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Abstract

Conflict undermines development, while poverty, in turn, breeds conflict. Policy interventions such as cash transfers could lower engagement in conflict by raising poor households' welfare and productivity. However, cash transfers may also trigger appropriation or looting of cash or assets. The expansion of government programs may further attract attacks to undermine state legitimacy. To investigate the net effect across these forces, this paper studies the impact of cash transfers on conflict in Niger. The analysis relies on the large-scale randomization of a government-led cash transfer program among nearly 4,000 villages over seven years, combined with geo-referenced conflict events that draw on media and nongovernmental organization reports from a wide variety of international and domestic sources. The findings show that cash transfers did not result in greater pacification but−if anything−triggered a short-term increase in conflict events, which were to a large extent driven by terrorist attacks by foreign rebel groups (such as Boko Haram) that could have incentives to "sabotage" successful government programs.

JEL-Codes: D740, I380, O170.

Keywords: conflict, terrorism, cash transfers, Sahel.

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1 Introduction

Armed conflict and political violence not only lead to tremendous human suffering, but also constitute a major blow to affected economies.[1] Hence, unsurprisingly, in the last decade economists have shown a booming interest in explaining the drivers of armed conflict. While this has led to an impressive body of knowledge, the lion's share of results does not directly relate to policy interventions. There is also a notable scarcity of large-scale experimental evidence linking policy interventions and conflict, as discussed below. This is the gap in the literature that this paper addresses.

One striking fact is that civil conflicts have been predominantly concentrated in poorer countries in the post–World War II period (see, e.g., Fearon and Laitin 2003; Collier et al. 2009). While this association may not reflect causality, there is ample evidence that negative productivity shocks (typically measured by adverse climatic shocks) tend to fuel conflict (see, e.g., Miguel et al. 2004; Dell et al. 2014).

A natural idea, hence, is to reverse engineer productivity shocks and put in place policies to increase the earnings and welfare of the poor. Cash or in-kind transfers constitute one such type of policy instrument, particularly as they have been shown to facilitate human capital investments, induce asset accumulation, foster higher-productivity income-generating activities and reduce poverty (see, e.g. Fizsbein and Schady 2009; Alderman and Yemtsov 2014; Millán et al. 2019; Kondylis and Loeser 2021; Banerjee et al. 2022). Although cash transfer programs do not typically have direct peacebuilding objectives, the idea that aid and transfers also necessarily foster peace is widespread in government agencies and public debates.[2]

While even in the absence of pacifying benefits there remain plenty of powerful arguments for anti-poverty programs, an empirical investigation of the argument that they also contribute to peace is key. Indeed, the nexus between transfers and conflict or peace has a range of policy implications, including for security policies: if foreign aid curbs conflict, peacekeeping troops could eventually be redeployed, while if aid fuels conflict, this may call for a reinforcement of security in targeted areas.

To address the question of whether cash transfers foster peace, we study the impact on conflict of a large-scale, randomized government-led cash transfer program in Niger. The national cash transfer program ("Filets Sociaux") was set up by the government of Niger to deliver unconditional cash transfers for two years through monthly payments. Yearly benefits amounted to about 11 percent of household consumption. The program has had well-documented positive impacts on its core objectives related to household consumption and food security, as well as asset accumulation, income-generating activities, and psychosocial well-being (see, e.g., Stoeffler et al. 2020; Bossuroy et al. 2022; Premand and Barry 2022; Premand and Stoeffler 2022). We leverage the randomization of treatment assignment among nearly 4,000 eligible villages to estimate the causal impact of these cash transfers on conflict events. Geo-referenced conflict events are obtained from the GDELT data set (Leetaru and Schrodt 2013), which draws on media and NGO reports from a wide variety of international and domestic sources (as discussed in more detail below).

We do not find that cash transfers contribute to foster peace, at least not in the short-to-medium run. If anything, treated villages experienced an increase in conflict events during program implementation. This increase was mostly driven

[1] Anderton and Brauer (2021) estimate 100 million mass-atrocity-related deaths since 1900. In terms of economic costs, Mueller and Tobias (2016) estimate an average drop in GDP of 18 percent after a civil war, with only a very slow economic recovery. Abadie and Gardeazabal (2003) find that terrorist conflict in the Basque country has led to a roughly 10 percent drop in GDP per capita compared to a synthetic control region.

[2] For instance, this manifests itself in the US "Food for Peace" program. Also see, e.g., "Japanese wage peace with talks and money, pleasing Asians," *New York Times*, 8 December 2002; "Entwicklungspolitik kann Frieden schaffen," German Government, 9 April 2016; "Building a lasting peace and combatting fragilities," French Government, January 2021.

by incidents perpetrated by actors from outside the villages, especially foreign groups linked to international terrorism (such as Boko Haram incursions from Nigeria). In particular, while severe conflict events remained quite rare, we find that their incidence increased by 0.63 percentage point, including a 0.41 percentage point increase in events involving terrorism and a 0.34 percentage point increase in events involving foreign terrorists.

We find that these effects are temporary: in the years after the end of the program, treated villages did not experience a persistently higher level of conflict events. In addition, we do not find evidence of local spillovers, as the cash transfer program did not affect the likelihood of conflict in nearby control villages relative to control villages further away. Lastly, using as a secondary data source a household-level survey in a subsample of villages, we show that (self-reported) within-village conflict events neither decreased nor increased during treatment.

Our results can be read through the lens of a framework with several countervailing forces (see the discussion in Rohner and Thoenig 2021): (i) productivity increases may raise the opportunity cost for locals of engaging in conflict; (ii) an increase in lootable wealth increases the "prize" that can be appropriated (both by local inhabitants as well as by foreign groups); (iii) international terrorist groups may have incentives to "sabotage" successful government-led poverty eradication programs which increase government legitimacy (see, e.g., Crost et al. 2014). Groups such as Boko Haram have indeed been observed to attack government services such as schools or health centers (see discussion below).

The interplay of these countervailing forces can account for the temporal patterns that we observe. In particular, during the program (when the cash payments took place), effects (ii) and (iii) may have gained traction, causing conflict to *increase*. The scope for mechanism (i) seems more limited in the context of this paper, given that much of the observed violence stemmed from foreign groups. (Indeed, throughout the sample, the role played by home-grown violence was limited relative to violence perpetuated by foreign groups.) Also—if anything—we would expect this effect to occur with a lag (as it may take some time for cash transfers to be invested and raise productivity).

After the end of the program, both mechanisms (ii) and (iii) may have become less salient. Given that the government-led cash transfer program only lasted for two years, the scope for appropriation activities and the incentives to undermine the expansion of government presence may have receded after program completion in a given location. Further, the Niger cash transfer program has had well-documented medium-term impacts on economic welfare, income-generating activities, and assets (such as livestock) (Bossuroy et al. 2022; Stoeffler et al. 2020). To the extent that these effects raise the marginal productivity of labor, they are consistent with a higher (local) opportunity cost of conflict, which tends to reduce incentives to engage in conflict for domestic actors. While this effect may well have contributed to the demise of the conflict spike after the end of the program, its role may be moderate, given the dominant importance of foreign actors.

Our findings are related to the literature on the natural resource curse and on higher rents fueling conflict (see, e.g., the survey by Rohner 2018 and recent papers by Caselli et al. 2015; Berman et al. 2017; McGuirk and Burke 2020), as well as the literature on how higher or lower productivity and opportunity costs of conflict attenuate or fuel fighting, respectively (Miguel et al. 2004; Dell et al. 2014; König et al. 2017; McGuirk and Burke 2020; Eberle et al. 2020).

Another relevant body of literature studies the impact on conflict of policy interventions that provide cash transfers or other forms of foreign aid (see also the surveys of Findley 2018; Rohner 2022). While earlier studies have focused on the country-year level (see, e.g., De Ree and Nillesen 2009, who found that foreign aid abbreviates conflicts), many recent studies have leveraged within-country variation. In particular, Nunn and Qian (2014) conclude, using a shift-share instrumentation strategy, that in-kind US food aid tends to increase the risk of conflict. Consistent with this, Crost et al. (2014) have found, relying on a regression discontinuity design, that a community development program in the Philippines induced an increase in conflict in treated villages (due to insurgent groups deliberately sabotaging the program). Crost et al. (2016) study a randomized conditional cash transfer program (CCT) in the Philippines, where, in exchange for cash, households fulfilled a set of conditions related to child vaccination and school attendance, finding that this CCT led to a reduction in conflict incidents.[3] As Crost et al. (2016) highlight, policy interventions may have differential effects on conflict depending on their design and whether they are government led or not. We study unconditional cash transfers, which are increasingly common in low-income and fragile settings and may affect a broader range of household investments than transfers conditioned on specific human capital investments (Beegle et al. 2018). One specificity of our setting (and the Sahel zone in general) is the large presence of international armed groups, which reduces the relative scope of the aforementioned opportunity-cost channel. Put differently, in the context of "home-grown" conflict, unconditional cash transfers may have a more favorable impact.

Besides cash transfers, other policy interventions have been analyzed in the literature. In particular, Berman et al. (2011) find that better service provision can attenuate the risk of insurgency, especially when public security is guaranteed (Berman et al. 2013). Related, Sexton (2016) finds that counterinsurgency aid only reaches its goals if disbursed in areas already under government control.[4]

In a nutshell, the findings of the above literature are still to a large extent ambiguous and sometimes contradictory, and further work on these questions is needed. In particular, our paper has three dimensions of novelty and added value: (i) it is the first randomized experiment studying the impact of a government-led, unconditional cash transfer program on actual armed fighting incidents;[5] (ii) our study is a large-scale intervention randomized among nearly 4,000 eligible villages; and (iii) we study violent conflict events in the Sahel zone, which is a key area in the fight against international terrorism and notoriously under-studied in economics (Porteous 2022).

The remainder of the paper is organized as follows: Section 2 presents the data and methodology, Section 3 presents the results, and Section 4 provides a discussion and conclusion.

2 Data and Methodology

2.1 Intervention and Data Sources

2.1.1 The Niger government-led unconditional cash transfer program

Landlocked in the Sahel at the edge of the Sahara Desert, Niger faces frequent climatic shocks and droughts. It is one of the poorest countries in the world, ranking last in the Human Development Index (UNDP 2020). As part of its efforts to tackle poverty, the Government of Niger has set up a national cash transfer program providing monthly payments of CFAF

[3] This contrasts with the difference-in-difference analysis by Weintraub (2016) of the Familias en Acción CCT in Colombia, for which he finds a surge in killings.

[4] Papers on employment interventions are also related, such as Blattman and Annan (2016), which finds, in a randomized experiment in Liberia, that employment programs can reduce self-reported interest in illegal activities. Similarly, Fetzer (2020) finds that the Indian National Rural Employment Guarantee Act has uncoupled productivity shocks from conflict, leading to persistently lower conflict levels. Lyall et al. (2020) carry out a randomized control trial of two interventions—vocational training and cash transfers—on (self-reported) combatant support in Afghanistan, finding that, conditional on training, the cash transfers increased support for the Afghan government. Finally, Bertrand et al. (2021) do not find impacts of a public works intervention in Côte d'Ivoire on criminal behavior.

[5] Existing RCTs focus on different types of interventions and either draw on a much smaller sample than ours (the number of villages covered is 30 times larger in our study than in Crost et al. 2016) or focus on self-reported attitudes towards violence or illegal activities as the dependent variable (Blattman and Annan 2016; Lyall et al. 2020).

10,000 for two years (US$15.95, $38.95 PPP). Yearly benefits correspond to about 11 percent of household consumption for the targeted poor, rural households.[6] The program is financed by the World Bank has had high visibility as it is managed by a safety nets unit in the office of the prime minister. It reached approximately 100,000 beneficiary households between 2012 and 2019 and has continued to expand. The cash transfers are unconditional but delivered together with parenting and child development promotion activities, as well as with complementary interventions to diversify livelihoods. The program did not have a specific objective to foster peace,[7] but questions on the linkages between cash transfers and conflict have become salient over time with the deterioration of the security situation in the Sahel.

The cash transfer program reached all eight regions of the country, and the selection of villages was randomized through public lotteries before using poverty targeting to select beneficiary households. In particular, geographical targeting was first applied within each region to select "communes" (middle-sized geographical units containing several villages) with the highest poverty rates. Within each selected commune, public lotteries to select beneficiary villages took place in the presence of village chiefs and commune and regional authorities.[8] Next, to determine beneficiary households within the selected villages, poverty-targeting methods were applied.[9] Finally, within the selected households, the recipient of the cash transfers was an adult woman, typically the (first) wife of the household head.

Our main explanatory variable is the assignment of a given village to the cash transfer treatment through the public lotteries within communes. To construct this variable, we rely on the national locality registry and an administrative database from the national cash transfer program. Specifically, we obtained a geo-localized list of the 1,190 villages that benefited from cash transfers at any point between 2012 and 2019.[10] We then merged this administrative data set with the national locality registry to identify which locality was assigned to the cash transfer program within each commune. The remaining localities became the pool of control localities.[11]

Our village data set characterizes which villages were assigned or not assigned to the cash transfer program, but it does not contain much information on covariates. It includes population numbers at the village level (estimated from a 2012 population census), as well as population numbers for the administrative units to which the village is mapped (commune, department, and region). We document further below the balance in locality populations and the absence of pre-trend in conflict incidence between treatment and control localities.[12]

[6]For a more detailed account of the "Filets Sociaux" cash transfer program, see Premand and Barry (2022); Bossuroy et al. (2022).

[7]For the same reason, this dimension was not the focus of the original RCT designed to analyze the program.

[8]Lotteries were initially introduced for a randomized control trial of the first phase of the program (Premand and Barry 2022). Program implementers found the public lotteries helpful to select beneficiary villages in a context of overwhelming need but limited data with which to rank villages by poverty level. In particular, the transparency of the public lotteries shielded them from interference and complaints, and they continued to use them for purely operational reasons after the initial trial. As such, the paper is not based on a specific trial, but instead uses secondary, observational data as part of a larger natural experiment.

[9]Premand and Schnitzer (2021) describe the household targeting methods in detail and analyze their performance.

[10]We worked with the national cash transfer project office to collect geo-coordinates for a subset of beneficiary villages that were not already geo-localized.

[11]Note that the agency that implements the cash transfer program is used to perform public lotteries, which have been ascertained to achieve a good balance in the smaller-scale trials in which household survey micro-data were collected (Bossuroy et al. 2022; Premand and Barry 2022). We rely on the locality registry as we could not obtain the list of localities that entered the public lotteries in each commune outside the samples from those smaller-scale RCTs.

[12]Merging the administrative data set and the locality registry presented two slight complications. First, the cash transfer program considered villages as the targeting unit, but this is not a formal administrative unit. Smaller "hamlets" may, at times, be considered part of a village and, at other times, a stand-alone community. Second, small villages were not considered eligible for the cash transfer program or were, at times, grouped with neighboring villages to facilitate implementation. Eligible villages were listed commune by commune, without strict rules that we could apply across the full locality database. When we use population data from the 2012 national census, we find that the pool of all the control localities in the locality registry is slightly smaller than the pool of treatment localities. This is consistent with hamlets or small villages being filtered out during the cash transfer program's targeting process. In communes where we find that non-treatment localities are smaller on average, we trim the smallest localities until (i) we get balance in population size within the commune or (ii) the smallest remaining locality has no more than 250 individuals (approximately 25

2.1.2 Conflict-event data

We draw on geo-referenced conflict-event data taken from the GDELT data set (Leetaru and Schrodt 2013). This established data source has been widely used in recent research (see, e.g., Manacorda and Tesei 2020; Armand et al. 2020; Gallea and Rohner 2021; Naidu et al. 2021) and draws on media and NGO reports from a wide variety of international and domestic sources, covering print, broadcast, and web outlets in over 100 languages.

One advantage of this data source for our purpose is that it is probably the most comprehensive database on conflict-related events available to date, including over a quarter billion geo-referenced records worldwide for the last three decades. The events are geo-coded with a high resolution. It is particularly critical for us that the events are not aggregated at a higher administrative level, as our identification strategy relies on within-commune variation in the incidence of events between villages assigned or not assigned to the cash transfer program. Because of this comprehensiveness and a high degree of resolution, the data set includes a sizeable number of not only large but also small conflict events, allowing us to have enough statistical variation.[13]

The GDELT data contain information about the geo-location of a given conflict event (longitude/latitude) as well as the actors involved and the type of event (based on the CAMEO classification of action categories). In some cases, there is also a link to the original news source.

We first build a village-year variable that captures whether a village is the nearest village to a recorded conflict event, using high-intensity events (based on CAMEO categories 18–20 covering assault, fighting, and mass violence events). We build an alternative variable that measures if a village is within a 10 km radius of such a conflict event. Making use of the event typology, actors involved, and original news source, we are also able to code a battery of different conflict variables, such as whether the event involved foreign or terrorist groups (i.e., events in which one of the actors was an insurgent, militant, or extremist group, such as Boko Haram, Al-Qaeda, or the Islamic State).[14] Disposing of such a range of different measures helps us narrow down the mechanisms at work.[15]

2.2 Descriptive Statistics

Figure 1 maps the program communes and indicates the year when villages in each commune were registered to start receiving cash transfers (top panel), as well as the location of conflict events and treated and control villages (bottom panel). To complete the full picture, Table A1 provides a further overview of the structure of our data set. The study considers 3,891 villages, of which 1,190 participated in the cash transfer program at some point in time. Recall that the program lasted two

households). This is consistent with the exact program eligibility rules varying by commune. Six percent of localities are trimmed using this procedure. We show in the appendix that results are robust when we include all small localities in the sample or instead trim all localities with fewer than 250 individuals (approximately 25 households). The appendix also shows that the results are robust when we identify treatment villages in the locality registry based on names only, rather than based on their geo-coordinates.

[13] In studies covering whole continents or very populous countries facing major civil wars, such as the Democratic Republic of Congo during the Second Congo War, other datasets with more restrictive inclusion criteria can be suitable, yet for our context in Niger—a country of roughly 24 million people—it is crucial to have the most comprehensive data set possible in order to exploit enough statistical variation to study within-commune variation in the incidence of conflict events.

[14] While GDELT contains "off-the-shelf" information on the country affiliation of groups, the coding of whether a given group has a terrorist background was done by hand, based on background readings on particular armed groups. See the discussion in the next section.

[15] For illustration, we list below some examples of press articles on conflict events contained in the GDELT data: "Witness: Gunmen attack and enter central prison in Niger's capital," Fox News, 1 June 2013; "Jihad in Niger: Bad omens," *The Economist*, 6 July 2013; "Thousands flee Boko Haram attack on Niger town," UNHCR, 6 June 2016; "Food aid to double in Niger after fresh Boko Haram attacks," *Daily Mail*, 14 June 2016; "5 soldiers, 30 militants die in Nigerien Army clash with Boko Haram," The News Nigeria, 14 September 2016; "3 US military service members killed in Niger," ABC News, 5 October 2017.

years for any given village. The first communes (and their 108 randomized villages) started participating in 2012, and the last communes (and their 63 randomized villages) exited the program in 2018 (see Table A1, cols. 3–4).[16]

The last two columns of the table display the evolution of conflict events over time. The level of violence is lower in Niger than in full-blown civil war contexts (such as, say, recently in the Syrian Arab Republic or the Republic of Yemen), which is reflected by the fact that less than 1 percent of villages experienced a severe conflict event in a given year. However, the likelihood of conflict events has increased over time, reflecting a worsening of the security situation during the study period across the Sahel region. In Niger, the increase in violence is due to an increase in terrorist activities by Boko Haram and other jihadist groups based in neighboring Nigeria and Mali (ICG 2017).[17] The likelihood of a village being located within 10 km of a conflict event ranges from 1 to 10 percent during the period the program was implemented, subsequently peaking at 14 percent in 2019.

2.3 Identification Strategy

We start by presenting results from a two-way fixed-effects (TWFE) specification

$$Y_i = bT_{it} + S + w_t + u_{it} \tag{1}$$

where Y is the conflict outcome for village i, and T is an indicator for treatment (taking a value of 1 during the two years a village is assigned to the cash transfer program and 0 otherwise).[18] The specification controls for commune S and year fixed effects w_t, with standard errors clustered at the commune level. This specification relies on variation in the incidence of conflict and exposure to treatment induced by the randomization within each commune, but may also exploit some variation in exposure to treatment across communes and over time. As such, it may suffer from the fact that the year when a commune entered treatment is not random, which represents a potential source of bias, as highlighted recently by de Chaisemartin and D'Haultfœuille (2020) and Goodman-Bacon (2021).

Our preferred (RCT) specification eliminates potential sources of bias in TWFE by relying fully on within-commune variation in exposure to treatment and conflict incidence. Specifically, it estimates the effects of cash transfers on conflict via an ordinary least squares (OLS) regression

$$Y_i = bT_{it} + S * w_t + u_{it} \tag{2}$$

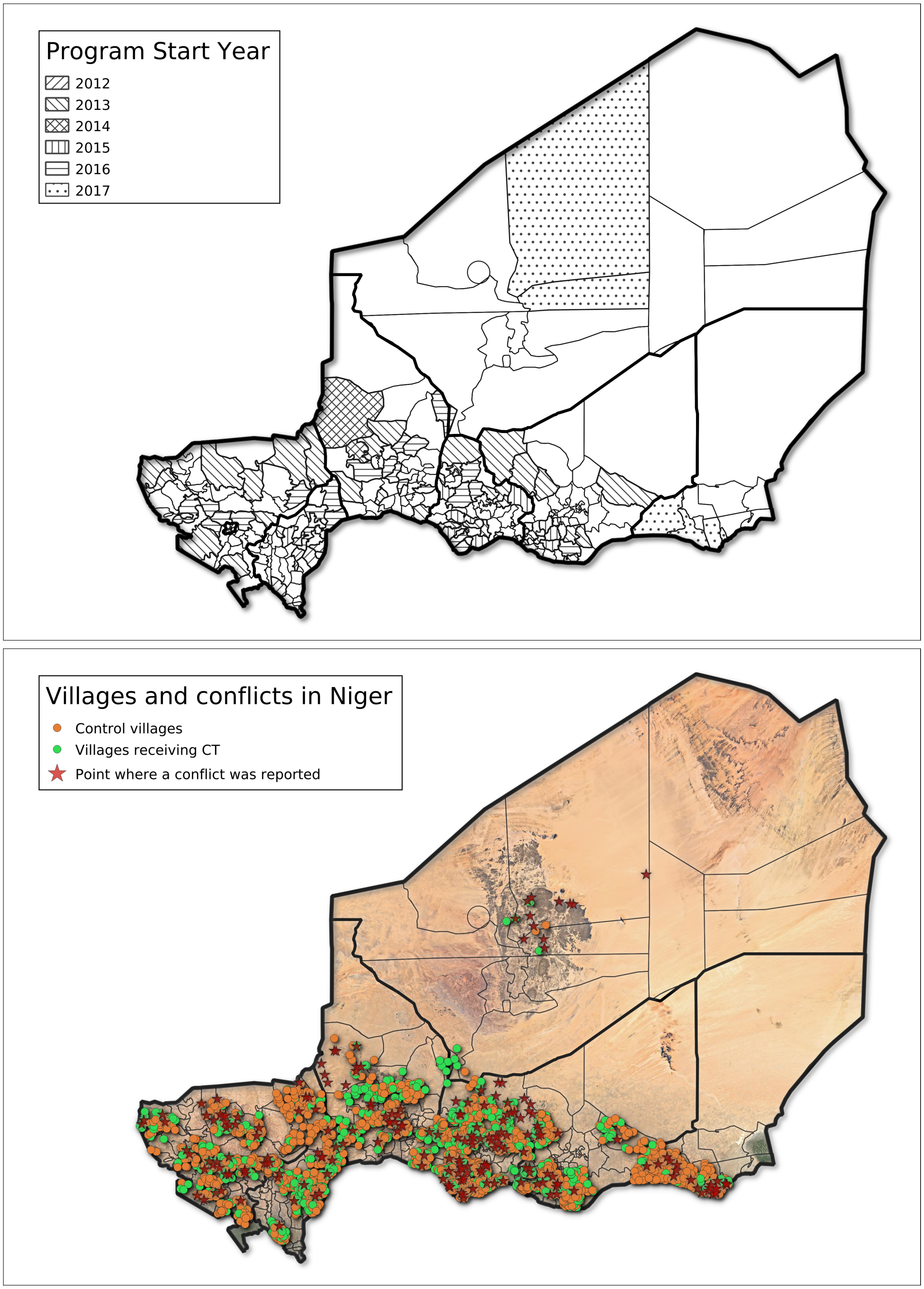
where Y is the conflict outcome for village i, T is an indicator for treatment (taking a value of 1 during the two years a village is assigned to the cash transfer program and 0 otherwise), and $S * w_t$ captures commune times year fixed effects. By controlling for commune times year fixed effects, we restrict our comparison to villages belonging to the same commune

[16] A second phase of the program started after 2019 with slightly different targeting mechanisms, so we focus on the first phase of the program for which sufficient data are already available.

[17] Boko Haram (meaning "Western education is forbidden") is a terrorist group that seeks to install its version of Islamic law. It is based in Nigeria, but makes regular incursions into Niger, Chad or Cameroon. It regularly attacks symbols of government and Western interests, including schools or government buildings. The Islamic State is active at the border with Mali and Niger. As Boko Haram, the Islamic State (Group) includes several factions whose importance has varied over time. It has grown in importance in recent years, for example with one faction of Boko Haram joining in 2015. Various Islamic subgroups (Al-Mourabitoun (AMB), Al-Qaeda in Islamic Maghreb (AQIM), Ansar al-Dine (AAD), Movement of Unity and Jihad in West Africa (MUJAO), Al-Shabaab, and, more recently, the Islamic State of Iraq and Syria (ISIS)) operate in both Mali and Nigeria, may at times affiliate with the broader groups, and also make incursions into neighboring Niger.

[18] Three communes entered two phases of the program at two different points in time. We code treatment villages from the first wave as missing for the subsequent wave as they would not constitute proper control villages.

Figure 1: Location of the Niger Cash Transfer Program and Conflict Events



Source: GDELT data set, Niger cash transfer program administrative data, and national locality registry.

in the same year, where some have been randomly assigned to treatment and others not. We thus call equation (2) our RCT specification, where identification fully stems from within-commune variation in exposure to treatment and incidence of conflict events. Robust standard errors are clustered at the commune level.

Table 1 shows that the randomization achieved balance. There is no statistically significant difference in population size between villages that were treated at any point in time and control villages (col. 1). We also perform a placebo test to see if there is any spurious pre-trend by checking whether there is a higher likelihood of conflict in (future) treatment villages one to four years before the start of the program. Results show that there is a common pre-trend before treatment starts (cols. 2-5), using exposure to conflict defined based either on the nearest neighbor village or a 10 km radius. As the treatment is randomized within communes, our estimates from equation (2) capture the causal impact of the unconditional cash transfer program treatment on conflict events.

Table 1: Balance and Placebo Test of Cash Transfer Effects on Conflict Before Treatment (Pre-Trend)

	(1) Village Population	(2) TWFE Nearest neighbor	(3) RCT Nearest neighbor	(4) TWFE 10 km radius	(5) RCT 10 km radius
Treated (Any time)	-0.644				
	(11.94)				
Pre-trend (1–4 yrs. before treatm.)		0.00230	0.00196	0.00614	-0.000134
		(0.00146)	(0.00147)	(0.00951)	(0.00680)
Commune and year FE	No	Yes	No	Yes	No
Commune x year FE	No	No	Yes	No	Yes
Observations	3,891	49,547	49,547	49,547	49,547
R^2	0.207	0.021	0.049	0.100	0.273

Note: Standard errors are in parentheses, clustered at the commune level. The variable in column (1) is village population size according to the 2012 census. The outcome in columns (2)–(5) is a dummy that equals 1 if the village is the closest to a severe conflict event or within 10 km of a severe conflict event. Results in (2) and (4) follow the specification in equation (1), while columns (3) and (5) follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

3 Results

3.1 Main Results

Table 2 displays our main findings. As mentioned above, the treatment variable takes a value of 1 during the two years when randomized villages received the cash transfer program and 0 otherwise. As far as the dependent variable is concerned, in col. (1) we start with a definition of conflict capturing whether a given village is the closest to a conflict event meeting some minimum severity threshold (i.e., levels 18–21 in CAMEO). While in col. (1) we have the less demanding two-way fixed effects structure (as in equation 1), col. (2) is our preferred RCT specification, including commune times year fixed effects (as in equation 2), which is identified based on the within-commune variation in cash transfer treatment assignment. Cols. (3)–(4) use the same specification as cols. (1)–(2) but apply a broader definition of conflict (taking a value of 1 if a conflict event occurred in a 10 km radius from a village). In all columns, we find that the cash transfer program induces a statistically significant increase in the likelihood of conflict events. Quantitatively, the program increases the likelihood of conflict occurring closest to the village by 0.63 percentage point (col. 2) and the likelihood of a conflict occurring within 10 km of the village by 1.68 percentage points (col. 4).[19] While the incidence of severe conflict events remains rare in the sample (see Table A1, cols. (5)-(6)), the likelihood of an event occurring in the immediate vicinity of the village approximately doubles. The likelihood of an event occurring within 10 km of the village increases by approximately 25 percent.

Table 2: Effect of Cash Transfers on Severe Conflict Events

	(1) TWFE Nearest neighbor	(2) RCT Nearest neighbor	(3) TWFE 10 km radius	(4) RCT 10 km radius
Treated (Last 2 years)	0.00511**	0.00631**	0.0385***	0.0168*
	(0.00214)	(0.00255)	(0.0143)	(0.00883)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	61,198	61,198	61,198	61,198
R^2	0.019	0.045	0.108	0.278

Note: Standard errors are in parentheses, clustered at the commune level. The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village or within a 10 km radius of the village. Results in columns (1) and (3) are based on the specification in equation (1), while columns (2) and (4) are based on equation (2). $^{*}\ p < .1$; $^{**}\ p < .05$; $^{***}\ p < .01$

3.2 Mechanisms

Table 3 is dedicated to better understanding what types of conflict events drive the main results and what underlying mechanisms may be at work. Cols. (1)–(4) show that the cash transfer program results in a statistically significant surge in conflict events involving terrorism, while conflicts unrelated to terrorism are not affected. Conflict events involving terrorism increase by 0.41 percentage point (significant at the 5 percent level in the RCT specification), while the coefficient for conflict events that do not involve terrorism equals 0.22 and is not statistically significant. Cols. (5)–(8) further distinguish between terrorist conflict events involving foreign actors and terrorist conflict events only involving domestic actors. Importantly, the cash transfer program only fuels terrorism perpetrated by foreign actors (which increases by 0.34 percentage point and is significant at the 5 percent level), with no impact on domestic terrorism (with a point estimate of 0.07 that is not statistically significant).

In a nutshell, these findings are in line with the notion that the surge in violence in areas treated by the program is substantially driven by the activities of international terrorist groups such as Boko Haram. To the extent that multiple media reports have shown that jihadist groups have deliberately targeted aid workers and government services such as schools or

[19]The appendix contains a series of robustness checks. It shows that the results are robust with respect to alternative definitions of conflict in the GDELT data set (Table A2), alternative coding of small villages (Tables A3–A4), and alternative geographical matching (Table A5). In Tables A6–A7, we also reproduce our results using an alternative data source, the Armed Conflict Location Event Data Project (ACLED) data set (Raleigh et al. 2010). We include severe conflict events involving battles, explosions/remote violence, protests, riots and violence against civilians (ACLED 2019). ACLED is less fine grained and contains fewer events than GDELT. For instance, 6 percent of communities are within 10km from a conflict event reported by GDELT, but 2.7 percent of communities are within 10km from a conflict event reported by ACLED. While in Table A6—which draws exclusively on ACLED—the coefficients of interest maintain the expected sign but lose statistical significance, in the following Table A7—which pools ACLED with GDELT data—results remain statistically significant.

health centers, one plausible mechanism at work is the sabotage of public policies designed to improve people's lives.[20] In addition, the capturing of assets or cash may also be part of the motivation for attacks, but it is difficult to detect or directly measure in the data, and we do not have qualitative evidence of such occurrences.

Table 3: Effect of Cash Transfers on Different Types of Severe Conflict Events

	(1) TWFE Severe conflict involving terrorism	(2) RCT Severe conflict involving terrorism	(3) TWFE Severe conflict not involving terrorism	(4) RCT Severe conflict not involving terrorism
Treated (Last 2 years)	0.00347**	0.00414**	0.00165	0.00218
	(0.00163)	(0.00175)	(0.00183)	(0.00185)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	61,198	61,198	61,198	61,198
R^2	0.015	0.049	0.011	0.041
	(5) TWFE Severe conflict involving terrorism and foreign actors	(6) RCT Severe conflict involving terrorism and foreign actors	(7) TWFE Severe conflict involving terrorism and domestic actors	(8) RCT Severe conflict involving terrorism and domestic actors
Treated (Last 2 years)	0.00283*	0.00344**	0.000637	0.000701
	(0.00144)	(0.00160)	(0.000603)	(0.000622)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	61,198	61,198	61,198	61,198
R^2	0.015	0.052	0.004	0.026

Note: Standard errors are in parentheses, clustered at the commune level. The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village. Results in uneven columns follow the specification in equation (1), while even columns follow equation (2). The dependent variable in columns (5) and (6) gets a value of 1 when a village has experienced both a terrorist attack and a conflict involving a foreign actor, while in columns (7) and (8) it gets a value of 1 when a village has experienced both a terrorist attack and a conflict involving a domestic actor; however, the same event may not meet both conditions. * $p < .1$; ** $p < .05$; *** $p < .01$

To further substantiate the role played by international terrorist actors, note that Niger only became a major theater of operations for Boko Haram and other jihadist groups from 2015 onward (ICG 2017). Hence, we expect the detected impact of the cash transfer program to be concentrated in the post-2015 period. Table A8 provides results separately for 2012–2014 and 2015–2018. While this reduces statistical power (especially for the nearest neighbor events, which are rarer), Table A8 clearly shows that the results are driven by the 2015–2018 years, when the incidence of terrorist conflict events perpetrated by Boko Haram and other jihadist groups was higher.

We can further document how the effects on conflict evolve depending on the cash transfer program cycle in participating villages. We have already shown in Table 1 that there are no significant effects for the one to four years before the program. Figure 2 depicts separate estimates for the two years when the program operated in treatment villages, and for the following two years. The impact materializes in the second year of the program but then vanishes once no more cash is disbursed and the program activities have been completed (i.e., in the third and fourth years after the start of the two-year program).

The absence of a persistent violence-inducing effect may be due to the presence of countervailing forces. During the program, international terrorist actors may have incentives to undermine an expansion of government presence or capture rents, but after the end of the program, the incentives to sabotage the program may recede, or the effect of a sustained increase in productivity and opportunity cost for local actors to engage in violence may become relatively more important,

[20]Examples of media reports include "Niger moves nearly 100 schools 'out of harm's way,'" news24, 19 January 2016; "Extremist attack on health post in Niger kills 6," Associated Press, 25 May 2016; "Niger says US aid worker likely kidnapped by Mali jihadists," France 24, 16 October 2016; "Boko Haram attacks hinder aid delivery in southeastern Niger," Reuters, 1 December 2016; "Gunmen kidnap German humanitarian worker in western Niger," Associated Press, 12 April 2018; "Six French aid workers and two local guides shot dead in Niger giraffe park," *Washington Post*, 10 August 2020.

counteracting the first two effects.

3.3 Spillovers

As part of our analysis of mechanisms, we also consider if there might be local spillovers, whereby cash transfers targeting a village may also affect the likelihood of conflict in neighboring villages. Those spillovers could be negative; for instance, if jihadist groups shift their attacks toward treated villages and away from nearby control villages, thereby reducing conflict events in the latter. They could also be positive if jihadist groups increase the likelihood of attacks in the general vicinity of program villages, thereby increasing the risk of conflict in nearby control villages.[21]

We have shown that our results are robust to two alternative definitions of conflict events, including whether a village is the nearest neighbor to a conflict event or is located in a radius around a conflict event. The use of the radius variable may hide the presence of spillovers. Importantly, however, we can use the nearest neighbor conflict variable to test for the existence of local spillovers. Specifically, we build a measure of the distance between control villages and the nearest treated village. Using the median of that variable (approximately 5 km), we define a group of "near" control villages and a group of "far" control villages.

Similar to the approach used by Miguel and Kremer (2004) and Crost et al. (2016) to analyze spillovers, we estimate treatment effects separately by comparing the likelihood of conflict (using the nearest neighbor variable) in treated villages relative to "near" control villages and "far" control villages. Table A9 shows that point estimates are positive and statistically significant using either the near or far control villages for both the TWFE (cols. 1–2) and RCT (cols. 3–4) specifications. The point estimates are also of similar magnitude when we use either the near or far control villages, suggesting no substantial local spillovers. To formally assess the presence of local spillovers, we test whether the likelihood of conflict events differs between near and far control villages. We do not find evidence of local spillovers based on this test (cols. 5–6). The results thus indicate that the cash transfer program does not shift the location of conflict events between villages at the local level or increase the likelihood of conflicts in neighboring villages.

3.4 Additional Results from Household-Level Data

The results disaggregated by type of event in Table 3 highlight that the increase in conflict was mostly driven by foreign terrorist groups. To better understand potential within-village effects, we complement the analysis of the village-level data with secondary household data from a smaller-scale RCT conducted during the first phase of the program (2012–2013).[22] We analyze households' self-reported information on the incidence of various types of conflict involving community members, based on a follow-up survey collected two years after the start of the program among 3,500 households, of which about two-thirds lived in communities randomly assigned to the cash transfer and one-third in control villages.[23] Table A10 shows that we do not find evidence of a significant decrease or increase in any self-reported conflict, including within households, within the community, or between the community and other nearby communities.[24]

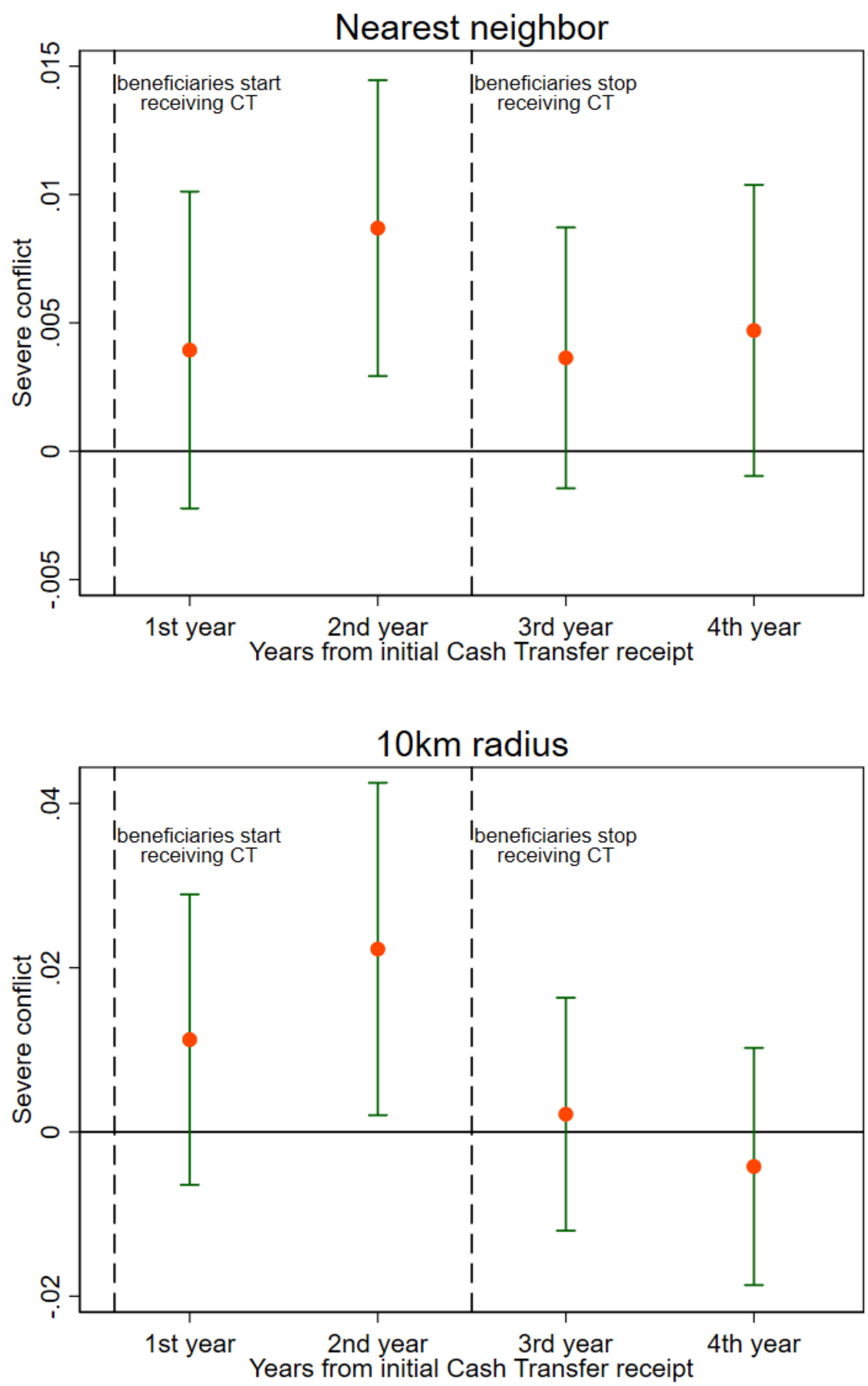
[21]For a discussion of the spatial decay of violence, see Mueller et al. (2022).

[22]This analysis should be considered exploratory as these were not the main outcomes of this smaller-scale RCT, which is discussed in Premand and Barry (2022).

[23]See Premand and Barry (2022) for details on the RCT design, sample, and baseline balance. While the experiment is generally well balanced across a large set of indicators, the variables we study here were not collected at baseline.

[24]The results are similar when focusing only on households eligible for the program rather than a sample of households in the community.

Figure 2: Effect of Cash Transfers on Conflict Events during and after the End of the Program



Note: Brackets show the 95 percent confidence interval. Standard errors are clustered at the commune level. Results are based on equation (2).

This absence of a conflict effect in the household data can be accounted for by three factors. First, it may be related to the sample time span (2012–2013), as for this earlier period the surge in international jihadism in the region had only started to materialize, and hence the mechanisms linked to sabotage and rent-seeking were less salient.[25] Second, the findings from the household data (based on domestic respondents only) are consistent with the notion that the likelihood of within-village conflict has barely changed, and the lion's share of the overall conflict surge can be attributed to international jihadist groups.[26] Finally, the smaller sample size of the household data may matter as well. In particular, note that a traditional, "small-scale" RCT based on 150 clusters of villages may not be powered to detect impacts on the incidence of (rare) conflict events. Our main results at the village level rely on a much larger set of observations (nearly 4,000 villages).[27] Another key distinction is that the household data is bound to rely on self-reported conflict information, while the village-level data draws on reports and articles from a broad set of media and NGO sources.

Taken together, the household-level results show no evidence of a decrease or increase in conflict between community members, while the village-level results highlight an increase in events perpetrated by foreign terrorist actors. This suggests that the cash transfer program affected conflict mostly by shifting the incentives of actors outside the village rather than by altering opportunity costs to engage in conflict for local populations.

4 Discussion and Conclusion

This paper has presented novel, large-scale evidence of the impact of a government-led unconditional cash transfer program on the likelihood of conflict in Niger. Causal identification relies on the randomization of the program through public lotteries within communes. The results show that, during treatment, there was a temporary increase in conflict events, which was mainly driven by attacks by foreign terrorist groups. These findings are consistent with the terrorists' attempts to sabotage successful government policies as well as incentives to appropriate rents. The fact that this increase in conflict proved temporary could be consistent with a reduced incidence of sabotage and rent-grabbing after the end of the program, when the government was less present and financial flows had ceased. In addition, medium-run productivity increases may have raised the opportunity costs for locals to engage in conflict.

In terms of magnitude, severe conflict events remain quite rare, and we find that cash transfers increase their likelihood by 0.6 to 1.7 percentage points. Hence the welfare costs of these conflict events would need to be between 59 (1/0.017) to 166 (1/0.006) times larger than the welfare benefits of cash transfers for the two effects to fully cancel out. Cash transfers have indeed been shown to have robust welfare effects in the international literature. In Niger, cash transfers were found to increase consumption by 10 percent on average (Premand and Barry 2022; Premand and Stoeffler 2022), and led to lasting effects on asset accumulation (Stoeffler et al. 2020) and income-generating activities when complemented with productive

[25] In fact, if we restrict the village-level data to the waves for which microdata are available, we do not find an increase in conflict events either (see Table A8, cols. 1–2).

[26] This evidence is consistent with other research on the Niger cash transfer program that shows positive effects of complementary livelihood interventions on social well-being and social cohesion, with no evidence of increased community tension (Bossuroy et al. 2022). Premand and Schnitzer (2021) also analyze the application of household targeting methods and find no effect on the self-reported likelihood of conflict, which—if anything—seems to decrease in the villages over time between the start and the middle of the program.

[27] As such, the paper illustrates that very large samples are required to analyze the impacts of policy interventions on rare outcomes such as conflict events. The sample size of typical RCTs for which micro-data is collected would not be sufficient in our setting. On a related note, the unavailability of micro-data across a very large number of villages also limits our ability to analyze a wide range of intermediary outcomes to tease out mechanisms.

interventions (Bossuroy et al. 2022).

As discussed in the review of the literature, existing studies find contradictory results, yet most of them are not able to draw on a randomized experiment. One exception is the study of Crost et al. (2016), who also employ an RCT. Hence, comparing our findings with theirs is particularly useful. Crost et al. (2016) find, based on a smaller-scale experiment, that a CCT program curbs conflict in the Philippines. The key mechanism they highlight is a reduction of local insurgent influence, with weaker rebel presence. In contrast, we find that an unconditional cash transfer program temporarily increases conflict in Niger, driven by events with involvement of foreign terrorist groups. The scope for cash transfers to reduce the opportunity cost of participating in conflict may be lower in a context when attacks by foreign terrorist groups predominate. As such, in the context of "home-grown" conflict (as in Crost et al. 2016), cash transfers may have a more favorable impact. There are other differences between the two interventions: the program in the Philippines is conditional and makes electronic payments, while the one in Niger is unconditional and makes cash payments. We encourage further experimental analysis of different types of cash transfer programs that include additional conditionalities or aim more specifically at raising labor productivity through economic inclusion components. A priority for future work is to understand under what conditions or with what specific designs might social protection or employment programs reduce conflict incentives across settings. One concrete question is whether digital payments attenuate risks linked to sabotage and rent appropriation.

Further, the interplay of security and development policies deserves attention. In contexts where foreign terrorist groups are absent or where the state is fully able to counter them, negative side effects of successful cash transfer programs may be less likely. The relationship between security and development calls for coordinated actions to address poverty and insecurity (Rohner and Thoenig 2021). Note that, beyond cash transfer programs' short- and medium-run economic benefits, they may also strengthen state presence, and this state capacity may also help secure future policies. As such, government-led cash transfer programs may still contribute to the emergence of stable and inclusive states over the longer term.

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Online Appendix - Additional Tables

In this online appendix, we display several additional tables and figures that are referred to in the main text.

Table A1 displays descriptive summary statistics, while the following tables are devoted to robustness checks. In particular, they focus on alternative conflict definitions (Table A2), the exclusion of all small villages (Table A3), the inclusion of all small villages (Table A4), matching by name only (Table A5), and alternative conflict data (Tables A6–A7).

Further, Table A8 displays separate results per wave/year, while Table A9 investigates spillovers. Finally, Table A10 performs an exploratory analysis at the household level based on secondary micro data from a smaller-scale RCT.

Table A1: Descriptive Statistics of Villages by Year

(1)	(2)	(3)	(4)	(5)	(6)
Year	Total	In Cash Transfer Program	In Cash Transfer Program (Cumulative)	% with severe conflict event	% with severe conflict event within 10km
2006	3891	0	0	0.103	1.079
2007	3891	0	0	0.283	3.058
2008	3891	0	0	0.180	1.928
2009	3891	0	0	0.283	4.729
2010	3891	0	0	0.206	2.519
2011	3891	0	0	0.283	3.290
2012	3891	108	108	0.411	5.885
2013	3891	636	636	0.437	5.114
2014	3891	551	659	0.334	7.376
2015	3891	129	768	0.745	10.383
2016	3891	445	1125	0.617	7.196
2017	3891	402	1190	0.540	6.682
2018	3891	63	1190	0.797	8.944
2019	3891	0	1190	1.079	14.392
2020	3891	0	1190	0.848	12.208
2021	3891	0	1190	0.797	9.920
			Mean :	0.458	6.044

Table A2: Robustness to Alternative Definitions of Conflict Outcomes

	(1) TWFE 5km radius	(2) RCT 5km radius	(3) TWFE 15k radius	(4) RCT 15k radius	(5) TWFE Nearest neighbor	(6) RCT Nearest neighbor	(7) TWFE 10km radius	(8) RCT 10km radius
Treated (Last 2 years)	0.0120**	0.00596*	0.0556***	0.0255***	0.00538**	0.00655**	0.0263*	0.0166*
	(0.00548)	(0.00351)	(0.0196)	(0.00879)	(0.00251)	(0.00249)	(0.0133)	(0.00844)
Commune and year FE	Yes	No	Yes	No	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	66329	66329	66329	66329	66329	66329	66329	66329
R^2	0.040	0.111	0.181	0.400	0.023	0.051	0.115	0.268

Note: Standard errors are in parentheses, clustered at the commune level. The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village or within a 5 km, 10 km, or 15 km radius of the village. Results in uneven columns follow the specification in equation (1), while results in even columns follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

Table A3: Robustness to the Exclusion of All Small Villages in the Sample

	(1) TWFE Nearest neighbor	(2) RCT Nearest neighbor	(3) TWFE 10km radius	(4) RCT 10km radius
Treated (Last 2 years)	0.00483**	0.00630**	0.0385***	0.0156*
	(0.00216)	(0.00265)	(0.0142)	(0.00892)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	57694	57694	57694	57694
R^2	0.019	0.046	0.110	0.279

Note: Standard errors are in parentheses, clustered at the commune level. Estimation is based on a sample removing all villages with fewer than 250 inhabitants (based on 2012 population census data). The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village or within a 10 km radius of the village. Results in columns (1) and (3) follow the specification in equation (1), while results in columns (2) and (4) follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

Table A4: Robustness to the Inclusion of All Small Villages in the Sample

	(1) TWFE Nearest neighbor	(2) RCT Nearest neighbor	(3) TWFE 10km radius	(4) RCT 10km radius
Treated (Last 2 years)	0.00532**	0.00623**	0.0381***	0.0166*
	(0.00213)	(0.00245)	(0.0144)	(0.00861)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	66329	66329	66329	66329
R^2	0.018	0.044	0.103	0.274

Note: Standard errors are in parentheses, clustered at the commune level. Estimation is based on a sample including all villages with fewer than 250 inhabitants (based on 2012 population census data). The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village or within a 10 km radius of the village. Results in columns (1) and (3) follow the specification in equation (1), while results in columns (2) and (4) follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

Table A5: Robustness to Identification of Treatment Villages by Name Instead of Geo-coordinates

	(1) TWFE Nearest neighbor	(2) RCT Nearest neighbor	(3) TWFE 10km radius	(4) RCT 10km radius
Treated (Last 2 years)	0.00617**	0.00742**	0.0408**	0.0133
	(0.00267)	(0.00310)	(0.0159)	(0.00856)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	57968	57968	57968	57968
R^2	0.021	0.048	0.107	0.283

Note: Standard errors are in parentheses, clustered at the commune level. Results are based on the identification of treatment villages based on a nominal match between the cash transfer program database and national locality registry (instead of a merge based on geo-coordinates). The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village or within a 10 km radius of the village. Results in columns (1) and (3) follow the specification in equation (1), while results in columns (2) and (4) follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

Table A6: Effect of Cash Transfers on Severe Conflict Events (ACLED Data)

	(1) TWFE nearest neighbor	(2) RCT nearest neighbor	(3) TWFE 10km radius	(4) RCT 10km radius
Treated (Last 2 years)	0.000966	0.000719	0.00692	0.00174
	(0.000953)	(0.000691)	(0.00586)	(0.00262)
Commune x year FE	No	Yes	No	Yes
Observations	61198	61198	61198	61198
R^2	0.037	0.107	0.181	0.475

Note: Standard errors are in parentheses, clustered at the commune level. Results in columns (1) and (3) follow the specification in equation (1), while results in columns (2) and (4) follow equation (2). The conflict events encompass the following categories: battles, explosions/remote violence, protests, riots and violence against civilians. * $p < .1$; ** $p < .05$; *** $p < .01$

Table A7: Effect of Cash Transfers on Severe Conflict Events (Pooled GDELT and ACLED Data)

	(1) TWFE Nearest neighbor	(2) RCT Nearest neighbor	(3) TWFE 10km radius	(4) RCT 10km radius
Treated (Last 2 years)	0.00489**	0.00521**	0.0433***	0.0170*
	(0.00220)	(0.00249)	(0.0144)	(0.00874)
Commune and year FE	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes
Observations	61198	61198	61198	61198
R^2	0.032	0.080	0.153	0.343

Note: Standard errors are in parentheses, clustered at the commune level. Results in columns (1) and (3) follow the specification in equation (1), while results in columns (2) and (4) follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

Table A8: Effect of Cash Transfers on Severe Conflict Events by Year

	(1) 2012-2014 TWFE Nearest neighbor	(2) 2012-2014 RCT Nearest neighbor	(3) 2015-2018 TWFE Nearest neighbor	(4) 2015-2018 RCT Nearest neighbor	(5) 2012-2014 TWFE 10k radius	(6) 2012-2014 RCT 10k radius	(7) 2015-2018 TWFE 10k radius	(8) 2015-2018 RCT 10k radius
Treated (Last 2 years)	0.00104	0.00249	0.0106***	0.0105**	-0.00420	-0.000899	0.0566***	0.0359**
	(0.00270)	(0.00300)	(0.00387)	(0.00426)	(0.0154)	(0.00790)	(0.0187)	(0.0162)
Commune and year FE	Yes	No	Yes	No	Yes	No	Yes	No
Commune x year FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	27438	27438	33760	33760	27438	27438	33760	33760
R^2	0.015	0.036	0.023	0.054	0.097	0.295	0.123	0.266

Note: Standard errors are in parentheses, clustered at the commune level. The outcome variable is a dummy that equals 1 if there has been a severe conflict event nearest to the village or within a 10 km radius of the village. Results in uneven columns follow the specification in equation (1), while results in even columns follow equation (2). * $p < .1$; ** $p < .05$; *** $p < .01$

Table A9: Local Spillover Analysis Using Near and Far Control Villages

	(1) TWFE Treated vs Near Control	(2) TWFE Treated vs Far Control	(3) RCT Treated vs Near Control	(4) RCT Treated vs Far Control	(5) TWFE Near Control vs Far Control	(6) RCT Near Control vs Far Control
Treated (Last 2 years)	0.00460**	0.00562**	0.00454*	0.00739**		
	(0.00219)	(0.00238)	(0.00255)	(0.00345)		
Near Control					0.000652	0.000633
					(0.000988)	(0.00100)
Commune and year FE	Yes	Yes	No	No	Yes	No
Commune x year FE	No	No	Yes	Yes	No	Yes
Observations	31766	31752	31766	31752	58850	58850
R^2	0.031	0.018	0.071	0.064	0.016	0.043

Note: Estimation uses the nearest neighbor conflict variable as the outcome. Standard errors are in parentheses, clustered at the commune level. We define "near" control villages and "far" control villages based on the median of the distance between each control village and the nearest treated village (0.04°, or approximately 5 km). Columns (1) and (2) are based on the specification in equation (1), using only near and far control villages, respectively. Columns (3) and (4) are based on the specification in equation (2), using only near and far control villages, respectively. Columns (5) and (6) estimate the effect on conflict of being located in a near control village relative to a far control village (dropping treatment villages from the sample), based on specifications in equations (1) and (2), respectively. * $p < .1$; ** $p < .05$; *** $p < .01$

Table A10: Effect of Cash Transfers on Conflict Events Reported within Villages (Based on Secondary Household Data from Micro RCT)

	(1) Inside family	(2) Conflict outside family	(3) Conflict with another community member	(4) Conflict between other community members	(5) Conflict with members of another community	(6) Any conflict (cols (1)-(5))
HH in treated village	0.0268	0.00606	0.0159	0.0274	0.00644	0.00285
	(0.0210)	(0.0212)	(0.0210)	(0.0247)	(0.0233)	(0.0306)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (Hh.)	3455	3455	3455	3455	3455	3455
Control Mean	0.104	0.110	0.114	0.151	0.111	0.233

Note: Standard errors are in parentheses, clustered at the strata level. Analysis of secondary household data from an RCT conducted during the first phase of the program (2012–2013), based on a follow-up survey collected two years after the start of the program among 3,500 households, of which about two-thirds lived in communities randomly assigned to the cash transfer and one-third in control villages. See Premand and Barry (2022) for details on the RCT design, sample, and baseline balance. * $p < .1$; ** $p < .05$; *** $p < .01$