*The Unintended Consequences of Conditional Cash Transfer Programs for Violence: Experimental and Survey Evidence from Mexico and the Americas**

Daniel Zizumbo-Colunga

ABSTRACT

Because conditional cash transfer programs (CCTs) can address the deep roots of violence, many scholars and policymakers have assumed them to be an effective and innocuous tool to take on the issue. I argue that while CCTs may have positive economic effects, they can also trigger social discord, criminal predation, and political conflict and, in doing so, increase violence. To test this claim, I take advantage of the exogenous shock caused by the randomized expansion of Mexico's flagship CCT, PROGRESA/Oportunidades. I find that that the experimental introduction of the program increased rather than decreased violence. Then, I analyze all the data compiled by LAPOP on the issue over the years. I find that, other things constant, Latin Americans are more exposed to violence and insecurity when they participate in CCTs than when they do not. These findings urge us to reconsider the effects of social programs on violence.

Keywords: CCT, social programs, homicides, PROGRESA, Mexico

Conditional Cash Transfer programs (CCTs) have become ubiquitous worldwide, attracting the attention of scholars across subfields. While their capacity to benefit the incumbent politically has triggered much debate over the last decade (De La O 2013; Imai et al. 2020; Díaz-Cayeros et al. 2016; Araújo 2021), there is a growing consensus that CCTs are effective in incentivizing human capital investments, reducing social inequality, and promoting political participation and inclusion (De Micheli 2018; Layton et al. 2017; Schober 2019; Morais de Sá e Silva 2017; Jenson and Nagels 2018; Molina-Millán et al. 2019). This evidence has led a growing body of scholars and policymakers to believe that as they tackle the deep roots of violence, CCTs may have downstream crime- and violence-reducing effects (Machado et al. 2018; Lance 2014; Dubois et al. 2012; Chioda

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et al. 2016; Watson et al. 2020). If confirmed, this consensus would mean that there exists a useful policy tool that can help countries in the region escape the rise in violence that has left them with social, political, and economic scars (Malone 2012; Albarracín and Barnes 2020; Visconti 2020).

While I do not deny that CCTs can help reduce social conflict in some contexts, this article challenges the consensus by highlighting the mechanisms by which these programs can generate negative externalities that might mute and even revert CCTs' violence-reducing effects. Then, to investigate the net effect of CCTs, I investigate the case of Mexico. Mexico is an ideal case of study because it was home to the most influential CCT ever implemented in the developing world—the PROGRESA/Oportunidades/PROSPERA program (henceforth PROGRESA)—and recently experienced a rapid rise in violence. The study takes advantage of an exogenous shock created by a randomized controlled trial implemented during the 1999 expansion of the program, in which 506 marginalized rural localities received 2 additional years of PROGRESA (Parker and Todd 2017). I find no evidence of the long-term violence-reducing impact of the program. If anything, I find that the villages that adopted the program earlier registered significantly more homicides and violence-related hospitalizations than villages that adopted the program later.

Next, to examine the generalizability of the findings across the continent, I analyze all the individual-level data collected by the AmericasBarometer on CCT participation and insecurity throughout the Americas from 2010 to 2019. The results are consistent. Accounting for other individual and contextual-level confounders, CCT beneficiaries are systematically more likely to report crime and insecurity and, in the countries with the highest murder rates, they are more likely to be afraid of being murdered.

The findings of this study contribute to our understanding of social programs by highlighting the difficulty of reducing violence and producing stability solely through antipoverty interventions. They suggest that far from generating political stability through specific and diffuse political support (De La O 2013; Layton et al. 2017), social programs can destabilize communities and, in doing so, increase the prevalence of crime and violence (Weintraub 2016; Blattman et al. 2018; Borraz and Munyo 2020). Of course, additional research would be necessary to identify the specific individual, social, and political mechanisms behind this study's findings.

Violence-reducing Effects of Cash Transfer Programs

Conditional Cash Transfer Programs involve making direct cash transfers to families to help them fulfill their most urgent needs and incentivize them to make human capital investments. While some scholars note that CCTs can reproduce norms and behavioral patterns that perpetuate gender, social, and economic inequality (Molyneux 2006; Molyneux et al. 2016; Molyneux and Thomson 2011), research on economic development has found strong evidence that these programs are effective in reducing school dropout rates, food insecurity, and economic deprivation in the short run (Morais de Sá e Silva 2017; Palmeira et al. 2020; Parker and Todd 2017) and economic inequality and intergenerational poverty in the long run (Lagarde et al. 2009; Rudgard 2019; Molina-Millán et al. 2019). While research on the impact of CCTs has focused most frequently on their stated objectives, a new wave of academics and policymakers have argued for the use of cash transfer programs as a socially responsible instrument to address the growing insecurity crises in many countries on the continent.

Some researchers argue that because they address the deep-rooted causes of antisocial behavior (Crutchfield and Wadsworth 2003; Enamorado et al. 2016; Fajnzylber et al. 2002; Webster and Kingston 2014), in the long run, CCTs can set the stage for peaceful, resilient, and law-abiding communities that can resist internal and external criminal pressures (e.g., United Nations 2015; IACHR 2015). Others, however, do not believe that we need to wait years to reap the violence-reducing benefits associated with cash transfer programs (Chioda et al. 2016; Camacho and Mejía 2013; Watson et al. 2020). If individuals engage in crime for economic reasons, by subsidizing households' income, CCTs reduce the utility of delinquency and consequently have a negative effect on various forms of crime in the short run. Furthermore, by design, CCTs typically reward families or individuals for enlisting their children in school. Some authors argue that, since children attending school have no time to engage in delinquency, as soon as the academic conditionalities of social programs come into effect, we should observe a reduction in various dimensions of crime, what some call the incapacitation effect of education (Jacob and Lefgren 2003; Luallen 2006).

Several studies have found correlational evidence consistent with the capacity of CCTs to reduce crime and violence. In Brazil, states with a faster adoption rate of the CCT Bolsa Familia have been found to register lower levels of crime (Loureiro 2012). In Mexico and Colombia, municipalities incorporated into PROGRESA and Familias en Acción tend to register lower levels of homicides, injuries, and property crime (Lance 2014; Machado et al. 2018; Rios Salgado and Llano Jaramillo 2021). And in cities like Bogotá and São Paulo, the expansion of CCTs has been found to coincide with lower levels of property crime (Chioda et al. 2016; Camacho and Mejía 2013; Watson et al. 2020).

While these findings are suggestive, they have not been based on experimental methods. They have been based either on cross-provincial and cross-temporal comparisons with limited statistical power or on the statistical analysis of municipal-level data. Moreover, although previous findings can be reconciled, they are frequently contradictory. The same studies that find a negative association between social programs and property crime show a null (Chioda et al. 2016; Camacho and Mejía 2013; Rios Salgado and Llano Jaramillo 2021; Watson et al. 2020) or positive link between CCTs and armed violence (Weintraub 2016; Zürcher 2017). Therefore, it seems critical to theorize about how CCTs can generate negative externalities that may attenuate and even revert their violence-reducing impacts.

Violence-inducing Effects of Cash Transfer Programs

Just as classic economic, social, and political theory can substantiate an optimistic view of CCTs, there are also theoretical mechanisms that back a link between antipoverty programs and an increase in crime and violence. It is critical to consider all aspects of the phenomenon to evaluate these programs fairly.

By incentivizing school enrollment, for example, CCTs can not only incapacitate the youth but also trigger a concentration effect. That is, they can draw students, gangs, and families into the same physical space and, in doing so, increase the likelihood that problems between conflicting groups will be expressed violently. In the United States, Jacob and Lefgren (2003) and Luallen (2006) investigated the impact of exogenous shocks in attendance (teacher in-service days and strikes) on the prevalence of misdemeanors. They consistently found that while property crime tends to decline when schools are not in session, violent crime increases by between 30 percent and 45 percent when schools are in session.

In the medium and long run, concentrating children in schools with insufficient resources to protect youngsters can facilitate rather than hinder criminal recruitment. In Colombia, thousands of children were recruited from poor, marginalized schools throughout the civil war (Kirk 1994); in El Salvador, gangs intimidate teachers and principals routinely to recruit students (Martínez-Reyes and Navarro-Pérez 2018; López Ramírez 2015). Meanwhile, in Mexico, drug cartels have been found to infiltrate schools, attempting to enlist children as informants, traffickers, and even hitmen (Barrena et al. 2019; Salomón 2019; Geremia and Pérez García 2011).

Furthermore, even as cash inflows can have positive economic effects, they can also expose beneficiaries to different forms of violence. Most directly, raising citizens' disposable wealth through cash transfer programs can turn citizens into better loot (Bueno de Mesquita 2020; Borraz and Munyo 2020). Borraz and Munyo (2020), for instance, evaluated the impact of the 2008 reformulation of Uruguay's Plan de Equidad on property crime and, contrary to previous studies, found that the increase in the payment to and the number of beneficiaries in the program raised property crime by 1.1 percent.

Cash inflows can also disrupt familial, communal, and political power equilibria (Adato et al. 2000; Hays 1982; Villarreal 2002; Díaz-Cayeros et al. 2016). When CCTs do not explicitly incorporate women-empowering components (Molyneux 2006; Molyneux and Thomson 2011) or when they are implemented in contexts of low state capacity, programs that rely on women as their point of contact can generate gender stigmatization and put women in a position in which they can be the target of extractive intrafamily violence (Díaz-Cayeros et al. 2016; Adato et al. 2000).

By increasing citizens' economic autonomy and by introducing authorities that are foreign to the community (CCT program managers and operators), in the medium and long term, CCTs can threaten the political and economic dominance of local traditional authorities (Blattman et al. 2018; Díaz-Cayeros et al. 2016). In doing so, they can trigger a reaction that might lead to violence against program operators and citizens willing to vote their conscience (Sesia 2001; Blattman et al. 2018). Moreover, since CCTs often target historically marginalized groups, they can trigger some degree of resentment among intolerant majorities and among those who perceive welfare to be unjust (Ellis 2012; MacAuslan and Riemenschneider 2011). To the extent that resentment propels radicalized majorities to exert violence against minorities as a form of social control (De la Roche 1996; King and Wheelock 2007), CCTs can leave beneficiaries exposed to retributive crime and violence.

In addition to their individual-level effects, cash transfer programs can increase the overall value of localities and, in doing so, trigger political, criminal, and armed territorial competition (Cameron and Shah 2013; Casas 2018; Díaz-Cayeros et al. 2016; Zürcher 2017). Cameron and Shah (2013), for example, examined the impact of the distribution of cash fuel subsidies in Indonesia and found the deployment of the program to be associated with increases in political and ordinary violence, revealing efforts by clientelist and criminal actors to capture the benefits of the program. Similarly, studies about civil war have examined the issue and found that the introduction of foreign and local aid exacerbates conflict between armed actors (Zürcher 2017). Wood and Molfino (2016) found that in sub-Saharan Africa the introduction of humanitarian aid led to an increase in the number of confrontations between rebel and government forces. For the Philippines, Crost et al. (2014) found that the distribution of poverty relief amid civil war increased civil casualties. Meanwhile, Weintraub (2016) examined the effect of Colombia's Familias en Acción and found that municipalities in which the program was implemented registered a significantly faster increase in the number of attacks perpetrated by the FARC. These findings imply that where political, criminal, or armed actors compete violently to dominate territory, CCTs can expose beneficiaries to higher levels of violence.

In sum, just as there are reasons to expect CCTs to have a crime- and violence-preventing effect, there are also reasons to hypothesize that CCTs can unintentionally increase social, political, and armed conflict. To evaluate the net effect of CCTs on violence, we turn first to the case of Mexico. This country was a pioneer in the design and implementation of CCT programs and has recently seen a vertiginous rise in violence.

Violence and CCTs in Mexico

Over the two decades that followed its transition to democracy, Mexico experienced high volatility in its levels of violence. Although the first political cycle after the advent of democracy was characterized by a steady decline in violence, the following three have been marked by a steep increase. Before former president Felipe Calderón's presidency (2006–12), Mexico recorded between 10,000 and 15,000 homicides a year (Calderón et al. 2019). By the end of Calderón's presidency, homicides had doubled, executions had increased tenfold, and the number of missing persons had reached nearly 26,000 (Schedler 2015). Since then,

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homicides, kidnappings, extortions, and executions have risen in urban and rural communities (Maldonado-Aranda 2012; Calderón et al. 2019). In fact, in 2019, Mexico registered the highest homicide rate since the start of the war on drugs.

To control rising levels of violence, all federal administrations since 2006 have continued to pursue the policies that, many argue, gave rise to the problem in the first place (Phillips 2015). They strengthened a prohibitionist approach to drugs, increased cartel decapitation operations, and redoubled their bet on the use of the military as a force to occupy territories. Yet it must be noted that since the start of the war on drugs, executives have also highlighted the importance of poverty reduction programs as a critical strategy to reduce violence. During his presidential campaign in 2018, Andrés Manuel López Obrador ("AMLO") summarized his security platform in four words: "*becarios sí, sicarios no*."[1] Throughout his presidency, he has also attempted to persuade the governments of North and Central America to collaborate in the expansion of the CCTs he has promoted in Mexico, to address the structural economic conditions that create violence and promote migration in the region.

Although no president before AMLO underlined the violence-reducing potential of poverty alleviation programs so strongly, all federal administrations have framed PROGRESA as a central part of their long-term security strategy.[2] Therefore, to estimate the long-term impact of these programs, we need not implement a new CCT and wait for years to see its effects. We can investigate previous CCTs paying special attention on how well they have fared in their efforts to achieve their tangential but stated objective to reduce violence.

The Long-term Effects of PROGRESA on Violence

Mexico's Education, Health, and Nutrition Program (PROGRESA) was the second iteration of a long history of cash transfer programs implemented in Mexico since 1988. It was the successor of the National Program of Solidarity, PRONASOL (1988–96) and the predecessor of OPORTUNIDADES (2002–14) and PROSPERA (2015—18). PROGRESA started with an objective similar to that of PRONASOL, to serve as a stepping stone for low-income families to escape poverty. Meanwhile, it improved over its predecessor by making cash transfers conditional on families enrolling their children in school, complying with an 85 percent school attendance, preventing their children from repeating a single school year more than two times, and taking their children to regular medical check-ups. During its first year, PROGRESA incorporated four hundred thousand families, but by the end of 2016, the program had been taken to more than six million households and had inspired the creation of dozens of similar CCTs around the world (Morais de Sá e Silva 2017; Robles et al. 2017).

Although there is a consensus over the positive educational, health, and economic impacts of PROGRESA, evidence of its effect on violence is mixed. Whereas Lance (2014) found a negative association between PROGRESA and homicide rates at the

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municipal level, qualitative analyses have found a link between PROGRESA and social conflict. Adato et al. (2000) and Díaz-Cayeros et al. (2016), for instance, conducted in-depth interviews with citizens in Mexico and found evidence of conflict between beneficiaries and nonbeneficiaries, traditional authorities and program managers, and women recipients and their extractive husbands. Similarly, research journalists have investigated the modus operandi of drug cartels in rural areas and have reported how criminal organizations intimidate doctors, teachers, program workers, and beneficiaries to control communities and extract resources from their inhabitants (*Proceso* 2009). In the words of a member of Mexico's Council of Farmers and Settlers, "[The cartels] have the list of beneficiaries, they know how much they receive, and they extort and threaten those who do not pay their dues" (*MVS Noticias* 2010).

The mixed evidence found by previous studies might be due to methodological and epistemological differences. However, it may also be due to the inherent limitations of observational and qualitative designs. While PROGRESA had clearer rules than its predecessors, it was implemented and expanded to localities in which the political, criminal, and structural conditions allowed it (Skoufias 2006; Díaz-Cayeros et al. 2016). This situation opens our inferences to observed and unobserved confounders that can introduce selection bias. To extract the long-term causal effect of PROGRESA, it would be necessary to identify an exogenous shock that altered the program's implementation and then follow its effect for years. While finding such a shock is difficult, it is not impossible.

In the fall of 1997, the International Food Policy Research Institute (IFPRI) started a randomized controlled trial (RCT) in which hundreds of rural, marginalized localities, the lowest statistical unit studied by Mexico's National Institute of Statistics (INEGI), were randomly assigned either to receive PROGRESA immediately or to serve as controls for two years.

The RCT advanced in four stages. In the first stage, researchers selected seven states with high levels of poverty and inequality: Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz. Then, in the second stage, they selected 506 localities within the entire sample. Eligible localities had to score "high" or "very high" on Mexico's marginalization index, have more than 50 inhabitants, and have at least one school in the area. In the third stage, IFPRI identified poor households within the localities in the trial using a quantitative and qualitative cross-validation process. First, households were ranked by their levels of poverty, using data from the Demographic and Socioeconomic Household Survey (ENCASEH). Then, households were validated as poor by a popular assembly. In the fourth stage, the 506 localities were randomly divided into a treatment ($N = 320$) and a control group ($N = 186$). Households in treatment localities were incorporated into PROGRESA in 1998, and households in the control group were incorporated into the program two years later.[3] This means that families with children in first through sixth grade started receiving incentives two years earlier, and that families with children in eighth and ninth grade received incentives only if they were in the treatment group.[4] As figure 1 shows (and online appendix A2

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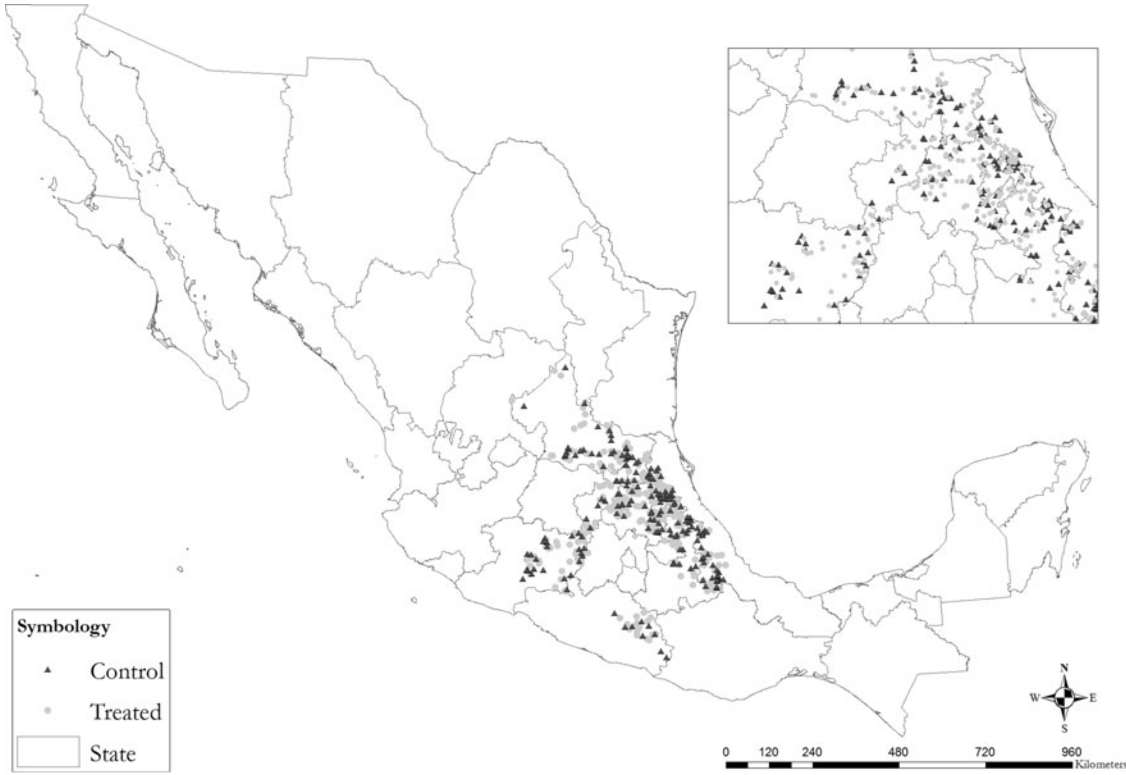
Figure 1. Distribution of Treatment and Control Localities



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confirms), although more localities were assigned to be early PROGRESA adopters, the treatment and control groups tended to be demographically and geographically balanced.

In total, over the course of the trial, the government made transfers to 4,546 households in the 320 treatment localities. It translates to a total of 109,104 monthly cash transfers and—at a rate of 24.6 1999 dollars per transfer—a total of an additional US$2.68 million in 1999. Adjusting for inflation, these numbers imply that PROGRESA distributed an average of 14.7 thousand (2022 inflation-adjusted) dollars per locality and that the government distributed a total of 4.7 million (2022 inflation-adjusted) additional dollars in the treatment group.

The Mexican government collected numerous variables to measure the economic, educational, and social development of the children and families living in the localities included in the trial (Skoufias 2006; Parker and Todd 2017). Unfortunately, it did not collect information on the levels of crime and violence that occurred in these areas. Thus, the only way to study the shock caused by the trial is to examine data outside of what were explicitly collected for the RCT.

Since the 1990s, INEGI has published event-level data on the causes of the deaths registered in the country, in line with the tenth revision of the International Classification of Diseases (ICD-10). The municipal-level identifiers that INEGI has included in this database have allowed scholars to investigate the determinants of violence at this level (e.g., Dell 2015; Enamorado et al. 2016; Lance 2014). Yet since IFRI randomized PROGRESA at the local and not the municipal level, an analysis of municipal-level trends in crime cannot help extract the causal effect of interest. Fortunately, only two years after the end of the trial, INEGI started identifying deaths at the local level. Thus, while we cannot calculate the number of locality-level homicides immediately before and after the trial, we can estimate the downstream impact of the trial starting two years after its conclusion.

I coded as homicides all the deaths caused by armed and unarmed direct violent attacks (X85-Y09 in the ICD-10 list), as well as all the deaths from gunshots of "undetermined intention" (W32–W34 and Y22–Y24) recorded by INEGI. All events recorded extemporaneously were counted in the year in which they occurred.[5] To analyze the long-term impact of PROGRESA on violence, I examined the first decade of available data; this means the 5,566 locality-years between 2002 and 2012.[6] I constrained the period under study in this way because it allows me to capture the differences in wealth and school-age children introduced by the program before they erode.

Homicides emerged as a rare event in the localities under study. In total, authorities registered 37 during the period analyzed. This may be explained by the fact that localities are in rural areas, authorities filtered out violent states from the trial, or a combination of both.[7] Because selection occurred before treatment assignment and authorities did not stop recording homicides at any time, selection affected neither the *N* of the study nor its ability to remove bias from the econometric analyses. Still, to prevent any potential selection bias and increase the efficiency of the estimates, I specify a negative binomial regression model in each locality-year as a function of the following equation:

$$Violence_{sjt} = \beta_0 + \beta_1 Treated_j + \delta' Size'_j + \varphi' Violence97'_j + \Theta' State'_s + \Phi' Year'_t + e_{sjt}$$

In this equation, $Violence_{sjt}$ is the locality-year number of homicides; $Treated_j$ is a dichotomous variable that identifies whether the locality was chosen to be an early adopter of PROGRESA. $Size'_{sj}$ and $Violence97'_{sj}$, represent two vectors of indicators that identify the size of the locality in the t^{th} year and the levels of violence in the locality's municipality before the study (1997).[8] $\Theta' State_s$ and $\Phi' Year_t$ represent a set of state and year fixed effects, and e_{sjt} represents a residual error term clustered at the locality level.

As expected, the model shows that populous localities located in municipalities with higher levels of violence before the trial have a higher probability of registering homicides during the period of analysis (column 1 in table 1). More important, it shows little evidence in favor of the violence-reducing effect of CCTs. If anything, it suggests that localities experiencing the PROGRESA RCT shock registered a significantly higher number of homicides ($p = 0.07$) than localities not experiencing this shock. In relative terms, the risk of observing a homicide in localities randomly to start PROGRESA early is 2.16 times larger than the risk in localities chosen to serve as controls.

While this finding brings initial evidence for the unintended effects of PROGRESA, it is important to acknowledge that not all incidents of violence involve homicides. Fortunately, there is an additional data source that can help assess further the effects of the program—Mexico's Automated Hospital Discharge System (SAEH).

The SAEH, operated by the General Directorate of Health Information (DGIS), documents each case that is discharged in 98 percent of Mexico's public hospitals and clinics (DGIS 2001). The system records patients' demographic characteristics, date of admission, and condition for which they were treated. Importantly, the SAEH records the cause of all the hospitalizations that involve an external cause (7.21 percent) according to the ICD-10. This allows us to categorize hospitalizations as nonviolence- and violence-related hospitalizations (VRH). The latter include hospitalizations caused by aggressions, gunshots, stabbings, or explosive attacks. The former is a residual category.[9]

While these data are of great value, they have a key limitation. As the randomization of the experiment occurred within municipalities, only the SAEH can be used to obtain a count of the number of VRHs at the locality-year level after 2004, the first year in which INEGI made public this information. Regardless, evaluating the long-term effect of the exogenous implementation of the trial on nonlethal violence is critical to fully understand the effects of this program. Thus, in line with the previous section, I analyze the first decade of available data on VRHs (2004 and 2014).

Perhaps due to a lack of medical access in rural localities or the inherent rarity of the phenomenon, during the period under analysis (5,566 locality-years), the SAEH

Table 1. Effect of PROGRESA on Homicides and Violence-related Hospitalizations

	(1)	(2)
	Number of Homicides	Number of Hospitalizations
PROGRESA	0.771*	0.740**
	(0.431)	(0.317)
Population[a]		
20–118	–15.52***	–1.203
	(0.714)	(0.824)
119–178	–0.743	0.0720
	(0.766)	(0.588)
253–377	1.945***	1.178**
	(0.748)	(0.478)
378–2089	2.527***	1.666***
	(0.742)	(0.483)
Homicides (municipality)[b]		
0	–1.807**	–0.909
	(0.916)	(0.659)
1–5	–0.620	–0.527
	(0.641)	(0.596)
11–15	0.438	–0.0528
	(0.648)	(0.624)
> 15	0.126	–0.555
	(0.763)	(0.681)
Constant	–22.49	–5.393***
	(0.976)	(1.256)
Year fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Localities	506	506
Years	2002–2012	2004–2014
Locality-years	5,566	5,566

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Clustered standard errors at the locality level are shown in parentheses.
[a]The omitted category is 179–252 inhabitants.
[b]The omitted category is 6–10 homicides.
Locality-level percentages and averages were calculated from the Skoufias et al. (1999).

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recorded only 62 violence-related hospitalizations. Still, these data allow us to produce a secondary test of hypotheses based on cases of violence that do not result in the victim's death.[10]

With this objective in mind, I specified a negative binomial regression model identical to the one presented in equation 1. This time, however, instead of representing the count of homicides, $Violence_{sjt}$ represents the number of VRHs recorded in a locality-year. Its results are presented in column 2 of table 1. Consistent with previous analyses, this analysis finds no evidence for the violence-reducing effect of PROGRESA. Instead, it finds that VRHs are more prevalent in localities that adopted the program early than in localities that adopted the program late. More precisely, I find that, once all factors are held constant, exposure to the PROGRESA trial produced a statistical and substantive increase in the incidence of VRH.

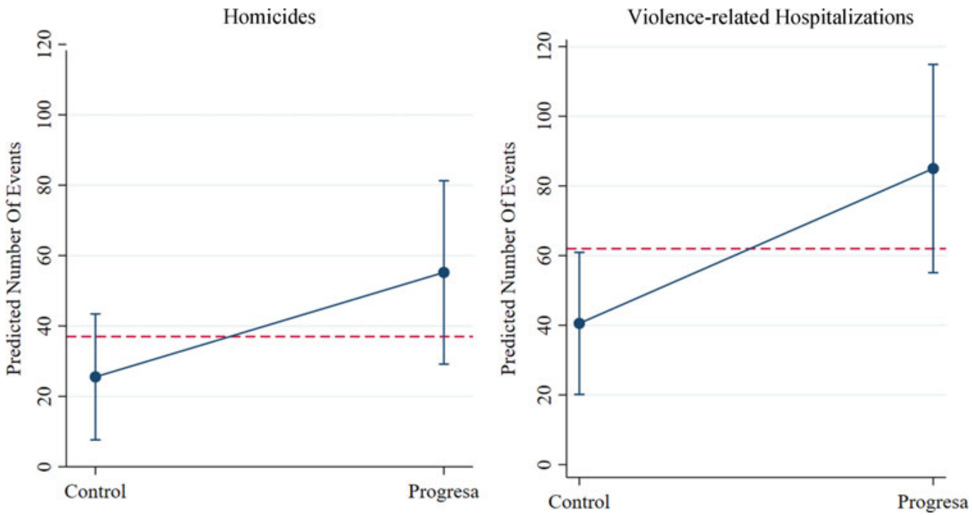
All things considered, data on both lethal and nonlethal violence suggest that PROGRESA had negative externalities for violence during its first six years of implementation. How meaningful were these negative externalities? To answer this question, we can use the parameters in columns 1 and 2 of table 1 to calculate the number of events (murders and VRHs) that would have been observed in the years under study had all the localities been assigned to start PROGRESA early, and the number of events that would have been observed had all localities been assigned to wait until the year 2000 to start the program.[11]

As the panels in figure 2 show, estimating that had no locality started PROGRESA until the year 2000, we would have expected to see about 26 homicides and 41 VRHs during the subsequent decade. In contrast, had the entirety of the localities started PROGRESA in 1998, we would expect to see about 55 homicides and 101 VRHs in the same period. Those are 18 more homicides and 23 more VRHs than observed in the data (dotted line), and some 30 additional murders and 44 more VRHs than expected under the opposite counterfactual.[12] This might seem like a marginal effect, but if we consider that the same year that the trial took place, PROGRESA was introduced in another 43,485 rural marginalized localities, these findings suggest that PROGRESA could be associated with as many as 2,319 excess homicides and 3,472 excess VRHs.[13]

Although the rigor of the PROGRESA trial allows us to derive internally valid estimates, it is important to consider three limitations of the results so far. First, as previously noted, since the Mexican government does not make locality-level data on violence available before 2002 (2004 for VRHs), only the long-term effect of PROGRESA can be estimated. While accurate, these estimates are not inconsistent with the program's having virtuous short-term effects (Camacho and Mejía 2013; Chioda et al. 2016). PROGRESA could have reduced crime violence in the first two years in which it was implemented, had null effects over a second phase, and generated sociopolitical conflict in the long term. Additional information would be necessary to estimate the effect of PROGRESA in its first two years.

Second, these results are consistent with causal mechanisms related and unrelated to PROGRESA's stated objectives. As noted, PROGRESA aimed to increase

Figure 2. Long-term Effect of the PROGRESA Trial on Violence



The figure illustrates an estimation of the number of events (homicides, hospitalizations) that could be expected in the sample under a counterfactual treatment reassignment. Vertical lines reflect 95% confidence intervals. The dotted line indicates the observed number of homicides and hospitalizations in the period.

wealth, human capital, and school attendance (Schultz 2004; Parker and Todd 2017). Yet it also exposed communities to program operators of questionable repute and opened the opportunity for social and political conflict. Further qualitative and quantitative research would be necessary to distinguish whether the increases in violence detected here are a byproduct of rapid economic development, criminal predation, conflicts between central and traditional authorities, or another causal mechanism.

Third, while the results derived from the PROGRESA trial have been used to justify the implementation of CCTs across contexts, it is critical to acknowledge that the sample is relatively small (it encompasses only 506 of about 190,000 localities) and not representative of the country, and should be used with care to make strong claims about the impact of PROGRESA—or other programs—in urban areas, in other periods, or in other countries. Furthermore, the findings so far do not necessarily represent a strong rebuttal to the optimistic results found by others in urban areas (Camacho and Mejía 2013; Chioda et al. 2016; Machado et al. 2018). The violence-inducing effect of PROGRESA could very well be idiosyncratic to the characteristics of the program, the kind of violence experienced by Mexico, the rural context in which the trial was implemented, or a combination of these factors. Additional experimental and observational research would be necessary to investigate whether the findings of this study are consistent with what is observed in other environments.

To explore the generalizability of the findings presented, the next section analyzes all the data on social program participation and violence collected by the Latin American Public Opinion Project (LAPOP). While the observational nature of LAPOP's data prevents deriving strong causal inferences, it provides a unique opportunity to investigate the issue at an individual level across a broad number of countries.

Social Program Participation and Violence in the Americas

Since 2004, LAPOP's flagship project, the AmericasBarometer, has compiled firsthand information on the political culture of the citizens of the Americas. In addition to asking citizens about their democratic attitudes and dispositions, since 2010, the AmericasBarometer has questioned interviewees across the continent about their participation in cash transfer programs and their exposure to crime and violence.[14] These data allow us to account for citizens not reporting crime to authorities (Levitt 1998; Skogan 1976) and for the difficulty of inferring individual-level experiences from aggregate data (Seligson 2002).

To investigate the effect of CCT participation on crime and violence, this study compares the experiences of interviewees living in households receiving CCT transfers with those not receiving them. All in all, the analysis is based on 141,268 interviews collected in Mexico, Guatemala, El Salvador, Honduras, Nicaragua, Costa Rica, Panama, Colombia, Ecuador, Bolivia, Peru, Paraguay, Chile, Uruguay, Brazil, Venezuela, Argentina, the Dominican Republic, Haiti, Jamaica, Guyana, Trinidad and Tobago, Belize, and Suriname. Online appendix A6 describes the number of interviews in each of the 86 country-waves.

Three regression models were estimated. The first model evaluates the effect of social program participation on citizens' probability of being the victims of a crime (*Crime Victim*). The second model evaluates the impact of program participation on individuals' perception of the levels of insecurity in their neighborhood (*Neighborhood Insecurity*). The third model analyzes the effect of CCT participation on citizens' *Fear of Being Murdered*. This latter question was asked only to citizens living in the countries with the highest murder rates in the Americas (Mexico, Guatemala, El Salvador, Honduras, Brazil, and Venezuela) and was included only in 2016. Equation 2 shows the general statistical equation specified in each model presented in table 2.

$$\begin{aligned} Violence_{sjt} = {} & \beta_0 + \beta_1 CCT\ Participant_{sjt} + \lambda' Demographic'_{sjt} + \delta' Size'_{sjt} \\ & + \varphi' Economic'_{sjt} + \xi' Political'_{sjt} + \Theta' Subnational\ Region'_s \\ & + \Phi' Year'_t + e_{sjt} \end{aligned}$$

In equation 2, $Violence_{sjt}$ represents one of the three dependent variables described before (*Crime Victim*, *Neighborhood Insecurity, Fear of Being Murdered*) and $CCT\ Participant_{sjt}$ represents a dummy variable that indicates whether or not the interviewee lives in a household participating in a conditional cash transfer program. Since the independent variables are not assigned exogenously, it is necessary to account for potential confounders to approximate the net causal effect of interest. The special terms in equation 2 represent six clusters of relevant statistical controls.

The term $\lambda' Demographic'_{sjt}$ stands for the demographic variables—age, education, and gender. It is important to control for these variables because CCTs are frequently directed toward women, school-age individuals, and the elderly, and citizens belonging to these groups tend to have different experiences with crime (Singer 2017). The term $\varphi' Economic'_{sjt}$ represents two relevant economic controls, income level and wealth. Accounting for these factors is critical since, by design, CCTs are systematically directed to families of lower economic strata, and these families have a different propensity to be involved in crime.

To approximate income, I relied on individuals' self-reported income, and to approximate wealth, I relied on Córdova's relative wealth measure (Córdova 2009). The term $\delta' Size'_{sjt}$ represents the size of the individual's community, and $\xi' Political'_{sjt}$ represents the two indices included in all waves of the AmericasBarometer that capture two key dimensions of citizens' political attitudes—*Support for the System* and *Political Tolerance*.[15] I accounted for the size of individuals' place of residence because rural and urban areas experience radically different levels of violence and insecurity. I controlled for political attitudes because—in countries with weak levels of the rule of law—those supportive of the political system and those willing to condemn dissidents are more likely to become beneficiaries of social programs, more likely

Table 2. Effect of Social Program Participation on Crime Victimization and Perceptions of Insecurity

	(1)	(2)	(3)
Variable	Crime Victim	Neighborhood Insecurity	Fear of Being Murdered
Beneficiary	0.0665***	0.695***	2.535**
	(0.0202)	(0.253)	(1.159)
Women	–0.148***	4.055***	7.893***
	(0.0146)	(0.168)	(0.762)
Education	0.676***	–1.279***	1.378
	(0.0356)	(0.448)	(1.736)
Age	–0.403***	–0.890***	–5.942***
	(0.0247)	(0.292)	(1.303)
Capital city	0.134***	2.852***	2.955
	(0.0396)	(0.592)	(2.246)
Large city	0.154***	3.572***	1.458
	(0.0297)	(0.470)	(1.284)
Small city	–0.175***	–4.391***	–3.495**
	(0.0313)	(0.485)	(1.373)
Rural area	–0.347***	–5.349***	–2.653*
	(0.0300)	(0.433)	(1.406)
Income	0.178***	–1.651***	1.097
	(0.0265)	(0.338)	(1.164)
Wealth	–0.0192	–2.617***	–1.054
	(0.0249)	(0.301)	(1.236)
System support	–1.023***	–17.08***	–18.30***
	(0.0345)	(0.441)	(1.731)
Political tolerance	0.204***	0.882**	9.216***
	(0.0314)	(0.399)	(1.678)
Constant	–0.954***	53.36***	46.74***
	(0.0820)	(1.228)	(2.839)
Observations	132,239	131,456	10,189
Country's region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

***p < 0.01, **p < 0.05, *p < 0.1.
Notes: Estimates in column 1 are derived from a logistic regression model. Estimates in columns 2 and 3 are derived from an OLS model. Design-based standard errors in parentheses.

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to receive public security services, and less likely to suffer or report crime (Layton et al. 2017; Singer 2017).

In addition to these substantive controls, I included wave- and strata-level fixed effects ($\Phi' Year_t'$, $\Theta' Subnational\ Region_s'$). Strata-level fixed effects allow for accounting not only for country-level differences but also for geographical variations within countries (e.g., distinguishing between northern and southern Mexico).[16] Table 2 shows the results of the three models. When the dependent variable is dichotomous (*Crime Victim*), I specified a logistic regression model. When the dependent variable is on a 1-to-5 Likert scale (*Neighborhood Insecurity* and *Fear of Being Murdered*), I rescaled it to run from 0 to 100 and specified a multivariate OLS model.

The results in table 2 are clear. Holding individuals' wealth, income, gender, age, education, support for the system, and political tolerance constant, CCT beneficiaries are significantly more likely to be victims of crime, to report their neighborhoods as being more insecure, and to be more fearful of being murdered. Focusing on the first of the three findings, the odds of an average Latin American becoming the victim of a crime are 5.46 percent higher if the person is a program participant than if not. This suggests that expanding social programs to an additional 10,000 households could lead to as many as 107 additional crimes a year. In relative terms, this effect is equivalent to 45.53 percent of the effect of gender, 37.37 percent of the effect of income, and 20.84 percent of the effect of living in an urban area.

While these estimates are in line with experimental data and are robust to controlling for preexisting levels of neighborhood and municipal-level insecurity (see online appendix A8), they should also be taken with care, as they cannot fully account for the bidirectional relation between the variables of interest.[17] Additional cross-national experimentation would be necessary to fully establish the causal link of interest. While such an ambitious exercise may be undertaken, LAPOP's data suggest that the findings in the first section of this article should not be dismissed as a fluke emerging from the uniqueness of the context in which the PROGRESA trial took place.

Conclusions

Because of their economic and political importance, conditional cash transfer programs have attracted the attention of economists and political scientists over the last two decades. While academics and policymakers have suggested that these programs could be an effective policy tool to prevent and reduce crime and violence, empirical and journalistic evidence suggests that in contexts of low rule of law, CCTs can expose beneficiaries to social, criminal, and political violence. In this article, I have reviewed the potential impacts of CCTs, paying particular attention to how CCT programs could have an unexpected and unintended violence-inducing effect.

To assess empirically the net effect of cash transfer programs, I looked at the case of Mexico. This country played a leading role in the design and implementation of CCTs during the 2000s, has become one of the most violent countries in the region, and has recently promoted the use of cash transfer programs as a strategy to reduce violence. I took advantage of an exogenous shock created by the 1998–2000 experimental expansion of Mexico's PROGRESA to estimate the causal long-term impact of the program on locality-level homicides and violence-related hospitalizations.

I found little support for the optimistic view of CCTs, but some support for the idea that the introduction of PROGRESA caused an unintended increase in violence. Localities exposed to PROGRESA for two additional years recorded significantly more violence in the decade that followed the study. While these data bring valuable insights to the understanding of the impact of CCTs, they are not broadly applicable. Therefore, to assess the generalizability of these findings, I analyzed what is arguably the broadest comparable database on CCT participation and violence—the AmericasBarometer. The data compiled across 25 countries between 2012 and 2016 bring little support for the rosy view of CCTs and, on the contrary, show that once other factors are accounted for, beneficiaries are more likely to become victims of crime, perceive their neighborhood to be insecure, and be fearful of being murdered.

These findings bring support to a growing literature critical of CCTs. Just as some antipoverty programs can fail to empower women, actually consolidating traditional gender roles (Molyneux et al. 2016), this study's results suggest that when CCTs are not accompanied by strategies to reduce crime and violence, they generate the conditions and incentives in which these phenomena can be exacerbated (Borraz and Munyo 2020).

Moreover, the results of this article have implications for understanding the political consequences of CCTs. Other researchers have found that although redistributive cash transfer programs are deployed with a political objective in mind (Díaz-Cayeros et al. 2016; Penfold-Becerra 2007; González and Mamone 2015), they fail to deliver the political benefits expected (Imai et al. 2020), are seldom able to distract citizens from corruption and economic underperformance (Pavão 2016), can generate contention among partisans (Corrêa and Cheibub 2016), and can even lead to the growth of the opposition in the long run (Blattman et al. 2018). The findings in this study suggest that—to the extent that the context allows them to do so—political actors may use violence to secure the electoral gains they cannot obtain through redistribution, to resolve intrapartisan conflict, or to contain the losses that a wealthier and more independent electorate entails. Of course, additional qualitative and quantitative research would be necessary to deepen our understanding of how the expansion of CCTs can intensify political and social conflict at the local level.

An open question that is not answered in this article is whether the violence-inducing effect estimated here varies across social programs. While further research is needed in this area, it is important to point out that this heterogeneous impact is likely to be contingent on the mechanisms underlying the violence-inducing effects of CCTs and the very nature of the programs. If violence emerges from political elites' frustration with their incapacity to control a wealthier and more independent electorate, the negative effects of social programs might be stronger where they are better shielded against clientelist manipulation. If the effects are driven by the medium-term predation of the additional human and economic capital introduced to communities (Kaufman and Trejo 1997; Díaz-Cayeros 2008), the violence-inducing effects of social program might be even stronger in places where elites can distribute cash more privately and discretionally.

More theoretical and empirical research would be necessary to identify the institutional and contextual factors that moderate the effects of cash transfer programs. Instead of bringing closure to this debate, this article seeks to inspire others to incorporate the unintended effects of social programs into their models of development policy and our understanding of the emergence of violence.

Supplementary material

To view supplementary material for this article, please visit https://doi.org/10.1017/lap.2022.67

Notes

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1. “Yes to scholarship recipients, no to hitmen” (author’s translation).

2. Online appendix A1 shows how not only AMLO but also other presidential candidates—as well as the national development plan—consistently see social programs as part of the strategic actions implemented to prevent violence.

3. Data available at https://evaluacion.prospera.gob.mx

4. When PROGRESA was introduced in control localities, families with children in eighth and ninth grade were no longer eligible.

5. Counting deaths by gunshots of “undetermined intention” as homicides is important because, during conflict, it is difficult for government officials to identify the causes of murders.

6. The period analyzed includes a total of 11 years. As online appendix A4 shows, the results become violence-inducing after 2005 (five years after the trial) and remain significant between 2005 and 2016 (the last year under analysis), independent of the period under analysis.

7. Skoufias notes that “localities in the states of Campeche, Chiapas, Chihuahua, Coahuila, Guanajuato, and Oaxaca were screened out of the trial for a variety of socioeconomic reasons, including the possibility of having security problems for the enumerators” (2006, 28).

8. These variables are specified as categorical to account for nonlinearities within them. Since INEGI does not identify murders at the locality, it is the only way to account for *ex-ante* levels of violence. Yet as online appendix A3 shows, the results are robust to removing all substantive controls (population and homicides).

9. The SAEH database contains between 1.79 and 2.9 million records each year. I classify an external cause to be violence-related when it involves the following ICD-10 codes: W32-W34, X85-Y09, Y22, Y23, Y24, Y36.

10. Especially if one considers that, by 2004, all localities had been included in PROGRESA.

11. As online appendix A5 shows, the results are robust to conducting a clustered randomized inference test. In total, I find that under rerandomization, the results presented here are replicated in fewer than three of every thousand iterations.

12. Decimals were rounded to the nearest whole number for simplicity. The locality-year AMTE is 0.0053 (0.0029) for homicides and 0.00798 (0.00333) for VRHs.

13. This extrapolation should be taken with a grain of salt, as a proper extrapolation would require incorporating sampling and propagation error.

14. In 2010 and 2012, LAPOP asked citizens whether they received monthly assistance in the form of money or products from the government. In the rest of the years (including a half-sample in 2012), it asked interviewees if someone in their household was a beneficiary of the country's main conditional cash transfer program. Online appendix A6 lists the questions analyzed in this study.

15. Online appendix A6 describes the questions that compose both indexes.

16. All analyses were conducted using STATA 15.1 svy module.

17. Online appendix A7 shows that the violence-inducing impact of CCTs extends to considering moving for fear of crime, limiting places of recreation, perceptions that shootings were a problem, and perceptions that assaults were a problem. Online appendix A8 shows that the results are robust to controlling for perceptions of neighborhood insecurity, the number of interviewees victimized in the municipality, and the government-reported municipal homicide rates. Furthermore, while omitted-variable bias influences the results for crime victimization and fear of homicide, the results for perceptions of insecurity remain unchanged when omitting all controls (see online appendix A9).

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Supporting Information

Additional supporting materials may be found with the online version of this article at the publisher's website: Appendix.