Reparations as Development? Evidence from Victims of the Colombian Armed Conflict

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Abstract

Can reparations for victims of human rights violations help rebuild lives? We estimate the effects of reparations across the life cycle, leveraging variation induced by Colombia's program for victims of the internal armed conflict. The reparations consist of large, one-off, lump-sum payments of up to 10,000 USD (PPP 26,000 USD) and represent, on average, three times recipients' annual household income. We link comprehensive national administrative panel microdata and measure numerous individual- and household-level outcomes, including work and living standards, health care utilization, and intergenerational impacts on victims' children. We exploit the staggered rollout of reparation payouts and the unexpected timing of their receipt using event study approaches and document three main sets of results. First, reparations cause an immediate decrease in the probability of formal employment driven by shifts out of low-pay, high-risk, salaried jobs. Three years after receipt, victims have higher wages and are more likely to own an active business. Second, reparations cause an economically meaningful decrease in health care utilization, consistent with improved health due to better working and living conditions. Third, reparations increase high school test scores and college attendance rates of victims' children. We conclude that the large transfers of money provided by reparations allow households to make fundamental investments, narrow the gaps formed due to conflict, and appear to be an effective policy tool to promote recovery and development.

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1 Introduction

Can reparation programs for victims of human rights violations help them rebuild their lives? More than three dozen countries—from post-conflict Côte d'Ivoire and Indonesia, to post-World War II Germany and the United States (with respect to interned Japanese-Americans), to post-authoritarian Argentina and Chile—have implemented such programs, which often transfer money to victims to recognize and address the harms they have suffered (United Nations, 2005).[1] Reparations are thought to be critical for justice and recovery; scholars have posited that they may generate significant impacts on well-being, especially since victims tend to be poor or otherwise disadvantaged (de Greiff, ed, 2006; OHCHR, 2009; Roht-Arriaza and Orlovsky, 2009; Uprimny and Saffon, 2009). Yet, despite these hypothesized impacts and the political and economic costs of reparations, there is scant quantitative evidence of the effects of reparations on victims.

There are reasons to think that reparations may not have an impact. For instance, victims may suffer from physical and mental health problems and lack of other inputs, and may end up not investing reparations in a way that leads to sustained income gains. In addition, there are empirical challenges to estimating the impacts of reparations. First, the data infrastructure required to identify victims and track their outcomes is often lacking in post-conflict societies. Victims' registries are either non-existent or miss a substantial portion of victims; even when they are complete, there is a lack of individual- or household-level data that follows victims before and after receiving reparations (OHCHR, 2009; Sikkink et al., 2015). Second, it has been difficult to find plausibly exogenous sources of variation in reparations that would allow for estimating causal effects. Because of these challenges, little is known about the effectiveness of reparations as a policy tool to narrow the gaps formed by victimizations.

This paper provides one of the first quantitative evidence on the effects of reparations on victims' well-being. We use evidence from Colombia—which, as part of its transition into peace, committed to compensating more than 7 million individuals, making it the largest reparations program in history.[2] The reparation consists of a massive one-time, lump-sum, non-means-tested, unconditional cash award of up to 10,000 USD per individual or 26,200 USD per capita at purchasing power parity (PPP) in 2019, representing over three times recipients' annual household income. Much like a "labeled" cash transfer, reparations are presented as tools for victims to rebuild their lives.

Between January 2009 and August 2021, 8.4 trillion pesos (2.2 billion USD), were paid out

[1] While reparations also encompass restitution, rehabilitation, satisfaction, and guarantees of non-repetition (United Nations, 2005), we focus on the financial compensation component of reparations; henceforth, we use the terms "reparation" and "compensation" interchangeably.

[2] Reparation programs have rarely benefited more than 180,000 victims in other countries (Carranza et al., 2017; Sikkink et al., 2015). Moreover, Colombia represents a new model of implementing transitional justice, as the conflict is still ongoing.

to more that 1.1 million victims over the course of those eleven years (UARIV, 2021), that is, roughly 0.07 percent of Colombia's GDP per year.[3] Importantly, the rollout of reparations was staggered due to binding budget and operational constraints. Moreover, the receiving victim cannot predict when they will receive the reparation—a fact backed by anecdotal and survey evidence and confirmed empirically with our data. We leverage these features of our setting to estimate the causal effects of reparations using event study and before/after approaches. To track outcomes over time, we construct a novel administrative panel dataset that links the national victim registry to individual-level data on employment and earnings; entrepreneurship and business survival; consumption; land and homeownership; health care utilization; and human capital formation.[4] This ability to link multiple data sets for millions of Colombians allows us to quantify the impacts of reparations on victims' well-being across the life cycle.

We divide our findings into three mains parts: (i) impacts on work and living standards; (ii) effect on health care utilization; and (iii) intergenerational impacts on human capital investments. For impacts on work and living standards, we find that reparations cause a 2–3 percent drop in the probability of formal employment. This effect is economically small, as only 16.4 percent of the comparison group has formal work, but it is very precisely estimated and highly statistically significant. It is primarily driven by young workers shifting out of high-risk and low-paying jobs, resulting in a mechanical increase in daily wages. However, wages continue to rise after the drop in formal employment stabilizes, consistent with workers also responding to the cash windfall by taking longer unemployment spells and ultimately finding better jobs. As a result, three years after receiving reparations, total formal earnings 2.1 percent are higher; for this reason, we estimate that the long-run marginal propensity to earn (MPE) out of unearned income is close to zero and non-significant among recipients. Part of the drop in formal employment may also be explained by victims investing in income-generating activities outside of the formal wage labor market. Indeed, we find a sizable effect on the probability of starting a new business: recipients are 37 percent more likely to obtain a new business license the quarter after receipt. We also find that reparations boost firm survival and increase microcredit use without increasing delinquency.

Moreover, we find that victims consume more durables and non-durables thanks to reparations and are wealthier. Using administrative data on the universe of formal loans, we find that reparations induce a short-term decrease in credit card debt, a long-term increase in credit card spending, and an increase in auto loans. Moreover, we find increases in cumulative land and home purchases using cadastral records. Taken together, these results suggest that

[3] Funding for reparations primarily comes from the national budget. The annual budget allocated for the government agency in charge of victims is roughly similar to K–12 public education or *Familias en Acción*, Colombia's main conditional cash transfer program.

[4] In Guarin et al. (2021), we plan on using a unique survey to estimate the effects of reparations on soft outcomes, such as victims' attitudes towards the State and illegal armed groups, satisfaction with the reparation program, perception of restorative justice, psychological well-being, and intent to return (for victims of forced displacement).

reparations relieved borrowing constraints and induced long-term increases in consumption and asset ownership.

Regarding impacts on health outcomes, we find that reparations cause economically meaningful drops in health care utilization. Individuals are 5.3 percent less likely to visit the emergency department (ED) one year after receipt and this effect compounds over time: four years after receipt, ED visits have dropped by 15.9 percent. Hospitalizations and medical procedures follow a similar pattern. The causes of these ED visits shed light on the underlying mechanisms. For instance, there is an immediate drop in musculoskeletal and external causes of morbidity, consistent with improved work and living conditions. In the longer run, the decrease in ED visits is also driven by chronic conditions, consistent with healthier behaviors. We view these health results as reflecting changes in intermediate outcomes (e.g., dietary habits, access to clean water) that may not be captured using administrative data but which affect health conditions and physical well-being.

Finally, we show that victims invest the money from reparations in their children's human capital. Reparations increase first-time enrollment in university by 24 percent and overall university attendance by 18 percent. The relative gain in matriculation is larger at private institutions, where expensive tuition fees traditionally discourage attendance for low-income students (Londoño-Vélez et al., 2020). Moreover, while we do not see that reparations affect children's likelihood of graduating from high school, they do improve their academic performance. We estimate a gain of 2.37 percentiles in Colombia's national standardized high school exit exam. The gains are larger for victims who were younger when their household received the reparation and therefore had more time to benefit from the boost in parental wealth. Taken together, these findings indicate that reparations may have positive intergenerational effects thanks to investments in human capital.

We conclude with a stylized cost-benefit analysis that compares the cost of reparations relative to the measured long-term benefits. In calculating the dollar-equivalent value of each outcomes, we err on the conservative side and, therefore, our estimates should be seen as a lower bound of the total benefit of the policy. Notwithstanding, aggregating the benefits along the different dimesions, we find that reparations are likely to be cost-beneficial.

This study contributes to five literatures. First, we contribute to the literature on reparations, which largely consists of qualitative work by political scientists, lawyers, sociologists, and other experts on transitional justice (Cilliers et al., 2016; de Greiff, ed, 2006; Díaz, ed, 2008; Dixon, 2015; Hirsch et al., 2012; Pham et al., 2016; Sánchez-León and Sandoval-Villalba, 2020; Sveaass and Sonneland, 2015; Vallejo, 2019; Vargas et al., 2019; Weber, 2020).[5] Despite this work, reparations remain controversial as a policy tool among both scholars and practitioners (Dixon, 2017; Dixon et al., 2019). For example, in the United States, there is

[5] An exception is Miller (2011, 2020), who examines the effect of the Cherokee Nation providing free land to its former enslaved people after the American Civil War.

an ongoing debate about whether reparations are owed to African-American descendants of enslaved people (Boerma and Karabarbounis, 2021; Coates, 2014; Darity and Frank, 2003). On the one hand, proponents argue that reparations promote justice and a more inclusive society by narrowing the socioeconomic gaps formed by victimization. On the other hand, detractors say reparations are costly and may not be effective in meeting their goal. We contribute to this literature and policy debate by offering one of the first known quantitative evidence on a large-scale reparation program.

Second, we contribute to a vast and growing literature on the effectiveness of unconditional cash transfers (UCTs) for poverty alleviation (e.g., Balboni et al., forthcoming; Blattman and Dercon, 2018; de Mel et al., 2012; Egger et al., forthcoming; Handa et al., 2018; Haushofer and Shapiro, 2016). Despite sharing similar features, Colombia's reparations differ from traditional UCTs in two crucial ways. First, the average reparation is over three years' worth of household income and, therefore, substantially larger than UCTs. Second, reparations target victims of human rights violations, a uniquely vulnerable population. Adverse shocks in the setting of conflict, like forced displacement, can have lifelong detrimental effects and trap victims in a low equilibrium of poverty (Fiala, 2015; Ibáñez and Moya, 2010).[6] Similar to Balboni et al. (forthcoming) and Banerjee et al. (2021), we show that by providing households with a large, lump-sum grant, reparation can serve as a "big push" policy for the victim to transform their life and escape poverty traps.

Third, we contribute to research on the effects of cash windfalls on employment and occupational choices. Standard labor supply models predict that an exogenous increase in income should reduce labor supply through income effects. This prediction is supported by evidence on lottery winners in developed countries (Cesarini et al., 2017; Golosov et al., 2021; Imbens et al., 2001). In contrast to these findings, the literature on developing economies finds no systematic evidence that cash transfers discourage work (Banerjee et al., 2019, 2017). We find that the large lump-sum payments from reparations induce an immediate drop in the probability of formal employment. However, our estimated long-term MPE is close to zero and not statistically significant. In our context, part of the effect is explained by workers spending more time out of formal employment to find better jobs and on income-generating activities outside of the formal wage labor market, such as entrepreneurship (Andersen and Nielsen, 2012; Bandiera et al., 2017; Bianchi and Bobba, 2012; de Mel et al., 2013; Gertler et al., 2012; Giorgi et al., 2018; Holtz-Eakin et al., 1994; Jayachandran, 2019; Ulyssea, 2018).

Fourth, we contribute to the literature on the effects of cash transfers on health. A recent review by Pega et al. (2017) concluded that the impacts of UCTs on the use of health services and health outcomes are uncertain. Most prior estimates on the relationship between income

[6] In the United States, there is mixed evidence on whether adverse shocks like the Depression (Malmendier and Nagel, 2011), the Great Recession (Rothstein, forthcoming) or Hurricane Katrina (Deryugina et al., 2018) have long term impacts on people.

and health have been based on correlations across individuals, across countries, or over time, thus suffering from reverse causality, omitted worker characteristics, and measurement error (see Cutler et al., 2011).[7] Two notable exceptions are Cesarini et al. (2016), which exploits random assignment of Swedish lottery prizes, and Haushofer and Shapiro (2016), which draws from randomized UCTs in rural Kenya. Importantly, neither study finds strong effects of windfall income on physical health: Cesarini et al. (2016) find no impact on hospitalizations and health, while Haushofer and Shapiro (2016) find no impact on a survey-measured health index. Contrary to these null results, we find that cash awards cause significant and economically meaningful drops in health care utilization, which is consistent with improvements in health.

Finally, we contribute to the literature on the effects of cash transfers on children's educational outcomes. Previous evidence, reviewed in García and Saavedra (2017), shows that cash transfer programs improve primary and secondary schooling in developing countries. In developed countries, Dahl and Lochner (2012) leverage large and non-linear changes in the US Earned Income Tax Credit using an instrumental variables strategy and find that a plausibly exogeneous increase in income results in a short-run increase in math and reading test scores. Other studies show that *negative* income shocks, induced by parental job loss around the time youth complete high school, are negatively related with enrollment at university and community college (Coelli, 2011; Shea, 2000).

The remainder of this paper is organized as follows. Section 2 describes the institutional context. Section 3 introduces the data and the methodology. Section 4 presents results on the effects of reparations on victims throughout the life cycle. Section 5 provides a stylized cost-benefit analysis and Section 6 briefly concludes.

2 Background: Colombia's Conflict and Victim Reparations

Colombia has had the most prolonged internal armed conflict in the Western Hemisphere. In the mid-Sixties, left-wing rebel groups like FARC–EP (Revolutionary Armed Forces of Colombia–People's Army, the largest guerrilla group) and ELN (National Liberation Army) emerged in remote regions of the country. Fueled by an increasingly profitable cocaine trade during the Eighties, the violence escalated as right-wing paramilitary groups developed to contain the emergence of these guerrillas and protect landowners and drug lords. The intensification of the conflict caused an ever-rising number of attacks against civilians, as depicted in Appendix Figure A.1. The frequency of victimizations peaked in the early 2000s, following a failed peace negotiation between the government and FARC–EP. After the peak, the number of

[7] A handful of studies use self-reported health measures from household surveys while also exploiting variation from lotteries (Apouey and Clark, 2015; Gardner and Oswald, 2007; Lindahl, 2005). A separate literature has used administrative data to study the relationship between income and life expectancy in the United States (Aizer et al., 2016; Chetty et al., 2016; Sullivan and von Wachter, 2009). Moreover, the literature on cash transfers have examined the effect of money awarded to poor pregnant women on children's birth outcomes (e.g., Amarante et al., 2016).

victimizations decreased with Colombia's attempts to transition into peace and reconciliation. In 2005, Colombia sought to transition into that direction by demobilizing paramilitary groups and reintegrating them into civilian life through the Peace and Justice Law. Further, in 2016 the government negotiated and signed a peace treaty with FARC–EP.

Colombia's armed conflict was widespread, with over 90 percent of municipalities suffering victimizations and rural areas being disproportionately affected (see Ibáñez, 2008, and Appendix Figure A.2). In all, between 1985 and 2019, the conflict victimized 8.9 million people and claimed hundreds of thousands of deaths. Figure 1 reports the number of victims by type of victimization caused by the conflict. Nearly 8 million individuals—i.e., 16 percent of Colombia's total population—were forcibly displaced during the internal armed conflict.[8] This represents the world's second-largest number of forcibly displaced people after Syria (UNHCR, 2019). Moreover, 1.2 million people had their relatives murdered or forcibly disappeared as a result of the conflict (henceforth, "indirect" victims). Thousands of others were raped, kidnapped, tortured, injured by landmines, or forcibly recruited as minors.

Throughout the years, the Colombian State has identified and collected information on victims and provided them with humanitarian aid and other services, like food and shelter, in addition to financial compensation. In 1997, the *Registro Único de Población Desplazada*, RUPD, was created, eventually becoming the world's largest and broadest victim registry (Sikkink et al., 2015). Victims declared, under oath, and informed about the facts leading to the victimization. They also reported the date of the victimization and provided some personal information (e.g., name, identification number, date of birth, contact information).[9] In addition, the State established the first victim reparation program that year (Law 387/1997 and Law 418/1997), which gave a handful of reparations to indirect victims. Later, in 2008, Colombia standardized and broadened the victim reparation program (Decree 1290/2008), benefitting some 28,000 people between 2009 and 2011.

The Victims' Law, adopted in 2011, radically expanded Colombia's reparation program for victims in what is considered one of the world's largest and most ambitious peace-building and recovery programs (Law 1448/2011 and Decree 4800/2011). The Victims' Law seeks to award reparations to individuals victimized by guerrilla, paramilitary, or state forces on or after January 1, 1985, by 2031. It is broad in scope: all 7.4 million registered victims with valid contact information are eligible for reparations—i.e., roughly one in seven Colombians. In contrast, no other country has ever sought to compensate more than one percent of its population (Sikkink et al., 2015). In addition to reparations, the law aims to provide truth, restitute dispossessed lands, award humanitarian aid to households in emergency conditions, and enhance access to

[8] A victim of forced displacement is defined as having been forced to migrate from their municipality after being a victim of an attack(s) from illegal groups, or migrating to prevent aggression from such groups.

[9] Once the victim submitted the report, the State had to evaluate within a certain period of time whether the declaration was valid or not. Moreover, victims could be registered in RUPD as a result of a judicial decision. Appendix Figure A.3 plots the number of victimizations reported over time.

micro-credit and subsidized housing.

The reparations consist of one-time, lump-sum payments. Their size, defined in multiples of the monthly minimum wage, depends on the type of victimization suffered and under which regime the reparation is paid (i.e., Law 418/1997, Decree 1290/2008, or Law 1448/2011; see Appendix Table A.1). In particular, the Victims' Law sets the size of the reparation as follows. First, indirect victims and direct victims of kidnap and personal injuries resulting in permanent disability receive the biggest reparation: 40 monthly minimum wages or around US$ 10,000. Next, victims of personal injuries resulting in partial disability, victims of child recruitment, and victims of sexual abuse receive up to 30 monthly minimum wages (US$ 7,600). Lastly, victims of forced displacement—which, as shown in Figure 1, represent the vast majority of victims—receive either 17 or 27 monthly minimum wages (US$ 4,300 or US$ 6,900).[10] If a direct victim is compensated for more than one victimization, they can accumulate reparations up to a total of 40 monthly minimum wages. Instead, indirect victims can receive more when compensated for several murdered or forcibly disappeared relatives.

For most types of victimizations, reparations are assigned at the individual level. However, for indirect victims and victims of forced displacement, reparations are given to the household and shared among the surviving members. Suppose more than one person claims a reparation for a murdered or missing relative. In that case, the intra-household allocation depends on the claimant's relationship with the victim. For example, consider a widow with two children receiving compensation for her murdered spouse. In that case, the widow will receive 50 percent of the reparation and the siblings will share the remaining 50 percent, i.e., 25 percent each, as illustrated in Appendix Figure A.4 (see Appendix Table A.2 for other examples). If the household member who is compensated is a minor, their money is placed in a trust fund and collected once they turn 18.

To manage the delivery of reparation checks and coordinate aid delivery with local authorities, a new agency was created, called *Unidad Administrativa para la Atención y Reparación Integral a las Víctimas*, UARIV. UARIV centralizes, unifies, and updates information from RUPD, local historical records, and other sources into a single database, the *Registro Único de Víctimas*, RUV, which is one of our primary datasets. Registration in RUV is a prerequisite for receiving a reparation.

Due to binding government budget, operational, and technical constraints, Colombia staggered the rollout of the reparation payouts (Article 19 of Law 1448/2011; Conpes 3712, 2011; Conpes 3726, 2012). The central government annually allocates a specific budget for reparations to prevent draining public coffers and ensure financial sustainability. Panel (a) of Figure 2 plots the quarterly number of reparations paid over the last decade. After adopting the Victims'

[10]Before the Victims' Law, forcibly displaced people received 27 monthly minimum wages. The Victims' Law reduced this amount to 17 monthly minimum wages for those who had not registered their victimization by April 22, 2010; instead, registered victims could receive 27 monthly minimum wages. See Appendix Table A.1.

Law, the number of reparations paid to victims significantly increased, reaching over 45,000 reparations paid in 2012q3. By August 2021, over 1.1 million victims had received reparations, totaling 8.4 trillion pesos or US$ 2.2 billion (UARIV, 2021). Our empirical strategy leverages this staggered rollout of reparations by comparing outcomes between eligible victims compensated sooner rather than later and victims who were not compensated at all during our study period.

The Victims' Unit distributes reparations to victims with the allocated resources, but the prioritization process has changed over the years. Certainly, the Victims' Unit has not awarded reparations on a first-come, first-served basis. In practice, indirect victims and disabled victims have been prioritized (Decree 4800/2011), as shown in Panel (b) of Figure 2 (green and blue bars). A more detailed prioritization scheme was defined for the first time in 2013 (Resolution 0223, 8 April 2013), which prioritized victims previously eligible for reparations (as defined in Law 418/1997 and Decree 1290/2008), victims with a terminal illness or a disability, female heads of households with two or more dependents living under the poverty line, victims aged 60 or above under a poverty line, LGBTI victims, victims of sexual abuse, and ethnic minorities. In a surprising turn of events, in July 2013, the Constitutional Court ruled that victims of forced displacement must receive reparations (Decision SU-254/2013), forcing the government to compensate them beginning in 2014 (black bars). Given the colossal fiscal cost of compensating the millions of forcibly displaced victims, the government prioritized those who wished to return to their home of origin (Decree 1377/2014), had a disability, were aged 70 and above, or had already fulfilled their basic needs (Decree 1084/2015).[11] For all other victims, the Victims' Unit prioritized victims who were previously eligible for reparation, victims with a terminal illness or disability, female heads of households living with a sick or disabled dependent, victims aged 70 or above, LGBTI victims, victims of sexual abuse, and indirect victims whose relatives' bodies are being given back to them (Resolution 090/2015).[12]

Despite these efforts at establishing *de jure* prioritization rules, it is unclear whether these rules were systematically followed in practice. The broad criteria, and the lack of detailed socio-economic information in the RUV, made too many victims eligible for reparations, and asking them to provide additional information proved extremely burdensome (UARIV, 2018). For this reason, the government was unable to inform victims in the long waitlist when they could expect compensation (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2018). This led to substantial uncertainty regarding when an eligible victim will be able to receive the cash award, both on behalf of the government and the victims themselves (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2015; ICTJ,

[11] Requiring forcibly displaced victims to fulfill their basic needs sought to avoid them spending the reparation on items like food, housing, healthcare, which UARIV provided to them via humanitarian aid (UARIV, 2018).

[12] In 2019, the prioritization procedure substantially changed (Resolution 1958/2018; Resolution 1049/2019). While this reform took place after our period of study, we leverage the reform-induced variation in assignment to reparations in other work-in-progress (Guarin et al., 2021).

2015).[13] In fact, many compensated victims believed Colombia's current system of prioritizing victims for reparation was random or based on luck (Sikkink et al., 2015).

From the victim's perspective, the process of receiving the reparation is as follows. First, they receive an unexpected phone call from the Victims' Unit. The caller instructs them to attend an "important" meeting at a specified time and location but does not mention a reparation. A few days later, the victim arrives at said meeting (see Appendix Figure A.6). During the meeting, the victim is informed they will receive a reparation and is given a dignification letter (see Appendix Figure A.7). The letter describes when the reparation check can be collected from *Banco Agrario*, Colombia's state bank, which is usually 1–2 weeks later. (Victims are not required to have a bank account to collect the check.) Moreover, the dignification letter includes a message about what the reparation means that reads roughly as follows:

> "*As the Colombian State, we deeply regret that your rights have been violated by a conflict that never should have happened. We know that the war has differentially affected millions of people in the country and we understand the serious consequences it has had—it is impossible to imagine how much pain this conflict has caused. However, from the Victims' Unit, we have witnessed conflict survivors' capacity for transformation over these years. We have witnessed their spirit to keep going, their strength to raise their voices against those who have wanted to silence them, their ability to rebuild their lives... For this reason, with your help, we are working so that you can live in a peaceful Colombia since it is the victims who actively contribute to the development of a new society and a better future.*" [Authors' translation]

Given how the reparation process works in practice and based on substantial additional qualitative evidence, victims are *de facto* unable to anticipate when they receive a reparation. The unanticipated timing of reparation receipt is the second policy characteristic leveraged by our empirical methodology to identify causal effects.

Throughout the debates in Congress, opponents of the Victims' Law expressed their concerns regarding the high fiscal cost of the reparations and their potential misuse by recipients.[14] To assuage these fears, the government "labeled" the cash transfer: reparations were presented as seed money for victims to transform their lives (*indemnizaciones transformadoras* or "transformative reparations"); specifically, using the reparation to invest in postsecondary attendance, create and strengthen small businesses, and acquire housing or improve housing conditions:

[13] There is some evidence that the government tries to spread reparations across departments (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2018). Given the wide across-department variation in the number of victims, this implies a large spread in the share of compensated victims (see Figure A.5).

[14] Similar concerns had been raised by state authorities regarding reparations awarded by Decree 1290/2008. After interviewing a portion of recipients, in 2010 the Ombudsman's Office of Colombia believed that reparations had not made sizeable impacts in recipients' living standards because recipients did not know how to invest them and, instead, spent the money on fulfilling basic needs or paying off old debts (Defensoría del Pueblo, 2010).

"The National Government, through the Victims' Unit, will implement an accompaniment program to promote an adequate investment of the resources the victim receives as reparation to rebuild their life project, mainly oriented towards:

1. *Technical or university training for victims or their children.*
2. *Creating or strengthening productive enterprises or productive assets.*
3. *Acquiring or improving new or used housing.*
4. *Acquiring rural real estate."*

—Article 134 of Law 1448/2011 [Authors' translation].

Reparations were proposed as a tool for development to be invested in self-sustaining, income-generating activities, much like micro-finance development interventions (Vallejo, 2019). The government galvanized recipients to use the reparation wisely through a process of education, training and planning around finances and small investment opportunities. Starting in 2013, the government held fairs at the time of reparation disbursements to connect recipients with local public and private institutions laying out options in education, housing, land and small businesses (*feria integral de servicios*). Victims could also voluntarily participate in investment workshops, where they would receive training in budgeting and investing, including getting help to obtain small business or student loans and pay off old debts (*programa de acompañamiento de inversión adecuada de los recursos*). While in principle this would mean that our estimator conflates the effect of money and these additional services, it is worth noting that, in practice, less than 10 percent of victims compensated between 2016 and 2019 attended these workshops (UARIV, 2019). As a result, our estimated coefficients can be interpreted as the effect of money.

3 Data and Methodology

3.1 Data

We merge comprehensive national administrative panel microdata to measure effects on numerous outcomes. In particular, our data comes from the following nine sources:

1. **Eligible victims and treatment:** We use microdata from the universe of registered victims, RUV, and all compensated victims, Indemniza, provided by UARIV. RUV is a unified and centralized registry covering all individuals ever reporting to have been victimized during the Colombian internal conflict by August 2019. RUV has detailed information regarding victims' household demographics, the type of victimization, the date and municipality of the victimization, and the date and municipality of victimization registration. In

addition, Indemniza has information regarding the financial reparation size and the date and location of the payout.

2. **Household sociodemographics and composition:** We use microdata from the Department of National Planning's *Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales*, SISBEN, the main instrument used by the government to target social welfare program recipients. The scheme uses data from a proxy-means survey to assign households a single and continuous score from 0 to 100 (poorest to richest) based on housing quality, possession of durables, public utility services, and human capital endowments, among others. Over 25 million individuals—more than one in two Colombians—were included in SISBEN in 2010.

3. **Formal employment and earnings:** We use social security records from Colombia's Ministry of Health and Social Protection's *Planilla Integrada de Liquidación de Aportes*, PILA. This dataset represents the census of all individual-by-month contributions to healthcare, pension funds, and workers' compensations. The information is available for both formal workers—that is, both wage-earners and self-employed individuals—from 2010q1 to 2019q4. Critically, however, it excludes non-employed individuals and informal workers. This is an important caveat because one in two workers in Colombia is informal (DANE, 2019). Each monthly PILA dataset has detailed information on payroll, earnings, days worked, and other information for over 10.5 million individuals and more than 300 thousand firms.

4. **Entrepreneurship and business survival:** We use microdata from the Colombian Confederation of Chambers of Commerce (*Confecámaras*) firm registry or *Registro Único Empresarial y Social*, RUES. In Colombia, firms must obtain a license (*matrícula mercantil*) from the local Chamber of Commerce. This license is required for many regular business activities, including access to credit, subsidies, and government training programs. A fee of US$ 13 to US$ 638 is charged for obtaining this license, depending on the value of the firm's assets.[15] Firms must renew their license every year by March 31.[16] Notably, .Colombia is unique in its firm registration requirements: no other Latin American country requires all firms to register nor renew their license every year (Salazar et al., 2017). For instance, in 2018, there were roughly 1.5 million firms with an active license in Colombia. Having a license is a strong indicator of the initial decision to engage in the formal economy in Colombia and, in fact, many studies define firm formality in Colombia as whether or

[15] In addition, firms generally have to pay firm registration taxes to the state department (state), as well as payroll taxes, value-added taxes, and corporate income taxes to the central government. However, new firms created between 2010 and 2016 with less than 50 employees and assets worth less than 5,000 monthly minimum wages were exempt from the license fee and enjoyed significant tax benefits (Law 1429/2010).

[16] Failure to renew a license can result in heavy sanctions (17 monthly minimum wages in penalties, or closing business if license was not renewed for more than 5 years).

not a business has this license (Cárdenas and Mejía, 2007; Galiani et al., 2017; Ydrovo-Echeverry, 2010). Since 2011, *Confecámaras* has centralized and unified the business registry and renewal data for all the 57 local Chambers of Commerce in Colombia. Our dataset includes firms that obtained or renewed their license between 2011 and 2018.

5. **Access to and use of credit:** We use data from the universe of formal loans managed by the Financial Superintendence of Colombia (*Superfinanciera*), format number 341. This individual-by-quarterly-level longitudinal dataset contains detailed credit information for the universe of individuals in the formal financial sector. In addition to identifying all individuals with a formal loan, this dataset also includes end-of-quarter snapshot information about the type of institution providing the loan (e.g., bank, cooperative), the type of credit (e.g., microcredit, consumption), credit risk scores, collaterals, loan size, interest rate, and debt terms, among other variables. Student loans are excluded from this dataset. The information is available from 2008q1 to 2019q4.

6. **Land and home ownership:** We use cadastral data covering all real estate transactions in almost all municipalities in Antioquia from *Dirección de Sistemas de Información y Catastro*, managed by Catastro Antioquia.[17] Antioquia, the second-largest department by population, provides an interesting case study because it has suffered acutely from violence: it accounts for one-fourth of victims and one-fourth of reparations in Colombia. The dataset includes information on land and home characteristics and values based on cadastral records for all transactions between 2011q1 and 2019q4.

7. **Health care utilization:** We use panel microdata from the Ministry of Health and Social Protection's *Registro Individual de Prestaciones de Salud*, RIPS. RIPS is a national database of healthcare service use that captures data on medical visits, diagnostic and therapeutic procedures, and other services for every patient in Colombia. Healthcare providers (primary care physicians, hospitals, clinics) report detailed information about their patients (e.g., ID number, sex, age, user type, municipality of residence). They also report every patients' medical appointment, emergency visit, hospitalization, procedure, and diagnosis. The four-digit codes for diagnosis comply with the International Classification of Diseases (ICD-10). A wealth of other information about the service is provided, including the price of the consultation(s) and procedure(s) charged by the healthcare provider. The information is available between 2009s1 to 2019s1.

8. **Postsecondary attendance:** We use panel microdata from the Ministry of Education's *Sistema para la Prevención de la Deserción en la Educación Superior*, SPADIES, which

[17] We have information for 120 of Antioquia's 125 municipalities. We do not observe real estate transactions in the following four municipalities: Copacabana, El Retiro, Rionegro, and Medellín (they have a separate cadastral information system). Moreover, we do not observe transactions for Murindó (no data is available).

tracks students along the postsecondary education system. The data provides a wealth of individual-by-semesterly level information on student observable characteristics, including enrollment status, HEI, major of study, the share of courses passed, and graduation or dropout status. The average postsecondary enrollment for the observed period is around 2 million individuals per semester and the data covers roughly 90 percent of all these postsecondary enrollees; information from a handful of institutions is omitted due to poor or inconsistent reporting. We use SPADIES data from 2006 to 2016.

9. **High school graduation and test scores:** We use data from the *Instituto Colombiano para el Fomento de la Educación Superior*, ICFES, the institution in charge of standardized testing, including Colombia's national standardized high school exit exam, called SABER 11. SABER 11 is taken by virtually all high school seniors in Colombia, regardless of their postsecondary intentions, and is often used as a proxy measure for high school graduation. In addition to information on individual test scores, the dataset includes sociodemographic information (e.g., socioeconomic stratum, parental education, municipality of residence) for all the 6.7 million test-takers between 2010 and 2019.

We link individuals across these nine separate datasets using social security numbers (the Colombian *cédula de ciudadanía* or *tarjeta de identidad* for minors). When this information is not available, we use the date of birth, municipality, household composition, and personal names.

3.2 Methodology

We exploit the staggered rollout of reparations to victims and the unanticipated timing of receiving a reparation to identify causal effects using an event study approach. We define "event time" d_i as the date at which individual (or household) i receives the reparation and $D_{it}^k = 1(t = d_i + k)$ as an indicator variable that equals one if the individual (or household) received the reparation k periods ago (where k may be negative).[18] Our event study model compares outcome y around event time across individuals using the following OLS specification:

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=\underline{C}}^{\bar{C}} \beta^k D_{it}^k + u_{it} \tag{1}$$

where α_i is an individual fixed effect, γ_t is a calendar time fixed effect, $\bar{C} > 0$ and $\underline{C} < 0$ are constants, and u_{it} is the error term. The β^k coefficients are our main parameters of interest and provide estimates of the mean outcome in event time after having taken out the individual- and time-specific effects. We normalize the first lead to be zero to interpret the effects as relative

[18]The date will be measured in quarters, semesters, or years, depending on the database. For instance, college attendance in SPADIES is measured in semesters, while Confecámaras measures firm survival annually.

to event time -1. Standard errors are clustered at the household level. We do this because, as previously described, reparations can be assigned to the household; such is the case for reparations to indirect victims and victims of forced displacement.

Note that the inclusion of individual fixed effects in specification (1) controls for time-invariant personal characteristics that may be correlated with reparation receipt (e.g., sex, victimization type). As with a difference-in-difference design, the identification requires parallel trends between treated and control groups in the absence of reparation, which we can support empirically by testing for pre-event parallel trends.

Because only a fraction of victims has received reparations by 2019, we can compare changes in the outcomes of treated units with those of control units that have not yet received the cash reparation ("eventually treated") and units that will not receive a reparation during our period of study ("never treated"). In the main text, we present results using the latter comparison for most of the results and report the results using former comparison in the Online Appendix. The two analyses lead to quantitatively similar conclusions.[19]

The announcement and adoption of the Victims' Law may have generated the expectation of receiving money among victims. However, only around 1 percent of eligible victims received reparations every year, on average. Indeed, the expected probability of receiving a reparation is very small because the government budget, technical, and operational constraints were binding. Incidentally, this implies that the likelihood of reparations triggering general equilibrium effects is low. Moreover, the feature of the design we exploit for identification relies on *non-differential* anticipation between victims who receive reparations sooner versus later.

3.3 Summary Statistics of Victims and Recipients of Reparations

Table 1 presents the descriptive statistics for our data on victims and beneficiaries of reparations. Column (1) shows the information for all 8.9 million individuals victimized in the Colombian internal armed conflict, as registered in RUV. 50.1 percent of these victims are female. By 2019, 25.7 percent are minors, 62.0 percent are aged 18 to 60, and 12.3 percent are aged above 60. The average victimization took place 16.6 years before 2019, that is, *circa* 2002. 87.8 percent of victims suffered forced displacement, 3.6 percent are direct victims of homicide or forced disappearance, and 9.8 percent are indirect victims. (These victimization shares do not sum to 100 percent because victims can report more than one type of victimization.) Lastly, 12.7 percent of victims are members of an ethnic minority group and 4.0 percent have a disability.

In practice, some of these victims in RUV will not be able to receive a reparation because they are unreachable to the government. For instance, they may not have included their ID number (*cédula de ciudadanía*) or their contact information when registering their victimization.

[19] We were unable to match never-treated victims to the Catastro and RIPS datasets, given their very large size. For outcomes that use these datasets, we report the results only for eventually treated victims.

Column (2) restricts the sample to the 7.7 million victims that have valid contact information. These victims are very similar in observable characteristics to the universe of victims in Column (1). Column (3) further restricts the sample to eligible victims by excluding direct victims of homicide or forced disappearance who are, by definition, deceased or missing. Further, their distant relatives (e.g., grandparent, sibling, aunt, cousin) do not receive a reparation on their behalf when there is a spouse, child, or parent present because immediate family is prioritized (see Appendix Table A.2). This results in a sample of 7.4 million victims eligible to receive reparation.

Column (4) presents descriptive statistics for the eligible victims who have received a reparation by June 2019, when we obtained the RUV data from UARIV. Almost 822,000 victims had received a reparation by this date. Reflecting the prioritization scheme, relative to the eligible victims in Column (3), reparation recipients in Column (4) are more likely to be female, disabled, older than 60, and indirect victims. Further, because victims deemed eligible for reparations in Law 418/1997 and Decree 1290/2008 received priority, compensated victims are victimized longer ago than other eligible victims.

For many outcomes, we are interested in estimating the effects on the household and/or on the minors, who do not receive a reparation before turning 18. However, the Victims' Registry does not have updated information on household composition. Because victims are particularly prone to household reconfiguration, it is uniquely challenging to link individuals to households using household information from RUV, as household composition most likely changed from the date they registered the victimization to the date of reparation payout. For instance, there are many deceased relatives and forcibly displaced families who no longer live under the same roof. Because of this, we link individuals to households by finding them in SISBEN in 2010, immediately prior to the adoption of the Victims' Law. Being the main government instrument to target social welfare program recipients, SISBEN also provides rich sociodemographic information about individuals. Moreover, SISBEN enables us to recover victims' national identification numbers, which we then use to link victims across datasets. We merge the two datasets using victims' date of birth, municipality, and first names.[20] If no match is found, we merge individuals when two or more members of their household share the same date of birth in the two datasets.[21]

Column (5) presents summary statistics for the 25.8 million individuals appearing in SISBEN in 2010. Column (6) shows we identify almost 3 million eligible victims in SISBEN in 2010, that is, two-fifths of those from Column (2). Relative to the average individual in SISBEN from Column (5), victims identified in SISBEN are more likely to be female, younger, less educated, and poorer (both measured in SISBEN score and household income). They also

[20] We match the location where the household was surveyed in SISBEN to the municipality in RUV where either (i) the victimization took place, (ii) the victimization was registered, or (iii) the victim was forcibly displaced to.

[21] This implies that we cannot identify the 5 percent of victims that do not have information on the date of birth, nor victims without information on household structure, as is the case for some indirect victims.

are more likely to live in rural areas and have larger families. In addition, they are less likely to participate in the labor force, have a job, and be formally employed. Thus, victims identified in SISBEN represent a remarkably vulnerable population, even relative to the average person in SISBEN.

Columns (7) and (8) report descriptive statistics for recipients of reparations identified in SISBEN in 2010. Victims who received financial reparations are very similar in observable characteristics to all victims identified in SISBEN (Column 7 relative to Column 6). This is an important insight, as the population in Column (6) includes the control sample in some of our specifications. Notwithstanding, and reflecting the prioritization rules, recipients are older, more likely to be disabled, and were victimized earlier on. They are also more likely to be an indirect victim.

Finally, 87,668 recipients have missing information about the date they received the financial reparation, i.e., the "event time" in our event study design described in Section 3.2. The overwhelming majority of these cases are minors since, by law, financial reparations to underage victims are paid out only once the victim turns 18. Column (8) presents the descriptive statistics for this final sample of 262,136 recipients for which we observe the date of reparation payout, which will be the main treated sample in our estimation strategy. As expected, the main difference between this sample and all victims receiving reparations is that the former excludes minors, for which reparations have not yet been distributed.

Table 1 further shows that the reparations were large, especially relative to victims' mean income. The last rows of Column (8) show that, on average, recipients received 11.7 monthly minimum wages in financial reparations. With the average household income being three-quarters of the monthly minimum wage, a household compensation of 29.575 monthly minimum wages represents roughly 39.4 times the household monthly income, that is, 3.3 times their annual household income. Reparations were therefore substantial in magnitude. Lastly, given the aforementioned challenge of determining household structure at the time of reparation receipt, the last row in Table 1 re-estimates average household reparation using the household structure as observed in SISBEN in 2010, the year prior to the Victims' Law. As new households are formed (likely with individuals who have not received a reparation), average reparations received by households drop from to 19.0 monthly minimum wages or 2.1 times recipients' annual household income. In some analyses below, we use these SISBEN-defined household reparations to estimate the effects of cash awards on outcomes for minors.

4 Results

We are interested in understanding the effect of reparation on victims' well-being across the life cycle. We divide our analysis into three parts. First, we examine effects on work and living standards. Next, we study health care utilization. Finally, in Section 4.3, we study impacts on human capital accumulation among the children of victims.

4.1 Work and Living Standards

This section examines whether reparations impact work and living standards. First, we test whether reparations affect victims' employment, occupational choice, and earnings using social security records. We then examine impacts on entrepreneurship and business activity, leveraging information from Colombia's business registry. Some entrepreneurs, however, may decide to keep their business informal and not register it. For this reason, we complement the analysis using census-level panel microdata on microloans. Next, we study how reparations affect consumption patterns using census information on all credit card debt and automobile loans from banks and financial institutions. Lastly, we examine impacts on land and homeownership with cadastral records, using Antioquia Department as a case study.

4.1.1 Employment and Occupational Choice

We first examine whether the reparation, by providing a large wealth shock to victims, discouraged labor supply. To estimate effects on employment, we use information from social security contributions covering all formal workers in Colombia. We focus on the working-age population, defined as those aged 18 to 55 in 2010.

Figure 3 plots the likelihood of being formally employed in the quarters leading to and after reparation payout. The vertical red dashed line marks the event, i.e., the period in which the money is paid. The x-axis shows time relative to the event, and the y-axis plots the event study coefficients from Specification (1), which are expressed in percentage terms relative to the average outcome in 2014q1 for "never-treated" victims, i.e., those that never received a reparation during our period of study. Two striking results emerge from this figure. First, the series rule out pre-event trends: the difference in formal employment between treated and control victims is not statistically significant before the reparation payout, which supports our identifying assumption. Second, formal work falls by 0.55 percentage points the quarter after reparation payout ($k = 1$). Relative to a baseline mean of 16.4 percent of never-treated victims having formal work, this represents a 3.3 percent drop in formal employment. While the coefficient is precisely estimated and highly statistically significant, it is economically small. Moreover, the effect persists over time: 12 quarters after reparation, formal employment is still 1.6 percent below the rate immediately before the reparation payout. Table 2, which summarizes the estimated coefficients by event time bins, reports an average drop in formal employment of

2 percent 9 to 12 quarters after reparation.[22]

Further, Table 2 decomposes the drop in formal employment by type of occupation, i.e., wage- versus self-employment. While formal *self*-employment remains roughly constant after treatment, *wage* employment drops after reparation and remains 1.68 percent lower even three years later (see also Appendix Figure A.10). The number of days of salaried work also drops by 2 percent in the longer run. Notwithstanding, victims who remain employed *increase* their daily wages: Table 2 and Figure 4 show that the average daily wage is nearly 1 percent higher as soon as the victim receives the reparation. The coefficient rises in magnitude and significance over time, reaching 3.11 percent 9 to 12 quarters after reparation payout. Since, instead, the drop in formal wage employment stabilizes after $k = 1$, the higher long-run wage rate cannot be driven by selection but, rather, by an improvement in the quality of the jobs for victims—a finding we turn to below.[23] Consequently, the reparation induces a marginally-significant 2.05 percent *increase* in formal earnings three years after the reparation payout. The implied long-run marginal propensity to earn out of unearned income is close to zero and non-significant.

To understand who leaves formal wage employment, we decompose the effect by occupational risk and the wage rate. First, we compare wage employment for low- and high-risk occupations. We define the level of risk leveraging information from Colombia's workers' compensation insurance system. Employers are required to pay for their employees, and the contribution rate is occupation-specific. Some occupations, like education and retail, have a contribution rate of 0.5–1 percent of the worker's salary. Other occupations, like manufacturing, construction, and mining, have a contribution rate above 2.4 percent. We define "low-risk" and "high-risk" occupations if the contribution rate is below or above the median, respectively. Column (1) of Table 3 shows that most victims work in high-risk occupations. Column (2) rules out pre-event trends. Columns (3) through (6) show that, while reparation does not affect employment in low-risk occupations, it induces victims to shift out of high-risk occupations: there is an immediate 1.7 percent drop in employment in high-risk occupations that increases to over 3 percent three years after reparation payout (see also Appendix Figure A.12).

Table 3 further decomposes the effects between low- and high-pay salaried jobs. We define a low-paying job as paying the legal monthly minimum wage or less and a high-paying job as paying more than that. Two-thirds of victims with formal salaried work are in low-paying jobs.

[22] The results are quantitatively identical when we use a two-way fixed effect regression and include only the not-yet-treated individuals as the comparison group (see Appendix Figure A.8). Moreover, we results are also robust to using the Callaway and Sant'Anna (forthcoming) valid inference procedures for staggered adoption and limited treatment anticipation (see Appendix Figure A.9). Accounting for heterogeneity in the effects does not quantitatively affect our results.

[23] Two additional exercises help us rule out composition effects driving the increase in daily wages. First, we regress people's baseline characteristics on time fixed effects and the event time dummies to see whether and how covariates change among salaried victims. Appendix Table A.3 shows that the victims who remain in salaried work are not different in their baseline education levels, income or Mincer equations' predicted wages, suggesting compositional effects do not drive the higher wages. Second, we run a regression using Specification (1) for a balanced sample of salaried workers. Appendix Figure A.16 shows that the increase in wage rates also replicates for this sample.

Moreover, the probability of having a low-paying job drops by 2.72 percent in the quarter of reparation payout, and this drop remains highly significant even three years later. In contrast, the effect on high-paying jobs is not statistically significant at conventional levels (see also Appendix Figure A.13).

Finally, the last rows of Table 3 show the results when interacting the two sets of job risk and pay indicators. Consistent with reparations shifting workers out of high-risk and low-paying jobs, we find a 3.98 percent drop in the likelihood of being employed in a high-risk and low-paying job three years after reparation. (Victims also shift out of high-risk, high-pay and low-risk, low-paying jobs, but we are underpowered to detect significance for those categories.) In contrast, the likelihood of having a low-risk, high-paying job is 6.3 percent *higher* three years after reparation—a point we return to later on. Notwithstanding, because very few victims have these high-quality jobs at baseline, the overall effect is a shift out of low-quality jobs. This finding could be consistent with individuals responding to the cash windfall by leaving the labor force, spending more time unemployed, or switching to informal jobs. We dedicate the remainder of this subsection to investigating each of these possibilities.

First, we examine whether workers who receive reparations choose to retire or invest in their education by decomposing the effect by age group. Younger workers—defined aged 18 to 39—are significantly more likely to drop out of formal work (see Appendix Figure A.14). In contrast, the effect is close to zero and not statistically significant for older workers. This suggests that retirement is not the main driver of the employment drop. Instead, it is possible that younger victims leave their (low-quality) jobs to invest in their human capital, a hypothesis we test and indeed confirm in Section 4.3.

Second, the shift out of low-quality jobs may also be driven by improved outside options. For example, an increased reservation wage would extend workers' search for better jobs, raising their unemployment spell. The last row of Table 2 tests whether the reparation affected salaried workers' out-of-formal-employment spell, defined as the number of quarters since the last time they were observed in a salaried job.[24] Consistent with improved outside options, there is a persistent increase in out-of-formal-employment spells, and this effect remains highly significant even three years after the reparation payout. This, coupled with the above-documented increase in daily wages, suggests that reparations improve the quality of jobs people take on.

Third, victims can choose to switch from *formal* wage employment to *informal* employment, defined as not contributing to social security. Indeed, informality is particularly pervasive in our context: Table 1 showed that less than one in three working victims identified in SISBEN were in the formal sector. Moreover, if money makes salaried workers shift to *self-employment*, as some previous work has found (Blattman and Dercon, 2018; Falco and Haywood, 2016), they may likely do so without contributing to social security. Indeed, Table 2 showed that formal self-

[24]We cap this variable at 8 quarters to avoid the distortion implied by those that leave the labor market permanently.

employment is rare among victims: the baseline control mean is only 1.56 percent. Our results appear to be consistent with this self-employment channel. As we document below, victims invest the lump-sum grant in creating new businesses. This shift to self-employment may well occur without victims contributing to social security.

Indeed, the reparation can directly foster entrepreneurship by alleviating liquidity and credit constraints, helping people afford the startup costs of establishing a new business. This symbiotic relationship between money and entrepreneurship is particularly relevant in our setting since the delivery of the reparation checks was permeated with a message of economic accountability, microfinance, and small-business creation (Vallejo, 2019). Victims were encouraged to invest the money from reparation in income-generating activities.

We use two different sources of microdata to investigate impacts on entrepreneurship. First, we utilize data from the national firm registry managed by Colombia's Confederation of Chambers of Commerce. Since firms are required by law to obtain a license from a Chamber of Commerce to operate, we generate a dummy for whether a person registered a new business to proxy for formal entrepreneurship. Active firms must renew their license every year, enabling us to estimate impacts on firm survival by examining whether the license has been renewed in a given year. Some entrepreneurs, however, may decide not to register their new business and keep their firm informal. For this reason, we complement the analysis using census data on all microloans owed to banks and financial institutions from Colombia's Financial Superintendence. Microcredit is aimed mainly at self-employed professionals and (formal and informal) small entrepreneurs. Thus, we view any uptake of microfinance as reflecting an increase in self-employment and entrepreneurial activity.

Figure 5 plots the likelihood of registering a new business in the quarters leading up to and after reparation payout. The figure shows that reparation stimulates formal entrepreneurship: the probability of registering a new business increases by 37 percent a quarter after cash payout, and this effect is significant at the 1 percent level. This represents a sizable impact on formal business creation since only 0.17 percent of control victims register a new firm in a given quarter. This suggests that liquidity is a crucial barrier to entrepreneurship, with reparations helping victims afford the costs associated with starting (and formalizing) a business.

Moreover, by injecting capital into existing businesses, reparation can also extend firm survival. Figure 6 shows that reparations also improve business survival, measured as having a firm with an active license. The likelihood of having an active business increases immediately after reparation payout. The magnitude of the effect remains large and statistically significant over time: the probability of having an active business is 14.1 percent larger three years after payout, reflecting the importance of resource misallocation when financial constraints are binding.

As mentioned above, reparations may encourage entrepreneurship, but individuals can choose not to register their new business with a Chamber of Commerce. While having a business license does not necessarily imply paying taxes, the pecuniary and non-pecuniary costs of obtaining this license can discourage firm formality. Moreover, firms in Colombia often do not perceive sizable benefits from formalizing (Galiani et al., 2017). Thus, if reparation induces people to establish a new business—whether formal or informal—our estimates using firm registration represent a lower bound on the overall impact on entrepreneurship and business survival. If reparation also induces some existing informal firms to formalize, then Figures 5 and 6 will conflate the two effects. We therefore complement the analysis by exploring the effect of reparations on the use of microcredit for self-employed professionals and small entrepreneurs.

Table 4 and Appendix Figure A.19 report the effects of reparation on the use of microcredit. Around 8.9 percent of control victims owe some microcredit in 2014q1. The money from reparation enables victims to pay off their microloans: the probability of owing any microcredit drops by 4.8 percent the quarter they receive the reparation ($k = 0$). This effect drastically changes over time. One year after receiving reparation, victims are just as likely to owe any microcredit than before. Two years after reparation, the money has *raised* the likelihood of owing any microcredit by 3.2 percent. There are similar effects on outstanding balance (including zeros for people who do not owe any microcredit). Three years after payout, the amount of microcredit owed has increased by 9.5 percent.[25] Together, these results imply that victims use the reparation to pay off old debt and, over time, they use microcredit more intensively. If microloans funded unproductive investments, the more intensive use of microcredit would lead to more delinquency. Instead, Table 4 shows the opposite takes place: delinquency drops significantly and permanently after reparation (see also Appendix Figure A.21).

Taken together, we conclude that reparations have a small (but precisely-estimated) negative effect on formal employment, driven by salaried workers shifting out of risky and low-paying jobs. Because of improved outside options, reparations improve the quality of victims' jobs and raise their daily wages. Some victims shift into self-employment and entrepreneurship, with victims using the money to create and strengthen their small businesses. Indeed, the wealth shock enables victims to access the credit market, improve their credit history, and take on larger loans. We conclude that reparations fund productive investments and income-generating activities.

4.1.2 Consumption and Land and Homeownership

This section investigates how reparation affects victims' consumption patterns. We use two distinct datasets to gauge consumption. First, we leverage information on consumer debt—credit card debt and automobile loans, the two most common categories—from Colombia's Financial Superintendence. Second, we estimate impacts on land and homeownership, which the Victims' Law explicitly sought to encourage. To do this, we leverage detailed information

[25] Given 8.9 percent of never-treated victims owe *any* microcredit in 2014q1, this means the average microloan among those who have a positive amount is 1,643 USD (=145.6/0.0886).

on real estate transactions taking place in the department of Antioquia, one of the country's largest departments, and that accounts for a disproportionate number of victims and reparation recipients.

Table 4 reports the effects of reparations on consumption, as proxied by credit card debt. Immediately after receiving the money, victims can pay off old credit card debt: the likelihood of owing any debt drops by 5.62 percent in $k = 0$, while the outstanding balance (which includes zeros for people without credit card debt) falls by 10.82 percent. However, both the magnitude and the sign of these coefficients change drastically after the initial reduction (see also Appendix Figure A.22). Two quarters after reparation payout, the effect on credit card debt is close to zero and non-significant; by $k = 4$, the effect is positive and highly significant, suggesting reparations *raise* consumption through the credit market in the longer run. For instance, outstanding balances are 32 percent larger three years after the reparation.

Moreover, we can infer people's consumption from the credit card records by adding the observed quarterly payments to the change in the outstanding balances (excluding interest). Money has a long-term effect on inferred consumption. Three years after the cash payout, inferred consumption has increased by 38.9 percent.

Table 4 shows that the reparation had similar positive effects on consumption of durable assets; specifically, motorized vehicles like cars and motorcycles, which we can observe using data on auto loans. Since most Colombians use loans to buy motor vehicles (Fasecolda, 2014), greater usage of automobile loans would suggest more automobile ownership. Indeed, the cash windfall induced victims to take on loans to purchase motorized vehicles. Three years after the reparation payout, victims owe 61.55 percent more in auto loans (see also Appendix Figure A.24).

Next, we focus on land and homeownership. Many victims of forced displacement lost their land and home due to the conflict, and the reparation program explicitly labeled the reparation as a tool for victims to purchase land or a house. To measure land and homeownership, we use data from Colombia's department of Antioquia, where we observe all real estate transactions from 2011q1 to 2019q4 from cadastral records. Antioquia, the country's second-largest department, provides an interesting case study because it has suffered acutely from the internal armed conflict: it accounts for one-fourth of all victims and one-fourth of all reparations.

Figure 7 presents the effect of reparations on the household's cumulative number of land and home purchases in the quarters before and after any member receives a reparation. Before the reparation payout, households have on average 0.1 cumulative real estate purchases, i.e., roughly a 10 percent chance of having purchased real estate since 2011q1. The coefficient increases after reparation and becomes statistically significant three quarters later, indicating victims use the money to buy land or a home. Two years after the household first received reparation, the cumulative number of land or home purchases by victim recipients is almost

16.9 percent higher.

In sum, reparations provide short-term debt relief and increase consumption. Moreover, they enable victims to invest the money in real estate—a particularly relevant margin of response given the intentions of Colombia's reparation program.

4.2 Health Care Utilization

This section examines how reparations affect health care utilization. The effect of a positive wealth shock from reparation on health care utilization is theoretically ambiguous. On the one hand, money could *increase* health care utilization if it enables a financially constrained individual to afford contact with the healthcare system. On the other hand, money can indirectly improve a person's health condition and therefore *reduce* utilization by improving their work environment, health behaviors (e.g., excessive alcohol consumption, smoking), nutrition, or mental well-being (e.g., stress).

There are four main reasons why the positive wealth shock from a reparation likely reduces health care utilization in our setting. First, there is universal healthcare, and over 95 percent of Colombians have access to health care services, and only 2 percent report unmet health care needs (OECD, 2015). Second, the basket of health services is the same for those in and out of formal employment. Third, out-of-pocket health spending is low, even compared to rich countries: 16 percent in Colombia compared to an OECD average of 20.6 percent (OECD, 2019).[26] Lastly, the system as a whole is not adversely biased toward hospital care: 85 percent of health system contacts in Colombia are ambulatory, only 10 percent are emergency services, and only 5 percent are hospitalization (OECD, 2015).

We report estimates of the impact of the reparation on three types of health care utilization: emergency department (ED) visits, hospitalizations, and procedures. We define treatment at the household level; an individual is treated if anyone in their household receives a reparation. The event time is the first date on which a household member received reparation.

Table 5 reports the likelihood of any ED visit in the period leading up to and after reparation payout. ED visits are relatively rare: only 2.67 percent of treated victims visited an ED the semester before receiving reparation. Reparation reduces the probability of an ED visit by 5.3 percent the year after reparation payout, and this coefficient is highly significant (see also Panel (a) of Figure 8). The effect is compounding over time but stabilizes such that, four years after reparation, ED visits are 15.9 percent less frequent.

To understand the drivers of this reduction in ED visits, Panel (b) of Figure 8 decomposes the effect by primary diagnosis (the coefficient on ED visit for each diagnosis—in percentage points—is divided by the likelihood of any ED visit at $k = -1$). The drop in ED visits a year after

[26]Out-of-pocket expenditure is low partly because enrollees of the subsidized regime make no co-payment for services if they belong to SISBEN I (most vulnerable households) and pay only a 5 percent coinsurance rate if they belong to SISBEN II (OECD, 2015).

reparation si explained by infections, musculoskeletal illnesses, and poorly-defined conditions. Improvements in work and living conditions, like the ones we documented above, could drive this effect. Over time, reparations also lower ED visits from circulatory, genitourinary, or digestive system conditions, possibly due to improvements in health behaviors (e.g., dietary habits) that take longer to emerge.

Next, Table 5 reports the effect of reparation on the likelihood of being hospitalized in a given semester. Hospitalization is an even rarer outcome: less than 1.2 percent of treated victims were hospitalized the semester before reparation. Reparation reduces hospitalization: two years after reparation payout, there is a 12.8 percent reduction in hospitalization (see also Figure 9). Again, the effect is compounding over time: four years after reparation, hospitalizations have dropped by 22.5 percent.

The last row of Table 5 plots the effect of reparation on the number of medical procedures in a given semester. On average, victims had 0.93 procedures done in the semester before reparation. Consistent with improved health, the number of procedures drops significantly after reparation; four years after the event, procedures have dropped by 22.1 percent.

In sum, our results on health care utilization are consistent with reparations significantly improving victims' health. Both ED visits and hospitalizations—which most likely indicate adverse health conditions—significantly drop after reparation, with compounding effects over time. Similarly, medical procedures also fall following reparation. We view these health results as reflecting changes in intermediate outcomes, like changes in dietary habits or access to clean water, that may not be captured using administrative data but which affect health conditions and physical well-being.

4.3 The Intergenerational Impacts on Human Capital Investments

Having documented the effects of reparation on adult recipients, we next turn our attention to estimating the impacts of reparations on the victims' children. Specifically, we test whether victims invest the reparation in the next generation's human capital. First, we estimate impacts on college access and persistence. Then, we show the effects on high school graduation and test scores.

4.3.1 College Attendance

The Victims' Law was explicit about wanting victims to invest the money from reparation in postsecondary education. Therefore, in this section, we test whether reparations improved postsecondary attendance.

Postsecondary institutions in Colombia offer four- or five-year undergraduate programs and/or two- or three-year technical and technological programs. The admission cycle takes place every semester: two cohorts graduate from high school every year, meaning a new cohort of prospective students applies, receives admission, and enrolls in college each semester.

We first study enrollment in an undergraduate program in a given semester. For this outcome, we adjust specification (1) in three ways. First, we restrict the sample to individuals aged 15 to 25 the semester before reparation receipt.[27] Second, since college attendance is strongly correlated with age, we include age fixed effects in the regression specification. Third, we define treatment at the household level; that is, an individual is treated if anyone in their household receives a reparation. We do this because minors do not directly receive reparation; their money is deposited in a fiduciary account. Moreover, their schooling choices and financial ability to pay for tuition largely depend on other household members (e.g., their parents). Since we use data from SISBEN in 2010, household composition refers to that observed the year before the adoption of the Victims' Law.

Figure 10 plots enrollment in a four- or five-year undergraduate program in the semesters leading up to and after reparation payout. Three results emerge from this figure. First, there is a large and persistent increase in enrollment after the cash windfall. The effect is relatively small and marginally significant at $k = 0$ but large and highly significant the semester after payout; we observe an 11 percent increase from a base of 2.5 percent among never-treated victims aged 15 to 25 at $k = 1$. Arguably, it takes one semester for money to boost undergraduate attendance, as applying and receiving admission is a prerequisite for enrollment. Second, part of the effect at $k = 1$ is driven by a gain in *access*: the likelihood of first-time attendance increases by 24 percent at $k = 1$ (see Appendix Figure A.26). Third, reparations also improve undergraduate *persistence*: the magnitude of the enrollment effect is rising over time and reaches 18.3 percent four semesters after the payout.[28,29]

The enrollment gains from the cash windfall are likely to vary depending on how tightly financial constraints bind. While private institutions in Colombia can be prohibitively expensive for low-income individuals, public institutions charge low tuition fees thanks to government subsidies (Ferreyra et al., 2017; Londoño-Vélez et al., 2020). Thus, if reparations relax financial constraints, we should expect the enrollment effects to be larger at private institutions—especially when expressed as a percentage change relative to the baseline mean. In contrast, the low tuition fees charged by public institutions suggest any enrollment gain will disproportionately reflect changes in the (net-of-tuition-fee) opportunity cost of attending college. Consistent with binding credit constraints, reparations cause large enrollment gains at

[27] For never-treated individuals, we restrict the sample to those who were the same age in the same calendar time as treated individuals the semester before their reparation receipt.

[28] Unfortunately, we are underpowered to detect impacts on graduation, as we do not observe postsecondary attendance data after spring 2016. Since the typical undergraduate program lasts between 4 and 5 years in Colombia, we are restricted to reparations paid out before 2011—i.e., before the Victims' Law.

[29] We do not detect any statistically significant difference in the child's enrollment gain when comparing the identity of the household member receiving the reparation, for instance, mother versus father or grandparents versus parents (not reported).

private institutions (see Appendix Figure A.27). By $k = 4$, the point estimate is more than twice as large in percentage terms at private than public institutions. The gain was more modest—but still economically meaningful—at public institutions, reflecting supply-side restrictions.

Lastly, we examine postsecondary enrollment more broadly. Appendix Figure A.28 presents matriculation at two- or three-year undergraduate programs. Reparations did not affect attendance at two- or three-year programs; if anything, the effect is negative. This suggests that victims respond to reparation also on the intensive margin, substituting technical or technological education with university education. The average effect of reparations on overall postsecondary enrollment is 10.3 percent by $k = 4$.

4.3.2 High School Graduation and Test Scores

In this section, we study the effects of reparations on children's likelihood of graduating from high school and their performance in Colombia's national standardized high school exit exam, SABER 11. To measure these outcomes, we use microdata for children taking SABER 11 between 2010 and 2019. Virtually all high school seniors take SABER 11 in their final semester before graduation, regardless of their post-secondary intentions. Therefore, taking SABER 11 is a proxy measure for being a high school graduate.

Since students graduate from high school only once, we do not use the event study approach to estimate the effect of reparation on schooling outcomes. Instead, we compare school-aged children whose households received reparation when the children were younger versus older. Intuitively, if reparation affects children's schooling outcomes, we should expect it to have stronger effects among children who have more time left in school. Older children—for instance, children aged 16 and above—have less time left in school and, therefore, their schooling behavior will be less affected by reparation.

We, therefore, compare outcomes for ever-treated test-age children whose households received reparation before versus after they turned 16 using the following OLS specification:

$$Y_i = \beta \cdot 1(Age < 16)_i + \psi_{t(i)} + \gamma_{c(i)} + \delta_{v(i)} + \alpha_{m(i)} + X_i' \Phi + \epsilon_i \tag{2}$$

where Y is the outcome for person i, $1(Age < 16)$ is a dummy that equals 1 if the child's household received reparation when the child was aged 15 or younger and 0 otherwise, $\psi_{t(i)}$ are year-of-birth fixed effects, $\gamma_{c(i)}$ reparation year fixed effects, $\delta_{v(i)}$ are victimization type fixed effects, $\alpha_{m(i)}$ are municipality fixed effects, X_i is a vector of baseline covariates, and ϵ is the error term. β is the parameter of interest and compares the effect of receiving reparation on schooling outcomes when the child has more versus less time left in school.

We perform this comparison among the sample of test-age children, defined as being aged 10 to 15 in 2010 and whose household received a reparation when they were aged 13 to 19 (see Appendix Figure A.32). Table 6 presents the β coefficient and associated standard errors using specification (2). Column (1) shows that only 51 percent of test-age children graduate from high school. Reparation does not seem to affect the likelihood of graduating high school: the β coefficient is close to zero and not statistically significant at conventional levels. Column (2) compares the average age children took the standardized exam. Reparation appears to induce students to take the exam—and therefore graduate from high school—10.1 weeks sooner (=-0.194*365/7), and this effect is highly statistically significant. Appendix Table A.4 decomposes this result by age bins and shows that this happens by reducing the fatness of the right tail of the distribution, i.e., fewer children graduate when older than age 18 thanks to the reparation. Moreover, thanks to the above-documented positive effects on living conditions and health outcomes, the reparation may reduce children's schooling disruption.

Columns (3)–(8) report the effect of reparation on students' performance in SABER 11. Reparation significantly improved students' test scores. On average, reparation raised children's test scores by 2.37 percentiles or 0.07 standard deviations, and this effect is highly statistically significant. Figure 11 decomposes the percentiles result by age bins. Consistent with reparation causing larger gains for younger children, who have more time left in school, the effects are more sizable and precisely estimated for children younger than 16 years old.

To interpret this positive effect of reparation on children's test performance, we examine how reparation affects the high schools' characteristics, like private or public, average cohort size, and geographic location. In addition, we test whether reparation affects children's peers by examining effects on their school's average test scores and socioeconomic composition. Reparation does not affect any of these observable characteristics: the coefficients are close to zero and not statistically significant at conventional levels (see Appendix Table A.5). This suggests that the test score gains are not driven by changes in the observable features of the high school children attend nor in the characteristics of their peers. Instead, reparation may have raised test scores by improving their family's living and health conditions, as we previously documented. Moreover, the money may have boosted students' aspirations, resulting in greater effort.

5 Discussion

It is argued that reparations may be essential to fostering peace and reconciliation and re-establishing trust between citizens and the state (Greiff, 2009; Roht-Arriaza and Orlovsky, 2009). By recognizing suffering and responsibility, reparations have inherent symbolic value. Notwithstanding, the money awarded to victims through reparation also has a high fiscal cost. Without prior knowledge of its effect on victims, some detractors deemed this cost too hefty a burden to impose on the taxpayer. Indeed, concerns regarding the implications of a reparation program on the state's fiscal sustainability featured prominently in the political debate about the

Victims' Law. Our results enable us to assess the cost of reparations in light of their measured benefits. While victims had complete discretion on how to spend (or misspend) the money, we found it is invested in income-generating activities and human capital and improves victims' health. This section contributes to this discussion by providing a back-of-the-envelope cost-benefit analysis in light of these findings.

We make three main assumptions to contrast the cost of reparations relative to measured long-term benefits. First, we project the long-term gain based on the event-study coefficients estimated three or four years after reparation. Second, we translate these coefficients into a net present value (NPV) money metric using a 5 percent interest rate. We do this only for outcomes that represent a quantifiable benefit to the victim. The outcomes, plotted in Figure 12 and summarized in Table A.6, include the value of increased formal earnings; having an active business; fewer ED visits, hospitalizations, and medical procedures; and accumulating more human capital. That is, we ignore the quantified effects of reparation on the consumption of durables and non-durables; we do not take a stand on the dollar-equivalent value of consuming more through the credit market or greater homeownership. We also ignore any other (positive or negative) effect of reparation that we cannot measure with our administrative data, like satisfaction or trust in the State—though we recognize reconciliation and trust are key objectives of reparation programs. Third, we ignore spillovers or general equilibrium effects. Our assumptions err on the conservative side and lead us to estimate a lower bound of the true benefits of reparation. We briefly summarize the steps taken to obtain our cost-benefit calculation in what follows.

Regarding the long-term effects of reparations on recipients' formal earnings, we observe that the average age for adult victims who receive reparation is 45 years of age. Colombia's retirement age is 62 for men and 57 for women; therefore, we assume recipients have 15 years left in the labor force on average. Consequently, we estimate reparations will raise formal earnings by 0.31 percentage points (2.05 percent from Table 2 multiplied by the control mean) or 3.05 USD per year. Assuming a 5 percent interest rate, this leads to an NPV of 34.8 USD per adult. Since there are 2.05 adults per household, this translates to an NPV of 71.60 USD per household.

Moreover, reparations raised the likelihood of having an active business by 0.28 percentage points (14.1 percent multiplied by the control mean). Without information about profits earned from these businesses, we assume they perform similar to the average micro-firm in Colombia. Colombia's national statistics agency, DANE, estimates the average monthly profit for micro-firms to be worth 94 percent of the legal minimum wage. This would imply an additional 7.95 USD per adult per year or 186.33 USD per household in NPV.

Next, we estimate that reparations reduce health care utilization by 15.9 percent for ED visits, 22.5 percent for hospitalizations, and 22.1 percent for medical procedures four years after receiving reparation. We assume this reduction will persist for 40 additional years based on Colombians' life expectancy. The average cost of a medical procedure in Colombia is 25 USD, meaning reeparations reduce the yearly cost of procedures by 10.23 USD per person or 757.6 USD in NPV per household. The average length of hospitalization is 5.8 days (RIPS, 2014), and the average daily cost of hospitalization is 401 USD (MinSalud, 2014). This means reparations reduce the yearly cost of hospitalizations by nearly 12.14 USD per person or 895.4 USD in NPV per household. We could not find information about the cost of an ED visit, so we assume it is worth one-fourth the daily cost of hospitalization. We estimate that reparations reduce the annual cost of ED visits by 62.8 USD in NPV per household. We err on the side of caution by ignoring any potential welfare gains from better health, longer life expectancy, or the value of a saved life.

Lastly, we compute the benefits from more human capital accumulation thanks to reparation. We found that reparations increased university attendance, and we assume this will lead to a similarly higher likelihood of graduating from university. For private universities, the cost of attendance is the tuition fee plus the foregone earnings (which we observe to be one-third of the monthly minimum wage for victims aged 15 to 25). Instead, public universities are heavily subsidized by the central government, so the tuition fee paid by the student is a fraction of the actual cost of enrolling an additional student. Since we are interested in calculating the cost for the taxpayer (not for the student), we assume the cost of enrolling an additional student in a public university for the government, which is 85 percent of the cost for a private university (Londoño-Vélez et al., 2020). In terms of benefits, we estimate that the attendance gain is 0.29 percentage points (32.3 percent relative to the control mean) for private universities and 0.17 percentage points (10.6 percent relative to the control mean) for public universities. Colombia's Ministry of Education estimates annual earnings to be 16,214 USD for private university graduates and 14,034 USD for public university graduates (Observatorio Laboral para la Educación). Instead, the average victim with no college education earns around 70 percent of the minimum wage. This translates to an annual gain of 57.3 USD or 937 USD in NPV for the household. Moreover, reparations increased students' performance in the high school exit exam by 2.37 percentiles, and we show this is likely due to a behavioral effect, rather than changing the quality of the high school attended. Previous work suggests test score gains raise college attendance through motivational effects (Laajaj et al., 2022). Using the estimates from Laajaj et al. (2022), we expect an additional increase in postsecondary matriculation of 0.9 percent for the next generation, raising households' income by 1,160 USD in NPV. We ignore the positive externalities from a more educated workforce, like reduced crime.

Aggregating the benefits of reparations along the different dimensions, we obtain a household gain worth 4,071 USD in NPV. Compared to the average household reparation of 4,250 USD, this means the benefit of reparation is worth around 95 percent its cost. Notwithstanding, this result is conservative and sensitive to the assumptions we are willing to make. For instance, assuming the average additional taxes thanks to the higher wages is 16 percent, the benefit would increase to 4,612 USD, which would exceed the cost of reparation.

Moreover, adding any positive education externalities or the welfare gain from homeownership or improved health status would likely make the gains from reparation far outweigh the costs.

6 Conclusion

This paper evaluates the effects of reparations on victims of human rights violations using quasi-experimental evidence from Colombia's reparation program, which awarded sizable, lump-sum cash payments to hundreds of thousands of victims of the internal armed conflict.

We construct a novel individual-level panel dataset that links comprehensive national administrative panel microdata and find evidence that reparations have positive long-term impacts on victims' lives. Contrary to the notion that cash transfers fully discourage employment, we observe a very small drop in wage employment. Notwithstanding, there is a considerable reallocation of workers across jobs, resulting in higher wages. We also see an increased business survival and higher levels of consumption. Reparations also cause an economically meaningful decrease in health care utilization, consistent with improved health due to better working and living conditions. Finally, reparations increase human capital investments for the next generation by improving high school test scores and postsecondary attendance rates of victims' children.

Our results suggest that reparation programs can be an effective policy tool that helps individuals overcome poverty and reduces the gaps formed due to conflict. Moreover, since reparations recipients tend to be historically marginalized populations, these programs may also serve as a starting point for promoting social inclusion that is key to development. An important limitation of our work is that we are restricted to estimating the effects of reparation on outcomes available in administrative datasets. However, reparations potentially have numerous effects on several outcomes we cannot capture, including trust in the state, forgiveness, and satisfaction with the reparation program. In ongoing work, we investigate the effects of reparation on such outcomes using original surveys. Doing so will enable us to shed further light on reparations as tools for peace-building and recovery.

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Tables and Figures

Table 1: Summary Statistics

	Victims of Colombia's Internal Armed Conflict				Individuals Identified in SISBEN (2010)			
		With valid contact information				Eligible for Reparation§		
	All	All	Eligible for Reparation§		All	All	Received	
			All	Received			All	w/ Date
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Women	0.501	0.502	0.519	0.570	0.512	0.540	0.582	0.594
Birthdate info	0.985	0.996	0.996	0.999	1.000	1.000	1.000	1.000
Younger than 18*	0.257	0.252	0.262	0.105	0.155	0.202	0.101	0.000
Aged 18-60*	0.620	0.631	0.625	0.661	0.676	0.680	0.653	0.728
Older than 60*	0.123	0.117	0.113	0.234	0.168	0.118	0.246	0.272
Years since victimization*	16.600	17.140	16.442	25.398		16.066	23.082	22.461
Years since report*	10.412	10.645	10.641	11.867		10.116	12.050	12.235
Forced displacement (direct)	0.878	0.876	0.909	0.693		0.966	0.893	0.886
Homicide (direct)	0.036	0.038	0.000	0.000		0.000	0.000	0.000
Homicide (indirect)	0.098	0.103	0.106	0.507		0.078	0.362	0.374
Other victimization	0.079	0.079	0.080	0.084		0.086	0.104	0.116
Minority	0.127	0.121	0.125	0.090		0.135	0.117	0.114
Disabled	0.040	0.042	0.044	0.068		0.053	0.084	0.090
Identified in SISBEN I, II or III	0.529	0.610	0.629	0.686	1.000	1.000	1.000	1.000
Employment in PILA (26-60y.o.)					0.305	0.159	0.174	0.174
Labor force part. (26-60y.o.)					0.617	0.534	0.523	0.527
Working (26-60y.o.)					0.572	0.497	0.482	0.485
School attendance (5-25y.o.)					0.661	0.672	0.569	0.461
Postsec. enrollm. (15-25y.o.)					0.099	0.051	0.072	0.072
Years of education (>25y.o.)					6.579	5.381	5.260	5.331
SISBEN hh wealth score					39.036	29.383	33.107	34.047
Rural					0.193	0.242	0.212	0.214
Home ownership					0.494	0.460	0.502	0.513
Household size					4.655	5.137	4.627	4.555
Female head of hh					0.345	0.344	0.401	0.398
Hh income (min. wages)					1.002	0.690	0.735	0.750
Ind. reparation (min. wages)				11.536			12.140	11.710
Hh reparation (min. wages): RUV				33.964			30.546	29.575
Hh reparation (min. wages): SISBEN							19.532	18.999
Observations	8,895,006	7,717,774	7,422,689	821,579	25,786,953	2,968,173	349,804	262,136

Notes: *As of 2019. §We restrict the sample by dropping direct victims of homicide or forced disappearance. *Source:* Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Table 2: The Impact of Reparation on Formal Labor Market Outcomes

Outcome	**Average in 2014q1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,4] (4)	**2 Years** k ∈ [5,8] (5)	**3 Years** k ∈ [9,12] (6)
Employment	16.40%	-0.20	-0.96**	-2.40***	-2.18***	-2.03***
		(0.47)	(0.45)	(0.53)	(0.66)	(0.73)
Self-employment	1.56%	-3.84*	0.09	-1.34	-2.93	-5.42
		(2.08)	(1.93)	(2.46)	(3.15)	(3.45)
Wage employment	14.80%	0.19	-1.07**	-2.52***	-2.11***	-1.68**
		(0.50)	(0.48)	(0.57)	(0.70)	(0.77)
Days of salaried work (includes zeros)	9.53	0.70	0.14	-2.36***	-2.42***	-2.03**
		(0.54)	(0.51)	(0.64)	(0.82)	(0.91)
Days of salaried work (excludes zeros)	66.43	0.37	0.67	-0.30	-0.17	-0.06
		(0.38)	(0.43)	(0.37)	(0.40)	(0.40)
Daily wage (In min. wages)	1.373	0.21	0.94**	1.63***	2.14***	3.11***
		(0.36)	(0.39)	(0.41)	(0.46)	(0.54)
Earnings (In min. wages)	0.149	0.46	1.45**	-0.38	0.10	2.05*
		(0.65)	(0.64)	(0.77)	(0.97)	(2.13)
MPE		0.02	0.03	-0.03	-0.02	0.01
		(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
Quarters out-of-formal-employment†	6.48	-0.10	0.08	0.38***	0.43***	0.38***
		(0.07)	(0.05)	(0.09)	(0.13)	(0.15)

Notes: This table presents the event study coefficients from Specification (1) but collapsing the event time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparation during the period of study. Columns (2) through (6) report the difference in the outcome between treated and control victims relative to the period immediately before reparation. The effects are expressed in percentage terms relative to Column (1). Column (2) shows the effects before reparation; the coefficients are close to zero and not statistically significant, supporting the identification assumption. Column (3) shows the immediate effect at event time 0, while Columns (4), (5), and (6) show the effects 1–4, 5–8 and 9–12 quarters after reparation, respectively. † Capped at eight quarters. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Table 3: Heterogeneity by Job Type: Occupational Risk and Pay

Type of salaried job	**Average in 2014q1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,4] (4)	**2 Years** k ∈ [5,8] (5)	**3 Years** k ∈ [9,12] (6)
Low-risk	6.72%	-0.01	-0.33	-1.04	-0.69	0.23
		(0.82)	(0.76)	(0.97)	(1.21)	(1.35)
High-risk	8.11%	0.35	-1.70**	-3.74***	-3.28***	-3.25***
		(0.75)	(0.75)	(0.86)	(1.05)	(1.13)
Min wage	9.76%	-0.48	-2.72***	-3.91***	-3.51***	-3.45***
		(0.84)	(0.89)	(0.90)	(1.04)	(1.12)
Above min wage	5.07%	1.47	2.11*	0.19	0.61	1.75
		(1.07)	(1.19)	(1.18)	(1.43)	(1.57)
Min wage high-risk	5.27%	0.04	-2.98**	-4.54***	-3.78***	-3.98***
		(1.21)	(1.31)	(1.29)	(1.46)	(1.53)
Min wage low-risk	4.49%	-1.09	-2.45*	-3.19**	-3.21*	-2.83
		(1.31)	(1.36)	(1.42)	(1.67)	(1.81)
Above min wage high-risk	2.83%	0.94	0.67	-2.25	-2.35	-1.89
		(1.44)	(1.58)	(1.63)	(2.00)	(2.20)
Above min wage low-risk	2.24%	2.15	3.93**	3.26*	4.34*	6.34**
		(1.73)	(1.89)	(1.90)	(2.32)	(2.56)

Notes: This table presents the event study coefficients from Specification (1) but collapsing the event time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparation during the period of study. Columns (2) through (6) report the difference in the outcome between treated and control victims relative to the period immediately before reparation. The effects are expressed in percentage terms relative to Column (1). Column (2) shows the effects before reparation; the coefficients are close to zero and not statistically significant, supporting the identification assumption. Column (3) shows the immediate effect at event time 0, while Columns (4), (5), and (6) show the effects 1–4, 5–8 and 9–12 quarters after reparation, respectively. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Table 4: The Impact of Reparations on Microcredit and Consumer Debt

Outcome	**Average in 2014q1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,4] (4)	**2 Years** k ∈ [5,8] (5)	**3 Years** k ∈ [9,12] (6)
Microcredit						
Has any microcredit	8.86%	-0.10	-4.80***	-2.37***	0.96	3.21***
		(0.37)	(0.34)	(0.51)	(0.71)	(0.83)
Outstanding balance	145.6 USD	-0.64	-3.58***	-0.21	5.66***	9.50***
		(0.54)	(0.46)	(0.72)	(1.09)	(1.34)
Days delinquent	3.79	0.79	-12.95***	-24.90***	-26.96***	-22.11***
		(1.62)	(1.45)	(2.40)	(3.50)	(4.37)
Credit card debt						
Has any credit card debt	3.59%	-0.94	-5.62***	0.72	7.40***	12.00***
		(0.75)	(0.75)	(0.98)	(1.37)	(1.62)
Oustanding balance	19.31 USD	-1.12	-10.82***	1.70	17.73***	31.97***
		(1.49)	(1.30)	(1.92)	(3.01)	(4.05)
Consumption	10.26 USD	13.18	-3.84	11.93	30.60**	38.87***
		(14.13)	(12.63)	(10.94)	(12.49)	(14.12)
Auto loans						
Has any automobile loan	0.11%	-8.65	8.48	21.68***	21.34**	18.06
		(6.02)	(5.89)	(7.89)	(10.67)	(12.43)
Outstanding balance	6.17 USD	-9.45	-1.54	38.89***	49.39**	61.55***
		(7.86)	(5.73)	(12.33)	(17.22)	(23.09)

Notes: This table presents the event study coefficients from Specification (1) but collapsing the event time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparation during the period of study. Columns (2) through (6) report the difference in the outcome between treated and control victims relative to the period immediately before reparation. The effects are expressed in percentage terms relative to Column (1). Column (2) shows the effects before reparation; the coefficients are close to zero and not statistically significant, supporting the identification assumption. Column (3) shows the immediate effect at event time 0, while Columns (4), (5), and (6) show the effects 1–4, 5–8 and 9–12 quarters after reparation, respectively. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

Table 5: The Impact of Reparations on Health Care Utilization

Variables	**Average in** **k=-1**	**Pre-event** k ∈ [-4,-2] (1)	**Immediate** k=0 (2)	**1 Year** k ∈ [1,2] (3)	**2 Years** k ∈ [3,4] (4)	**3 Years** k ∈ [5,6] (5)	**4 Years** k ∈ [7,8] (6)
Any ED visit	2.67%	0.5	-1.6	-5.3***	-7.8***	-13.8***	-15.9***
		(1.5)	(1.7)	(2.0)	(2.7)	(3.2)	(3.7)
Any hospitalization	1.16%	-0.7	-2.9	-2.5	-12.8***	-19.7***	-22.5***
		(2.3)	(2.7)	(2.8)	(3.5)	(4.2)	(4.7)
Number of procedures	0.93	-1.0	-1.0	-4.0***	-10.1***	-16.4***	-22.1***
		(1.0)	(1.2)	(1.3)	(1.8)	(2.3)	(2.7)

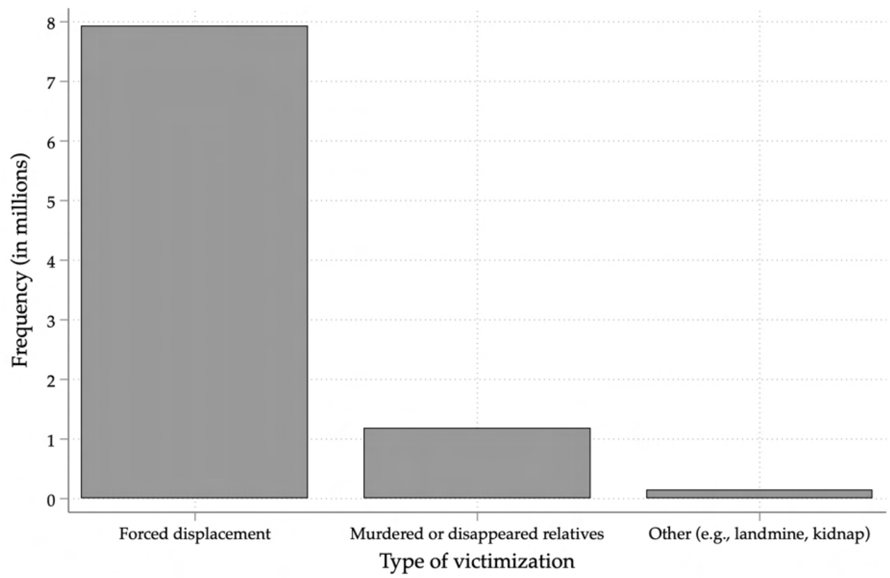
Notes: This table presents the event study coefficients from Specification (1) but collapsing the event time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparation during the period of study. Columns (2) through (6) report the difference in the outcome between treated and control victims relative to the period immediately before reparation. The effects are expressed in percentage terms relative to Column (1). Column (2) shows the effects before reparation; the coefficients are close to zero and not statistically significant, supporting the identification assumption. Column (3) shows the immediate effect at event time 0, while Columns (4), (5), and (6) show the effects 1–4, 5–8 and 9–12 quarters after reparation, respectively. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and RIPS.

Table 6: The Effect of Reparation on High School Graduation and Test Scores

	Takes SABER 11 Exam	Conditional on Taking SABER 11 Exam						
		Age	Test Score Percentile					
			Total		Math		Reading	
			Percentile	SD	Percentile	SD	Percentile	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	0.013	-0.194***	2.367***	0.070***	2.092***	0.063***	1.347**	0.045*
Standard Error	(0.01)	(0.03)	(0.66)	(0.02)	(0.67)	(0.02)	(0.68)	(0.02)
Observations	39,763	20,239	20,238		20,238		20,238	
Conterfactual mean	0.51	17.67	41.23	-0.32	40.52	-0.28	40.52	-0.27
Counterfactual S.D.	(0.5)	(1.38)	(26.22)	(0.82)	(26.63)	(0.86)	(26.73)	(0.9)
Year of birth FE	Y	Y	Y	Y	Y	Y	Y	Y
Reparation year FE	Y	Y	Y	Y	Y	Y	Y	Y
Victimization type FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls SISBEN (2010)	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table presents the effect of reparation on high school graduation and performance in Colombia's national standardized high school exit exam. The table report the β coefficient and robust standard errors using OLS specification (2). The outcome is the likelihood of taking SABER 11 in Column (1). For those who take the SABER 11 exam, the outcome is age in Column (2), test scores percentile in Columns (3), (5) and (7) and test scores national standard deviations in Columns (4), (6) and (8). The sample is restricted to children aged 10 to 15 in 2010 according to SISBEN and whose households received a reparation when they were aged 13 to 19. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using data from SRNI, SISBEN, and ICFES.

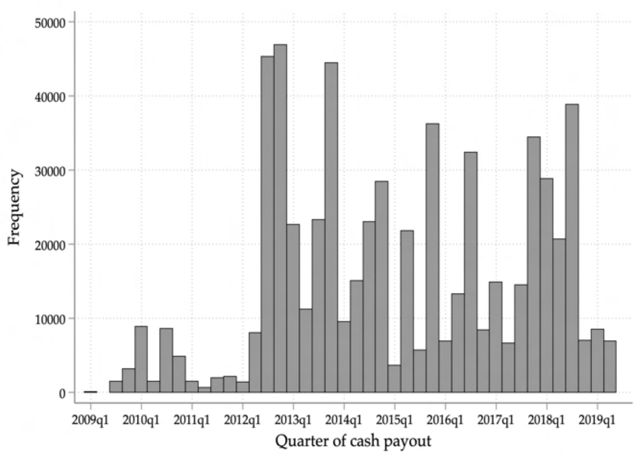
Figure 1: Number of Victims by Type of Victimization



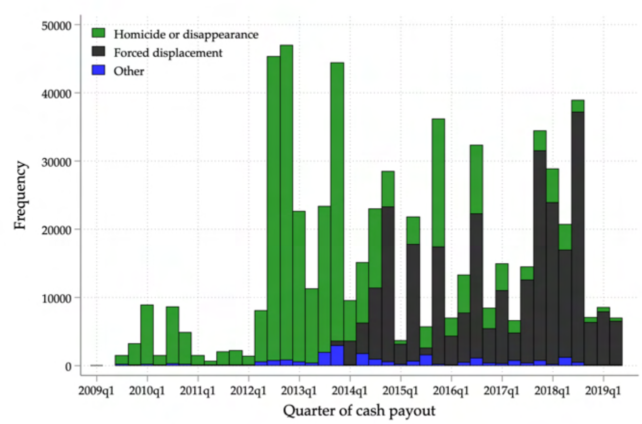
Notes: This figure plots the number of victims of the Colombian internal armed conflict by victimization type. A victim can count among forced displacement *and* homicide or forced disappearance if (s)he was both forcibly displaced and has relatives victims of homicide or forced disappearance. Victims that have suffered neither type of these victimizations are included in the category "Other". This category includes, for instance, victims of torture, rape, or kidnap. The figure shows there are 7.9 million victims of forced displacement and 1.2 million individuals whose relatives were murdered or disappeared by the conflict.
Source: Authors' calculation using RUV data from SRNI.

Figure 2: Financial Reparations by Quarter

(a) All victimizations

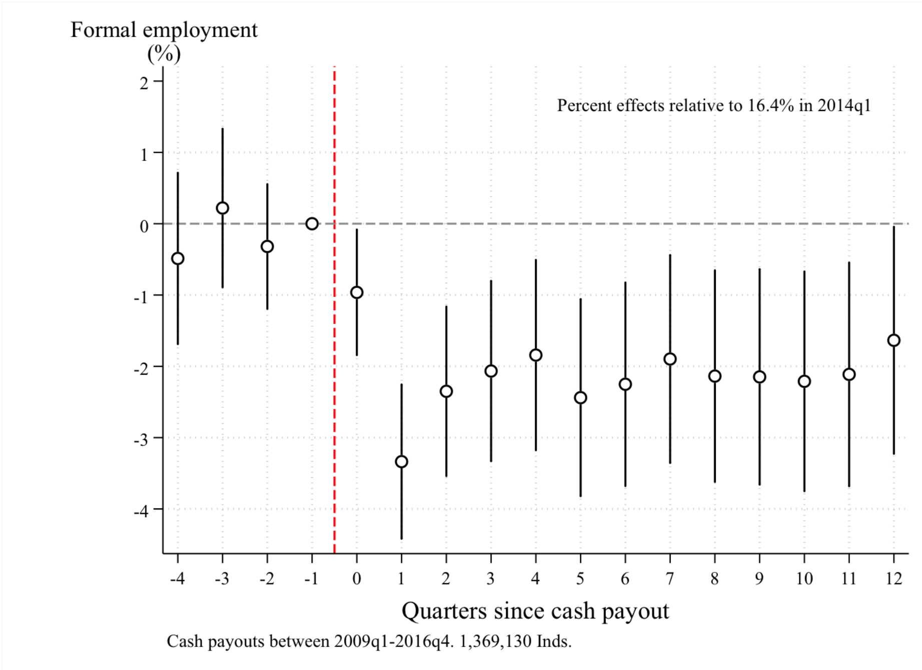


(b) By victimization type



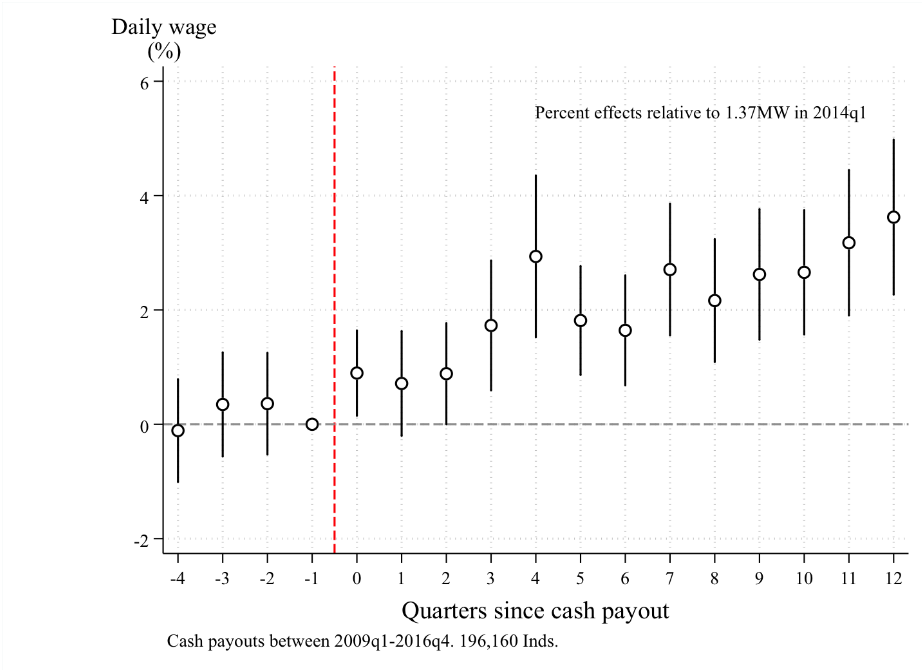
Notes: This figure plots the frequency of financial reparations by quarter of cash payout. Panel (a) plots the series for all types of victimizations. Panel (b) plots the series by victimization type: homicide or forced disappearance, forced displacement, and all other types of victimizations. A victim can receive more than one reparation.
Source: Authors' calculation using RUV data from SRNI.

Figure 3: Formal Employment



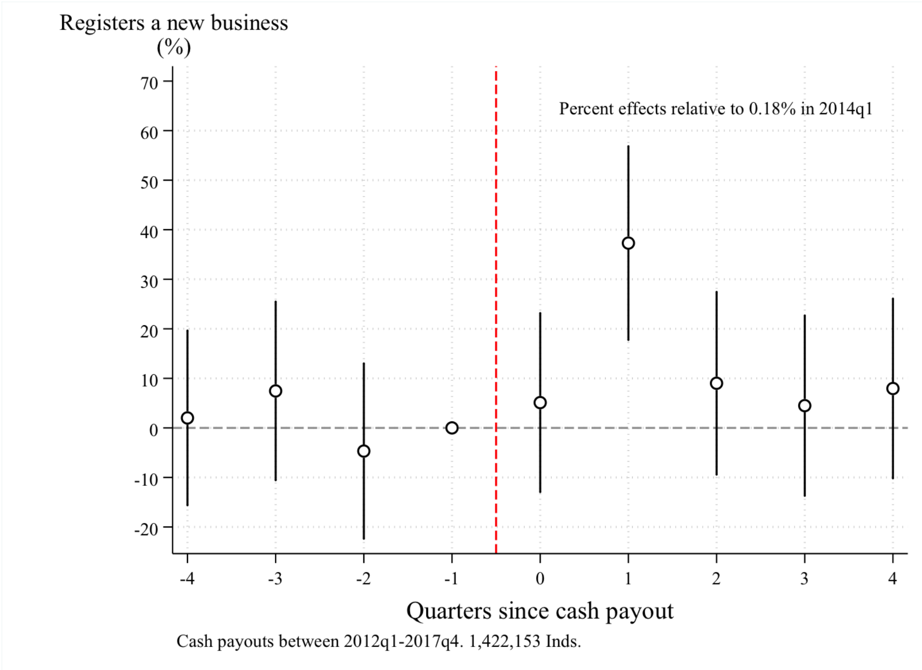
Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual works in the formal sector (i.e., contributes to social security)—is plotted against the distance to reparation payout (in quarters). The figure shows the reparation reduces formal employment by 3.3 percent relative to a baseline mean of 16.4 percent at $k = 1$. This drop is statistically significant at the 1 percent level and remains roughly constant over time. The treatment is defined at the individual level, and the sample is balanced in event time and includes victims that did not receive a reparation, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.8 presents these results excluding never-treated individuals and shows quantitatively similar effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure 4: Formal Daily Wages



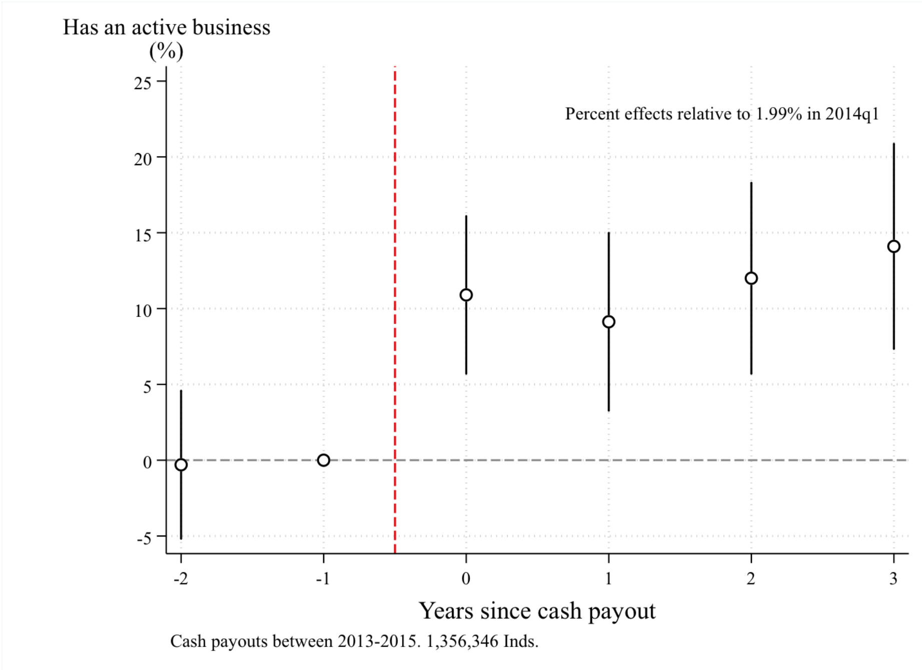
Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—daily wage for salaried workers (expressed relative to the minimum wage)—is plotted against the distance to reparation payout (in quarters). The figure shows the reparation significantly increases daily wages. The effect is statistically significant at the 1 percent level and increases over time, reaching 3.6 percent by $k = 12$. The treatment is defined at the individual level, and the sample is balanced in event time and includes victims that did not receive a reparation, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.15 presents these results excluding never-treated individuals and shows similar although less precisely estimated effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure 5: Registering a New Business



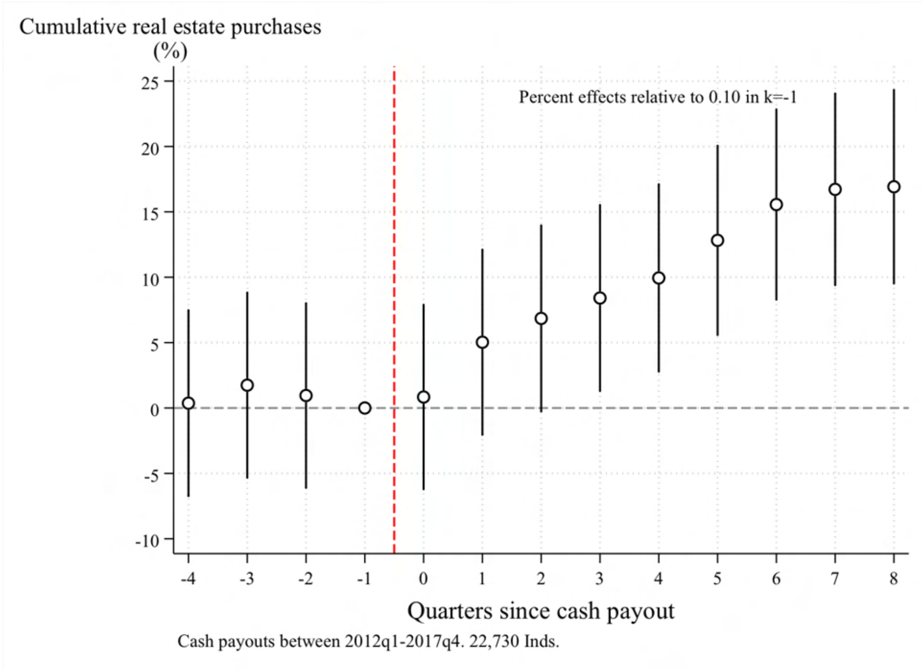
Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual registered a new business that quarter—is plotted against the distance to cash payout (in quarters). The likelihood of registering a new business one quarter after cash payout ($k = 1$) increased by 37 percent from a base of 0.17 percent, and this effect is only significant at the 1 percent level. The treatment is defined at the individual level, and the sample is balanced in event time and includes victims that did not receive a reparation, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.17 presents these results excluding never-treated individuals, and shows quantitatively similar effects of cash payout on entrepreneurship.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and Confecámaras.

Figure 6: Firm Survival



Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual has an active business that year—is plotted against the distance to cash payout (in years). We measure active business as having a valid license, which firms are required to renew every year. The likelihood of having an active business increases by 14.7 percent the year of cash payout ($k = 0$) from a base of 1.6 percent, and the coefficient is significant at the 1 percent level. The effect persists at least three years after the reparation payout. The treatment is defined at the individual level, and the sample is balanced in event time and includes victims that did not receive a reparation, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.18 presents these results excluding never-treated individuals and shows quantitatively similar but less precisely estimated effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN and Confecámaras.

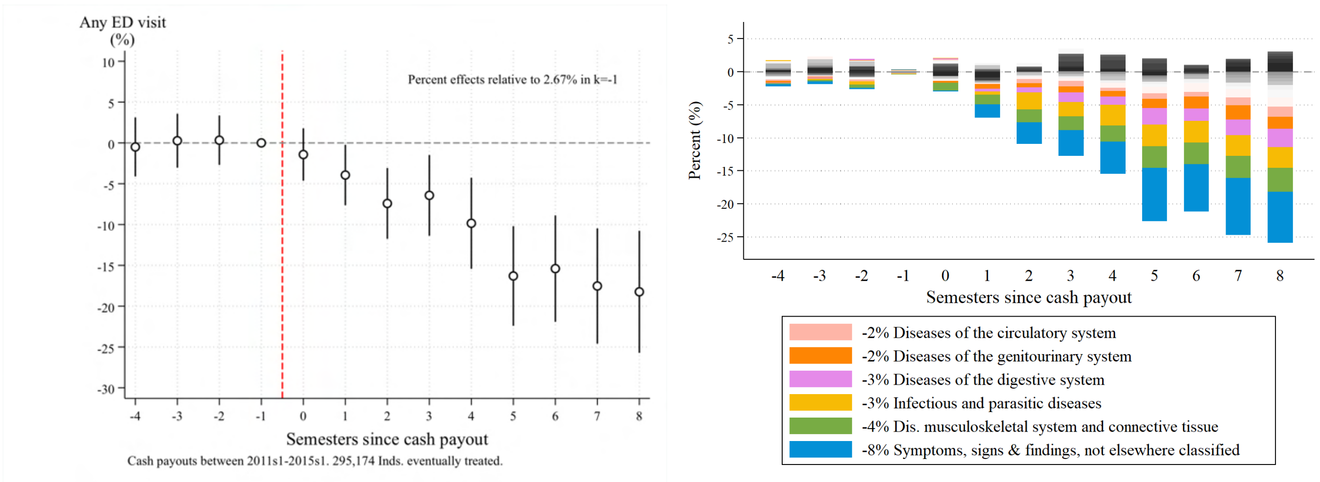
Figure 7: Cumulative Real Estate Purchases



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—the cumulative number of real estate purchases—is plotted against the distance to reparation payout (in quarters). The figure shows the reparation significantly increases real estate purchases. The effect is statistically significant at the 1 percent level and increases over time, reaching 16.9 percent by $k = 8$. The treatment is defined at the household level, and the sample is balanced in event time and excludes victims that did not receive a reparation, i.e., never-treated individuals. Standard errors are clustered at the household level. *Source:* Authors' calculation using RUV data from SRNI, SISBEN, and Catastro Antioquia.

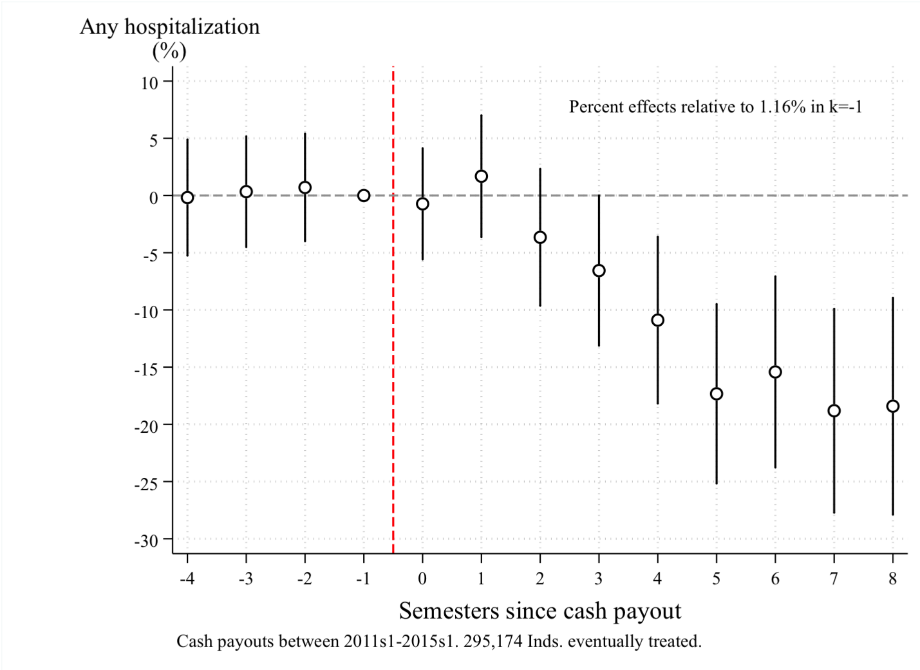
Figure 8: ED Visits

(a) Any ED Visit (b) Decomposed by diagnosis



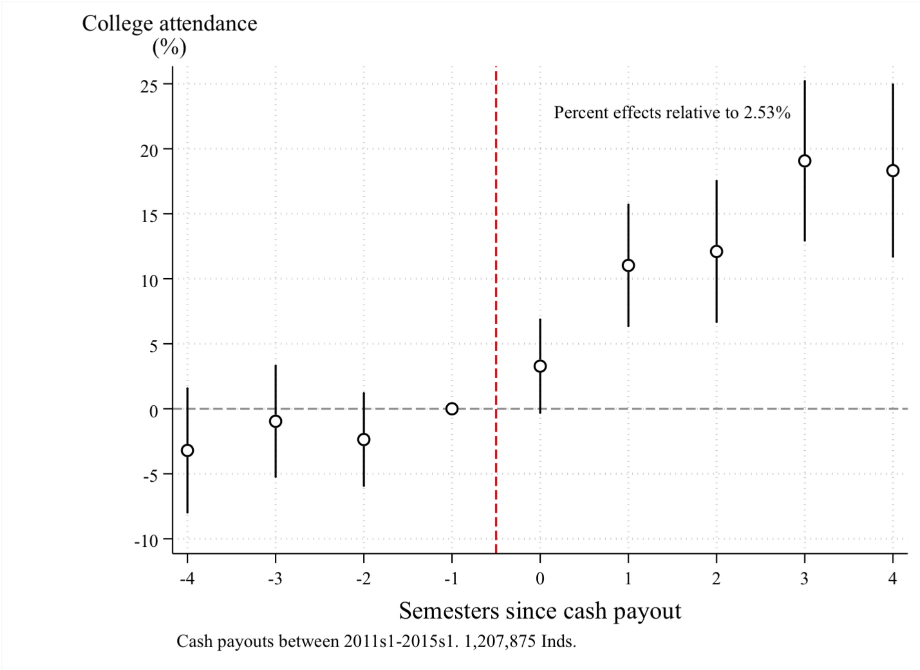
Notes: This figure presents the effect of cash payout on ER visits. Panel (a) plots the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—the likelihood of an Emergency Department visit in a given semester—is plotted against the distance to reparation payout (in semesters). Reparations reduce ED visits, with compounding effects over time: by $k = 8$, ED visits are 18.2 percent less frequent. Panel (b) decomposes ED visits by diagnosis, where the coefficient on an ED visit for each diagnosis (in percentage points) is divided by mean likelihood of any ED visit at $k = -1$. Reparations reduced ED visits associated with maladies from external causes (e.g., various symptoms, musculoskeletal illnesses, infectious and parasitic diseases). Over time, reparations also lower ED visits due to maladies associated with dietary and other habits that affect the circulatory, genitourinary, and digestive systems. Treatment is defined at the household level, and event time is defined as the first date in which any member received reparation. The sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and RIPS.

Figure 9: Hospitalizations



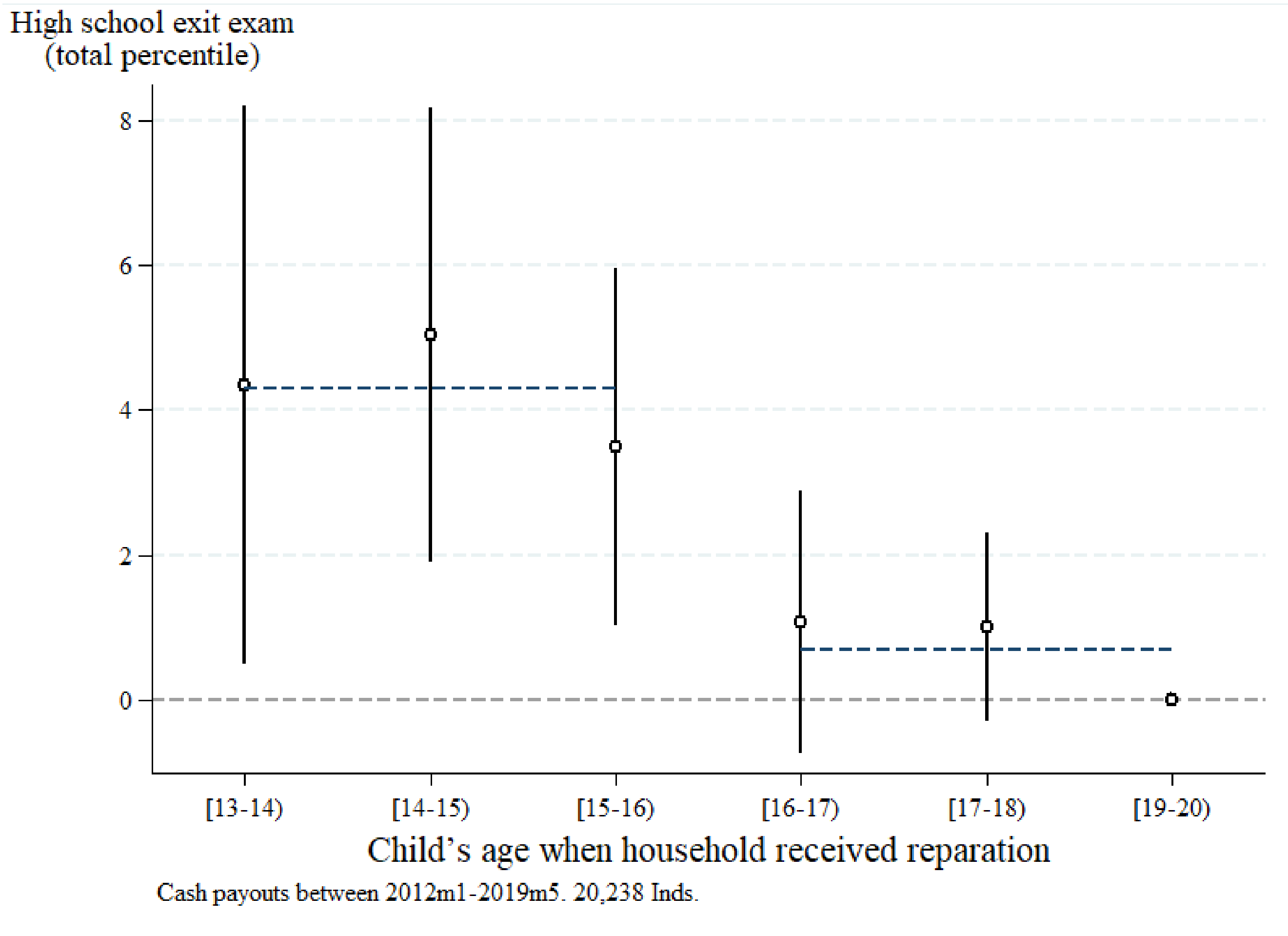
Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—the likelihood of being hospitalized in a given semester—is plotted against the distance to reparation payout (in semesters). Reparations reduced hospitalizations, with compounding effects over time. By $k = 8$, hospitalizations are 18.4 percent less frequent, and this effect is significant at the 1 percent level. Treatment is defined at the household level, and event time is defined as the first date in which any household member received a cash reparation. The sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and RIPS.

Figure 10: Enrollment in a Four- or Five-Year Undergraduate Program



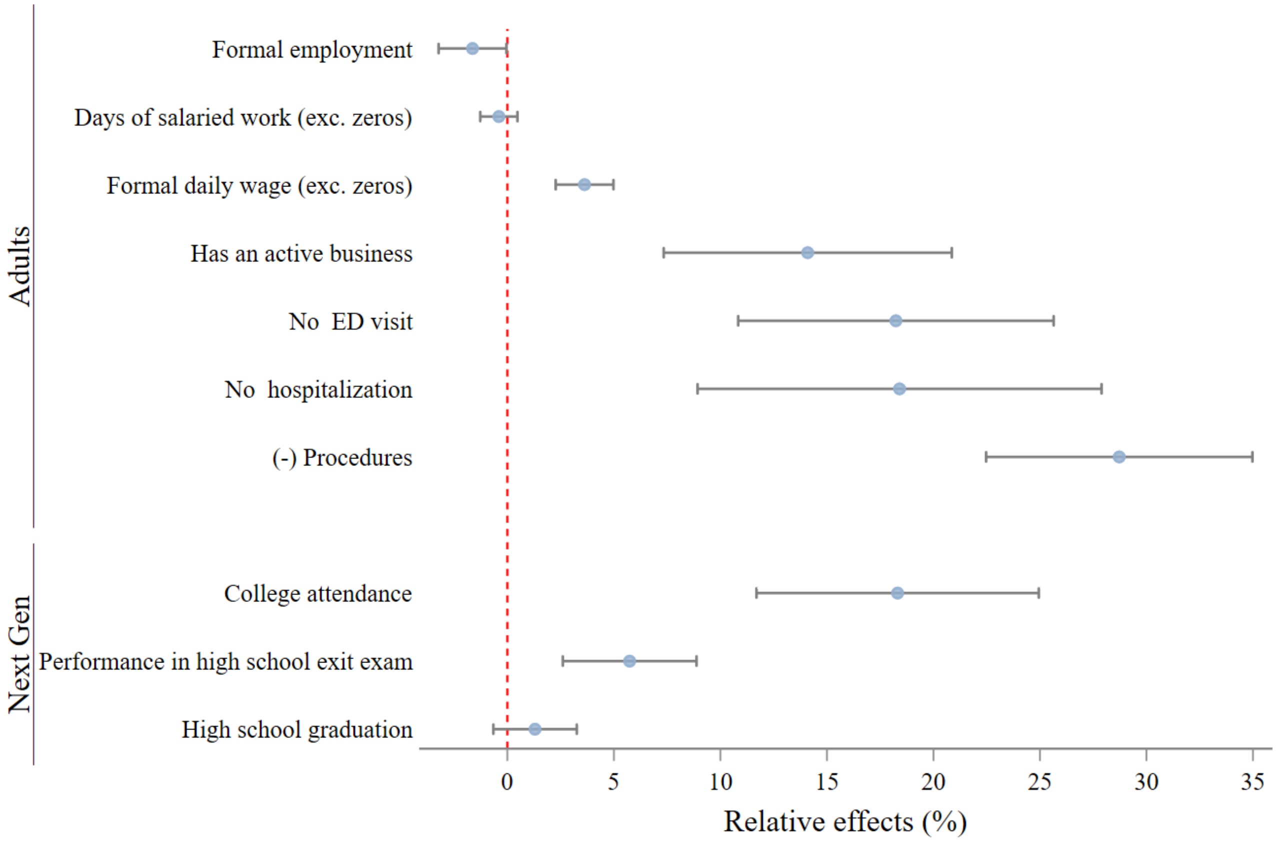
Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a four- or five-year undergraduate program in a given semester—is plotted against the distance to cash payout (in semesters). Treatment is defined at the household level, and event time is defined as the first date in which any household member received a cash reparation. The sample (i) is balanced in event time; (ii) is restricted to individuals aged 15 to 25 at $k = -1$; and (iii) includes never-treated individuals. Standard errors are clustered at the household level. The figure suggests cash reparations encouraged undergraduate attendance, with compounding effects over time: by $k = 4$, attendance is 18.3 percent higher. Figure A.26 shows that part of the effet at $k = 1$ is driven by a gain in access: the likelihood of attending an undergraduate program for the first time ever increases by 24 percent. Figure A.28 presents enrollment at two-year colleges and overall postsecondary enrollment. Figure A.29 presents these results excluding never-treated individuals, and shows quantitatively similar effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

Figure 11: Effect of Reparation on Performance in High School Exit Exam



Notes: This figure plots the relative effect of reparation on performance in SABER 11 exam by the child's age when their household received reparation (19 years old is the excluded dummy). The sample is restricted to ever-treated children aged 10 to 15 in 2010.
Source: Authors' calculation using data from RUV, SISBEN, and ICFES.

Figure 12: Summary of the Longer-Term Effects of Reparation on Adult Victims and Children



Notes: This figure summarizes the relative effects of reparation on adult victims and their children three or four years after payout. The effects are reported in percentage terms (i.e., relative to the control mean). Each row reports the event study coefficient and associated 95 percent confidence interval on the latest period after reparation payout available; for most outcomes, this is 12 quarters after payout. For high school performance, this is the β coefficient from Specification (2).
Source: Authors' calculation using data from RUV, SISBEN, PILA, Confecámaras, SuperFinanciera, Catastro Antioquia, RIPS, SPADIES, and ICFES.

Appendices

A Online Appendix Tables and Figures

Table A.1: Reparation Size and Distribution by Type of Victimization and Decree

Type of Victimization	Reparation Size (in Multiples of Minimum Wage)			Distribution
	Law 418/1997	Decree 1290/2008	Law 1448/2011	
Indirect victims				
Homicide	40	40	Up to 40	Household
Forced disappearance	40	40	Up to 40	Household
Direct victims				
Permanent disability	Up to 40*	Up to 40	Up to 40	Individual
Non-permanent disability		Up to 30	Up to 30	Individual
Torture		30	Up to 30 or 10**	Individual
Kidnap		40	Up to 40	Individual
Rape		30	Up to 30	Individual
Children product of rape			Up to 30	Individual
Child recruitment		30	Up to 30	Individual
Forced displacement		27	Up to 27 or 17***	Household

Notes: * Resolution 7381/2004. ** Resolution 00552/2015 reduced this amount from 30 minimum wages to 10. *** The Victims' Law reduced the reparation amount from 27 to 17 monthly minimum wages for people who had been forcibly displaced after April 22, 2008, or who had not registered this victimization in RUV (and therefore were not included in the waitlist for reparations) by April 22, 2010 (Decree 1377/2014).

Table A.2: Intra-Household Distribution of Reparations by Household Composition and Decree

	Household Composition	Law 418/1997	Decree 1290/2008	Law 1448/2011
1	Single without children	Parent(s)	50% Parent(s) 50% Sibling(s)	Parent(s)
2	Single with children and parents	Children	50% Parent(s) 50% Children	50% Parent(s) 50% Children
3	Single with children, but no parents (with or without siblings)	Children	Children	Children
4	Single without children, parents nor grandparents, with sibling(s)	Closest relative	Siblings	No cash award to sibling(s)
5	Single without children nor parents, with sibling(s) and grandparent(s)	Closest relative	Sibling(s)	Grandparent(s)
6	Single without children nor sibling(s), with parent(s)	Parent(s)	Parent(s)	Parent(s)
7	Married or cohabitating, with children	50% Partner 50% Children	50% Partner 50% Children	50% Partner 50% Children
8	Married or cohabitating, without children	50% Partner 50% Parent(s)	50% Partner 50% Parent(s)	50% Partner 50% Parent(s)
9	Married or cohabitating, without children nor parents	Partner	Partner	Partner
10	Single without children, parents nor sibling(s)	Closest relative	Closest relative	Grandparent(s)

Notes: Two individuals are considered co-habitating in Colombia if they have lived together for two or more years (Law 54/1990). The closest relative must show proof that (a) he/she is related to the victim and (b) provided, financially, for the victim before his/her death or disappearance. A Constitutional Court ruling expanded the definition of relatives able to receive this reparation to siblings, uncles, and grandchildren (*Sentencia C-052*, February 8, 2012).

Table A.3: Wage Earners Composition

Variable	**Average in 2014q1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,4] (4)	**2 Years** k ∈ [5,8] (5)	**3 Years** k ∈ [9,12] (6)
Years of education	7.53	0.33	0.07	0.18	-0.17	-0.24
		(0.26)	(0.25)	(0.30)	(0.36)	(0.38)
Income in min wages	0.50	-0.09	0.15	1.20	0.58	0.56
		(0.72)	(0.69)	(0.91)	(1.07)	(1.26)
Predicted wage from Mincer equation	0.70	0.03	-0.02	0.16	-0.09	0.17
		(0.15)	(0.15)	(0.18)	(0.22)	(0.23)
Predicted wage from Mincer equation + City FE	0.69	0.01	-0.02	0.21	-0.07	0.10
		(0.15)	(0.15)	(0.18)	(0.21)	(0.23)

Notes: This table presents the event study coefficients from Specification (1) but collapsing the event time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparation during the period of study. Columns (2) through (6) report the difference in the outcome between treated and control victims relative to the period immediately before reparation. The effects are expressed in percentage terms relative to Column (1). Column (2) shows the effects before reparation; the coefficients are close to zero and not statistically significant, supporting the identification assumption. Column (3) shows the immediate effect at event time 0, while Columns (4), (5), and (6) show the effects 1–4, 5–8 and 9–12 quarters after reparation, respectively. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Table A.4: The Effect of Reparation on Test Age

	Average age	Age at the date of SABER 11 exam					
		≤16	17	18	19	20	≥21
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient	-0.194***	0.046***	-0.009	-0.004	-0.006*	-0.009***	-0.005***
Standard Error	(0.03)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Observations	20,239	39,763	39,763	39,763	39,763	39,763	39,763
Conterfactual mean	17.67	0.19	0.16	0.09	0.04	0.02	0.01
Counterfactual S.D.	(1.38)	(0.39)	(0.36)	(0.29)	(0.2)	(0.14)	(0.12)
Year of birth FE	Y	Y	Y	Y	Y	Y	Y
Reparation year FE	Y	Y	Y	Y	Y	Y	Y
Victimization type FE	Y	Y	Y	Y	Y	Y	Y
Controls SISBEN (2010)	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y

Notes: This table presents the effect of reparation on the age at which at test-age ever-treated child took Colombia's national standardized high school exit exam. The table reports the β coefficient and robust standard errors using OLS specification (2). The outcome is the average age in Column (1). Columns (2)–(7) report the effects using dummies for different age bins. The sample is restricted to children aged 10 to 15 in 2010 according to SISBEN and whose households received a reparation when they were aged 13 to 19. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using data from SRNI, SISBEN, and ICFES.

Table A.5: The Effect of Reparation on High School Characteristics

	Calendar A HS (1)	Migration (2)	Socioecon. Stratum 1 (3)	Socioecon. Stratum 2 (4)	Socioecon. Stratum >2 (5)	Private HS (6)	HS cohort size (7)	HS LOM Total Percentile (8)	HS LOM Math Percentile (9)	HS LOM Reading Percentile (10)
Coefficient	0.008	-0.001	0.000	0.001	0.000	-0.012	2.172	-0.276	-0.277	-0.277
Standard Error	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(2.85)	(0.18)	(0.18)	(0.18)
Observations	20,239	20,239	20,239	20,239	20,239	20,239	20,239	20,239	20,239	20,239
Conterfactual mean	0.94	0.18	0.62	0.25	0.04	0.12	110.54	12.09	12.13	12.12
Counterfactual S.D.	(0.23)	(0.38)	(0.49)	(0.43)	(0.19)	(0.33)	(108.02)	(9.6)	(9.57)	(9.58)
Year of birth FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Reparation year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Victimization type FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls SISBEN (2010)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

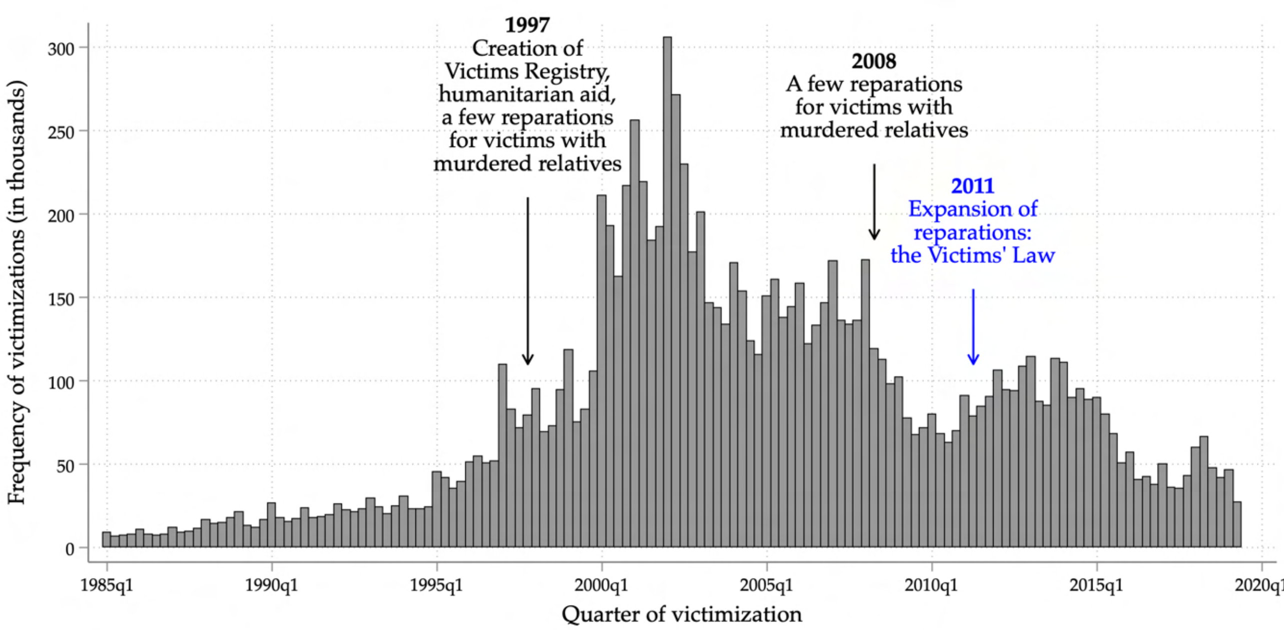
Notes: This table presents the effect of reparation on the age at which at test-age ever-treated child took Colombia's national standardized high school exit exam. The table reports the β coefficient and robust standard errors using OLS specification (2). The outcome is the average age in Column (1). The sample is restricted to children aged 10 to 15 in 2010 according to SISBEN whose households received a reparation when they were aged 13 to 19. $*p < 0.1, **p < 0.05, ***p < 0.01$.
Source: Authors' calculation using data from SRNI, SISBEN, and ICFES.

Table A.6: A Stylized Cost-Benefit Analysis

	% effect	pp effect	Annual Dollars	Years Proj.	NPV	hh. NPV
Total formal wage earnings per adult	2.05%	0.0031	3.05	15	34.8	71.6
Profit from having an active business	14.1%	0.0028	7.95	15	90.5	186.3
ED visits	15.9%	0.4%	0.85	40	15.46	62.8
Hospitalizations	22.5%	0.3%	12.14	40	220.5	895.4
Procedures	22.1%	20.6%	10.28	40	186.6	757.6
Undergraduate attendance: Private	32.3%	0.00292	38.23	40	694.3	625.5
Undergraduate attendance: Public	10.6%	0.00172	19.04	40	345.8	311.5
High school standardized test score		2.367	51.95	40	943.4	1160.4
Taxes and SS contributions					494.1	540.6
					Total NPV	$ 4,612
					Reparation	$ 4,250

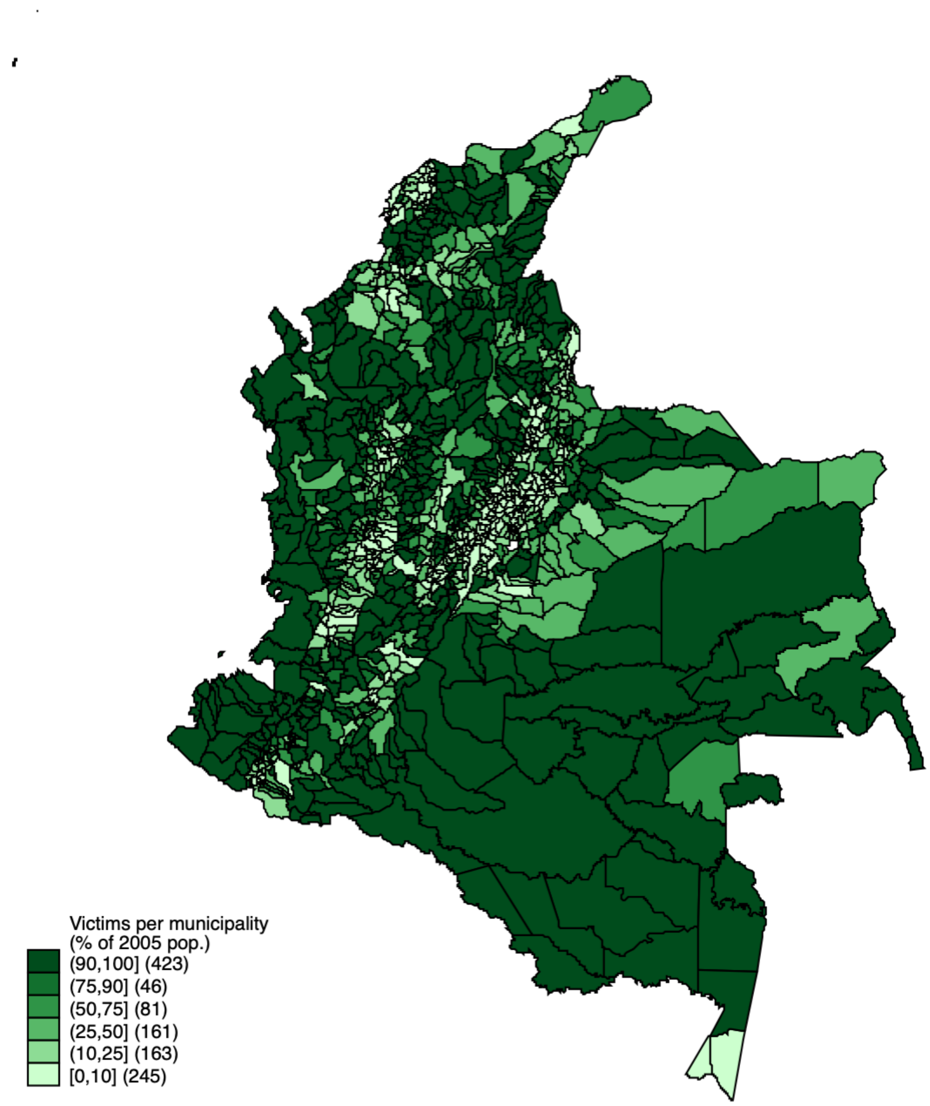
Notes: For the cost-benefit analysis we use an annual interest rate of 5%. On average, a recipient household has 1.52 minors, 0.48 seniors, and 2.06 adults. At the time of the SISBEN 2010 survey, there were on average 0.9 household members aged 15 to 25 years old and 1.23 children aged younger than 15.

Figure A.1: The frequency of victimizations between 1985 and 2019



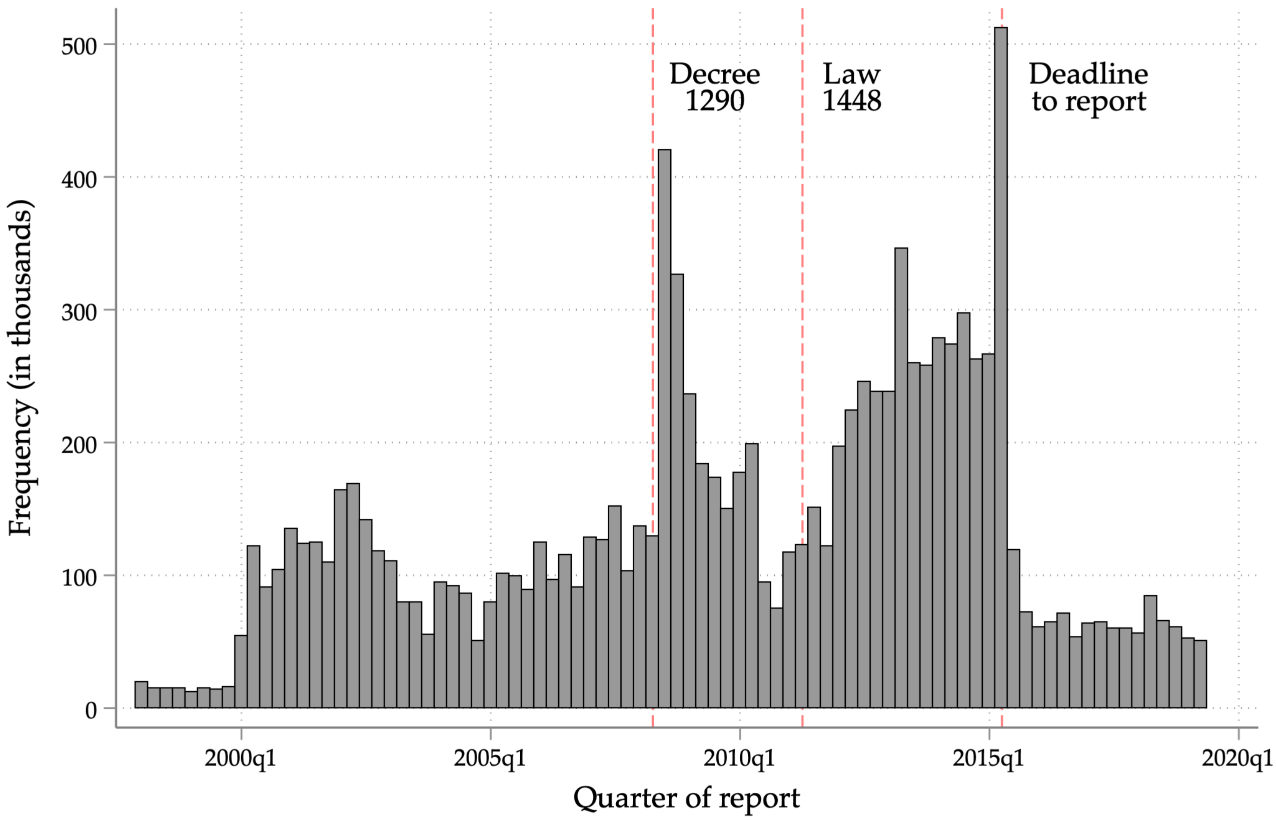
Notes: This figure plots the frequency of victimizations of the Colombian internal armed conflict by quarter of victimization. The figure shows victimizations peaked in the early 2000s and have since steadily fallen. In 1997, Colombia created the Victims Registry. A handful of reparations to victims with murdered relatives were provided between 1997 and 2010. In 2011, the Victims' Law expanded the number of reparations, as shown in Figure 2.
Source: Authors' calculation using RUV data from SRNI.

Figure A.2: Victims represent 90+% of population in some regions



Notes: This figure plots the total number of victims by geographic location, expressed as a share of 2005 population. For victims with more than one victimization, we take the municipality where the first victimization took place. The figure shows there is substantial heterogeneity in the intensity of the internal conflict across regions. For instance, less than 10 percent of the population in Bogota were victims of the internal conflict, while this share is greater than 90 percent in many regions in the country.
Source: Authors' calculation using RUV data from SRNI and SISBEN.

Figure A.3: Self-reporting as victim by date of report

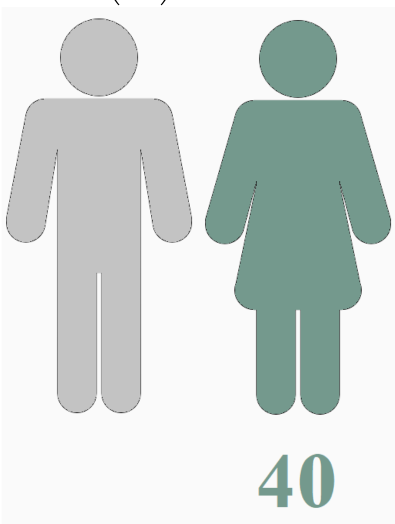


Notes: This figure plots the frequency of reporting of victimizations by quarter of report. The evolution of reported victimizations is influenced by (i) the definition of who is a victim; (ii) how long victims have to report the victimization after it occurred; and (iii) requiring people to report their victimization to be eligible for reparation. In particular, Decree 1290/2008 established that, to be eligible for a reparation, the victimization must have taken place before April 22, 2008 and be registered by April 22, 2010. Moreover, it allowed victims to report past victimizations regardless of when they took place. Both of these factors led to an uptick in the number of registered victims. Later, Law 1448/2011 expanded the number of reparations and required people who had suffered a victimization and had not reported it to do so by June 10, 2015. (Individuals victimized *after* the adoption of the Victims' Law had two years to report it.) In 2013, victims of paramilitary successor groups and criminal bands, BACRIM, were considered eligible for reparation. All of this lead to an increase in the number of reported victimization after the adoption of Law 1448/2011.
Source: Authors' calculation using RUV data from SRNI.

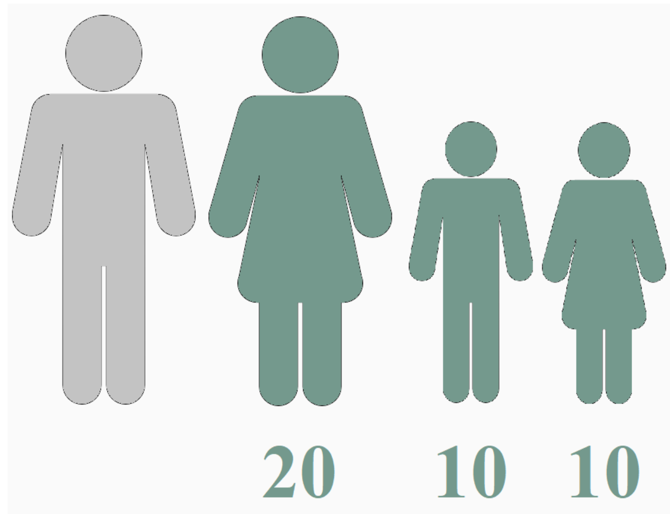
Figure A.4: Example of transfer amounts

(a) Relatives of homicide victim (40 minimum wages)

Case (a.1): Household without children



Case (a.2): Household with children

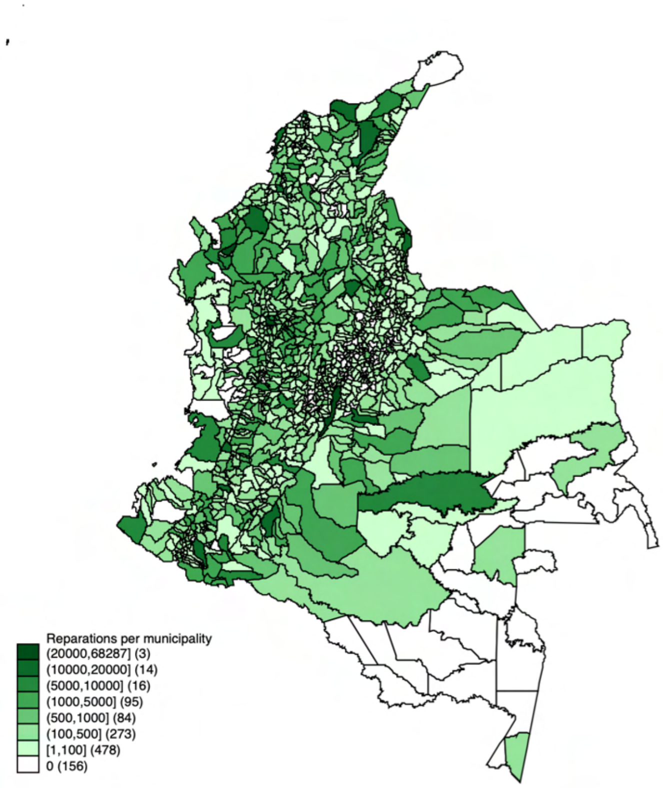


(b) Forcibly displaced household (27 minimum wages)

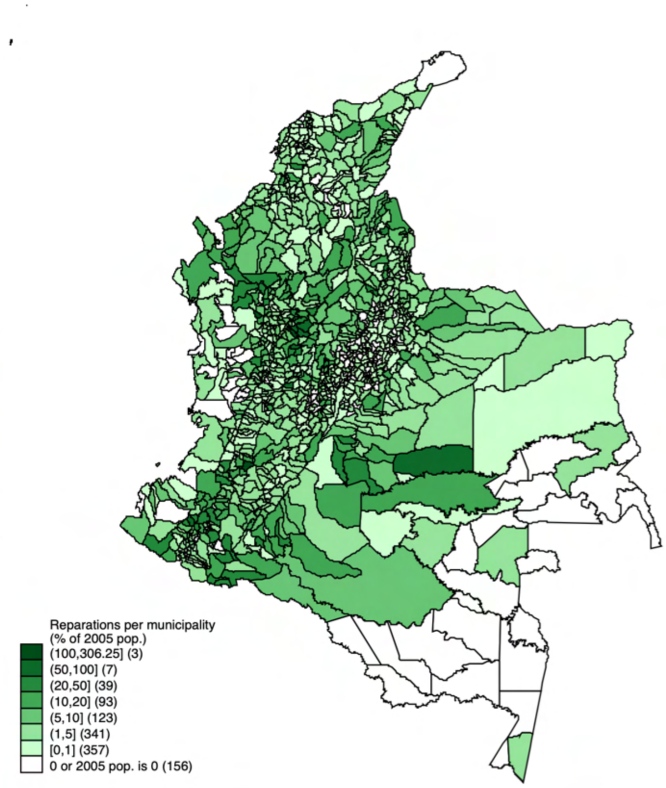
Notes: This figure illustrates several examples of intra-household distribution of victim reparations, in multiples of the legal monthly minimum wage. Panel (a) presents two examples of how the 40 minimum wages awarded to victims of homicide or forced disappearance can be distributed, depending on household composition. If the murdered or disappeared victim does not have children, 100 percent of the reparation is awarded to the spouse or civil partner (case a.1). If, instead, the murdered or disappeared victim has two children, 50 percent of the reparation is awarded to the spouse or civil partner and the remainder is split equally among the children (case a.2). Panel (b) shows that the 27 minimum wages awarded to victims of forced displacement are distributed equally among all four members of the household. See Tables A.1 and A.2.

Figure A.5: Geographic Distribution of Reparations

(a) Reparations per municipality (frequency)



(b) Reparations per municipality (% of 2005 pop.)

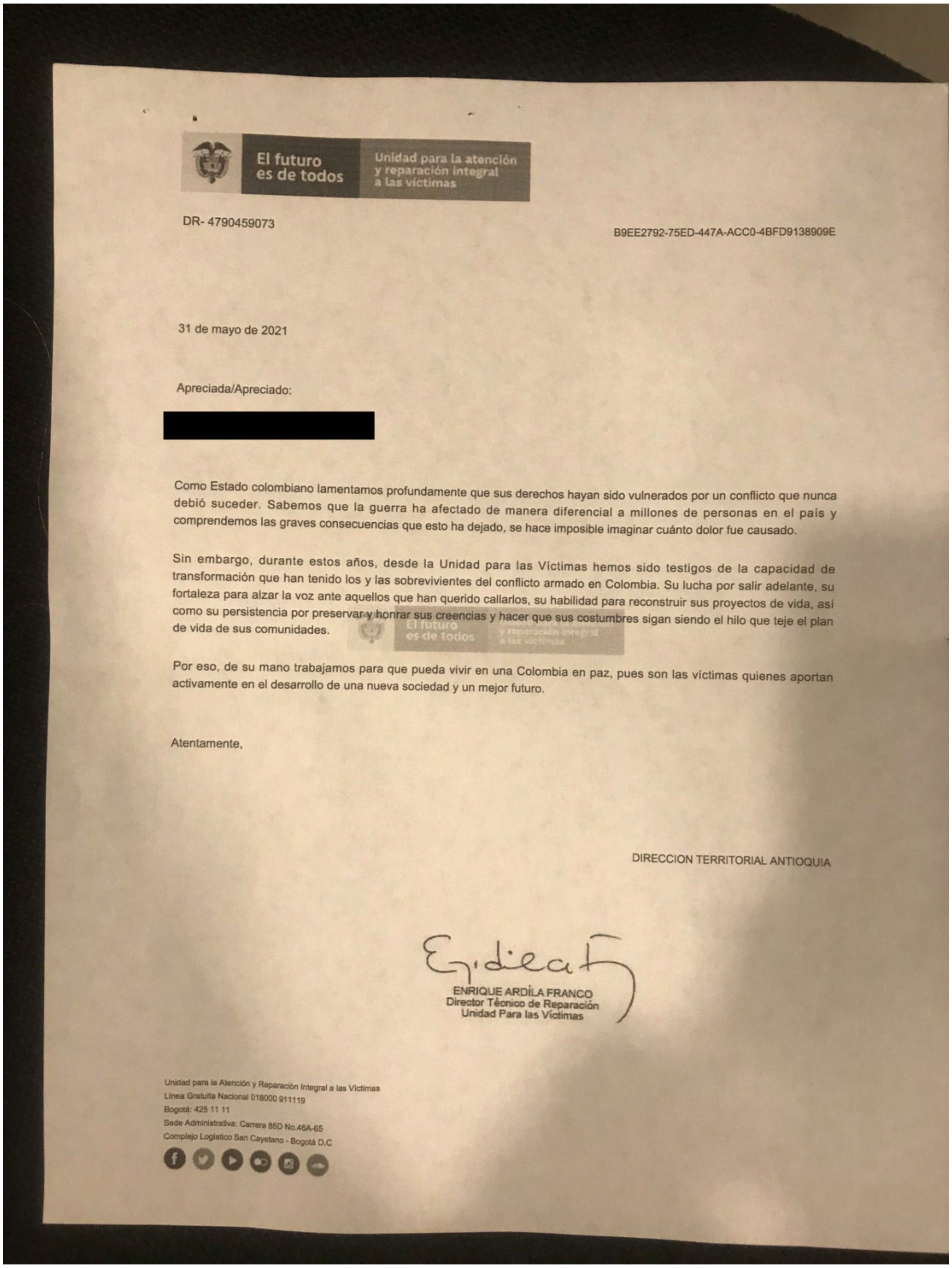


Notes: This figure plots reparations by municipality. There are around 1,120 municipalities in Colombia. Panel (a) presents the frequency of reparations and shows most municipalities received at least one reparation by June 2019. Panel (b) expresses this number as a share of the 2005 population in that municipality. A victim can receive more than one reparation.
Source: Authors' calculation using RUV data from SRNI and DANE.

Figure A.6: Victims are Informed about the Reparation

Notes: This picture was taken in one of the victim reparation meetings between UARIV and the beneficiaries in Medellin, Colombia. The UARIV informs victims they will receive a reparation check within the next days. *Photo credit:* Arlen Guarin.

Figure A.7: Victims are Informed about the Reparation



El futuro es de todos

Unidad para la atención y reparación integral a las víctimas

DR- 4790459073

B9EE2792-75ED-447A-ACC0-4BFD9138909E

31 de mayo de 2021

Apreciada/Apreciado:

[redacted]

Como Estado colombiano lamentamos profundamente que sus derechos hayan sido vulnerados por un conflicto que nunca debió suceder. Sabemos que la guerra ha afectado de manera diferencial a millones de personas en el país y comprendemos las graves consecuencias que esto ha dejado, se hace imposible imaginar cuánto dolor fue causado.

Sin embargo, durante estos años, desde la Unidad para las Víctimas hemos sido testigos de la capacidad de transformación que han tenido los y las sobrevivientes del conflicto armado en Colombia. Su lucha por salir adelante, su fortaleza para alzar la voz ante aquellos que han querido callarlos, su habilidad para reconstruir sus proyectos de vida, así como su persistencia por preservar y honrar sus creencias y hacer que sus costumbres sigan siendo el hilo que teje el plan de vida de sus comunidades.

Por eso, de su mano trabajamos para que pueda vivir en una Colombia en paz, pues son las víctimas quienes aportan activamente en el desarrollo de una nueva sociedad y un mejor futuro.

Atentamente,

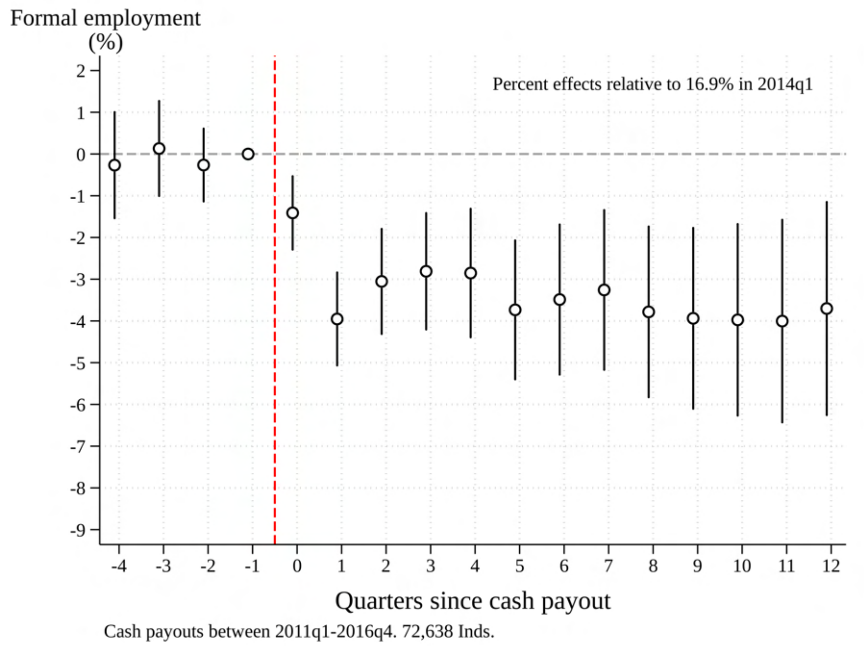
DIRECCION TERRITORIAL ANTIOQUIA

ENRIQUE ARDÍLA FRANCO
Director Técnico de Reparación
Unidad Para las Víctimas

Unidad para la Atención y Reparación Integral a las Víctimas
Línea Gratuita Nacional 018000 911119
Bogotá: 425 11 11
Sede Administrativa: Carrera 85D No.46A-65
Complejo Logístico San Cayetano - Bogotá D.C

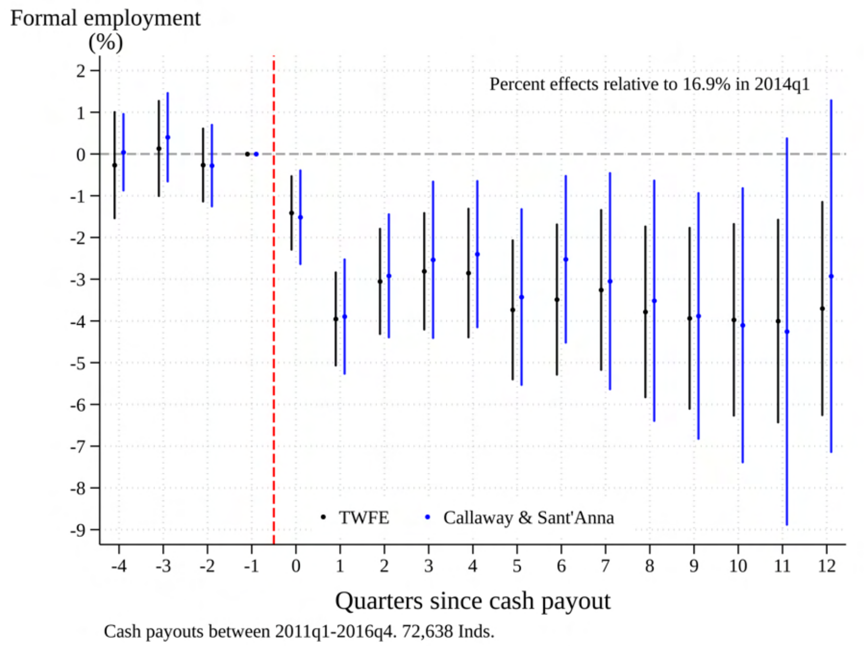
Notes: This picture was taken in one of the victim reparation meetings between UARIV and the beneficiaries in Medellin, Colombia. The dignification letter, signed by the Director of UARIV's victim reparation program, describes what the reparation means: "*As the Colombian State, we deeply regret that your rights have been violated by a conflict that never should have happened. We know that the war has differentially affected millions of people in the country and we understand the serious consequences it has had - it is impossible to imagine how much pain this conflict has caused. However, from the Victims' Unit we have witnessed conflict survivors' capacity for transformation over these years. We have witnessed their spirit to keep going, their strength to raise their voices against those who have wanted to silence them, their ability to rebuild their lives... For this reason, with your help, we are working so that you can live in a peaceful Colombia, since it is the victims who actively contribute to the development of a new society and a better future.*" [Authors' translation.] *Photo credit:* Arlen Guarin.

Figure A.8: Formal Employment (Excluding Never-Treated)



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual works in the formal sector (i.e., contributes to social security)—is plotted against the distance to reparation payout (in quarters). The figure shows the cash payout reduces formal employment by 3.9 percent relative to a baseline mean of 17.2 percent at $k = 1$, and this drop remains statistically significant at the 1 percent level even 12 quarters after reparation payout. The treatment is defined at the individual level and the sample is balanced in event time and excludes victims that did not receive a reparation, i.e., the never-treated. Figure 3 presents these results including these individuals. Standard errors are clustered at the household level. *Source:* Authors' calculation using RUV data from SRNI and PILA.

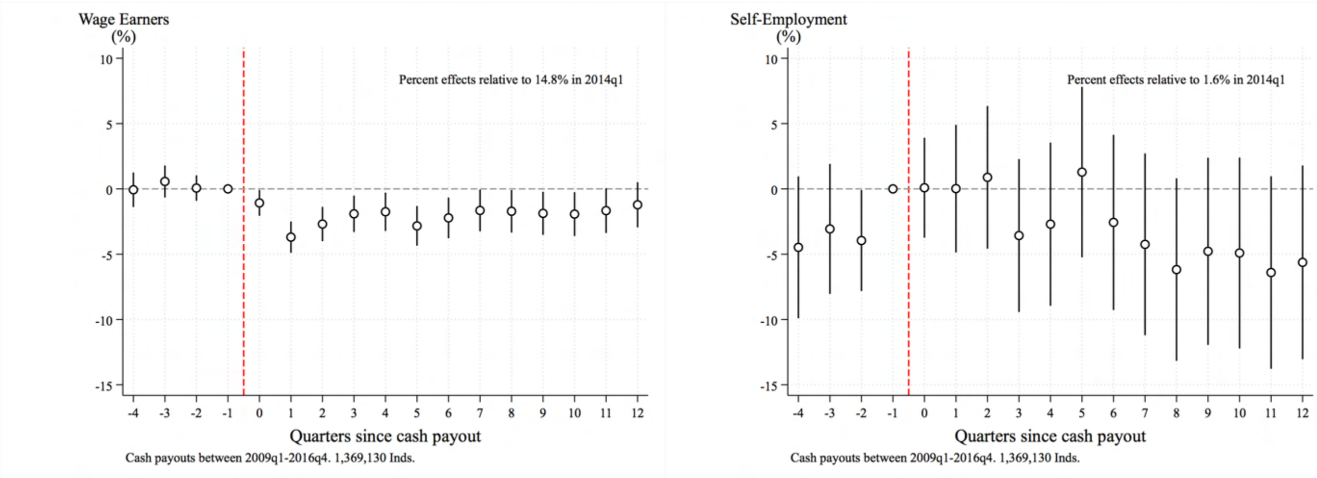
Figure A.9: Formal Employment (Using inference procedure from Callaway-Sant'Anna)



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1) (in black) and the coefficients and associated 95 percent confidence intervals using the Callaway and Sant'Anna (forthcoming) valid inference procedures for staggered adoption and limited treatment anticipation (in blue), where the outcome—whether an individual works in the formal sector (i.e., contributes to social security)—is plotted against the distance to reparation payout (in quarters). The figure shows that both analysis lead to quantitatively similar conclusions. The treatment is defined at the individual level and the sample is balanced in event time and excludes victims that did not receive a reparation, i.e., the never-treated. *Source:* Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.10: Formal Wage Employment vs. Self-Employment

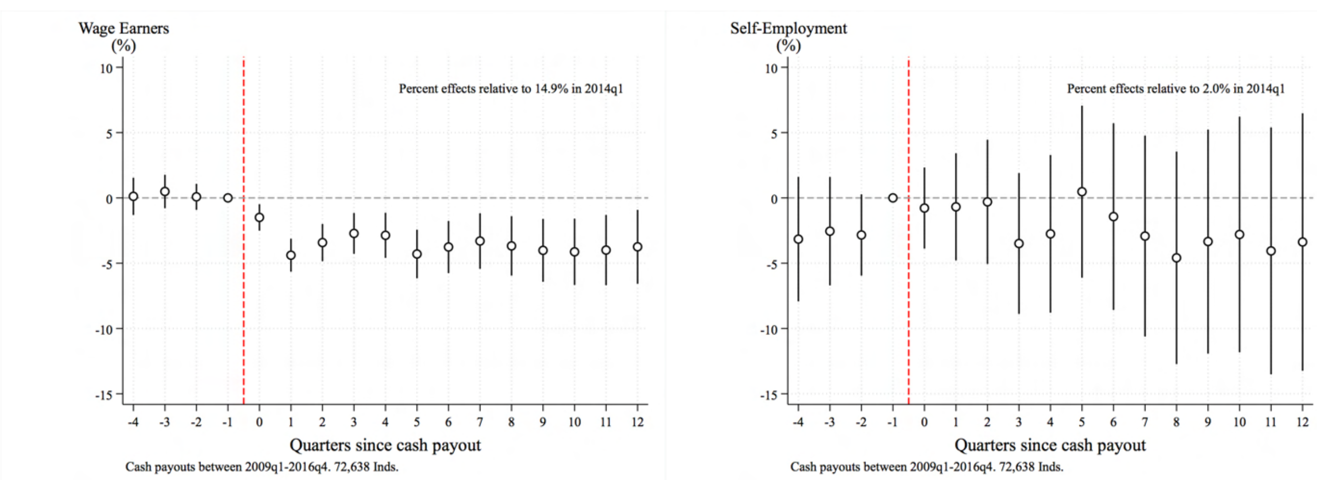
(a) Wage Employment (b) Self-Employment



Notes: These figures decompose the negative employment result from Figure 3 between wage- and self-employment in Panels (a) and (b), respectively. The employment drop is driven by wage employment, which drops the quarter after cash payout and remains constant and significant at the 5 percent level even ten quarters after reparation payout. Figure A.11 presents these results excluding never-treated individuals and shows quantitatively similar effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.11: Wage Employment vs. Self-Employment (Excluding Never-Treated)

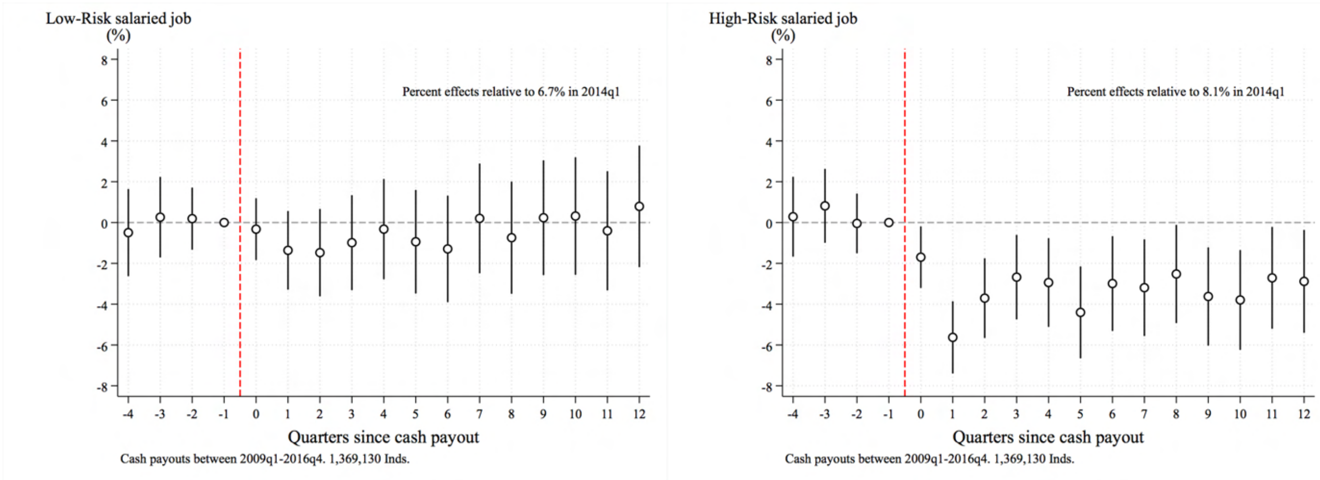
(a) Wage Employment (b) Self-Employment



Notes: This figure reproduces Figure A.10 when excluding never-treated victims from the estimation sample.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.12: Low-Risk vs. High-Risk Wage Employment

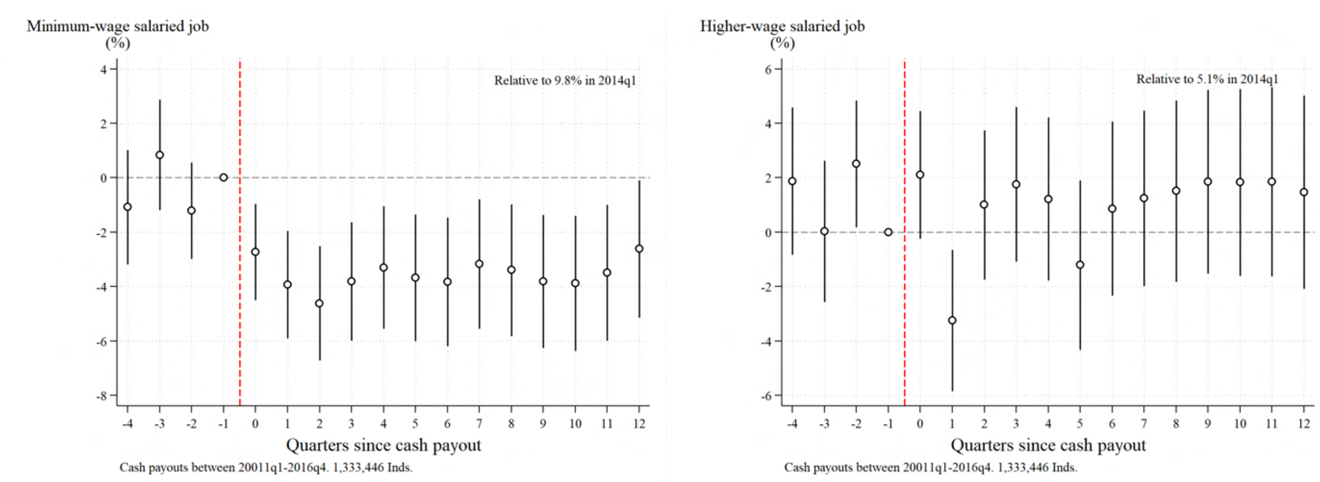
(a) Low-Risk (b) High-Risk



Notes: These figures decompose the negative wage-employment result from Panel (a) of Figure A.10 between low- and high-risk salaried jobs in Panels (a) and (b), respectively. The risk category is based on employers' contribution rate for the workers' compensation system; occupations with above-median contribution rate are coded as "high risk." The drop is driven by low-risk salaried jobs.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.13: Low-Pay vs. High-Pay Wage Employment

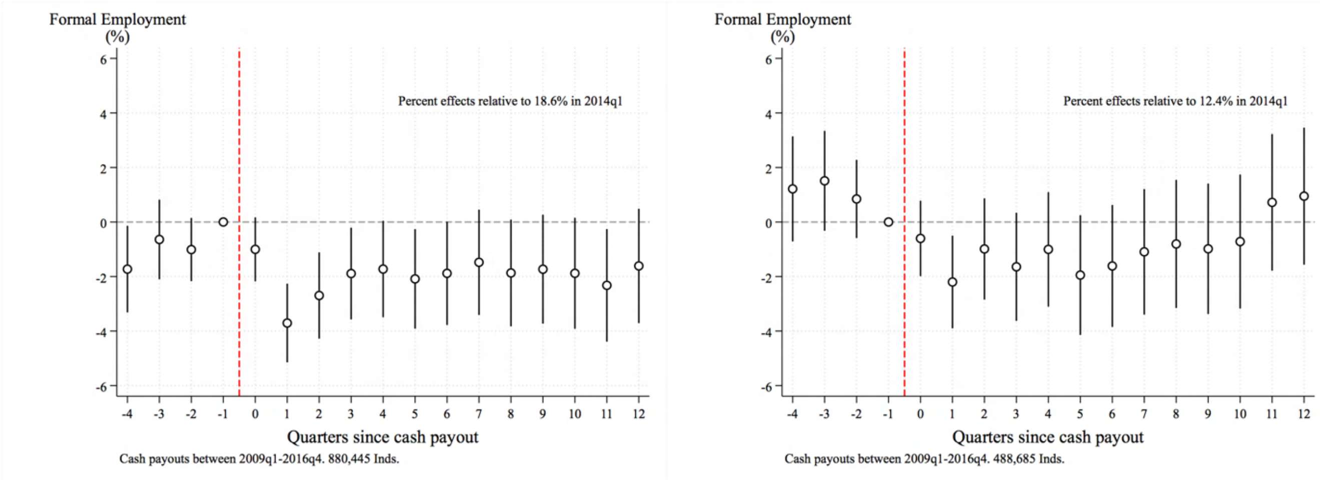
(a) Minimum-Wage Job (b) Higher-Wage Job



Notes: These figures decompose the negative wage-employment result from Panel (a) of Figure A.10 between minimum- and higher-wage salaried jobs in Panels (a) and (b), respectively. The wage-employment drop is driven by minimum-wage salaried jobs.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

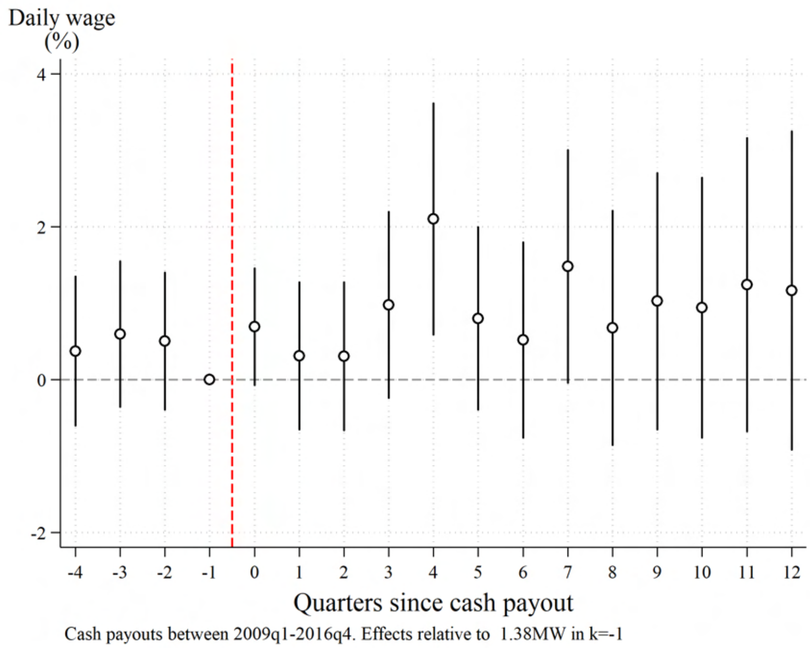
Figure A.14: Employment by Age Group

(a) Ages 18–39 (b) Ages 40–55



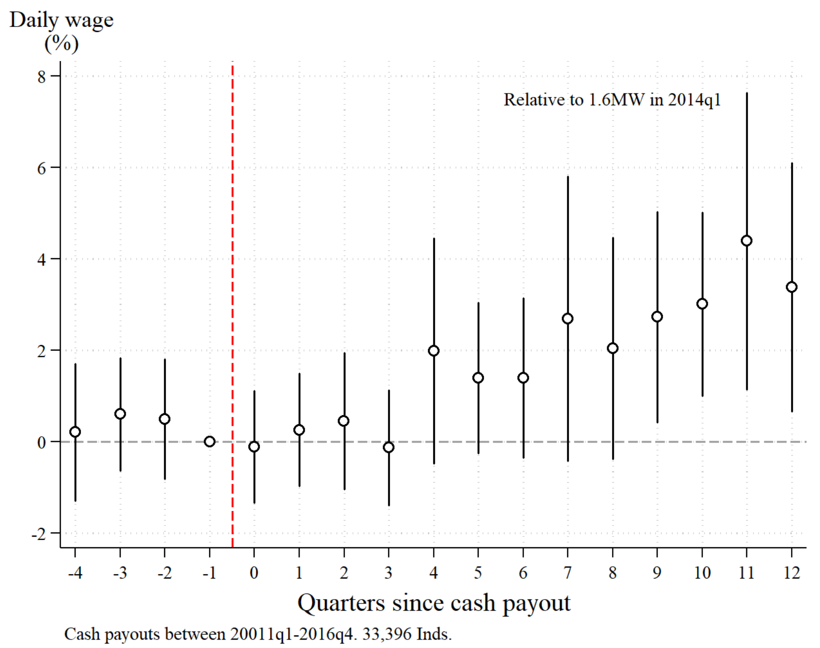
Notes: These figures decompose the negative employment result from Figure 3 between younger and older individuals in Panels (a) and (b), respectively. The employment drop is driven by younger workers aged 18–39, for which the formal employment drop remains around 2 percent lower and significant at the 5 percent level even three years after reparation payout.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.15: Daily Wages (Excluding Never-Treated)



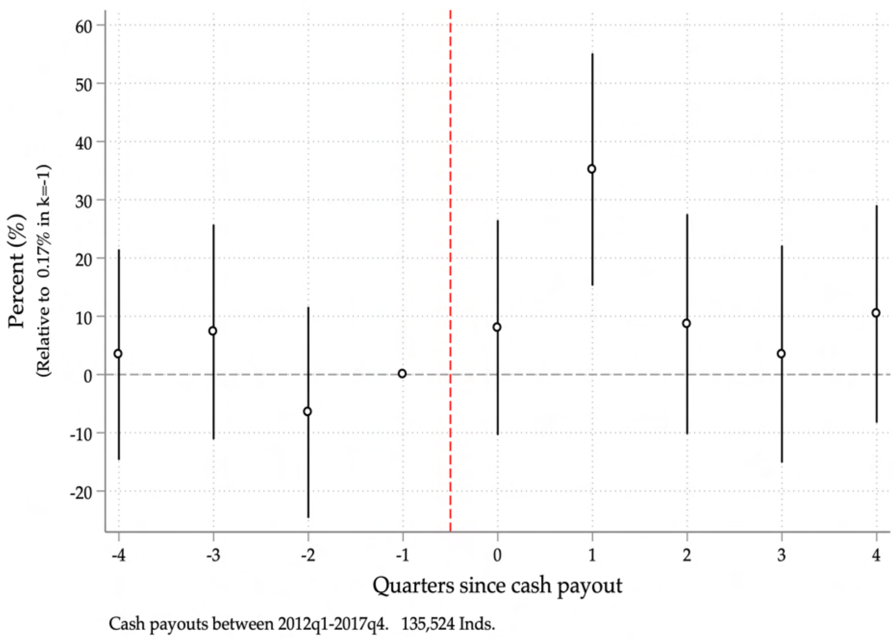
Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—daily wage for salaried workers (expressed relative to the monthly minimum wage)—is plotted against the distance to reparation payout (in quarters). The treatment is defined at the individual level and the sample is balanced in event time and excludes victims that did not receive a reparation, i.e., the never-treated. Figure 4 presents these results including these individuals. Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.16: Daily Wages (Balanced sample)



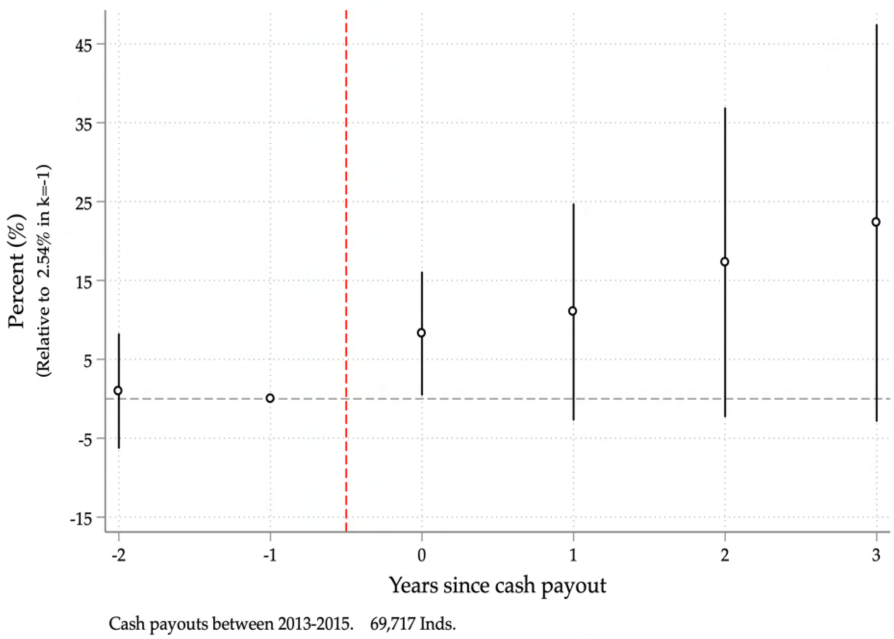
Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—daily wage for salaried workers (expressed relative to the monthly minimum wage)—is plotted against the distance to reparation payout (in quarters). The treatment is defined at the individual level. The sample is balanced in event time and restricted to individuals that never shifted out of wage employment in our period of analysis. Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and PILA.

Figure A.17: Entrepreneurship: Registering a New Business (Excluding Never-Treated)



Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual registered a new business that quarter—is plotted against the distance to cash payout (in quarters). The likelihood of registering a new business one quarter after cash payout ($k = 1$) increased by 36 percent from a base of 0.18 percent, and this effect is significant at the 1 percent level. The treatment is defined at the individual level, and the sample is balanced in event time and excludes victims that did not receive a reparation, i.e., never-treated individuals (Figure 5 presents results including these individuals). Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and Confecámaras.

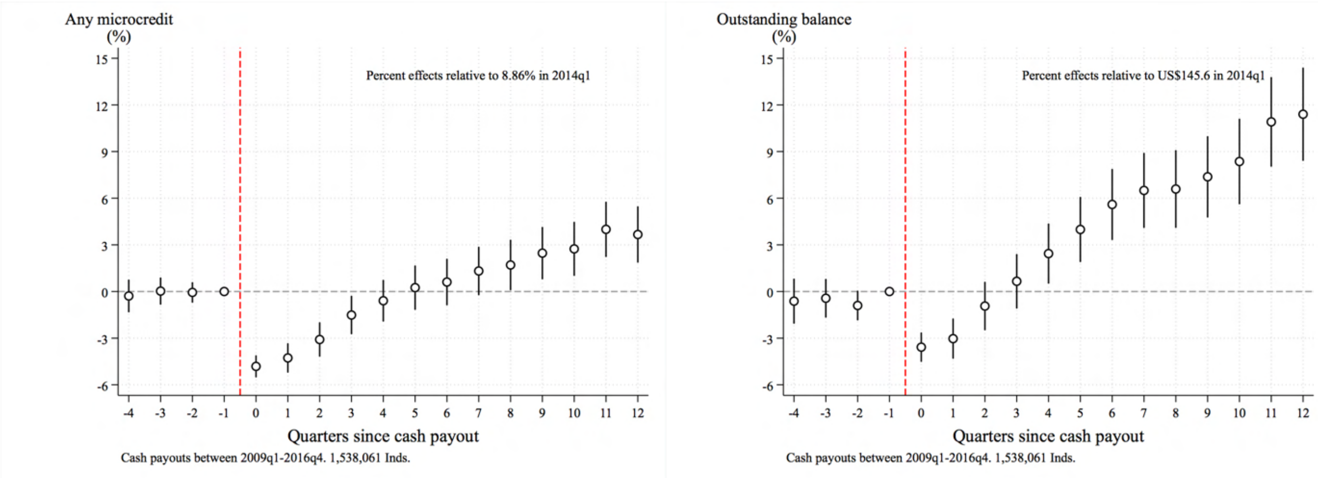
Figure A.18: Firm Survival (Excluding Never-Treated)



Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual has an active business that year—is plotted against the distance to cash payout (in years). We measure active business as having a valid license, which firms are required to renew every year. The likelihood of having an active business increases by 10.2 percent the year of cash payout ($k = 0$) from a base of 2 percent, and the coefficient is significant at the 5 percent level. The magnitude of the effect persists at least three years after the reparation payout, although it is significantly less precisely estimated and does not allow rejecting the null of no effect. The treatment is defined at the individual level, and the sample is balanced in event time and excludes victims that did not receive a reparation, i.e., never-treated individuals (Figure 6 presents results including these individuals). Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and Confecámaras.

Figure A.19: Microcredit

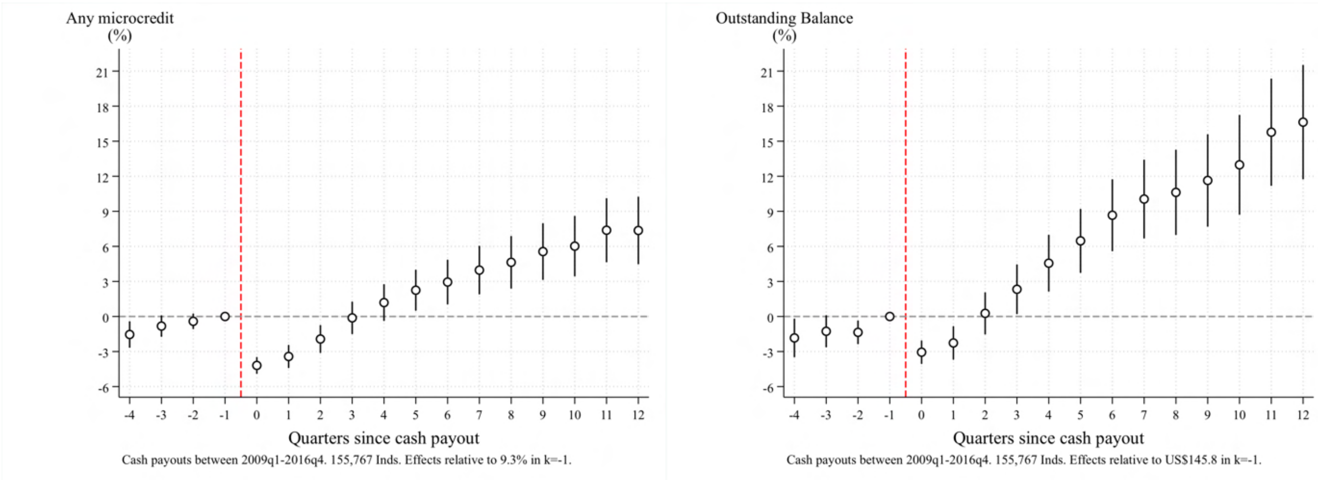
(a) Any Microcredit (b) Outstanding Balance



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the distance to reparation payout (in quarters). In Panel (a), the outcome is the likelihood of owing any microcredit. Around 8.9 percent of control victims owe some microcredit in 2014q1. In Panel (b), the outcome is outstanding balance, measured in constant US dollars and including zeros for those who do not owe any microcredit. Panel (a) shows that reparation induces victims to pay off their microloans: the probability of owing microcredit drops in $k = 0$. This effect is short-lived: three years after, the reparation raises the likelihood of owing any microcredit. Similarly, Panel (b) shows that the amount of money owed increases three years after receiving the money, suggesting reparation increases victims' intensive use of microcredit to fund productive investments. The treatment is defined at the individual level, and the sample is balanced in event time and includes never-treated individuals. Standard errors are clustered at the household level. Figure A.20 presents these results excluding never-treated individuals.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

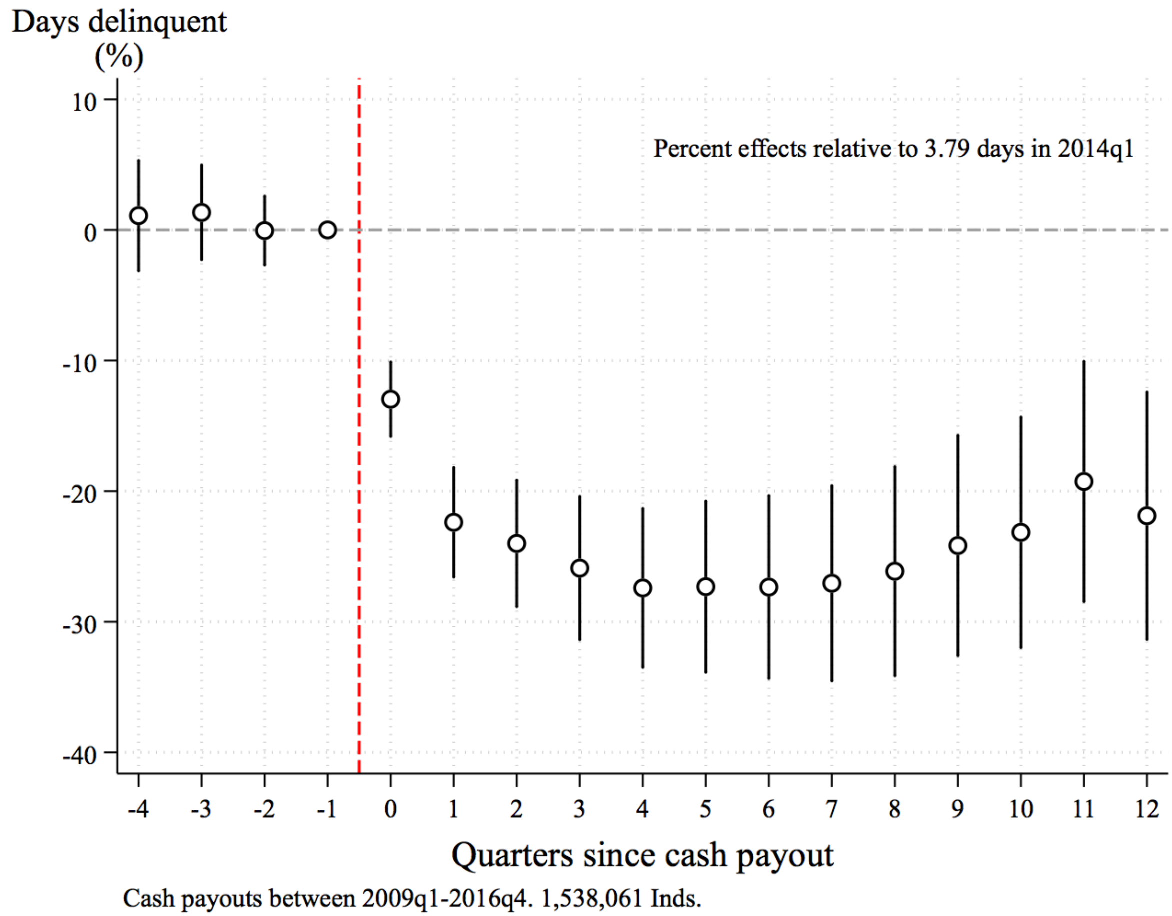
Figure A.20: Microcredit (Excluding Never-Treated)

(a) Any Microcredit (b) Outstanding Balance



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the distance to reparation payout (in quarters). In Panel (a), the outcome is the likelihood of owing any microcredit. Around 9.3 percent of victims owe some microcredit in $k = -1$. In Panel (b), the outcome is outstanding balance, measured in constant US dollars and including zeros for those who do not owe any microcredit. The treatment is defined at the individual level, and the sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level. Figure A.19 presents these results including never-treated individuals.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

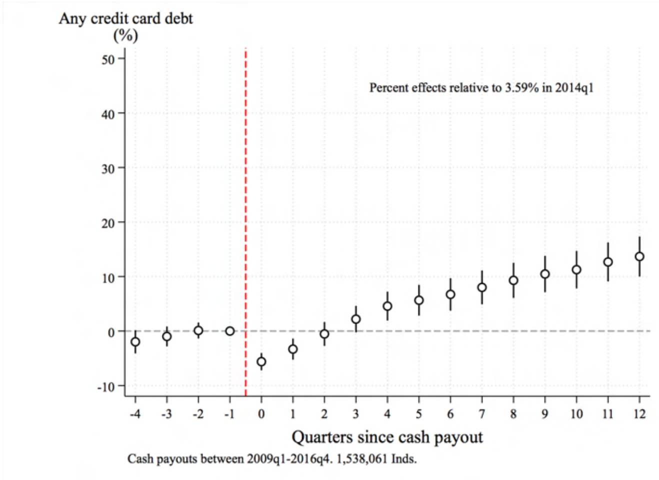
Figure A.21: Number of Days Delinquenct in Microcredit



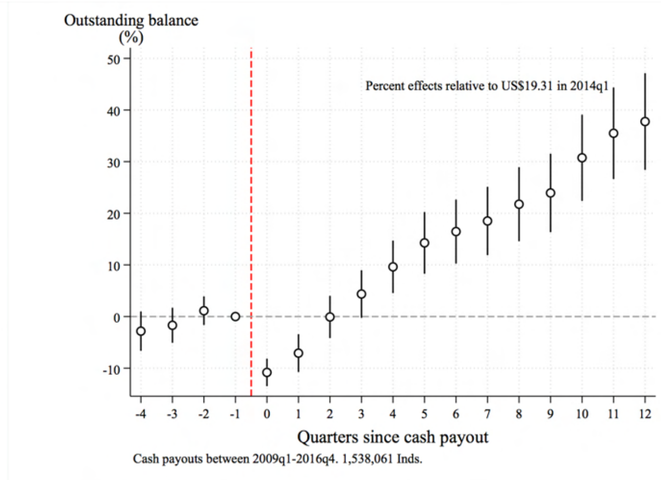
Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—the number of days delinquent for a microloan—is plotted against the distance to reparation payout (in quarters). On average, control victims are 3.8 days delinquent on a microloan in 2014q1. Despite the more intensive use of microcredit show in Figure A.19, delinquency falls significantly and remains lower three years after reparation. The treatment is defined at the individual level, and the sample is balanced in event time and includes never-treated individuals. Standard errors are clustered at the household level.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.22: Consumption

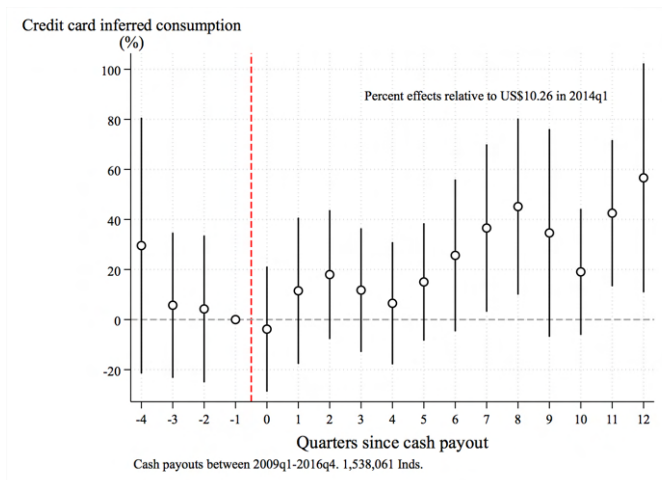
(a) Any Credit Card Debt



(b) Outstanding Balance on Credit Card Debt



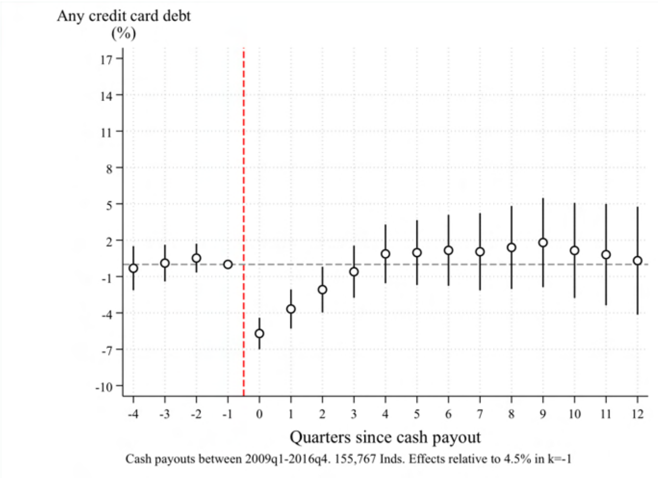
(c) Inferred Consumption



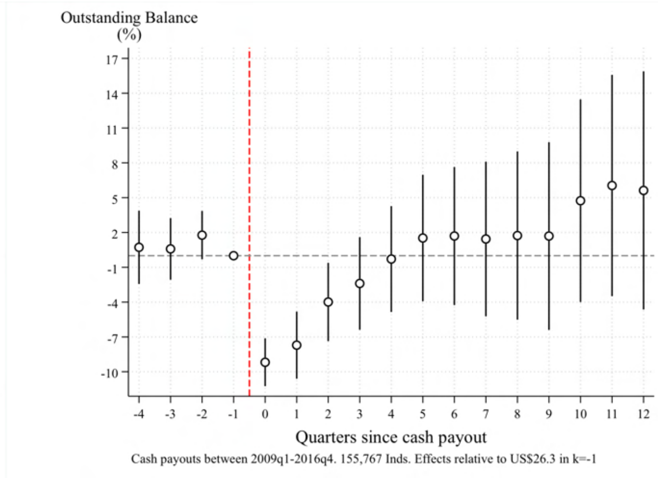
Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the distance to reparation payout (in quarters). The outcome is owing any credit card debt in Panel (a) and the outstanding balance on credit card debt in Panel (b). Panel (c) measures quarterly consumption or credit card activity as the difference between the outstanding balances (excluding interest) between the current and the previous quarter, plus the payments made in the quarter. The outcomes are measured in constant US dollars and include zeros for people who do not owe debt. Panel (a) shows the likelihood of owing any credit card debt decreases from a base of 3.6 percent for never-treated victims in 2014q1. The coefficient increases in magnitude over time, becoming close to zero and non-significant two quarters after payout, and then positive and significant four quarters after the event. Similarly, Panel (b) shows outstanding credit card debt has increased by nearly 40 percent. Panel (c) shows that consumption increases over time such that, three years after cash the payout, credit card activity has increased almost 60 percent due to the reparation. Treatment is defined at the individual level, and the sample is balanced in event time and includes never-treated individuals. Standard errors are clustered at the household level. Figure A.23 presents these results excluding never-treated individuals.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.23: Consumption (Excluding Never-Treated)

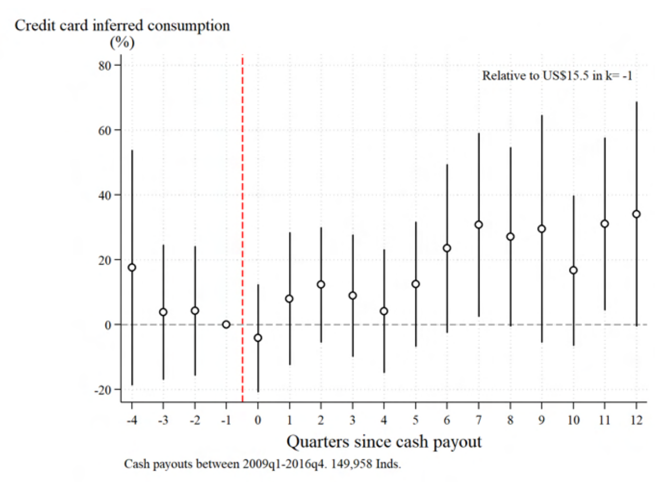
(a) Any Credit Card Debt



(b) Outstanding Balance on Credit Card Debt



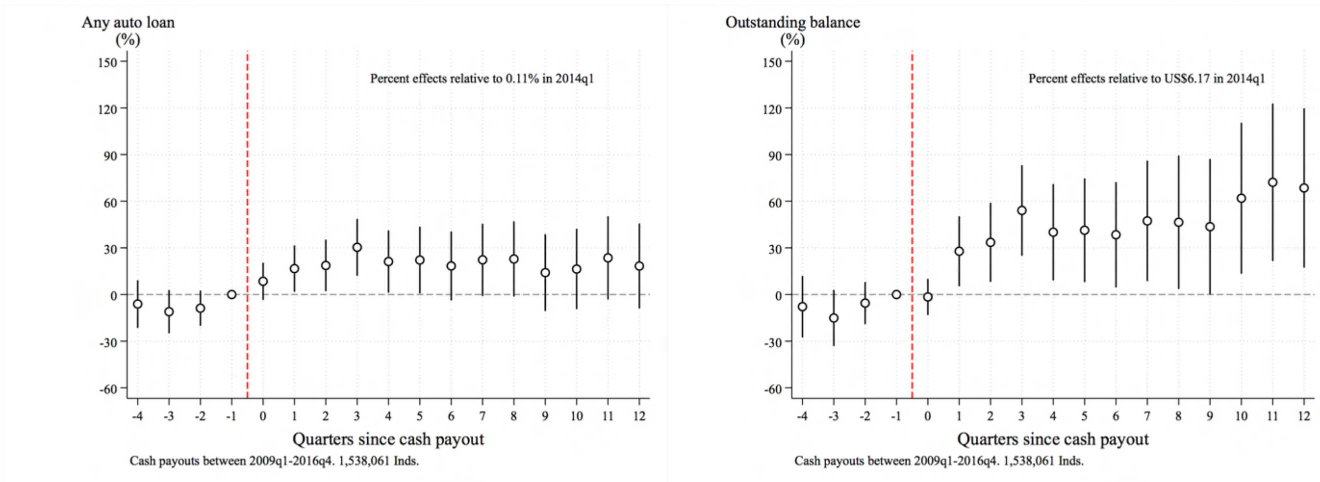
(c) Consumption



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the distance to reparation payout (in quarters). The treatment is defined at the individual level, and the sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level. Figure A.22 presents these results including never-treated individuals.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.24: Consumption of Durables Assets: Motor Vehicles

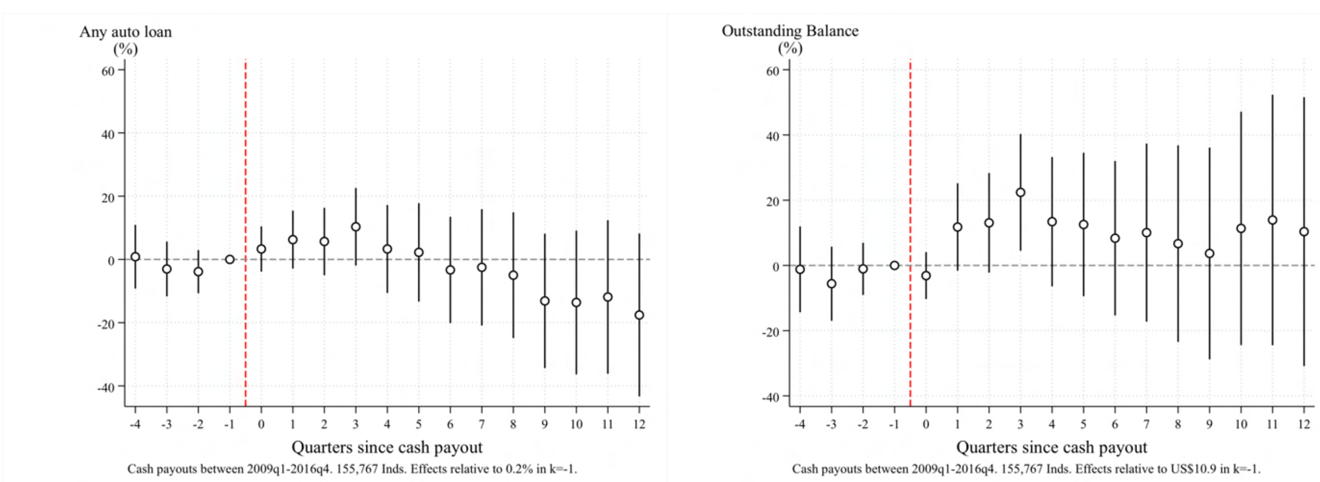
(a) Any Auto Loan (b) Outstanding Balance on Auto Loan Debt



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the distance to reparation payout (in quarters). The outcome is owing any auto loan in Panel (a) and the amount of outstanding balance on auto loan in Panel (b). The outcomes are measured in constant US dollars and include zeros for people who do not owe auto loans. Auto loans increase after reparation such that, three years after payout, victims have over 60 percent more auto loan debt. Treatment is defined at the individual level, and the sample is balanced in event time and includes never-treated individuals. Standard errors are clustered at the household level. Figure A.25 presents these results excluding never-treated individuals.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

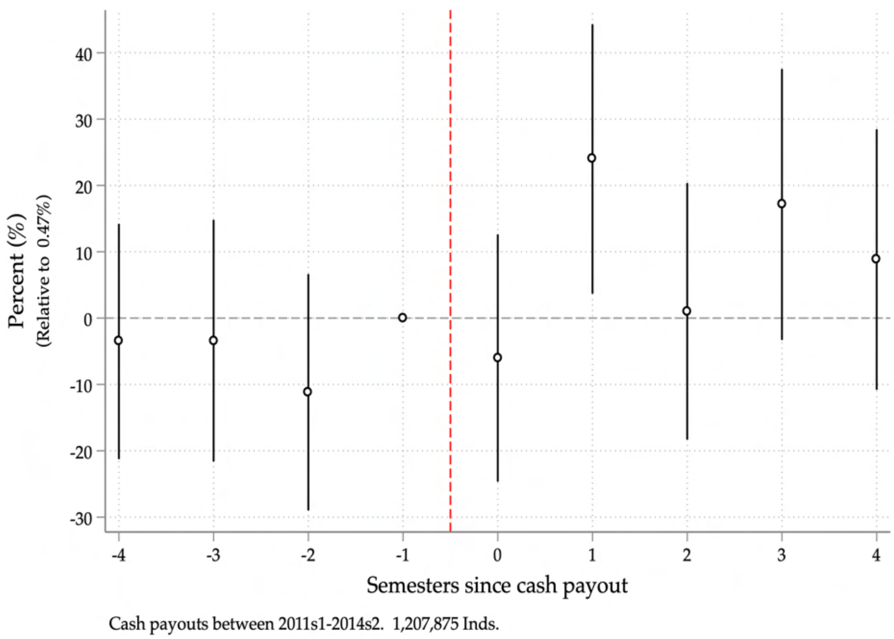
Figure A.25: Consumption of Durables Assets: Motor Vehicles (Excluding Never-Treated)

(a) Any Auto Loan (b) Outstanding Balance on Auto Loan Debt



Notes: This figure presents the event study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the distance to reparation payout (in quarters). The treatment is defined at the individual level, and the sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level. Figure A.24 presents these results including never-treated individuals.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and the Financial Superintendence of Colombia.

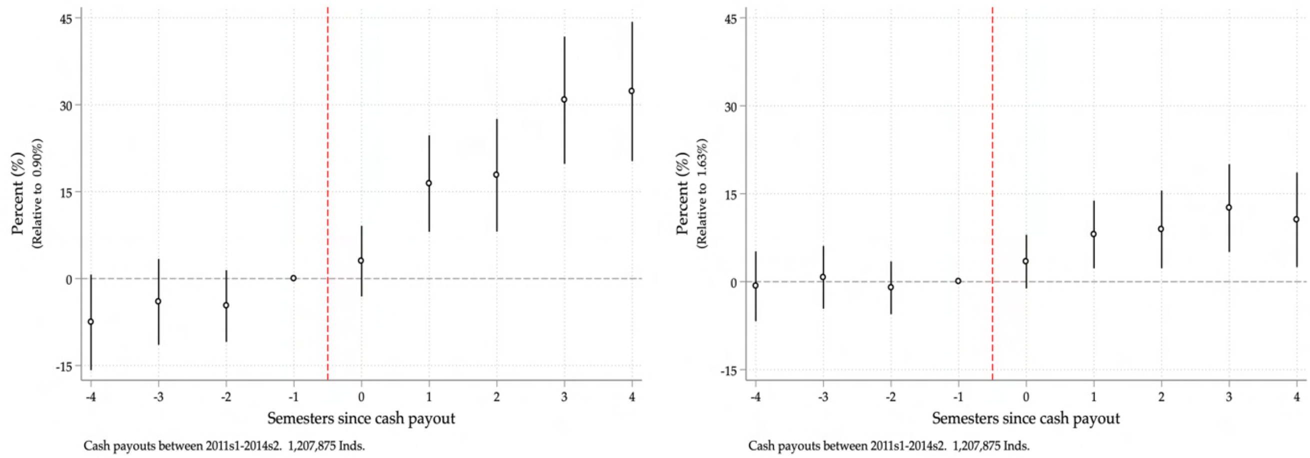
Figure A.26: First Enrollment in an Four- or Five-Year Undergraduate Program



Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a four- or five-year undergraduate program for the first time—is plotted against the distance to cash payout (in semesters). Treatment is defined at the household level, and event time is defined as the first date in which any household member received a cash reparation. The sample (i) is balanced in event time; (ii) is restricted to individuals aged 15 to 25 at $k = -1$; and (iii) includes never-treated individuals. Standard errors are clustered at the household level. The figures suggest cash reparations encouraged access: the likelihood of attending an undergraduate program for the first time increases by 24 percent from a base of 0.47 percent among never-treated individuals aged 15 to 25.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

Figure A.27: Undergraduate Enrollment: Private versus Public Institutions

(a) Private Institutions (b) Public Institutions

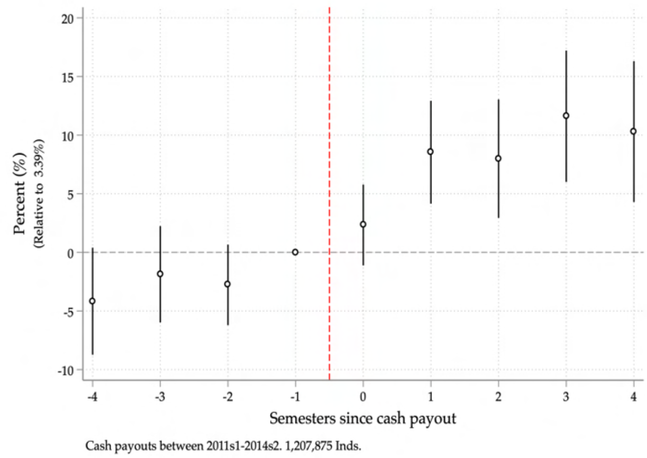


Notes: This figure decomposes the undergraduate enrollment result from Figure 10 by private versus public institutions. Relative to the baseline mean, cash reparations particularly encouraged enrollment at private institutions. Figure A.31 presents these results excluding never-treated individuals, and shows quantitatively similar effects.

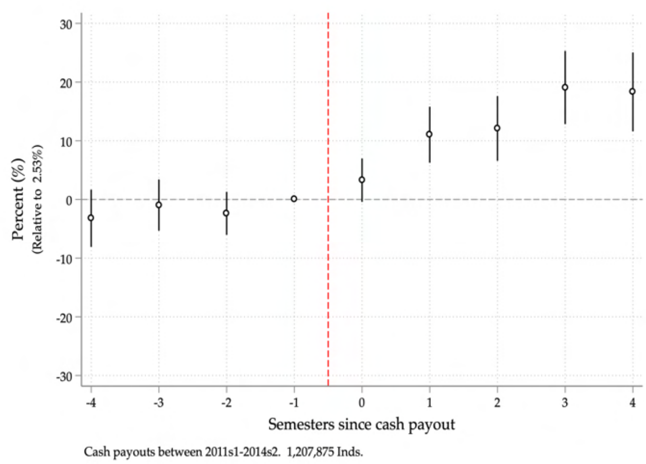
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

Figure A.28: Postsecondary Enrollment

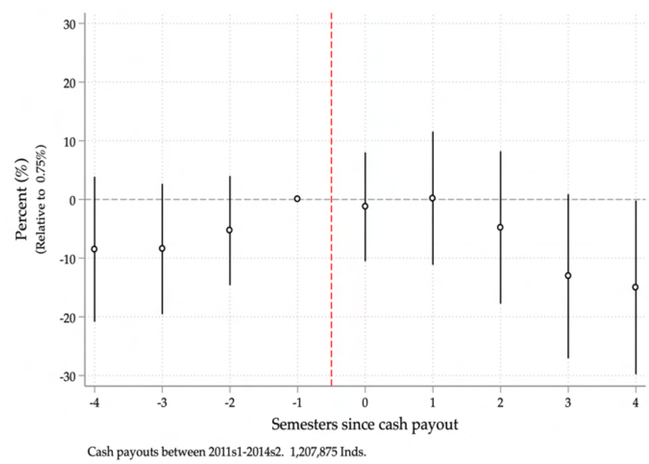
(a) Any Institution



(b) Universities



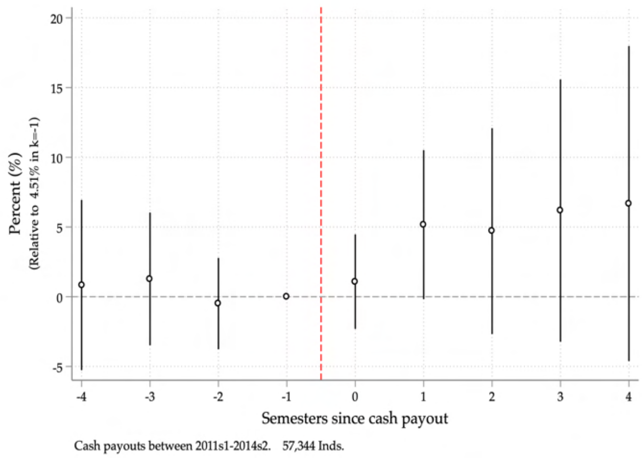
(c) Technical and Technological Institutions



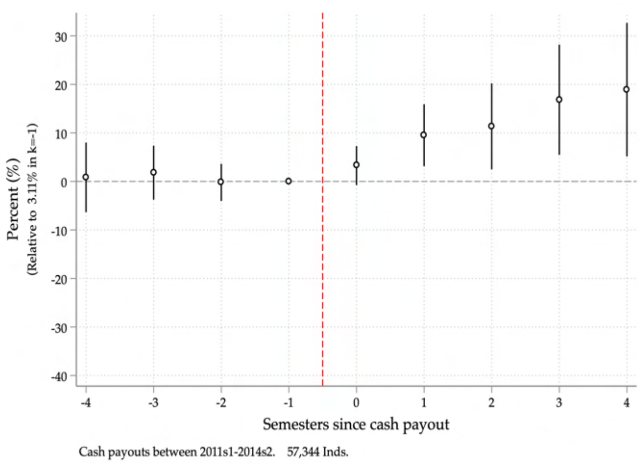
Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a postsecondary education institution in a given semester—is plotted against the distance to cash payout (in semesters). Panel (b) restricts the outcome to enrollment at a university, while Panel (c) does the same for enrollment at a technical or technological institution. Treatment is defined at the household level, and event time is defined as the first date in which any household member received a cash reparation. The sample (i) is balanced in event time; (ii) is restricted to individuals aged 15 to 25 at $k = -1$; and (iii) includes never-treated individuals. Standard errors are clustered at the household level. The figures suggest cash reparations encouraged postsecondary enrollment, with compounding effects over time. Panel (a) shows postsecondary enrollment increased by 10.3 percent by $k = 4$ on a control mean of 3.4 percent. Panel (b) shows the rise is particularly sizable at universities, where the increase at $k = 4$ is of 18.3 percent. Panel (c) shows enrollment at technical or technological institutions *decreased* by 15 percent, suggesting individuals are also switching across institutions. Figure A.29 presents these results excluding never-treated individuals, and shows quantitatively similar effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

Figure A.29: Postsecondary Enrollment (Excluding Never-Treated)

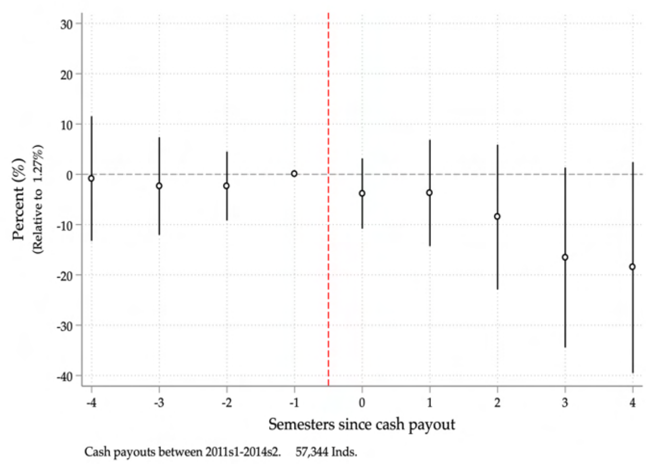
(a) Any Institution



(b) Universities



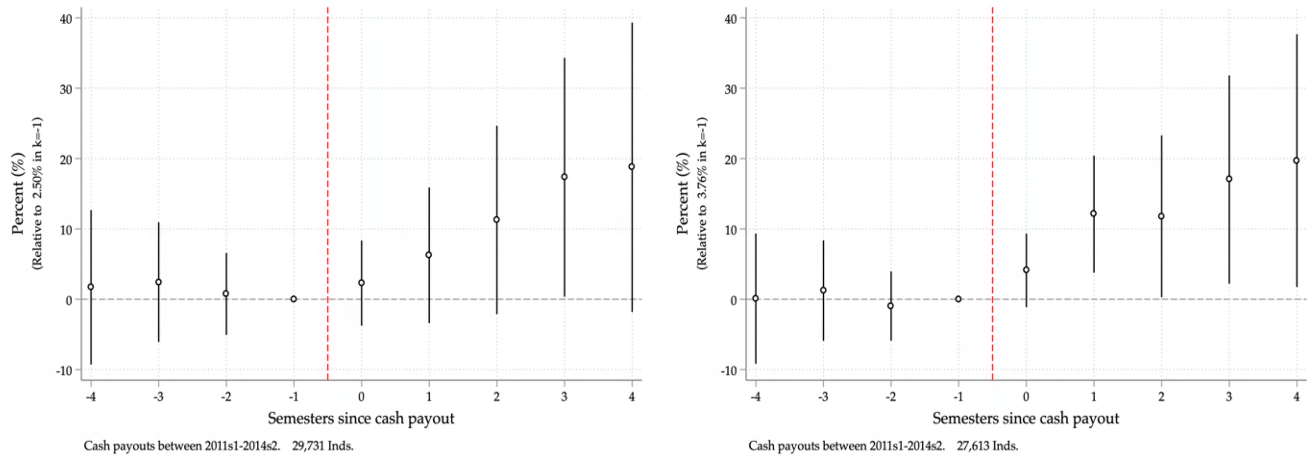
(c) Technical and Technological Institutions



Notes: This figure presents the event study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a postsecondary education institution in a given semester—is plotted against the distance to cash payout (in semesters). Panel (b) restricts the outcome to enrollment at a university, while Panel (c) does the same for enrollment at a technical or technological institution. Treatment is defined at the household level, and event time is defined as the first date in which any household member received a cash reparation. The sample (i) is balanced in event time; (ii) is restricted to individuals aged 15 to 25 at $k = -1$; and (iii) includes never-treated individuals. Standard errors are clustered at the household level. The figures suggest cash reparations encouraged postsecondary enrollment, with compounding effects over time. Panel (a) shows postsecondary enrollment increased by 6.7 percent by $k = 4$ on a control mean of 4.5 percent, although the effect is not statistically significant. Panel (b) shows the rise is particularly sizable at universities, with a 18.2 percent increase by $k = 4$. In contrast, Panel (c) shows that enrollment at technical or technological institutions *decreased* by 18.5 percent, though the effect is only marginally significant. Figure A.28 presents these results including never-treated individuals, and shows quantitatively similar but more precisely estimated effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

Figure A.30: Undergraduate Enrollment by Sex (Excluding Never-Treated)

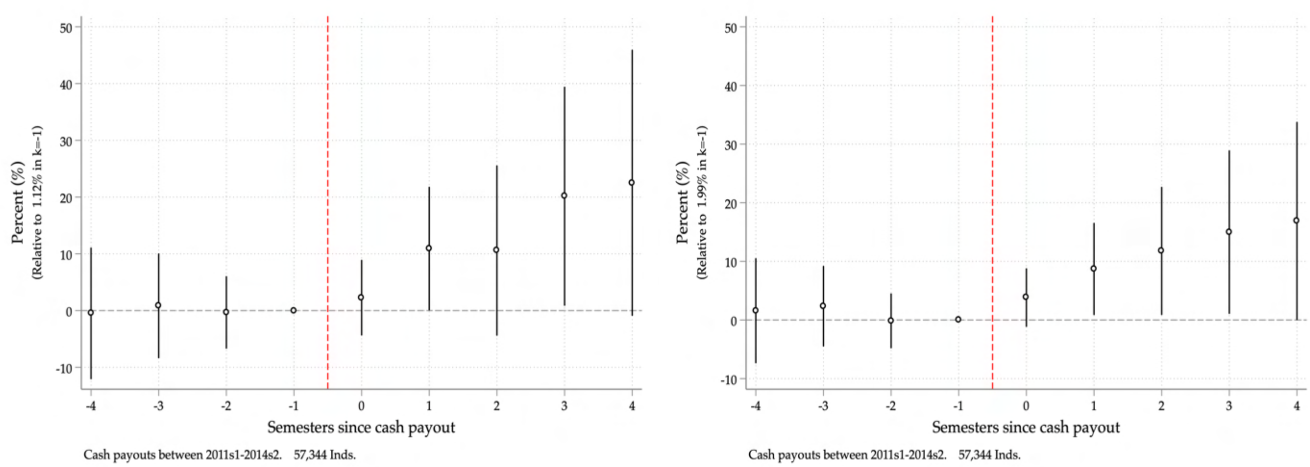
(a) Men (b) Women



Notes: These figures decompose the results from Figure A.29, Panel (b), by sex. The positive enrollment effect is more immediate and precisely estimated among women. Figure A.28 presents these results including never-treated individuals, and shows quantitatively similar but more precisely estimated effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

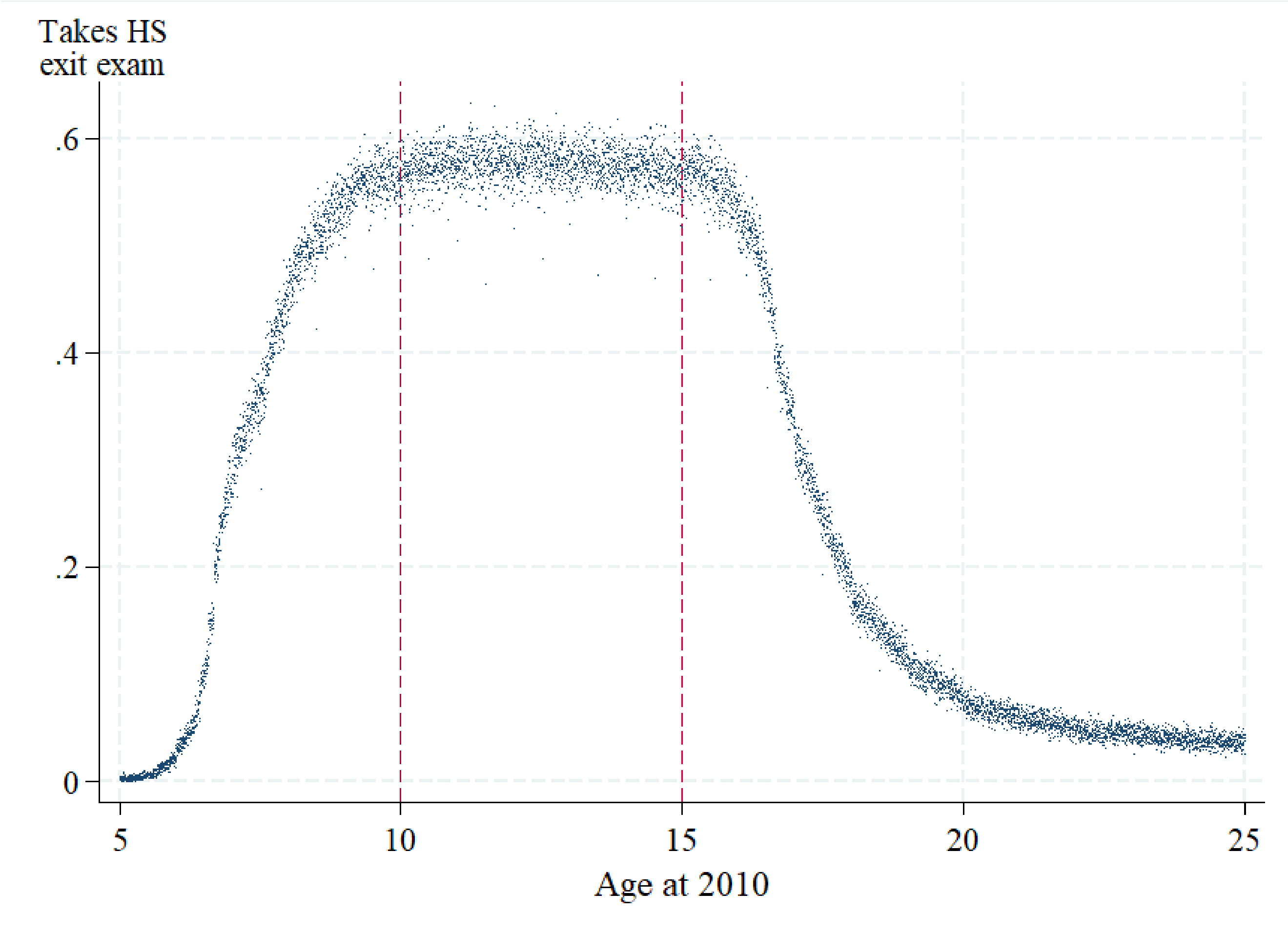
Figure A.31: Undergraduate Enrollment: Private versus Public Institutions (Excluding Never-Treated)

(a) Private Institutions (b) Public Institutions



Notes: This figure decomposes the undergraduate enrollment result from Figure A.29, Panel (b), by type of institution: private versus public. cash reparations encouraged enrollment at private universities, as well as not high-quality public universities. Figure A.27 presents these results including never-treated individuals, and shows quantitatively similar but more precisely estimated effects.
Source: Authors' calculation using RUV data from SRNI, SISBEN, and SPADIES.

Figure A.32: Likelihood of taking high school exit exam by age in 2010



Notes: This figure plots the likelihood of presenting SABER 11 between 2010 and 2019 as a function of the age in 2010 for people identified in SISBEN.
Source: Authors' calculation using data from SISBEN and ICFES.