social Capital				✓					✓	
Reason for Return					✓					✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights. ICW indicates an index constructed using inverse covariance-weighting. PCF indicates an index constructed using principal component factor analysis.

B.18 Robustness to Excluding Population Weights

Because violence varies with population size, our errors are likely to be heteroskedastic, even after we cluster by district. We incorporate population weights to address gravitation toward urban centers—what (Petrin, 2002) calls “urban bias”—in returnee flows. Our decision to incorporate population weights also follows Solon, Haider and Wooldridge (2015), who describe how weighting corrects for heteroskedasticity, thereby improving precision. In line with this logic, Table B-26 shows weighting improves precision of our core estimates. Columns 1 and 4 reproduce our baseline specifications from columns 2 and 4 of Table 1 for reference. Columns 2–3 and 5–6 estimate unweighted results. The unweighted results are similar in magnitude but marginally imprecise.

Table B-26: Robustness to Excluding Population Weights

	Insurgent-Initiated Attacks					
	Extensive Margin			Per 100k Pop.		
	(1) Baseline	(2)	(3)	(4) Baseline	(5)	(6)
2013 Returnee Share x Grant Increase	-0.016*** (0.005)	-0.012* (0.006)	-0.020** (0.010)	-0.160** (0.071)	-0.188 (0.238)	-0.214 (0.258)
Observations	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398
Pre-Treatment Mean	0.717	0.717	0.717	4.955	4.955	4.955
Pre-Treatment SD	398	398	398	398	398	398
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓		✓	✓		✓
Demographic Controls x Month	✓		✓	✓		✓
Development Controls x Month	✓		✓	✓		✓
Agricultural Controls x Month	✓		✓	✓		✓
Reconstruction Controls x Month	✓		✓	✓		✓
Lagged DV	✓		✓	✓		✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized night-lights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights in columns 1 and 4 only. The estimation sample includes district-months in 2016.

B.19 Population-Normalized Exposure to Encashed Returns

Our baseline exposure measure uses each district's share of 2013 returnees (relative to national flows) interacted with an indicator for the encashment period. We believe that this measure accurately captures treatment variation in the context of the shock we study. In particular, focusing on absolute exposure to encashed returns is sensible in our setting because each returnee brought a fixed cash grant (\$400), so the overall magnitude of the economic injection and potential local multiplier effects depend on the total volume of arrivals, rather than how large the encashed returnee population is relative to the stayee population in returnee-receiving communities. Put differently, in short-run general equilibrium, many local effects of returnee inflows (e.g., insurgent recruitment dynamics or demand for housing, food, jobs, or public services) depend primarily on the aggregate shock, not its proportion relative to the local population (Taylor et al., 2016). Relative measures, such as the inflow of returnees normalized by the size of the pre-treatment population of returnee-receiving districts, obscure the fact that the aggregate cash transfer and corresponding economic stimulus

rise linearly with absolute inflows. When the mechanism of interest, cash, is additive and tied one-for-one with assisted returnee inflows, absolute exposure to encashed returnees is the quantity that we believe best determines the scale of both economic and social impacts. This approach follows canonical work on the economic impacts of refugee aid and refugee camps (Maystadt and Verwimp, 2014; Taylor et al., 2016; Zhou and Shaver, 2021), and the economic effects of return migration (Bahar et al., 2024). Other prominent work on migration using Bartik-style instruments also examines treatments defined by absolute exposure to migrant inflows (e.g., Boustan, 2010; Fouka, Mazumder and Tabellini, 2022).

Table B-27: Heterogeneity in the Effect of Encashed Returns by Relative Exposure and Population Density

	Insurgent-Initiated Attacks				Communal Violence			
	Extensive Margin (0/1)	Per 100 Pop.	Extensive Margin (0/1)	Per 100k Pop.	Extensive Margin (0/1)	Per 100k Pop.	Extensive Margin (0/1)	Per 100k Pop.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low Returns as a Share of Population (<i>n</i> = 2955)	-0.007 (0.024)	0.798 (0.602)			-0.005 (0.006)	0.003 (0.012)		
High Returns as a Share of Population (<i>n</i> = 1357)	-0.018** (0.008)	-0.057 (0.160)			0.032*** (0.008)	0.021** (0.009)		
Low Population Density (<i>n</i> = 2200)			0.048 (0.120)	3.525 (3.403)			-0.029 (0.031)	-0.024 (0.061)
High Population Density (<i>n</i> = 2167)			-0.012* (0.006)	-0.200** (0.081)			0.016*** (0.003)	0.008** (0.004)
Pre-Treatment Mean	0.717	4.955	0.717	4.955	0.097	0.047	0.097	0.047
Pre-Treatment SD	0.451	11.851	0.451	11.851	0.297	0.302	0.297	0.302
Difference: High Relative Returns - Low Relative Returns	-0.011 (0.036)	-0.855 (0.888)			0.036*** (0.010)	0.018 (0.018)		
Difference: High Population Density - Low Population Density			-0.061 (0.153)	-3.725 (3.429)			0.045 (0.031)	0.032 (0.061)
PARAMETERS								
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓	✓	✓	✓	✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. Estimates show the effect of 2013 Returnee Share x Grant Increase in subsamples defined by the respective trait denoted in the row title. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

Still, it is reasonable to wonder whether our results are heterogeneous by the relative size of encashed returnee inflows to the total population of returnee-receiving communities, or whether estimated effects vary in more or less densely-populated areas. We take one main approach to examine effect heterogeneity in population density and the relative size of encashed returns. In Table B-27 we directly test for heterogeneous effects. We start from our basic reduced-form specifications in Table 1 and Table B-15. In columns 1-2 and 5-6 we study whether effects vary in districts where the relative intensity of assisted returns

during the encashment program was higher. We take the district-level sum of assisted returnees from Pakistan during the cash-for-return program, normalize this number by the pre-treatment population of the district, and then split the estimation sample at the median of the population-normalized encashed return measure. In columns 3-4 and 7-8 we study whether effects vary in districts with higher pre-treatment population density. We take pre-treatment population of the district, divide it by the size of the district in square kilometers, and then split the estimation sample at the median of the population density measure. Finally, we re-estimate our core reduced-form specifications in subsamples of districts defined by of low and high relative exposure and low and high population density.

We find that the negative effects of encashed return on insurgent violence and the positive effects of encashed return on communal violence are larger in magnitude in districts with more relative exposure to returns. However, the difference in effects between low and high exposure districts is only distinguishable for the extensive margin of communal violence. Similarly, the estimated negative effects of encashed return on insurgent violence and positive effects of encashed return on communal violence are larger in more population-dense districts, though the differences in effect size between more- and less densely-populated districts is not distinguishable. Together, these results give little evidence of effect heterogeneity by relative exposure or population density, and build confidence that our baseline design correctly captures the relevant treatment intensity.

B.20 Substitution in Tactics and Lethality

To test the reservation wage mechanism, we study tactical diversity and insurgent lethality in Tables 5 and 6. Here, we estimate complementary models. In columns 1 and 2 we study the share of all insurgent-initiated attacks that are explosive, expressed as $\frac{\# \text{ of Explosive Attacks}_{d,t}}{\# \text{ of Insurgent-Initiated Attacks}_{d,t}}$. This measure is undefined where no insurgent violence occurs. Hence, in column 1 we exclude observations without insurgent-initiated attacks, and in column 2 we fill these observations with zeroes. Column 2 also adds an indicator that takes the value one when a district experiences positive levels of conflict and zero otherwise. In columns 3 and 4 we study the share of all insurgent-caused counterinsurgent casualties from explosive violence, expressed as $\frac{\# \text{ of Counterinsurgent Casualties from Explosives}_{d,t}}{\# \text{ of Counterinsurgent Casualties}_{d,t}}$. This measure is undefined where no casualties occurs. Hence, in column 3 we exclude observations without counterinsurgent casualties, and in column 4 we fill these observations with zeroes. Column 4 also adds an indicator that takes the value one when a district experiences positive levels of casualties and zero otherwise.

Table B-28: Substitution Into Explosive Violence

	Tactical Substitution		Substitution in Lethality	
	(1) Explosive Share (Missing)	(2) Explosive Share (Filled)	(3) Casualty Share (Missing)	(4) Casualty Share (Filled)
2013 Returnee Share x Grant Increase	0.019*** (0.005)	0.023*** (0.004)	0.023*** (0.008)	0.025*** (0.003)
Observations	1794	4378	939	4378
Clusters	250	398	160	398
Pre-Treatment Mean	0.359	0.257	0.514	0.263
Pre-Treatment SD	0.324	0.318	0.410	0.390
PARAMETERS				
District FE	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓
Outcome (=1)		✓		✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. In column 2, outcome (=1) is an indicator for whether any insurgent-initiated attacks occurred. In column 4, outcome (=1) is an indicator for whether any counterinsurgent casualties occurred. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

B.21 Civilian Informing and Counterinsurgent Bomb Neutralizations

Rising reservation wages attributable to the repatriation cash assistance program would increase the cost of tips in a market for information from civilians about combatant activities (Berman, Shapiro and Felter, 2011; Vanden Eynde, 2018; Berman, Felter and Shapiro, 2018). We do not observe tips from civilians in our conflict microdata. However, we do observe an outcome highly sensitive to the flow of information from civilians to government forces—the success rate of counterinsurgent bomb neutralizations. In columns 1 and 2 of Table B-29 we study the share of all insurgent-emplaced explosives that are successfully found and cleared prior to detonation, expressed as $\frac{\# \text{ of Explosives Found and Cleared}_{d,t}}{\# \text{ of Explosives Emplaced}_{d,t}}$. This measure

is undefined where no insurgent explosive emplacements occur. Hence, in columns 1-2 we exclude observations without insurgent explosive emplacements, and in columns 3-4 we fill these observations with zeroes. Columns 3-4 also add an indicator that takes the value one when a district experiences positive levels of emplacements and zero otherwise. We find that a one standard deviation increase in exposure to encashed returns reduces the rate of successful bomb clearances by 6% (column 2) to 10% (column 4), relative to the baseline.

Table B-29: Counterinsurgent Bomb Neutralizations

	Counterinsurgent Bomb Neutralizations			
	Clearance Rate (Missing)		Clearance Rate (Filled)	
	(1)	(2)	(3)	(4)
2013 Returnee Share x Grant Increase	-0.007*** (0.003)	-0.020** (0.009)	-0.010*** (0.001)	-0.018*** (0.004)
Observations	1621	1005	4378	4378
Clusters	271	178	398	398
Pre-Treatment Mean	0.348	0.348	0.176	0.176
Pre-Treatment SD	0.409	0.409	0.339	0.339
PARAMETERS				
District FE	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓
UNHCR Area x Month		✓		✓
Demographic Controls x Month		✓		✓
Development Controls x Month		✓		✓
Agricultural Controls x Month		✓		✓
Reconstruction Controls x Month		✓		✓
Lagged DV		✓		✓
Emplaced (=1)			✓	✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized night-lights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Emplaced (=1) is an indicator for whether any explosive emplacements occurred. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

B.22 Social Capital Mitigates Communal Violence

In Table 7 we study an index of social capital constructed using inverse covariance-weighting (Anderson, 2008). The core results are robust if we instead construct a social capital index using principal component factor analysis of items described in the main text: returnees' self-reports about whether they selected their destination communities in order to be near family, co-ethnics, or other returnees, and returnees' self-reports that they reside near familial kin or other repatriates.

Table B-30: Robustness with an Alternative Measure of Social Capital

	Returnee–Stayee Relations Index (Z-Standardized)					
	ICW			PCF		
	(1) Baseline	(2)	(3)	(4) Baseline	(5)	(6)
Encashment Beneficiary	-0.233* (0.126)	-0.148*** (0.055)	-0.219* (0.129)	-0.199* (0.113)	-0.194** (0.074)	-0.189 (0.114)
Encashment Beneficiary x Social Capital (PCF)		0.195*** (0.054)	0.191*** (0.055)		0.181** (0.075)	0.175** (0.075)
Social Capital (PCF)		0.058*** (0.020)	0.052*** (0.019)		0.064*** (0.019)	0.054*** (0.018)
Observations	7391	7231	7231	7283	7134	7134
Clusters	65	65	65	65	65	65
Control Group Mean	0.035	0.035	0.035	0.033	0.033	0.033
Control Group SD	0.978	0.978	0.978	0.967	0.967	0.967
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓	✓	✓
Urbanicity	✓		✓	✓		✓
Gender	✓		✓	✓		✓
Age	✓		✓	✓		✓
Ethnicity	✓		✓	✓		✓
Education	✓		✓	✓		✓
Income	✓		✓	✓		✓
Dwelling	✓		✓	✓		✓
Marital Status	✓		✓	✓		✓
Tazkira	✓		✓	✓		✓
Country of Asylum	✓		✓	✓		✓
Registration Status x Month of Return	✓		✓	✓		✓
Social Desirability	✓		✓	✓		✓
Interview Order	✓		✓	✓		✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. Encashment beneficiary is an indicator for documented refugee returnees from Pakistan to Afghanistan between July–December 2016. Social capital is an PCF index comprising measures of returnees' proximity to kin and other repatriates. Country of asylum and registration status by month of return fixed effects absorb constituent terms of the interaction that comprises our indicator for likely program recipients. Urbanicity is an indicator for urban (vs. rural) respondents. Gender is an indicator for male (vs. female) respondents. Tazkira is an indicator for respondents with a national identity card. Age, education, and dwelling have five categories. Income has 10 categories. Marital status has four categories. Social desirability is a measure of respondent comfort with four categories. Interview order captures the order of interviews within sampling points. Estimates are scaled using sampling weights. ICW indicates an index constructed using inverse covariance-weighting. PCF indicates an index constructed using principal component factor analysis.

In Table 7 we use our survey-based data to show that social capital moderates the effect of encashment beneficiary status on relations between returnees and their non-migrant neighbors. In Table B-31 we take a comparable approach to understand the role of social capital in mitigating communal violence using our supplemental event data on local feuds. To proxy for social capital in these specifications, we calculate the 2013 share of assisted returnees from Pakistan to their origin province and district (see also Figure B-11). In columns 3-4 of Table B-31, we interact the reduced-form (2013 Returnee Share x Grant Increase) with the share of returnees to a district in their origin province. In columns 5-6, we interact the reduced-form (2013 Returnee Share x Grant Increase) with the share of returnees to their origin district. We find that social capital significantly attenuates the positive effect of returnee exposure on communal violence.

Table B-31: Social Capital Mitigates Observed Communal Conflict

	Communal Violence					
	Baseline		Share Returning to Origin Province		Share Returning to Origin District	
	(1) Extensive Margin (0/1)	(2) Per 100k Pop.	(3) Extensive Margin (0/1)	(4) Per 100k Pop.	(5) Extensive Margin (0/1)	(6) Per 100k Pop.
2013 Returnee Share x Grant Increase	0.013*** (0.002)	0.007*** (0.002)	0.023*** (0.004)	0.015*** (0.006)	0.021*** (0.004)	0.014*** (0.005)
2013 Returnee Share x Grant Increase x Social Capital			-0.031** (0.013)	-0.021 (0.015)	-0.039** (0.018)	-0.033* (0.019)
Grant Increase x Social Capital			-0.014 (0.016)	0.006 (0.025)	-0.002 (0.018)	0.011 (0.027)
Observations	4378	4378	4378	4378	4378	4378
Clusters	398	398	398	398	398	398
Pre-Treatment Mean	0.097	0.047	0.097	0.047	0.097	0.047
Pre-Treatment SD	0.297	0.302	0.297	0.302	0.297	0.302
PARAMETERS						
District FE	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓
Pashtun Share x Month	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓	✓	✓

Note: * p <.10, ** p <.05, *** p <.01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

B.23 Quantifying Local Institutional Quality in Afghanistan

In Table B-33 we study how strong, local institutions mitigate communal violence in the wake of repatriation. We use four cross-sectional measures of informal institutional strength drawn from a variety of sources. We describe these sources here.

First, we draw on three measures from the Afghan Quarterly Nationwide Assessment Research (ANQAR) survey. ANQAR is a NATO-funded, representative survey fielded throughout Afghanistan by the Afghan Center for Socio-Economic and Opinion Research (ACSOR). ACSOR hires and trains local enumerators to conduct field research. Districts are selected via a probability proportional to size systematic sampling approach, and within districts, villages and settlements are randomly selected. A random walk method is used to identify target households, and a Kish grid is used to randomize the respondent within selected households. Before fielding, ACSOR engages with local elders to secure permission for enumerators to enter sample villages. This culturally-sensitive approach ensures survey enumerators have access to contested and insurgent-controlled areas. We use data from all 2013 waves, and collapse the three questions described in Table B-32 to the district-level for use in Table B-33.

Second, we draw on one measure from ACSOR’s field reports, which record whether the Taliban contested or controlled influence in a district in 2013. This enumerability-based measure of insurgent control avoids challenges associated with measuring territorial control in ways explicitly endogenous to local violence.

Table B-32: Measuring Local Institutional Quality in Afghanistan

Variable Name	Question	Response	Source
Prefer Shura to State Court	If you had a legal dispute, would you take it to an Afghanistan state court or a local Shura/Jirga?	To a Shura/Jirga	ANQAR
Prefer Elders to Gov’t Officials	If you had a problem, would you go to your local tribal leaders or the official government representatives?	Local tribal leaders	ANQAR
Took Dispute to Shura	Have you taken a dispute to an Afghanistan state court or a local Shura/Jirga in the past year?	Yes, local Shura/Jirga	ANQAR
Taliban Control	Coded based on ACSOR field reports, which describe district enumerability for survey administrators	Average level of Taliban territorial control in 2013	ACSOR

Note: Took dispute to shura is only coded using the subset of respondents who reported having a dispute in the past year.

B.24 Strong, Local Institutions Mitigate Communal Violence

We calculate the four measures described in Table B-32, and use these measures to proxy for strong, local institutions. In columns 3-10 of Table B-33, we interact the reduced-form (2013 Returnee Share x Grant Increase) with these measures in turn. Columns 1-2 report our baseline estimates for reference. Across measures, we find that the positive effect of exposure to encashed returns on communal violence is substantially attenuated in districts with stronger local institutions.

Table B-33: Strong, Local Institutions Dampen Return-Induced Communal Violence

	Communal Violence									
	Baseline		Prefer Shura to State Court		Prefer Elders to Gov't Officials		Took Dispute to Shura		Taliban Control	
	(1) Extensive Margin (0/1)	(2) Per 100k Pop.	(3) Extensive Margin (0/1)	(4) Per 100k Pop.	(5) Extensive Margin (0/1)	(6) Per 100k Pop.	(7) Extensive Margin (0/1)	(8) Per 100k Pop.	(9) Extensive Margin (0/1)	(10) Per 100k Pop.
2013 Returnee Share x Grant Increase	0.015*** (0.003)	0.009*** (0.003)	0.031*** (0.006)	0.015* (0.008)	0.051*** (0.018)	0.034 (0.022)	0.031*** (0.009)	0.021* (0.012)	0.015*** (0.002)	0.008*** (0.003)
2013 Returnee Share x Grant Increase x Strong, Local Institutions			-0.092*** (0.031)	-0.048 (0.039)	-0.139** (0.068)	-0.097 (0.082)	-0.173* (0.104)	-0.150 (0.125)	-0.050* (0.026)	-0.042 (0.028)
Grant Increase x Strong, Local Institutions			-0.043 (0.047)	-0.117 (0.112)	0.001 (0.050)	-0.007 (0.079)	0.024 (0.047)	-0.043 (0.069)	-0.001 (0.019)	-0.017 (0.024)
Observations	4378	4378	3817	3817	3817	3817	3795	3795	4378	4378
Clusters	398	398	347	347	347	347	345	345	398	398
Pre-Treatment Mean	0.097	0.047	0.097	0.047	0.097	0.047	0.097	0.047	0.097	0.047
Pre-Treatment SD	0.297	0.302	0.297	0.302	0.297	0.302	0.297	0.302	0.297	0.302
PARAMETERS										
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: * p < .10, ** p < .05, *** p < .01. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Strong, local institutions are: 2013 average reliance on shuras (columns 3–4) or elders (columns 5–6); usage of shuras (columns 7–8); or Taliban governance (columns 9–10). Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

B.25 Communal Violence, Institutions, and Historical Violence

A natural concern is that districts with stronger local institutions are less violent in the first place. To address this concern we re-estimate the local institutions (Table B-33) specifications while controlling for levels of violence in a district in 2013 (i.e., total number of SIGACTs). In results omitted for space but available upon request we also verify robustness to controlling for historical levels of counterinsurgent, insurgent, and criminal SIGACTs. The core results are robust in every case.

Table B-34: Communal Violence, Local Institutions, and Historical Violence

	Communal Violence							
	Prefer Shura to State Court		Prefer Elders to Gov't Officials		Took Dispute to Shura		Taliban Control	
	(1) Extensive Margin (0/1)	(2) Per 100k Pop.	(3) Extensive Margin (0/1)	(4) Per 100k Pop.	(5) Extensive Margin (0/1)	(6) Per 100k Pop.	(7) Extensive Margin (0/1)	(8) Per 100k Pop.
2013 Returnee Share x Grant Increase	0.032*** (0.006)	0.015* (0.008)	0.054*** (0.019)	0.037 (0.023)	0.031*** (0.009)	0.021* (0.012)	0.015*** (0.002)	0.008*** (0.003)
2013 Returnee Share x Grant Increase x Strong, Local Institutions	-0.099*** (0.032)	-0.055 (0.040)	-0.151** (0.071)	-0.111 (0.086)	-0.174* (0.104)	-0.152 (0.126)	-0.050* (0.026)	-0.042 (0.028)
Grant Increase x Strong, Local Institutions	-0.050 (0.046)	-0.125 (0.113)	0.006 (0.050)	-0.002 (0.079)	0.025 (0.047)	-0.043 (0.069)	0.002 (0.019)	-0.013 (0.024)
Observations	3817	3817	3817	3817	3795	3795	4378	4378
Clusters	347	347	347	347	345	345	398	398
Pre-Treatment Mean	0.097	0.047	0.097	0.047	0.097	0.047	0.097	0.047
Pre-Treatment SD	0.297	0.302	0.297	0.302	0.297	0.302	0.297	0.302
PARAMETERS								
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-Specific Month FE	✓	✓	✓	✓	✓	✓	✓	✓
UNHCR Area x Month	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Development Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Agricultural Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Reconstruction Controls x Month	✓	✓	✓	✓	✓	✓	✓	✓
Lagged DV	✓	✓	✓	✓	✓	✓	✓	✓
Total SIGACTs x Month	✓	✓	✓	✓	✓	✓	✓	✓

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Robust, district-clustered standard errors are in parentheses. 2013 returnee share is each district's standardized share of the sum of assisted refugee returnees from Pakistan to Afghanistan in 2013. Grant increase is an indicator for July–December 2016, when the UNHCR offered an increased repatriation grant. Strong, local institutions are: 2013 average reliance on shuras (columns 1-2) or elders (columns 3-4); usage of shuras (columns 5-6); or Taliban governance (columns 7-8). Demographic controls are: Pashtun share and population (winsorized). Development controls are: distance to the border; travel time to the provincial center; ruggedness; provincial unemployment; provincial GINI coefficient; and population-normalized nightlights. Agricultural controls are: the share of land under cultivation; the share of cultivated land that is irrigated; and hectares of opium cultivation and eradication (inverse hyperbolic sine-transformed). Reconstruction controls are: per capita spending on governance, infrastructure, health, conflict stabilization, and refugees projects. All controls are pre-shock measures interacted with year-specific month fixed effects. Estimates are scaled using population weights. The estimation sample includes district-months in 2016.

C Data Appendix

C.1 Conflict

Our data on conflict come from official, government channels. These data are unclassified, but subject to access restrictions meaning they cannot be publicly shared. None of the data contain any potentially identifying information. The authors established access through a data agreement with the organization that maintains the data, NATO's Communications and Information Agency. Interested researchers can contact Blair (chris.blair@princeton.edu) or Wright (austinlw@uchicago.edu) for more information about the authorization process.

C.1.1 Insurgent Violence

Military combat records come from the **International Distributed Unified Reporting Environment** (INDURE), an unclassified but restricted-access platform maintained by the US Defense Department. Events described in INDURE are sourced from the US military's classified Combined Information Data Network Exchange. The INDURE files cover 2015-2017, and correspond with the Significant Activities (SIGACTs) database publicly available for earlier years in Afghanistan. These data were jointly collected by Afghanistan's military and

police forces and multinational forces of the North Atlantic Treaty Organization’s (NATO) International Security Assistance Force (ISAF). These records include information on combat activity as described in the manuscript. Additional information about the SIGACTs data is provided in [Berman, Shapiro and Felter \(2011\)](#); [Shaver and Wright \(2017\)](#); [Condra et al. \(2018\)](#); [Fetzer et al. \(2021\)](#). Although these data do not include any identifying information about individuals, the data has not been formally released for public use.

C.2 Communal Violence

We take multiple strategies to measure social conflict in Afghanistan. Our data come from a survey of returnees and a government-sponsored conflict event tracker.

C.2.1 Survey of Returnees

Our main analyses study returnees’ communal relations using individual-level data from a survey of Afghan refugee returnees. This data come from the Asia Foundation’s **Survey of the Afghan Returnees**, fielded in 2018–2019. This survey also offers data we use in a variety of supplemental tests (e.g., Tables [A-5](#) – [A-7](#)). More details about survey administration are discussed in Section [4](#). The data are available at this link: <https://bit.ly/36lW3yh>. See [Breslawski \(2021\)](#) for another source using Asia Foundation survey data from Afghanistan.

C.2.2 Event Data

We compliment our survey-based data with supplemental records of conflict events. Specifically, to measure social conflict we undertake an original coding process using data from a US Government and NATO-sponsored conflict tracker, which collates information from the Afghan National Police Command Center (NPCC), risk-reporting agencies contracted by the US State Department and Western embassies in Kabul, and open-source media. The data were secured through a legal agreement between the authors and NATO’s Communications and Information Agency. Following [Sexton \(2016\)](#), we use a supervised text analysis method based on a keyword-search algorithm available in Stata through the regular expressions command `regexm`. The tracker we use provides a categorical descriptor and an event narrative with 1-3 sentences describing the event. Using categorical descriptors, we filter out events pertaining to insurgent or counterinsurgent operations. To identify instances of social conflict, we code these filtered events using standard keywords in the event narratives, like LN (“local nationals”). An example code snippet is included below to illustrate:

```
keep if category=="Other"
```

```
gen communalviolence=(regexm(narrative, "conflict") & regexm(narrative, "tribes")
| regexm(narrative, "conflict") & regexm(narrative, "personal enmity") |
regexm(narrative, "conflict") & regexm(narrative, "nomad") | regexm(narrative,
"conflict") & regexm(narrative, "land issues") | regexm(narrative, "conflict"))
```

```
& regexpr(narrative, "grazing") | regexpr(narrative, "conflict") & regexpr(
narrative, "water") | regexpr(narrative, "LN") & regexpr(narrative, "dispute") |
regexpr(narrative, "LN") & regexpr(narrative, "clash") | regexpr(narrative,
"conflict") & regexpr(narrative, "irresponsible armed")
```

C.3 Migration

Our data on refugee return come from UNHCR and IOM.

C.3.1 Assisted Refugee Returns

We use two sources for data on assisted (i.e., documented) refugee returns. In Figures A-3 and A-4, we study the time-series of refugee stocks and returns for select countries. These records cover several decades, but entail limitations (see Fearon and Shaver, 2020). The data come from the UNHCR PopStats database available here: <https://bit.ly/3hNBCzx>. See Moore and Shellman (2007) and Zakirova and Buzurukov (2021) for more discussion and applications of the data.

Our granular measures of refugee return used in the main analyses come from the UNHCR-Afghanistan office, and describe the complete record of documented returns to Afghanistan for the period from 2013-2014 and 2016-2017. More details about these records are provided in the manuscript. The data are available through the Humanitarian Data Exchange and the UNHCR Microdata Library. We do not observe monthly inflows of returnees to Afghanistan for 2015, but reconstruct these using information contained in UNHCR field reports and the UNHCR's Population Movement Bulletin.

C.3.2 Spontaneous Refugee Returns

To reconstruct monthly inflows of spontaneous returnees reflected in Figure 1, we draw on Flow Monitoring Reports filed by IOM officials of the Cross Border Return and Reintegration, Protection, Humanitarian Assistance, and Reintegration and Development (RADA) program. These data contain no information on the destination district of returnees and are collapsed into a time series of return.

C.3.3 Internally Displaced People

In Tables B-23 – B-24 we exploit data on internally displaced people (IDPs). To track IDPs, we use data from the IOM's Displacement Tracking Matrix (DTM), which uses representative field surveys to provide retrospective estimates of displacement at the district level. We specifically draw on the product known as *Afghanistan – Baseline Assessment District Round 11*. These data are available at this link: <https://bit.ly/3xmWG1x>.

C.4 Administrative Boundaries and Geodata

Data on administrative boundaries (district and province) were retrieved from Empirical Studies of Conflict (ESOC) research group and are available at this link: <https://bit.ly/39oJrre>. These administrative boundaries are used for synchronizing combat event data and other geodata.

C.4.1 Population

We rely on data provided by WorldPop, a project housed at the University of Southampton, to measure population. We specifically use the product known as **Unconstrained individual countries 2000–2020 (100 m)**, which provides global population estimates at a resolution of 100 square meter grid cells. Following [Stevens et al. \(2015\)](#), WorldPop uses an unconstrained, top-down, dasymetric modeling approach incorporating geospatial census and ancillary data in a flexible, random forest estimation. Using an annual time-series raster, we calculate population per district in 2013.

C.4.2 Ethnicity

We use spatial data from the on the location of population settlements to measure the share of Pashto speakers (Pashtun) in a given district. The location of village settlements was compiled by the Afghanistan Information Management Service, Central Statistics Office, United States Agency for International Development (USAID), and Yale University. Information on each settlement’s share of Pashto speakers was collected during the USAID-funded Measuring Impact of Stabilization Initiatives (MISTI) project. Our measures of ethnic fractionalization and polarization are also drawn from a USAID data repository on MISTI. For details on MISTI, see [Egel and Glick \(2016\)](#).

C.4.3 Terrain

We rely on data provided by the US Geological Survey’s Earth Resources Observation and Science (EROS) Center to calculate terrain ruggedness. Raster data from the Shuttle Radar Topography Mission (SRTM) provide a global record of elevation. We use the product known as **SRTM 1 Arc-Second Global**, which captures void-filled data at a resolution of 1 arc-second. We calculate ruggedness as the standard deviation of elevation as in [Nunn and Puga \(2012\)](#).

C.4.4 Nightlights

We calculate variation in nighttime lights using the VIIRS: Visible Infrared Imaging Radiometer Suite. This data is compiled by the Earth Observation Group at the Colorado School of Mines using the Suomi National Polar-orbiting Partnership satellite. Following [Li et al. \(2020\)](#), we use the product known as **VIIRS Cloud Mask (vcm)**, which captures the detected radiance of city lights around the spectrum of 505–890 nanometers at a spatial

resolution of 15 arc-seconds across the latitudinal zone of 65°S-75°N. This measure excludes observations affected by stray light, and corrects effects caused by biogeophysical processes (e.g. snow) using the bidirectional reflectance distribution function. Using an annual time-series raster, we calculate nightlights per district in 2013.

C.5 Development Aid

We incorporate information from the **Development Assistance Database (DAD)**, maintained by Afghanistan Information Management Services (AIMS) and AidData. The data cover all external assistance projects in Afghanistan’s DAD, tracking more than \$16 billion in aid commitments for 1,561 projects across 7,168 locations between 2001 and 2014. We sum aid by category and district in 2013.

C.6 Economic Performance

We measure labor market and income-related covariates using data from the **Afghan Living Conditions Survey (ALCS)**. ALCS, previously known as the National Risk and Vulnerability Assessment (NRVA), is a representative survey fielded by the Central Statistical Organization of Afghanistan, and used to assess development outcomes. We study provincial unemployment and the provincial GINI coefficient using the 2013-2014 ALCS wave, which covered 20,786 households and 157,262 people. These data are available at this link: <https://bit.ly/2V88yLp>, and described in greater detail by [Child \(2019\)](#).

C.7 Travel Time and Land Quality

We measure travel time to the provincial center and agricultural land quality using data from the World Bank’s Afghanistan Spatial Database, and specifically, the **District Dashboard**. This source compiles information from Afghan censuses, reports of the Central Statistical Organization of Afghanistan, and proprietary World Bank sources. These data are available at this link: <https://bit.ly/2Van3yi>.

C.8 Opium Cultivation and Eradication

We measure cultivation and eradication of opium poppy using data from the United Nations Office on Drugs and Crime (UNODC), and specifically from the **Opium Survey 2013**. The UNODC-Afghanistan office estimates poppy production using ground-validated remote sensing techniques to track changes in vegetation during the spring harvest. After randomly spatially sampling potential agricultural zones within provinces, UNODC officials acquire pre- and post-harvest imagery, and examine them for changes in vegetative signatures consistent with the volatile wetness of opium plants after lancing. From this sampling technique, officers estimate the spatial risk of opium production, calculate gridded estimates, and then compile them as the annual amount of opium production in hectares for each district. To

measure eradication, UNODC collects official reports of Governor-led opium poppy eradication, and deploys enumerators to verify eradication campaigns. These data are available at this link: <https://bit.ly/3qRBinq>, and described in greater detail by [Sonin and Wright \(2024\)](#).

C.9 Local Institutions

C.9.1 Shuras/Jirgas

We capture local reliance on informal institutions for dispute resolution using survey data from the Afghanistan Nationwide Quarterly Assessment Research (ANQAR) platform. ANQAR is a NATO-funded effort, which tracks civilian attitudes on a host of pertinent outcomes. We offer a more detailed description of the enumeration process in Table B-32. Survey responses are collected on a quarterly basis by local contractors. Before administering each wave, elders are contacted to secure permission for enumerators to enter villages. When enumerators cannot access sampled villages, intercept interviews are used to collect information from residents traveling in neighboring areas. See [Condra and Wright \(2019\)](#) for a more comprehensive discussion of the data.

The individual-level ANQAR data are restricted-access only. The authors established access through a data agreement with the organization that maintains the data, NATO’s Communications and Information Agency. Interested researchers can contact Blair (chris.blair@princeton.edu) or Wright (austinlw@uchicago.edu) for more information about the authorization process.

D Ethical Appendix

In social science research there is a longstanding formal requirement, as well as a moral imperative, to protect human subjects. This section documents the steps the authors took to identify and mitigate risks associated with the research.

No institutional review board (IRB) approval was required by the authors’ institutions because this project does not meet the standards for “human subjects” research under existing federal guidelines (45 CFR 46.102(f)(1)(2)). This was confirmed to the authors in conversations with IRB administrators from two universities with which the authors are/were affiliated (Princeton University and the University of Pennsylvania). Under federal regulations, “human subjects” are defined as “living individual[s] about whom an investigator conducting research obtains (1) information or biospecimens through intervention or interaction with the individual and uses, studies, or analyzes the information or biospecimens; or (2) obtains, uses, studies, analyzes, or generates identifiable private information or identifiable biospecimen[s].” As we clarify below, this study is exempt on both points: the authors

never contacted or interacted with the research subjects (e.g., survey respondents), and none of the data we use contain personally identifiable or private information.

No interactions between the authors and the research subjects ever occurred because the data used throughout this project were collected by external partners (e.g., NATO, UNHCR, the Asia Foundation) and shared with the authors post collection. The authors had no role in the design, implementation, or collection of any surveys or administrative microdata we use. Additionally, none of the records we use contain personally identifiable or private information. In particular, all survey respondents across the various surveys we employ consented to sharing their opinions and were informed that their responses could be shared with researchers. Apart from the surveys we use, administrative records on repatriation shared by UNHCR are also potentially sensitive, since these contain detailed information on household location, size and gender composition. Before sharing these records, UNHCR staff deidentified the records, aggregating locational information about to the district-level (a large administrative unit) and removing additional information that could be used in combination with the shared information (e.g., on location and household size) to identify individual returnees or households. Finally, as a condition of access to the survey and repatriation microdata used in the study, the authors signed data access agreements in which we affirmed we would not attempt to identify or recontact any study subjects.

Although this research was deemed exempt from IRB protocols, the authors took a number of steps to ensure the ethical appropriateness of the research. Above all, we worked to ensure the research was guided by the principle of “do no harm” (Wood, 2006). To this end, we held multiple conversations with administrators and staff at the UNHCR, the Asia Foundation, NATO, and other organizations that played a role in collecting data used in this study. The goal of these conversations was to ensure that the organizations’ data collection procedures were aligned with contemporary ethical standards, and that our own use of the data complied with the terms of access agreements we signed with the relevant data providers.

Survey data we use in this study were initially collected by the Asia Foundation, which contracted with a local Afghan firm, the Afghan Center for Socio-Economic and Opinion Research (ACSOR), for implementation. We describe the ethical and data quality procedures the Asia Foundation and ACSOR followed in section D.1 below. Two points are particularly relevant for our conclusion that the Survey of Afghan Returnees was fielded in an ethical manner. First, as discussed in more detail below, the survey was implemented by a local survey firm working with local enumerators versed in local dialects and knowledgeable of local customs. This raises our confidence that survey implementation was culturally-sensitive and attentive to local norms and safety standards. Second, the Survey of Afghan Returnees underwent comprehensive ethical review by the Asia Foundation’s in-house Safeguarding team prior to fielding. This team leads ethical training sessions for humanitarian and development partners, and administers an ombudsmen’s hotline where relevant stakeholders, including survey respondents, can report misconduct or other concerns. The Australian government’s

Department of Foreign Affairs and Trade endorses the Asia Foundation’s Safeguarding team as an industry leader in ethical fieldwork (Jackson, Denney and Illingworth, 2021). To the best of our knowledge, no allegations of misconduct or other concerns were ever raised about the Survey of Afghan Returnees. After fielding, the Asia Foundation’s Safeguarding team also worked to back-check fieldwork to ensure data integrity and compliance with the American Association for Public Opinion Research (AAPOR) Code of Professional Ethics and Practices.

Apart from the survey data, microdata on repatriation shared by the UNHCR are also potentially sensitive. The data were de-identified by UNHCR staff prior to their public release, but given the sensitive nature of refugee return and the unique protection needs of displaced people, we also worked to continuously verify that our use of the data accorded with UNHCR standards on data protection. First, we spoke with UNHCR representatives from the Afghanistan country office about the collection methodology. These staff informed us that all repatriates represented in the data consented verbally to sharing information with UNHCR staff during the repatriation process, and were further informed that their anonymized data could be shared with researchers. Second, these UNHCR staff also informed us that the data collection effort was supervised by field staff trained on UNHCR’s guiding principles for ethical data collection. Third, we confirmed that the repatriation microdata shared with us were compiled and disseminated in accordance with the UNHCR’s General Policy on Personal Data Protection and Privacy, meaning, among other things, that the data were assessed prior to release by the UNHCR’s Personal Data Protection Review Committee and Chief Data Protection and Privacy Officer.

The third set of microdata we use in this study are incident reports filed by Afghan government and NATO forces deployed in Afghanistan. We describe the data collection procedures NATO followed in section D.2 below. As with the UNHCR microdata, these conflict records were de-identified by NATO staff prior to their release to us under a data sharing agreement negotiated in 2019. One condition of the agreement governing our access to these data is that the conflict records can only be released in an aggregated fashion (e.g., district-level maps documenting violence across districts over long temporal periods like months.). This condition is explicitly intended to mitigate risks of identification of victims of violence, and to reduce risks of Taliban retaliation against suspected government collaborators.

Finally, in addition to verifying that potentially sensitive data we use were collected in a manner consistent with contemporary ethical standards, we also carefully weighed the potential ethical benefits and costs of our study. We ultimately concluded that the potential ethical benefits of our research were significant, and outweighed any potential costs. In particular, we believe that insights from our study are important for tailoring humanitarian protection, facilitating safe and dignified repatriation, and improving repatriation policymaking—key priorities for development practitioners and international policymakers. To this end, we also take care in the manuscript to highlight the many ethical pitfalls of incentivized repatriation

schemes.

D.1 Survey of the Afghan Returnees

We supplement analyses of administrative records with analyses of 8,000+ individual-level survey responses from the representative Survey of the Afghan Returnees. We specifically study data from two waves fielded in 2018–2019. Survey data were gathered by the Afghan Center for Socio-Economic and Opinion Research (ACSOR), an Afghan subsidiary of the international research firm D3 Systems, which the Asia Foundation contracted to design and field various atmospherics surveys. ACSOR was contracted in part because the Asia Foundation viewed it as a high-fidelity implementing partner: it was led by survey methodologists, and its chairman held a social science Ph.D. The Asia Foundation’s determination that ACSOR was the most reliable implementing partner in Afghanistan was informed by a similar assessment made by NATO, which also contracted with ACSOR to administer surveys.

The survey was conducted in the provinces of Kabul, Balkh, Kandahar, Nangarhar, and Herat. For sampling, a frame of settlements from the International Organization for Migration (IOM) was used. The settlement was the primary sampling unit in the Survey of the Afghan Returnees, and settlements were selected via a probability-proportional-to-size systematic sampling approach. Findings can be taken as representative of returnees and host communities in the provinces where the survey was fielded. However, because of the nature of the sample, the findings are not projectable onto the national returnee or host community populations. In total, a sample of 8,044 Afghan citizens were surveyed face-to-face across the five provinces included in the study. All households were selected by random walk, and respondents were selected through a combination of screener questions and Kish grid among eligible household members. Respondents were 18 years and older; returnees had to have returned to Afghanistan within the past five years; and host community respondents had to know at least one returnee personally. Sampled respondents were gender-matched to enumerators, in keeping with local gender norms. Where weather-induced transportation issues (e.g., flooding) or threats to enumerator safety meant ACSOR could not conduct random selection interviewing, accessible replacement settlements were sampled.

To better understand how the survey was administered, we held several conversations with current or former employees of the contracting agency (the Asia Foundation), the implementing partner (ACSOR/D3 Systems), and NATO, an organization that also commissioned ACSOR surveys. All individuals we spoke with had direct knowledge of the survey from time working on the project or similar surveys. Conversation partners included a chairman at ACSOR, a managing director at ACSOR, a project manager at ACSOR, and an operational analyst at NATO’s Afghanistan Assessment Group. In all of these conversations, survey staffers highlighted several best-practices they used in survey administration:

- ACSOR teams hired and trained enumerators in every sampled province of Afghanistan. Training covered household and respondent selection, how to correctly record responses,

culturally-sensitive interview methods, and secure storage of contact information. Once trained by provincial-level teams from ACSOR, enumerators were assigned to enumerate districts in their province of origin. Consequently, all enumerators spoke local languages in local dialects, and were knowledgeable of important local customs.

- After the sampling set was identified and before fielding each wave, ACSOR entered negotiations with elders in selected settlements to secure permission for enumerators to operate. This locally-sensitive approach enabled enumerators to safely conduct fieldwork in areas of weak state reach.
- Under no circumstances were ACSOR enumeration teams accompanied by counterinsurgent or government personnel, including members of NATO, the Afghan National Security Forces (ANSF), or other security agencies.
- Field supervisors made note of political, social, or other newsworthy events that occurred during fielding and may have affected the survey. Where interviews may have been impacted, supervisors back-checked responses for quality assurance.
- After fielding, data were screened for keypunching errors. ACSOR randomly selected 10% of survey responses for duplicate entry. Double-punched questionnaires were compared to original questionnaires, and discrepancies were rectified.
- During the data processing phase, D3 examined all responses using a proprietary program called Valkyrie, which was built to search for patterns or anomalies in the data that may indicate an interview was not properly conducted by an interviewer. Valkyrie specifically conducted: (1) equality tests to compare interviews for similarities, grouped by interviewer, within sampling point and/or province; (2) “Don’t Know” tests of the percentage of “Don’t Know” responses for each enumerator; and (3) duplicate tests comparing cases across all interviewers and respondents to check for similarity rates. Across waves, fewer than 1.6% of all responses were removed by Valkyrie.

On behalf of the Asia Foundation, ACSOR tracked rates of response, cooperation, and refusal for all waves. Using these data, we conduct diagnostic tests. Encouragingly, the rate of response is high (mean = 74.57% across waves), the rate of cooperation is high (mean = 89.12% across waves), and the rate of refusal is low (mean = 7.10% across waves). These rates of non-contact, cooperation, and refusal are comparable to rates from well-known surveys like the General Social Survey (GSS) fielded in the US, as well as to rates from other surveys fielded by ACSOR for NATO in Afghanistan ([Condra and Wright, 2019](#); [Blair, 2025](#)).

D.2 Significant Activities (SIGACTs)

Administrative records in the main analyses come from the International Distributed Unified Reporting Environment (INDURE), an unclassified but restricted-access platform maintained by the US Defense Department. Events described in INDURE are sourced from the US military’s classified Combined Information Data Network Exchange. The INDURE files

cover 2015-2020, and represent a successor dataset to the declassified Significant Activities (SIGACTs) record, which covers the period from 2008–2014. Together, the INDURE and SIGACTS data form the most comprehensive account of insurgent and counterinsurgent operations during the War in Afghanistan (Shaver and Wright, 2017), totalling more than 580,000+ individual events. The data draw from a secure, classified platform populated using highly-detailed combat reports logged by NATO and Afghan troops and police. Equipped with satellite-linked GPS equipment in the field, these forces recorded the geolocation of every reported event at a highly-granular level, and the time-stamp of every reported event down to the minute. Because records were gathered by soldiers in the field, data collection was not subject to access constraints like insurgent territorial control, which plague survey- and media-based event trackers (Weidmann, 2016). While most extant work studies a subset of the SIGACTs data covering insurgent engagements against counterinsurgent forces (e.g. Fetzner et al., 2021; Blair, 2024), the rich INDURE and SIGACTs reports also cover a range of counterinsurgent-initiated operations, police actions, and other notable community events (e.g. Sonin and Wright, 2024; Blair, 2025).

All records were anonymized and scrubbed of potentially personally-identifiable information by NATO staff prior to release to us under the terms of a legal data-sharing agreement. In addition, to reduce the risk of harm to civilian collaborators of Afghan and NATO forces, NATO staff scrubbed the data of one especially sensitive category of SIGACTs—tips from civilians about insurgent operations. To further prevent risks of harm from our use of these detailed data, NATO also mandated that any release of information from the dataset would have to be aggregated to a sufficiently high level to prevent possible reidentification of victims. For instance, we must aggregate counts of violence to at least the district-month level in maps.

Although these records do not suffer reporting bias inherent in media-based conflict event data, they do have several relevant weaknesses (Berman, Shapiro and Felter, 2011, p. 790, 808-809). First, incidents are only detected when NATO or Afghan forces are present, and so undercount the total volume of events other than insurgent attacks against counterinsurgent forces. Second, counterinsurgent units may differ in their propensity for reporting any given event. The second concern raises the risk that changes in unit-level counterinsurgent leadership lead to shifts in reporting policies. We are sanguine because (Berman, Shapiro and Felter, 2011, p. 790) investigate this concern in the Iraqi context and find no evidence that errors from unit-level differences in reporting thresholds are nonrandom with respect to the variables of interest. Moreover, documentary evidence from Afghanistan suggests NATO advisory teams pushed Afghan security forces to standardize information and reporting guidelines, reducing concern about heterogeneous reporting standards.

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