REPARATIONS AS DEVELOPMENT? EVIDENCE FROM VICTIMS OF THE COLOMBIAN ARMED CONFLICT*

Arlen Guarin
Juliana Londoño-Vélez
Christian Posso

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Abstract

We estimate the effects of reparations on victims' well-being. We leverage variation from Colombia's reparations program for victims of forced displacement, homicide, landmines, and other human rights violations in armed conflict. We focus on financial reparations, which consist of a one-off, lump-sum, non-means-tested, and unconditional payment of up to US$10,000 (PPP US$26,000), or about three times the annual household income of victims. The transfer is highly progressive because most victims are poor. We exploit the staggered rollout of the reparations payments and their unanticipated receipt using event-study approaches to identify causal effects up to four years later. We construct novel and comprehensive administrative panel microdata and estimate effects on work, living standards, health care utilization, and the next generation's human capital. There are four main results. First, reparations induce a slight shift of workers from high-risk and low-paying formal jobs. Some workers spend more time out of formal employment and find higher-paying jobs. Others invest the money in new businesses, making businesses more sustainable. Second, reparations improve victims' living conditions, boosting consumption and homeownership. Third, reparations reduce victims' emergency department visits, hospitalizations, and medical procedures, consistent with improved health. Fourth, victims invest the reparations payment in their children, raising their standardized test scores and postsecondary school attendance. Despite their fiscal cost, reparations are cost beneficial. We conclude that reparations substantially improve victims' well-being without meaningfully distorting their labor supply and have value for development in addition to their value for transitional justice.

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*Corresponding author: Arlen Guarin is an Economist at the World Bank's Development Impact Evaluation (DIME) department, 1818 H St. NW, MC 4-404 Washington, DC 20433 (e-mail: aguarin@worldbank.org, 510-529-8376). Juliana Londoño-Vélez is an Assistant Professor of Economics at UCLA and NBER Faculty Research Fellow. Christian Posso is a Researcher at Banco de la República de Colombia. We are grateful to Emmanuel Saez, Edward A. Miguel, Hilary Hoynes, and David Card for their generous feedback, support, and encouragement. We also thank Natalie Bau, Arun Chandrasekhar, Kaveh Danesh, Nicolás de Roux, Fred Finan, Pamina Firchow, Rema Hanna, Ana María Ibáñez, Larry Katz, Supreet Kaur, Roxani Krystalli, Pat Kline, Adriana Lleras-Muney, Enrico Moretti, Paul Niehaus, Naomi Roht-Arriaza, Jesse Rothstein, Adriana Rudling, Yotam Shem-Tov, Catalina Vallejo, and Christopher Walters for insightful suggestions, as well as many seminar participants for helpful comments. We thank the Victims' Unit, the Department of National Planning, Confecámaras, MinSalud, Catastro Antioquia, and ICFES for their support. We thank Badir Ali Badran, Alejandra Hoyos, Lorena Trujillo, Juan Sebastián Vargas, Carlos Medina, Fabio Sánchez, and Santiago Rengifo for their generous help and feedback. Silvia Granados, Sara Londoño, Nicolás Mancera, Brayan Pineda, Estefanía Saravia, and Santiago Velásquez Bonilla provided outstanding research assistance. Arlen and Juliana gratefully acknowledge financial support from the Center for Effective Global Action, the Weiss Family Program Fund, and the Center for Equitable Growth at UC Berkeley. The findings, interpretations, and conclusions expressed in this paper do not necessarily reflect the views of Banco de la República or its Board of Directors. This study was approved by UC Berkeley's IRB (IRB 2020-08-13590).

1 Introduction

Can reparations help victims of human rights violations rebuild their lives? More than 30 countries—including post–World War II Germany and the United States, postconflict Côte d'Ivoire and Indonesia, and post-authoritarian Brazil and Chile—have implemented reparations programs to recognize and address the harms victims have suffered during episodes of war, conflict, and authoritarianism, either because the state directly committed atrocities or because it failed to prevent them. As a transitional justice mechanism, reparations may take the form of financial compensation, restitution, rehabilitation, satisfaction, and guarantees of non-repetition (United Nations, 2005). Scholars and practitioners claim that these measures' material and symbolic benefits are critical for pursuing truth, justice, non-repetition, and repair (de Greiff, 2009; de Greiff, ed, 2006). Moreover, the financial-compensation component of reparations—the payment of money to victims—might improve victims' well-being because many are poor or otherwise disadvantaged (Roht-Arriaza and Orlovsky, 2009; Uprimny-Yepes, 2009).

Despite these hypothesized effects, reparations remain controversial (Darity and Frank, 2003; Dixon, 2017; Dixon et al., 2019). On the one hand, proponents argue that reparations promote a more just and inclusive society and narrow the socioeconomic gaps formed by victimization. On the other hand, detractors point out that targeting recipients is difficult, payments are costly, and reparations may fail to improve long-run economic outcomes if recipients lack the necessary inputs to invest the money. Given the enormous political and economic costs of reparations programs, surprisingly few studies provide evidence of their causal effects. There are two main challenges in evaluating their impacts. First, victims' registries are either nonexistent or miss a substantial number of victims (OHCHR, 2009; Rivas, 2018; Sikkink et al., 2015). Second, simple comparisons of outcomes between reparations recipients and non-recipients suffer from omitted variable bias, and sources of exogenous variation in reparations receipt have been elusive.

This paper studies the effects of the largest reparations program in history. As part of its transition into peace, in 2011, Colombia committed to compensating more than seven million victims of forced displacement, homicide, landmines, rape, torture, and other human rights violations committed by guerrillas, paramilitary groups, and state forces. We focus on the financial-compensation component of reparations, which consists of one-time, lump-sum, non-means-tested, and unconditional cash transfers paid out to more than a million victims over the past decade (UARIV, 2021). The payment scale is based on the type of abuse the victim suffered and ranges from US$4,300 to US$10,200 per individual, or roughly US$26,000 at purchasing power parity (PPP) in 2019. While they are not means tested, reparations are highly progressive because most victims are within the poorest quartile of Colombia's households, with the average transfer representing over three times the recipients' baseline annual household income. Although the transfer is unconditional, the program has "labeled" reparations as transformative tools for victims to rebuild their lives and has explicitly endorsed investments in the next generation's human capital and victims' entrepreneurial activities and homes.

The funding for Colombia's reparations programs comes primarily from the national budget. Due to budgetary and operational constraints, Colombia has staggered the rollout of reparations over time, with fewer than 1 percent of eligible victims receiving reparations yearly. Crucially, victims cannot anticipate when they will receive the money—a fact backed by anecdotal and survey evidence (Sikkink et al., 2015) and confirmed empirically by our data. We exploit these features of our setting to estimate the causal effects of reparations using an event-study approach. To trace people's outcomes, we construct a novel administrative panel data set 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. This ability to link multiple data sets for millions of Colombians allows us to quantify the impacts of reparations on victims' well-being. We divide our findings into three main parts: (1) impacts on work and living standards, (2) effects on health care utilization, and (3) intergenerational impacts on human capital investments.

Regarding impacts on employment and work conditions, the canonical labor supply model predicts that an exogenous increase in income will reduce labor supply through income effects (Cesarini et al., 2017; Golosov et al., 2021; Imbens et al., 2001). Indeed, we find that the large lump-sum reparations payments discourage formal wage employment by 0.2 percentage points, or 1.7 percent. However, this effect is economically small and driven by young workers shifting out of high-risk and low-wage jobs. Some workers spend more time out of formal employment and end up in higher-paying jobs, consistent with reparations improving victims' outside options and increasing efficiency. Others change their occupation and invest the money in income-generating activities outside the formal wage-labor market. Indeed, reparations encourage victims to start new businesses and keep their businesses active even three years later. Moreover, the uptake of microcredit usage for self-employed professionals and small entrepreneurs further suggests that reparations stimulate entrepreneurial activity. These results are consistent with the explanation that imperfect financial markets, resource misallocation, and initial start-up costs hold low-income people below their potential (Andersen and Nielsen, 2012; Bandiera et al., 2017; Bianchi and Bobba, 2013; Blattman and Dercon, 2018; de Mel et al., 2013; Gertler et al., 2012; Giorgi et al., 2018; Holtz-Eakin et al., 1994; Jayachandran, 2020; Ulyssea, 2018).

Regarding impacts on living standards, we find that reparations raise victims' consumption of durables and non-durables. Specifically, data from credit card spending and auto loans show that the money enables victims to pay off their debts and boost their consumption levels, with these effects persisting even three years after households receive reparations. Cadastral property records for real estate transactions show meaningful increases in cumulative land and home purchases. These results are consistent with the explanation that large money transfers from reparations relieve borrowing constraints and enable people to increase both consumption and asset holdings.

Regarding impacts on health, we find that reparations cause significant and economically meaningful drops in health care utilization, including emergency department (ED) visits, hospitalizations, and medical procedures. For example, victims are 5.38 percent less likely to visit the ED one year after receiving a reparations payment, and this drop compounds over time. Four years after reparations, ED visits are 17.15 percent less frequent. Similarly, the chances of hospitalization and medical procedures also fall considerably, indicating that wealth improves people's health. While interpreting drops in health care utilization as welfare enhancing may seem counterintuitive to those accustomed to thinking of settings where care is rationed, financial barriers are unlikely to prevent contact with the health care system in Colombia: there is near-universal health care, low household out-of-pocket spending, and an equal basket of health services for people in and out of formal employment (OECD, 2019). Indeed, the diagnoses made during these contacts with the health care system support this conclusion and shed light on the underlying mechanisms. There is an immediate drop in musculoskeletal and external causes of morbidity, consistent with the explanation that better work and living conditions improve physical well-being. Four years after the receipt of reparations, chronic conditions also ameliorate, possibly because reduced stress, healthier behaviors, and better environments take longer to manifest as improved health (O'Connor et al., 2021).

Regarding intergenerational impacts on human capital investments, we first estimate the effects of reparations on high school outcomes by comparing test-age children whose households received reparations when the child had more versus less time left in school. Victims invest the money from reparations in their children's human capital. While we see no effects on high school graduation, combined high school exit-test scores increase by 7 percent of a standard deviation. These gains are larger for younger victims, who had more time to benefit from reparations. In addition to these learning effects, an event study shows improved access to postsecondary education, with reparations increasing first-time undergraduate enrollment by 24 percent and overall attendance by 18 percent. Consistent with the explanation that financially constrained households invest the money from reparations in children's college education, the relative gains in matriculation are bigger at private institutions, where expensive tuition fees traditionally discourage low-income students' attendance. These findings are consistent with previous work on the benefits of antipoverty programs for the next generation (Aizer et al., 2016; Barr and Gibbs, 2022) and show that cash transfers boost children's test scores and postsecondary attendance.[1]

Last, we offer a stylized cost-benefit analysis that compares the pecuniary cost of reparations to the estimated benefits three and four years afterward. In calculating the dollar-

[1]There is mixed evidence on cash transfers improving children's test scores (García and Saavedra, 2022; Molina-Millan et al., 2019) and scant evidence on the effects of wealth on postsecondary attendance (Coelli, 2011; Shea, 2000).

equivalent value of each outcome, we err on the conservative side and construct lower-bound estimates of reparations' economic benefits. Nevertheless, aggregating the estimated effects along the different dimensions, we find that reparations are cost beneficial. Indeed, the returns from Colombia's reparations program are particularly promising in light of our evidence that reparations significantly improve people's health outcomes and benefit children's human capital without meaningfully distorting the labor supply. Further, these effects will likely continue paying off in the long run as they benefit the next generations.

Two important points are worth discussing. First, the Colombian government has sought to spread out reparations geographically to avoid concentrating them in certain regions. Coupled with the fact that fewer than 1 percent of eligible victims have been compensated yearly, this implies that Colombia's reparations program is unlikely to have triggered general equilibrium effects. Second, all victims have access to other social services regardless of their compensation status (e.g., humanitarian assistance, free health care, and subsidized housing), and forcibly displaced victims have had preferential access to social protection programs since 2004. While our empirical approach identifies effects by leveraging variation from the timing of the reparations payout, which is orthogonal to receiving social assistance, this implies that economic well-being might improve for control victims thanks to social transfers. As a result, the sizable impacts we estimate suggest that treated victims substantially benefit from reparations above and beyond traditional welfare.

We contribute to three main bodies of literature. First, our findings contribute to the extensive literature on the effects of random wealth shocks, including unconditional cash transfers (UCTs) and lotteries (see, for instance, Baird et al., 2011, 2013; Balboni et al., 2022; Banerjee et al., 2021; Benhassine et al., 2015; Blattman and Dercon, 2018; Cesarini et al., 2017; de Mel et al., 2012; Egger et al., 2022; Handa et al., 2018; Haushofer and Shapiro, 2016). Unlike the case discussed in Bleakley and Ferrie (2016), the large wealth shock provided by reparations has been invested in the next generation's human capital, reflecting imperfect financial markets. Further, in contrast to Cesarini et al. (2016), money improves health outcomes and children's scholastic performance because recipients are impoverished. Indeed, development economists have shown that, in these contexts, cash transfers can have significant poverty-alleviating effects by increasing consumption and asset building (e.g., Haushofer and Shapiro, 2016). Reparations share this poverty-reducing potential with UCTs since they, too, provide financial resources to an exceptionally vulnerable population in a developing country. Moreover, the large size of Colombia's economic reparations not only raises consumption but also enables meaningful purchases or investments.

Beyond their sheer magnitude, economic reparations differ from traditional welfare in crucial ways. First, reparations target people whose rights have been violated and, as a result, are often not means tested—even though many recipients of reparations may also be poor. Second, reparations do not explicitly or necessarily have an anti-poverty objective. Instead, many transitional justice experts highlight reparations' purpose as acknowledging victims' suffering, expressing social solidarity, and promoting civic trust and reconciliation (e.g., de Greiff, ed, 2006; Firchow, 2017; Gallen and Moffett, 2022). This symbolic value of reparations is illustrated by the fact that Colombia grants economic reparations along with a Dignifying Letter that aims to reinforce the symbolic meaning of the money. It is a personalized letter from the state expressing remorse for the human rights violation and offering economic reparations to transform the victim's life. These distinguishing features of reparations might weaken the impulse to evaluate reparations programs based on their anti-poverty impact. However, our finding that reparations improve victims' socioeconomic well-being and are cost beneficial suggests that reparations programs like Colombia's can have value for development in addition to their value for transitional justice.

We contribute to a long-standing debate on reparations by a multidisciplinary group of political scientists, sociologists, lawyers, and other experts in transitional justice.[2] The scholarly and political debate about reparations centers on whether reparations should provide monetary compensation to recognize victims' suffering and promote civic trust, social cohesion, and reconciliation (de Greiff, ed, 2006; Firchow, 2017; Gallen and Moffett, 2022) or to improve victims' socioeconomic well-being and transform their lives (Roht-Arriaza and Orlovsky, 2009; Uprimny-Yepes, 2009). This is a crucial question for postconflict states aspiring to enforce human rights agendas and foster economic development under tight fiscal constraints (Vallejo, 2019). Unfortunately, there has been little quantitative evidence about the impacts of reparations on victims' socioeconomic well-being, with the literature on this topic primarily consisting of qualitative studies that focus on individuals' perceptions and experiences participating in reparations programs and other transitional-justice interventions.[3] But recent calls for reparations for African American descendants of enslaved people have sparked a renewed interest in understanding the potential economic effects of reparations (Boerma and Karabarbounis, 2022; Darity and Frank, 2003; Piketty, 2022). We contribute to this debate using a real-life reparations program to provide some of the first causal evidence supporting the "reparations as development" model.

Last, we contribute to the literature on programs that relieve extreme poverty by transferring capital to a postwar population (Blattman et al., 2016, 2014, 2020). As mentioned above, abundant evidence shows that cash transfers can lift people out of poverty. However, it is unclear if these results apply to the most marginalized people living in conflict-afflicted countries like Colombia, who may have little or no productive, physical, human, social, and psychological assets, due to forced displacement. Nevertheless, we find that reparations enable households

[2]Adhikari and Hansen (2013); Bogliacino et al. (2022); Casey and Glennerster (2016); Cilliers et al. (2016); de Greiff, ed (2006); Díaz, ed (2008); Dixon (2015); Firchow (2013, 2014, 2017); Gallen and Moffett (2022); Gready and Robins (2014, 2019); Hirsch et al. (2012); Pham et al. (2016); Sánchez-León and Sandoval-Villalba (2020); Sveaass and Sonneland (2015); Vallejo (2019); Weber (2020).

[3]A notable exception is Miller (2011, 2020), which examine the effect of the Cherokee Nation's providing free land to its former enslaved people after the American Civil War.

to improve their health and living standards and start and sustain small businesses, with these benefits persisting three and four years later. Moreover, since the money also benefits the next generation, the positive impacts of reparations are unlikely to dissipate fully in the future.

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. Finally, section 6 concludes.

2 Background: Colombia's Conflict and Victim Reparations

Colombia has had the most prolonged internal armed conflict in the Western Hemisphere.[4] In the early 1960s, left-wing rebel groups like FARC–EP (Revolutionary Armed Forces of Colombia–People's Army, the largest guerrilla group) emerged in remote regions of the country. Subsequently, right-wing paramilitary groups developed to contain the emergence of these guerrillas, increase their own territorial influence, and protect landowners and drug lords. Since then, guerrillas, paramilitaries, and state forces have been involved in a contest for the control of territory, with dire consequences for the civilian population (Comisión de la Verdad, 2022).

Colombia's armed conflict victimized 8.9 million people between 1985 and 2019 and claimed hundreds of thousands of lives, according to data from the Unitary Victims Registry (Registro Único de Víctimas, or RUV). After soaring in the 1990s and peaking in the early 2000s, the frequency of victimizations declined with Colombia's attempts to transition into peace and reconciliation, including peace accords with multiple armed actors (figure A.1). The conflict was widespread: over 90 percent of municipalities suffered victimizations, and rural areas were disproportionately affected (see Ibáñez, 2008, and Figure A.2).

Figure I reports the number of victims by the 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.[5] This represents the world's highest number of forcibly displaced people in 2021 (United Nations High Commissioner for Refugees, 2021). Moreover, 1.2 million people had relatives who were murdered or forcibly disappeared due to the conflict (henceforth, "indirect" victims). Thousands of others were raped, kidnapped,

[4]The precise starting point of Colombia's conflict is a matter of debate (LeGrand, 2003; Sánchez-León and Rudling, 2019). Some scholars argue that the conflict dates back to Colombia's independence from Spain, while others attribute it to the civil war in 1948–58 known as *La Violencia* (the Violence). Still other scholars blame the political exclusion caused by the consociational regime that ended *La Violencia* and alternated political power between the Liberal and Conservative parties.

[5]A victim of forced displacement is defined as someone who was forced to migrate from their municipality after being a victim of an attack(s) from illegal groups or who migrated to avoid aggression from such groups. 87 percent of all displacement episodes in 2004 were reactive, not preventive, meaning they occurred as a consequence of direct exposure to violence, like direct threats, homicide or homicide attempts, forced disappearances, kidnappings, sexual violence, confrontations between armed groups, or massacres (Ibáñez et al., 2022).

tortured, injured by landmines, or forcibly recruited as minors.

The Colombian government has identified and collected information on victims and provided them with humanitarian aid, access to basic services, and financial compensation (Ibáñez et al., 2022). 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 that they had been victimized and provided information about the victimization. They also reported the date of the victimization and provided some personal information (e.g., name, identification number, date of birth, and contact information).[6] In addition, the state gave monetary compensation to a handful of indirect victims (Law 387/1997 and Law 418/1997).[7]

In 2005, Colombia established its first transitional justice instrument, the Peace and Justice Law, as the state sought to demobilize paramilitary groups and reintegrate them into civilian life. Notably, this effort for reconciliation included the right to reparations for victims. After listening to the victim, a specialized judge assessed their damages and decided on the reparations measures (Law 975/2005). However, the pace of the proceedings was slow (Firchow, 2013; García-Godos and Lid, 2010; Sánchez-León and Rudling, 2019; Sánchez-León and Sandoval-Villalba, 2020; Vallejo, 2019). Consequently, in 2008, Colombia introduced a reform that standardized and broadened the victim reparations program (Decree 1290/2008). Victims registered in the judicial process of Law 975/2005 could obtain compensation from the public budget for homicide and forced disappearance, kidnapping, torture, sexual violence, and forced recruitment by guerrilla or paramilitary organizations. As long as the person was not a victim of state forces, they could receive financial reparations of up to US$10,000, with the scale of the payments based on the type of abuse the victim suffered, as defined by the National Reparations and Reconciliation Commission (CNRR).[8] Some 28,000 victims claimed compensation between 2009 and 2011.

Victims and human rights organizations pressured the state to expand the reparations program to victims of state forces and forcibly displaced victims. As a result, in 2011, Colombia adopted the Victims Law (Law 1448/2011 and Decree 4800/2011), creating one of the world's largest and most ambitious peace-building and recovery programs. It embraces a broad conceptualization of 'reparations,' encompassing the financial reparations and the more complex measures established in international law, namely, restitution, rehabilitation, satisfaction, and guarantees of non-repetition. That is, in addition to financial reparations, the Victims Law also aims

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

[7] In 2004, a landmark ruling by the Constitutional Court ordered the state to apply a differential approach to the delivery of assistance measures for internally displaced persons (Sentencia T-025/2004), putting internally displaced people on the map of transitional justice as a group with special vulnerable status.

[8] The CNRR justified excluding victims of state forces from the official definition of victimhood because "every act by the guerrilla or the paramilitary organizations was illegal, but violations by the army or the police needed to be proven" (Vallejo, 2019, p. 56). Moreover, this definition denied the existence of a civil conflict, emphasizing that Colombia was a case of terrorism—not political violence.

to guarantee rights related to education, health, housing, employment, and income-generation programs, and it includes measures aimed at returning victims' dignity, preserving memory, recovering the truth, and creating the necessary conditions for non-repetition.[9]

Regarding the financial-compensation component of the reparations program, which is the focus of this paper, and which we call "reparations" for simplicity, the Victims Law radically widened its scope to cover victims of the state and victims of guerrilla and paramilitary forces. As a result, virtually all 7.4 million people victimized on or after January 1, 1985, and registered with valid contact information were eligible for reparations by 2031—i.e., roughly one in seven Colombians.[10]

The financial reparations consist of a one-time, lump-sum payment. Its size, defined in multiples of the monthly minimum wage, depends on the type of victimization suffered and the payment regime (i.e., Law 418/1997, Decree 1290/2008, or Law 1448/2011 (see table A.1). The Victims Law sets the size of reparations as follows. First, both indirect victims and direct victims of kidnapping or personal injuries resulting in permanent disability receive the biggest reparations: 40 monthly minimum wages, or around US$10,000. Next, victims of personal injuries resulting in partial disability, child recruitment, or sexual abuse receive up to 30 monthly minimum wages (US$7,600). Last, victims of forced displacement—the vast majority of victims, as shown in figure I—receive either 17 or 27 monthly minimum wages (US$4,300 or US$6,900).[11] If a direct victim is compensated for more than one victimization, they can accumulate reparations of up to 40 monthly minimum wages. Indirect victims can receive even more when compensated for several murdered or forcibly disappeared relatives.

Reparations are assigned at the individual level for most types of victimization. However, for indirect victims and victims of forced displacement, reparations are given to the household and shared among its surviving members. If more than one person claims reparations for a murdered or missing relative, the intra-household allocation depends on the claimant's relationship to the victim. For example, in the case of a widow with two children receiving reparations for her murdered spouse, the widow would receive 50 percent of the reparations payment, and the children would share the remaining 50 percent (i.e., 25 percent each) as illustrated in figure A.4 (see table A.2 for other examples). If the household member who is compensated is a minor,

[9] Implementing non-monetary forms of reparations has been a slow and complex process. For instance, land restitution has been tremendously challenging due to the difficulty of showing tenure or ownership given Colombia's high levels of informality and the state's generalized lack of up-to-date information on landholdings (Sánchez-León and Rudling, 2019). Moreover, the other assistance measures (e.g., housing restitution) involve many other state agencies, rendering coordination difficult. For this reason, most of the progress made in implementing the Victims Law has been achieved in distributing financial reparations (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2019).

[10] Victims of violence linked to the armed conflict occurring before January 1, 1985, have the right to truth, symbolic reparations measures, and guarantees of non-repetition.

[11] 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 table A.1.

their money is placed in a trust fund to be collected after they turn 18.

To manage the delivery of reparations checks and coordinate aid delivery with local authorities, a new agency was created, the Unidad Administrativa para la Atención y Reparación Integral a las Víctimas (UARIV). The UARIV is also responsible for registering victims, advising reparations recipients on investing their money, and offering symbolic reparations and psychosocial support to recipients. The UARIV's Subdirección Red Nacional de Información (SRNI) centralizes, unifies, and updates information from the RUPD, local historical records, and other sources into a single database, the RUV, which is one of our primary data sets. Registration in the RUV is a prerequisite for receiving reparations.

Due to the binding government budget and operational and technical constraints, Colombia staggered the rollout of the reparations payouts (Articles 17, 18, and 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. Figure II(a) plots the quarterly number of reparations paid over the last decade. The Victims Law expanded the number of economic reparations paid to victims, reaching over 45,000 payments 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 1290/2008 and Decree 4800/2011), as shown in figure II(b) (green and blue bars). A more detailed prioritization scheme was defined for the first time in 2013 (Resolution 0223, April 8, 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 (partial or total) disability, female heads of households with a disabled child or with two or more dependents living under the poverty line, victims aged 60 or above living under the poverty line, LGBTI victims, victims of sexual abuse, and victims of child recruitment.

In a surprising turn of events, in July 2013, the Constitutional Court ruled that victims of forced displacement must receive financial reparations (Decision SU-254/2013), forcing the government to compensate them beginning in 2014 (black bars).[12] Given the colossal fiscal cost of compensating the millions of forcibly displaced victims, the government prioritized those who were disabled, who were aged 70 and above, who had already fulfilled their basic needs,

[12]Victims of forced displacement had not received financial reparations before this ruling because the Victims Law stated that reparations for forcibly displaced people would consist of other forms of social assistance—not economic reparations per se. Victims and human rights organizations filed a lawsuit, and the Constitutional Court ruled in favor of the plaintiffs.

or who wished to return to their place of origin (Decrees 1377/2014 and 1084/2015).[13] All other victims—namely, those who were not forcibly displaced—were prioritized if they were previously eligible for reparations, had a costly illness or disability, were a female head of household with a sick or disabled dependent, were aged 70 or above, identified as LGBTI, or were a victim of sexual abuse or forced recruitment, a landmine victim, or an indirect victim receiving the remains of the deceased relatives (Resolution 090/2015).

Despite these efforts at establishing de jure prioritization rules, the broad criteria made too many victims eligible for reparations at once. The RUV lacked socioeconomic information, and asking victims to provide additional information proved onerous (UARIV, 2018). For this reason, the government could not inform victims on the long waiting list when they could expect reparations (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2018). This led to substantial uncertainty regarding when an eligible victim would be able to receive the cash payment, both for the government and for victims themselves (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2015; ICTJ, 2015).

We show using the random forest model, a machine learning technique, that the prioritization rules did not make it easier to predict who would receive reparations. The model uses the available covariates to predict who receives reparations and when the payment is received. Appendix B discusses the details of the random forest model; here, we briefly summarize the key insights. First, the random forest model shows that Colombia's reparations program did not target recipients based on their socioeconomic needs but prioritized the surviving relatives of murdered victims.[14] Nevertheless, the model's performance in predicting who will receive reparations and when a household will receive them is low despite Colombia's well-defined prioritization rules, rich data infrastructure, and hundreds of thousands of reparations spanning many years. Therefore, it is extremely unlikely that victims themselves could predict who would receive reparations and when they would receive the money.

Surveys and anecdotal evidence further show that the timing of reparations payouts is unpredictable and unanticipated for victims. A group of researchers asked compensated victims about their experience with the reparations program and their views on the prioritization scheme. They concluded that victims believed Colombia's system of prioritizing victims for reparations was random or based on luck (Sikkink et al., 2015). In addition, we confirmed that

[13] By requiring forcibly displaced victims to fulfill their basic needs, the government sought to prevent their spending the reparations payment on immediate needs like food, housing, and health care, which the UARIV provided to them via humanitarian aid (UARIV, 2018). The idea is that a minimum level of subsistence is needed to enable victims to fully benefit from the transformative and long-term impacts of reparations (Dixon, 2017; Firchow, 2013).

[14] There is some evidence that the government tried to spread reparations across departments after July 2013 (see Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011 (2018) and appendix B for details). Given the wide variation in the number of victims across departments, this implies a large spread in the share of compensated victims (figure A.5). To reduce the uncertainty regarding the timing of the reparations payout, the Constitutional Court required the Victims Unit to change its prioritization scheme in 2017 (Auto 206 de 2017). Since 2019—i.e., after our period of study—the Victims Unit has used an index score calculated from observable characteristics (Resolutions 1958/2018 and 1049/2019).

the reparations payout was an unanticipated event when one of the authors' family members received financial reparations in July 2021. First, the victim received an unexpected phone call from the Victims Unit. The caller did not mention the reparations program but instructed the victim to attend an "important" meeting at a specified time and location. A few days later, the victim arrived at this meeting, where victims were informed they would receive financial reparations (figure A.6 shows a photograph taken during that meeting). The victim received a Dignifying Letter explaining that the reparations check would be available one or two weeks later from Banco Agrario, Colombia's state bank. (Victims are not required to have a bank account to collect the check.) Moreover, the Dignifying Letter included a message about the meaning of the reparations, which read 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; the original letter is available in Figure A.7]

Based on the implementation of the reparations process, the findings using the random forest model, substantial qualitative records, and anecdotes, the evidence overwhelmingly suggests that victims could not anticipate who would be compensated or when they would receive reparations. The unpredictable and unanticipated timing of reparations receipt is the second policy characteristic, in addition to the staggered rollout, leveraged by our empirical methodology to identify causal effects.

Throughout the debates in Congress, opponents of the Victims Law expressed concerns about the high fiscal cost of reparations and their potential misuse by recipients.[15] To assuage these fears, the government "labeled" the cash transfer: reparations were presented as seed money for victims to improve their socioeconomic conditions and transform their everyday lives. (They were referred to as *indemnizaciones transformadoras*, or "transformative reparations"; see also Sánchez-León and Rudling 2019.) Specifically, the program explicitly endorsed investments in the next generation's human capital, creating and strengthening small businesses, and acquiring housing or improving housing conditions:

[15] Similar concerns had been raised by state authorities regarding reparations awarded by Decree 1290/2008. After interviewing some recipients, in 2010, the Ombudsman's Office of Colombia concluded that reparations had not transformed recipients' lives 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 adequate investment of the resources the victim receives as reparations to rebuild their life project, mainly oriented toward:

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)

Indeed, reparations were proposed as a tool for development to be invested in self-sustaining, income-generating activities, much like micro-finance development interventions (see Vallejo, 2019). The government galvanized recipients to use reparations wisely through a process of education, training, and planning around finances and small investment opportunities. Starting in 2016, the government held fairs when disbursing reparations payments (*feria integral de servicios*) to connect recipients with local public and private institutions laying out options in education, housing, land, and small businesses. Victims could also participate in investment workshops (*programa de acompañamiento de inversión adecuada de los recursos*), which offered training in budgeting and investing and informed them about how to obtain small business or student loans and pay off old debts. In practice, however, records suggest that fewer than 10 percent of victims compensated between 2016 and 2019 attended these workshops (UARIV, 2019).

3 Data and Methodology

3.1 Data

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

1. **Eligibility, treatment, and covariates:**

   (a) **Eligible victims and financial reparations:** We use microdata from the universe of registered victims (the RUV) and all compensated victims (Indemniza) provided by the UARIV. The RUV is a unified and centralized registry covering all individuals who reported victimization during the Colombian internal conflict by August 2019. The 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 size of financial reparations and the date and location of the payout. While the data include reparations paid out by 2019q2, our event-study estimates are based on reparations between 2011q1 and 2016q4 to examine outcomes realized up to four years after the event.

(b) **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 other variables. Over 25 million individuals—more than one in two Colombians—were included in SISBEN in 2010.

(c) **Electoral outcomes**: We use data from the National Civil Registry (Registraduría Nacional), the institution responsible for the civil registry and identification of individuals. The National Civil Registry maintains updated official voter registries and information on electoral outcomes for each election. We use municipality-level data from all 1,119 municipalities.

2. **Work and living conditions**:

   (a) **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 data set represents the census of all individual-by-month contributions to health care, pension funds, and workers' compensation. The information is available for formal workers—that is, both wage-earners and self-employed individuals—from 2008m7 to 2019m12. 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 data set has detailed information on occupational choice, payroll, earnings, days worked, and other information for over 10.5 million individuals and more than 300 thousand firms.

   (b) **Entrepreneurship and business survival:** We use microdata from the Colombian Confederation of Chambers of Commerce (Confecámaras) firm registry, the Registro Único Empresarial y Social (RUES). In Colombia, firms are required by law to obtain a license (*matrícula mercantil*) from the local chamber of commerce to operate. This license is required for many regular business activities, including access to credit, subsidies, and government training programs. A fee of US\$13–US\$638 is charged for obtaining this license, depending on the value of the firm's assets. Active firms must renew their license annually by March 31. (Failure to renew a license can result in heavy pecuniary sanctions and even business closure.) Notably, Colombia is unique in its firm-registration requirements: no other Latin American country requires all firms to register and to renew their license every year (Salazar et al., 2017). For instance, in 2018, there were roughly 1.5 million firms with active licenses 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 according to whether 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 57 local chambers of commerce in Colombia. Our data set includes firms that obtained or renewed their licenses between 2011 and 2018.

(c) **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 data set contains detailed credit information for the universe of individuals and firms in the formal financial sector. In addition to identifying all individuals and firms with a formal loan, this data set also includes end-of-quarter snapshot information about the type of institution providing the loan (e.g., bank or cooperative), the type of credit (e.g., microcredit or consumption), credit risk scores, collateral, loan size, interest rate, and debt terms, among other variables. Most student loans are excluded from this data set (e.g., ICETEX and Sapiencia). The information is available from 2004q1 to 2019q4.

(d) **Land- and homeownership:** We use cadastral data covering real estate transactions in the department of Antioquia from the Dirección de Sistemas de Información y Catastro, managed by the Catastro Antioquia. 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 data set includes land and home characteristics and values based on cadastral records for all transactions between 2011q1 and 2019q4. We have information for 120 of Antioquia's 125 municipalities. We do not observe real estate transactions in Copacabana, El Retiro, Rionegro, and Medellín, which have a separate cadastral information systems. Moreover, no data is available for Murindó.

3. **Health:** 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 health care service use that captures data on medical visits, diagnostic and therapeutic procedures, and other services for every patient in Colombia. Health care providers (primary care physicians, hospitals, and clinics) report detailed information about their patients (e.g., ID number, sex, age, user type, and municipality of residence). They also report every patient's 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 health care provider. Information is available from 2009 to 2019.

4. **Human capital:**

   (a) **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 tracks students along the postsecondary education system. The data set includes a wealth of individual-by-semester-level information on student observable characteristics, including enrollment status, higher education institution, major of study, share of courses passed, and graduation or dropout status. The average post-secondary enrollment for the observed period is around 2 million individuals per semester, and the data cover roughly 90 percent of 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.

   (b) **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, 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 individual test scores, the data set includes sociodemographic information (e.g., socioeconomic stratum, parental education, and municipality of residence) for all 6.7 million test-takers between 2010 and 2019.

We link individuals across these ten separate data sets using social security numbers (the Colombian *cédula de ciudadanía* or *tarjeta de identidad* for minors). When this information is unavailable, we use date of birth, municipality, household composition, and personal name.

3.2 Methodology

We estimate the causal effect of financial reparations on various measures of victims' well-being. We use an event-study approach to estimate effects. We define "event time" d_i as the date when an individual (or household) i receives the reparations payment and $D_{it}^k = 1\,(t = d_i + k)$ as an indicator variable that equals one if the individual (or household) received the reparations payment k periods ago, where k may be negative.[16] Our event-study model compares outcome y

[16] 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.

around event time across individuals using the following ordinary least squares (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, which controls for time-invariant personal characteristics that may be correlated with reparations receipt (e.g., sex or victimization type). Moreover, 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 to event time -1. Since reparations are often assigned to the household, as occurs for indirect victims and forcibly displaced victims, we cluster the standard errors at the household level.

As with a difference-in-difference design, identification requires parallel trends between treated and control groups in the absence of reparations. We assess the plausibility of the parallel-trends assumption by checking for preexisting differences in trends ("pre-trends"). Moreover, because only a fraction of victims had received reparations by 2019, we can compare newly treated units and units that did not receive reparations during our study period ("never-treated" units). In addition, we can compare newly treated units and units that have not yet received reparations ("not-yet-treated" units). In the main text, we present findings including never-treated units for virtually all results and report the estimates using the latter comparison in the online appendix. The two analyses lead to quantitatively similar conclusions.

Recent advances in the literature on difference-in-difference designs with multiple periods have noted that two-way-fixed-effects (TWFE) linear regressions can also compare newly treated units and already-treated units. Under the presence of heterogeneous treatment effects, these so-called "forbidden comparisons" can severely bias the TWFE estimator, making it hard to give a clear causal interpretation (e.g., de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021). An advantage of our setting is that we have a large sample of never-treated units: for every victim who received reparations, we have at least six victims who did not, despite being eligible. This means that most of our control group units are never-treated units and not units that were treated in an earlier period, which substantially weakens the role of the forbidden comparisons between already-treated units and helps alleviate concerns about biases in TWFE estimators. Moreover, we will show that our results are robust when using the Callaway and Sant'Anna (2021) estimator, which avoids using already-treated units as the comparison group. Therefore, our TWFE-estimation procedure leads to estimating effects with an intuitive direct causal interpretation.

Before concluding this section, we discuss five institutional features that are key for identifying causal estimates, interpreting our results, and motivating the focus of our analysis. First, our identification strategy relies on non-differential anticipation between victims who receive reparations sooner versus later. While the announcement and adoption of the Victims Law could have led victims to expect to receive money, the expected probability of obtaining reparations is minuscule since fewer than 1 percent of eligible victims receive compensation yearly. Moreover, as explained above, the reparations program's prioritization rules were broad and rendered hundreds of thousands of victims eligible, making it virtually impossible to predict who would receive reparations or when. Indeed, the implementation details of the reparations process, the findings from the random forest model, substantial qualitative records, and anecdotes all overwhelmingly support the identifying assumption that victims could not anticipate who would receive reparations or when. Reassuringly, our series will rule out pre-trends, further supporting our identification assumption.

Second, reports to Congress show that the government sought to spread reparations across Colombia's 32 departments to avoid concentrating them in specific regions—a claim supported empirically by our machine-learning analysis (see appendix B for details). Coupled with fewer than 1 percent of eligible victims receiving reparations yearly, this implies that the likelihood of reparations triggering general equilibrium effects is low.

Third, in addition to financial compensation, the Victims Law expanded traditional social assistance for victims. While forcibly displaced victims have benefited from preferential access to social protection programs since 2004, the Victims Law expanded social services to all victims, regardless of compensation status. These services include, for instance, psychosocial treatments, special lines of credit for victims with productive activities, forgivable student loans, and free public housing for forcibly displaced persons. As a result, many victims have benefited from expanding the social safety net. Crucially, however, the rollout of these additional social services is orthogonal to the timing of the distribution of financial reparations. Since control victims increasingly benefit from social transfers, our event-study approach estimates the effect of financial reparations above and beyond social assistance. It is a lower-bound effect of money on victims.

Fourth, the financial reparations' payment scale depends on the type of abuse the victim suffered, as section 2 has detailed. For instance, indirect victims receive 40 monthly minimum wages, while rape victims receive 30 monthly minimum wages. Moreover, the scale for forcibly displaced victims of 27 or 17 monthly minimum wages depends on whether they registered their victimization before or after April 22, 2010, respectively. As a result, the type of abuse suffered is confounded with the payment amount, and the amount is confounded with the type of abuse suffered *and* the declaration date, so we cannot interpret the heterogeneous effects causally. For this reason, we do not report heterogeneous results (but they are available upon request).

Last, as discussed above, when disbursing reparations after 2016, the government also held fairs and investment workshops to help victims invest money in their human capital, small businesses, and homes. In theory, this would imply that our estimated effects for 2016—the last year used in our event-study approach—combine the impact of financial reparations and these additional services, which is the policy-relevant estimate of the reparations program. However, in practice, fewer than one in ten recipients attended these workshops in 2016 (UARIV, 2019). As a result, our estimated effects will be mainly driven by money's impact on recipients.

3.3 Summary Statistics of Victims and Recipients of Reparations

Table I presents the descriptive statistics for our data on victims and beneficiaries of reparations. Column (1) shows information for all 8.9 million individuals victimized in the Colombian internal armed conflict, as registered in the RUV. Of these victims, 50.1 percent are female. As of 2019, 25.7 percent are minors, 62.0 percent are aged 18–60, and 12.3 percent are aged above 60. The average victimization took place 16.6 years before 2019, circa 2002. Regarding the kind of victimization, 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. For instance, 64 percent of indirect victims were also forcibly displaced.) Last, 12.7 percent of victims are members of an ethnic minority group, and 4 percent have a disability.

In practice, some of these victims in the RUV will not be able to receive reparations because they are unreachable by the government. For instance, they may not have included their ID number or contact information when registering their victimization. Column (2) restricts the sample to the 7.7 million victims with valid contact information. These victims are 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., grandparents, siblings, aunts, or cousins) do not receive reparations on their behalf when there is a spouse, child, or parent present because immediate family is prioritized (see table A.2). This results in a sample of 7.4 million victims eligible to receive reparations.

Column (4) presents descriptive statistics for the eligible victims who received a reparations payment by June 2019, when we obtained the RUV data from the UARIV. Almost 822,000 victims had received reparations by this date. Reflecting the prioritization scheme, relative to the eligible victims in column (3), compensation 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 were victimized longer ago than other eligible victims.

For many outcomes, we are interested in estimating the effects on the household and on minors, who do not receive reparations before turning 18. However, the RUV 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 the RUV, as household composition most likely changed from the date members registered the victimization to the date of the reparations 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. As 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 data sets. We merge the two data sets using victims' date of birth, first name, and the municipality where the victimization occurred, the victim was registered, or the victim was forcibly displaced. 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 data sets. (We cannot identify the 5 percent of victims that do not have information on their date of birth or household structure, as is the case for some indirect victims.)

Column (5) presents summary statistics for the 25.8 million individuals appearing in SISBEN in 2010. Column (6) shows that 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, victims are more likely to be female, younger, less educated, and poorer (both measured by SISBEN score and by household income). They are also more likely to live in rural areas and to have larger families. In addition, they are less likely to participate in the labor force, have a job, or be formally employed. Thus, victims identified in SISBEN represent a remarkably vulnerable population, even relative to the average person in SISBEN. Arguably, poorer and rural areas were disproportionately affected by the conflict, and the conflict itself made victims worse off (Ibáñez et al., 2022).

Columns (7) and (8) report descriptive statistics for recipients of reparations identified in SISBEN in 2010. Victims who received financial reparations are 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. Nevertheless, and reflecting the prioritization rules, recipients are older, are more likely to be disabled, and were victimized earlier. They are also more likely to be indirect victims. Crucially, reparations recipients are similar to all other victims in terms of their socioeconomic conditions, consistent with Colombia's reparations program not being means tested, that is, not targeting recipients based on their socioeconomic needs.

Finally, 87,668 recipients lack information about the date they received financial reparations, 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 the reparations 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, whose reparations have not yet been distributed.

Table I further shows that reparations were large, especially relative to victims' mean income. The last rows of column (8) show that, on average, recipients received financial reparations equivalent to 11.7 times the monthly minimum wage. With the average household income being three-quarters of the monthly minimum wage, a household payment of 29.575 monthly minimum wages represents roughly 39.4 times the average monthly household income, that is, 3.3 times the average annual household income. Reparations were, therefore, substantial in magnitude. Last, given the aforementioned challenge of determining household structure at the time of the reparations payout, the last row in table I re-estimates average household reparations 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 reparations), average reparations received by households drop to 19 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 reparations on victims' well-being. First, we examine effects on work and living standards. Next, we study impacts on health care utilization. Finally, we estimate effects on human capital accumulation among the children of victims.

4.1 Work and Living Standards

First, we test whether reparations affect victims' employment, occupational choice, and earnings using social security records. Second, we examine impacts on entrepreneurship and business activity by leveraging information from Colombia's business registry. Some entrepreneurs, however, may decide not to register their businesses, so we complement the analysis using census-level panel data on microloans for self-employed professionals and small entrepreneurs. Third, we study how reparations affect consumption patterns using census information on all credit card debt, automobile loans, and mortgages owed to banks and financial institutions. Last, using the department of Antioquia as a case study, we investigate impacts on land- and homeownership with cadastral records.

4.1.1 Employment, Occupational Choice, and Earnings

We first examine whether reparations, by providing a large wealth shock to victims, diminish the 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 18–55 in 2010.

Figure III plots the likelihood of formal employment in the quarters before and after the reparations 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 reparations during our period of study. Three striking results emerge from this figure. First, the series rules out pre-event trends: the difference in formal employment between treated and control victims is not statistically significant before the reparations payout, which supports our identifying assumption. Second, formal work falls by 0.55 percentage points in the quarter after the reparations payout ($k = 1$). While the coefficient is precisely estimated and statistically significant, it is economically small because it represents a 3.3 percent drop in formal employment relative to 16.4 percent of never-treated victims with formal work. Third, the employment drop persists over time: 12 quarters after reparations, formal employment remains 1.6 percent below the rate immediately before the reparations payout. Figure A.8 shows that these results are identical when excluding never-treated victims from the estimation sample and robust to using the Callaway and Sant'Anna (2021) estimator.

The first row of table II summarizes these estimated employment coefficients by event-time bins. The second and third rows, which decompose the employment effect by type of occupation, show that *wage* employment drives this drop (figure A.9 presents the decomposition in figure form). To understand who leaves wage employment, the following rows of table II report the effect by occupational risk and wage rate (figure A.10 presents this decomposition in figure form). First, we define the level of risk by 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" jobs according to whether the contribution rate is below or above the median. Most victims work in high-risk jobs. However, reparations induce victims to shift out of these high-risk jobs. An immediate 1.7 percent drop in employment in high-risk jobs increases in magnitude to 3.25 percent three years after the reparations payout. By contrast, employment in low-risk jobs is entirely unaffected by victims' compensation.

Second, we contrast the employment effects by the wage rate, comparing the likelihood of working in jobs that pay at versus above the legal monthly minimum wage. Two-thirds of victims with formal salaried employment work in minimum-wage jobs. However, the probability of working in a minimum-wage job drops by 2.73 percent in the quarter of the reparations payout and is 3.45 percent lower three years later. By contrast, there is an immediate 2.11 percent *increase* in the likelihood of working in a higher-paying job upon receiving reparations. Three years later, this effect is positive but not statistically significant. By interacting the two sets of job-risk and pay indicators, table II shows that workers have shifted out of high-risk and low-paying jobs by 3.99 percent three years after reparations. Victims also move out of both low-pay-low-risk and high-pay-high-risk jobs, but we are underpowered to detect statistical significance. By contrast, there is an immediate and persistent *increase* in the likelihood of working in a high-pay-low-risk job, with the probability being 6.4 percent greater three years after reparations.

Figure IV shows that victims' average daily wage increases with these reparations-induced job transitions. The daily wage is nearly 1 percent higher as soon as the victim receives reparations, and this coefficient rises to 3.11 percent three years later. (Figure A.11 shows these results are robust to excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.) Since table II shows that the drop in formal wage employment shrinks over time, the higher wage rate three years after reparations is not driven by workers in salaried jobs becoming more selected.[17] Instead, it is most likely driven by reparations improving victims' job quality, proxied by pay and risk. Moreover, this effect is sizable enough to boost average earnings when including zeros for individuals without formal employment. Indeed, earnings increase by 1.45 percent immediately after reparations and by 2.05 percent three years later, although the gain loses statistical significance over time. As a result, the implied long-run marginal propensity to reduce earnings per dollar of unearned income (MPE) is nearly zero and is not significant, meaning victims do not give up earnings as a result of receiving reparations.

What do victims do after leaving their salaried jobs? The remainder of this section investigates victims who leave the labor force, spend more time unemployed, or work in informal jobs. First, we examine whether workers leave the workforce—for instance, by retiring or going to school—by decomposing the employment effect by age group. Figure A.13 shows that workers aged 18–39 are more likely to immediately drop out of formal work than older workers, suggesting that retirement is not the main driver of the employment drop. Instead, younger victims seem to leave their low-paying jobs to invest in their human capital, as shown in section 4.3.

Second, money may increase workers' willingness to spend time unemployed. This would be the case if reparations improved victims' outside options: an increased reservation wage would extend workers' search for better jobs, raising their out-of-formal-employment spells. Therefore, the last row of table II tests whether reparations affect salaried workers' out-of-formal-employment spells, defined as the number of quarters since they were last observed

[17] Two additional exercises help us rule out the possibility that composition effects drive the increase in daily wages. First, we regress people's baseline characteristics on time fixed effects and the event-time dummies to examine whether and how covariates change among salaried victims. Table A.3 shows that the victims who remain in salaried jobs are not different in their baseline education levels, income, or Mincer equation–predicted wages, suggesting that compositional effects do not drive the wage increase. Second, we run a regression using specification (1) for a balanced sample of salaried workers. Figure A.12 shows that workers employed every quarter also experience an increase in daily wages.

in a salaried job.[18] There is a persistent increase in out-of-formal-employment spells, and this effect remains highly significant even three years after the reparations payout. Coupled with the increase in daily wages, this suggests that reparations improve victims' outside options and raise the quality of jobs people take on.

Third, victims may choose to move from *formal* to *informal* employment, defined as employment not contributing to social security. Indeed, informality is particularly pervasive in our context: table I showed that fewer 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 (e.g., Blattman and Dercon, 2018; Falco and Haywood, 2016), they likely do so without contributing to social security. Indeed, table II shows that formal self-employment is rare among victims (the baseline control mean is only 1.56 percent) and unaffected by reparations receipt. In the next section, we will show evidence consistent with the explanation that reparations money boosts (informal) self-employment: victims invest the lump-sum grant in creating new businesses, and this shift to self-employment appears to occur without victims' contributing to social security.

4.1.2 Entrepreneurship and Business Survival

Reparations could foster entrepreneurship by alleviating liquidity and credit constraints, helping people afford the start-up costs of establishing a new business. The relationship between money and entrepreneurship is particularly relevant in our setting because Colombia's reparations program explicitly endorses investment in victims' entrepreneurial and income-generating activities. Moreover, the delivery of the reparations checks is permeated with a message of economic accountability, microfinance, and small-business creation (Vallejo, 2019).

We estimate impacts on entrepreneurship in two ways. First, we create a dummy for whether a person registered a new business with the local chamber of commerce to proxy for formal entrepreneurship and measure impacts on firm survival by examining license renewal. Second, we investigate victims' use of microcredit, which, in Colombia, is aimed mainly at self-employed professionals and (formal and informal) small entrepreneurs. Thus, an uptake of microfinance will indicate more self-employment and entrepreneurial activity.

Figure V plots the likelihood of registering a new business in the quarters leading up to and after the reparations payout. Reparations stimulate formal entrepreneurship: the probability of registering a new business increases by 37 percent a quarter after the reparations payout, which is a sizable impact since only 0.17 percent of control victims register a new firm in a given quarter. Furthermore, figure A.14 shows that these results are robust to changing the control group and using the Callaway and Sant'Anna (2021) estimator. Therefore, liquidity appears to

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

be a crucial barrier to entrepreneurship, and reparations help victims afford the costs associated with starting (and formalizing) a business.

Reparations can also extend firm survival by injecting capital into existing businesses. To examine firm survival, figure VI reports the effects when the outcome is an indicator for having a firm with an active license. The likelihood of having an active business increases immediately after the reparations payout. The coefficient, reported in table III, remains significant and large at 13.7 three years after the reparations payout. Again, these results are robust to changing the control group and using the Callaway and Sant'Anna (2021) estimator (figure A.15).

Nevertheless, some entrepreneurs may choose not to register their new business with a chamber of commerce and instead remain informal. Indeed, while having a business license does not necessarily imply paying taxes, the pecuniary and non-pecuniary costs of obtaining this license—and the lack of perceivable benefits—could discourage firms from registering their businesses (Galiani et al., 2017). To capture formal and informal entrepreneurial activity, we complement these results by leveraging microdata from the universe of microloans owed to banks and financial institutions in Colombia.

Table III reports the effects of reparations on the use of microcredit (figures A.16 and A.17 plot these results in figure form). Around 8.9 percent of control victims owe some microcredit in 2014q1. However, the money from the reparations payment enables victims to pay off their microloans: the probability of owing any microcredit drops by 4.65 percent the quarter they receive reparations ($k = 0$). This effect drastically changes over time. Victims are just as likely to owe any microcredit two years after receiving the money and 3.24 percent *more* likely to owe microcredit three years after reparations. There are similar patterns for the outstanding balance, with the amount of microcredit owed increasing by 9.29 percent three years after the reparations payout.[19] Again, these results are robust to changing the control group and using the Callaway and Sant'Anna (2021) estimator (figures A.18 and A.19). They imply that the wealth shock from reparations enables victims to access the credit market, improve their credit history, and take on larger loans to fund productive investments. Indeed, if microloans funded unproductive investments, the more intensive use of microcredit would lead to more delinquency as people accumulated unpayable debts. Instead, delinquency drops significantly and persistently after reparations.

Taking these observations together, we conclude that reparations have a tiny negative effect on formal employment, driven by salaried workers shifting out of risky and low-paying jobs. However, reparations improve victims' outside options and job quality, boosting daily wages and increasing efficiency. Furthermore, some victims shift into self-employment and entrepreneurship, investing the money from reparations to create and strengthen their small businesses. As a result, reparations fund productive investments and income-generating activities.

[19] Given that 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 US\$1,643 (=145.6/0.0886).

4.1.3 Consumption and Land- and Homeownership

This section investigates how reparations affect victims' consumption patterns and decisions regarding land- and homeownership. First, we leverage microdata on credit card debt and automobile loans, the two most common consumer debt categories from Colombia's Financial Superintendence. Next, we estimate impacts on land- and homeownership, which the Victims Law explicitly sought to encourage. To do this, we use two distinct data sets. First, we leverage detailed information on real estate transactions in the department of Antioquia, one of the country's largest departments, which accounts for a disproportionate number of victims and reparations recipients. In the online appendix, we complement this data using records from the census of mortgage loans.

Table IV reports the effects of reparations on consumption through the credit market (figures A.20 and A.21 plot these results in figure form). The first rows show that reparations enable victims to pay off old credit card debt: both the likelihood of owing any credit card debt and the outstanding balance drop by 5.63 percent and 11.11 percent in $k = 0$, respectively. However, the signs of these coefficients switch after the initial reduction. Indeed, one year after the reparations payout, the effect on credit card debt is close to zero and not significant. Two years afterward, the effect is *positive* and highly significant, consistent with reparations boosting consumption through the credit market. This effect is economically sizable; for instance, outstanding balances are 32.96 percent larger three years after the reparations payout. Admittedly, a larger outstanding balance does not necessarily mean individuals are consuming more but, rather, could indicate that they are more heavily in debt. For this reason, we infer people's consumption from the credit card records by adding observed quarterly payments to the change in outstanding balances (excluding interest). The third row of table IV shows that money has a positive and persistent effect on inferred consumption. Three years after the reparations payout, this outcome is 32.46 percent larger, thanks to the reparations payment.

Next, we examine the effect of reparations on people's consumption of durable assets: specifically, motor 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. The last rows of table IV show that the cash windfall induced victims to take on loans to purchase motor vehicles. Consistent with reparations boosting victims' consumption of durable assets, auto loans are 63.24 percent larger three years after the reparations payout.[20]

Next, we focus on land- and homeownership. Many victims of forced displacement lost their land and homes due to the conflict and, as a result, live in very unstable circumstances. For

[20] Figures A.22–A.24 show that the effects of reparations on consumption through the credit market are generally robust to the comparison group and using the Callaway and Sant'Anna (2021) estimator. There is one notable exception, however: after the initial drop, the effect on credit card debt stabilizes around zero when excluding never-treated victims from the comparison group.

this reason, Colombia's reparations program explicitly labels the reparations payment as a tool for victims to repurchase land or a house. To measure land- and homeownership, we first use cadastral records from Colombia's department of Antioquia, where we observe all real estate transactions from 2011q1 to 2019q4. 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 VII presents the effect of reparations on households' cumulative land and home purchases in the quarters before and after any member receives reparations. Before the reparations payout, households have accumulated, on average, 0.1 real estate purchases; i.e., the chance of having purchased real estate since 2011q1 is roughly 10 percent. However, this likelihood increases after reparations and becomes statistically significant one quarter later, indicating that victims use the money to buy land or a home. Three years after the household receives reparations, its cumulative number of land or home purchases is over 40 percent higher. Figure A.25 shows that these results are robust to changing the comparison group and using the Callaway and Sant'Anna (2021) estimator.

Last, table A.4 compares the estimates from cadastral records in Antioquia with those using census-level data on all mortgage loans in Colombia. Using mortgage loan data, we see a positive effect on both the likelihood of having a mortgage loan and on outstanding debt (figure A.26 plots these results in figure form, and figure A.27 shows the robustness checks). The magnitudes are similar to those obtained using cadastral records and indicate that reparations enable victims to invest in homeownership. However, there are pre-trends for these outcomes based on mortgage loans, so the effects must be interpreted with caution.

Taken together, the results show that reparations provide short-term debt relief and a longer-term increase in the consumption of durable and non-durable assets. Moreover, the money enables victims to invest in land- and homeownership—a particularly relevant margin of response given the intentions of Colombia's reparations program.

4.2 Health Care Utilization

This section examines how the positive wealth shock from reparations affects victims' health care utilization. This effect is theoretically ambiguous. On the one hand, money enables financially constrained individuals to afford contact with the health care system and could increase people's alcohol consumption and smoking, leading to an *increase* in health care utilization. On the other hand, money can improve people's health, for instance, by improving their work environment, nutrition, and mental well-being (e.g., by reducing stress). Better health makes people less likely to visit the emergency department (ED), be hospitalized, or undergo a medical procedure, leading to a *reduction* in health care utilization.

In our setting, the positive wealth shock from reparations likely *reduces* health care utilization because financial barriers are unlikely to prevent access to medical care. First, Colombia has universal health care, with over 95 percent of the population having access to health care services and only 2 percent reporting unmet health care needs (OECD, 2015). Second, the basket of health services is the same for people in and out of formal employment. Third, out-of-pocket health spending is low, even compared to rich countries, and especially for low-income individuals (OECD, 2019).[21]

We examine the impact of reparations on three measures of health care utilization: ED visits, hospitalizations, and medical procedures. Since ED visits and hospitalizations most likely indicate adverse health conditions, a drop in the probability of visiting the ED or being hospitalized indicates improved health. We define treatment at the household level: an individual is assigned to treatment if anyone in their household receives reparations, and the event time is the first date when any household member received reparations. This enables us to estimate the effects of reparations on all household members, including minors, older adults, and household members who are not themselves victims. We base our primary analysis on victims who eventually receive reparations during the study period and report the results when including never-treated victims in the online appendix.[22]

Figure VIII(a) and table V report the likelihood of any ED visit before and after the reparations payout. ED visits are relatively rare: only 2.67 percent of treated victims visit the ED the semester before receiving reparations. Moreover, money reduces the probability of an ED visit by 5.38 percent the year after the reparations payout, and this coefficient is highly significant. The effect compounds over time but stabilizes after three years. As a result, reparations have reduced victims' ED visits by 17.15 percent four years after the reparations payout.

Figure VIII(b) decomposes the effect by primary diagnosis to understand the drivers of this reduction in ED visits. We divide the coefficient on an ED visit for each diagnosis—in percentage points—by the likelihood of any ED visit at $k = -1$. A reduction in infections, musculoskeletal illnesses, and poorly defined conditions explains the drop in ED visits one year after the reparations payout. This result is consistent with reparations improving victims' work and living conditions, as we documented above, and improving their health. For instance, work injuries may drop if victims no longer work in risky jobs. Over time, reparations also lower ED visits from circulatory, genitourinary, and digestive system conditions. This result is possibly caused by improvements in stress levels and health behaviors, like dietary habits, that take longer to manifest as improved health.

Next, figure IX(a) and table V report the effect of reparations on the likelihood of hospi-

[21] 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).

[22] Unfortunately, we were unable to access the full hospitalization records of never-treated victims before 2015. By contrast, we have complete hospitalization records for eventually treated victims and base our primary analysis on this sample.

talization. Hospitalization is an even rarer outcome than ED visits: fewer than 1.2 percent of treated victims are hospitalized at any moment in the semester before reparations. Moreover, reparations significantly reduce hospitalization: there is an 8.32 percent reduction in hospitalization two years after the reparations payout. Again, the effect compounds over time such that hospitalizations have dropped by 17.49 percent four years after the reparations payout.

Last, figures IX(b) and IX(c) and table V report the effect of reparations on the likelihood of undergoing a medical procedure and on the number of procedures in a given semester. On average, 18.3 percent of treated victims undergo a medical procedure the semester before receiving reparations. Because many individuals undergo multiple medical procedures, the average number of procedures the semester before the reparations payout is 0.93. Consistent with improved health, both the likelihood of undergoing a medical procedure and the number of procedures drop significantly after the reparations payout. Again, the effect compounds over time, reaching a 10.88 percent drop in the likelihood of undergoing a medical procedure and a 14.78 percent drop in the number of medical procedures four years after reparations.[23]

In sum, our results suggest that reparations meaningfully improve victims' health. ED visits, hospitalizations, and medical procedures significantly drop after households receive the money from reparations, with compounding effects over time. In addition to reparations improving victims' health, we view these results as reflecting positive changes in intermediate outcomes that affect health conditions and physical well-being, like dietary habits and nutrition.

4.3 Intergenerational Impacts on Human Capital Investments

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

4.3.1 College Access and Persistence

This section tests whether reparations improve young victims' postsecondary attendance, a human capital investment explicitly endorsed by the Victims Law. Colombian postsecondary institutions offer four- or five-year "professional" programs and two- or three-year "technical and technological" programs, akin to the American bachelor's and associate's degrees. The undergraduate admissions cycle takes place every semester because there are two graduating high

[23]Figures A.28, A.29, A.30, and A.31 show that the results are robust using the Callaway and Sant'Anna (2021) estimator. Moreover, the estimates are similar for ED visits and medical procedures when including never-treated victims. However, we could not access the full hospitalization records of never-treated victims before 2015. Table A.5 and figure A.29(a) show that the incomplete data in the earlier years cause a pre-trend, making it difficult to interpret the results causally.

school cohorts per year.

To estimate the impacts of reparations on college attendance, we use panel microdata from Colombia's census of postsecondary attendees, SPADIES. Moreover, we adjust specification (1) in three ways. First, we restrict the sample to individuals aged 15–25 the semester before the reparations payout. (For never-treated individuals, we restrict the sample to those of the same age as treated individuals in the semester before their reparations payout.) Second, since college attendance strongly correlates 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 reparations. We do this because minors do not directly receive reparations; their money is deposited in a fiduciary account. Moreover, their schooling choices and ability to pay tuition largely depend on other household members (e.g., their parents).

Figure X reports the effect of reparations on the likelihood of being enrolled in a four- or five-year undergraduate program in a given semester. The figure shows that, first, there is a large and persistent increase in enrollment after reparations. The enrollment effect is positive at $k = 0$, then increases to 11 percent (from a base of 2.5 percent) and becomes highly significant one semester after the reparations payout. This pattern is consistent with applying and receiving admission being a prerequisite for enrollment. Second, part of the effect at $k = 1$ is driven by increased *access*: the likelihood of first-time attendance increases by 24 percent at $k = 1$ (figure A.32). Third, reparations also improve undergraduate *persistence*: the magnitude of the enrollment effect rises over time and reaches 18.3 percent four semesters after the reparations payout.[24]

Enrollment gains from reparations are likely to vary depending on financial constraints. 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 enrollment effects to be larger at private institutions. By 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. Therefore, figure A.33 and table A.7 compare enrollment at private and public universities. Reparations cause large enrollment gains at private universities. By $k = 4$, the point estimate is significantly larger at private than at public institutions. These results suggest that binding credit constraints prevented human capital investments for victims.

Last, in figure A.34, we examine impacts on two- and three-year programs and any postsecondary enrollment. Reparations do not seem to affect attendance at two- or three-year programs; if anything, the effect is negative, indicating that money induces victims to respond on

[24] Unfortunately, we are underpowered to detect impacts on graduation, as the SPADIES database ends in 2016. Since a bachelor's program typically lasts four or five years in Colombia, we are restricted to reparations paid out before 2011, i.e., before the Victims Law.

the intensive margin, substituting bachelor's degrees for short-cycle education programs. The average effect of reparations on any postsecondary enrollment is 10.3 percent by $k = 4$ and is highly significant (table A.7). These results are robust to the comparison group and using the Callaway and Sant'Anna (2021) estimator (figure A.35 and table A.8).

4.3.2 High School Graduation and Test Scores

This section studies 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. High school seniors take Saber 11 in their final semester before graduation, regardless of their postsecondary intentions. Therefore, taking Saber 11 is a proxy measure for graduating from high school. Since most people graduate from high school only once, the Saber 11 microdata is a cross-section and not a panel. As a result, we do not use the event-study approach to estimate the effect of reparations on schooling outcomes.

Instead, our empirical approach compares school-aged children whose households received reparations when the children were younger versus older. Intuitively, if money affects children's schooling outcomes, we expect it to have stronger effects among children with more time left in school. Older children—defined as children aged 16 and above—have less time left in school; therefore, their schooling behavior should be less affected by reparations. Indeed, recent evidence suggests that the length of exposure to greater economic resources matters and that young children benefit disproportionately from transfers (Bailey et al., 2020; Chetty et al., 2016).

We, therefore, estimate the effects of reparations on high school outcomes by comparing test-age children whose households received reparations when they were aged 13–15 ("younger") versus 16–18 ("older").[25] We use 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 child i, $1(Age < 16)$ is a dummy that equals one if the child's household first received reparations when he or she was aged 13–15 and zero otherwise, $\psi_{t(i)}$ are year-of-birth fixed effects, $\gamma_{c(i)}$ are reparations-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_i is the error term. β is the parameter of interest and compares the effect of receiving reparations on schooling outcomes when the child has more versus less time left in school. The identifying assumption is that children whose household receives reparations when they have more versus less time left in school are comparable after controlling for year-of-birth, reparations-year, victimization-type,

[25] Note that, first, this approach restricts the estimation sample to eventually treated households. Second, our results hold when excluding 16-to-17-year-olds from the control group (see table A.10). Third, we define "test-age" children as those 10–15 years old in 2010, where the likelihood of taking Saber 11 is the highest and constant (figure A.36).

and municipality fixed effects and baseline covariates. Arguably, this assumption is plausible because reparations were not prioritized based on children's ages or even on a household's having children at all. Nevertheless, to empirically support the identifying assumption, we show how β varies when progressively including FE and controls. Table A.11 shows how the differential effects for children whose households receive reparations when they are younger remain positive and significant regardless of the number of controls that we add. Moreover, the last column shows that the estimate is larger and highly significant using nearest-neighbor matching.

Table VI 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. Moreover, reparations do not seem to affect the likelihood of graduating high school: the β coefficient is close to zero and not statistically significant at conventional levels.

Next, column (2) compares the average age at which children took Saber 11. Reparations appear 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. Table A.9 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 reparations. This effect might be driven by reparations reducing children's schooling disruption thanks to positive effects on living conditions and health.

Last, columns (3)–(8) report the effect of reparations on students' performance in Saber 11. Reparations significantly improve students' test scores. Relative to older victims, younger victims score 2.37 percentiles higher in the high school exit exam, or 7 percent of a standard deviation. To benchmark this finding, note that this effect is similar to that estimated by Dahl and Lochner (2012) on the effect of income on test scores in the United States. Furthermore, studies from the United States suggest that a one standard deviation increase in teacher quality implies an increase in test scores of about 12 percent of a standard deviation (see Rose et al., 2022, and references therein).

Figure XI decomposes the percentile results by age bins. Consistent with reparations 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.

To interpret this positive effect of reparations on children's test performance, we examine how they affect the high schools' characteristics: whether it is a private or public school, its average cohort size, and its geographic location. In addition, we test whether reparations affect children's peers by examining impacts on their school's average test scores and socioeconomic composition. The results, reported in table A.12, suggest that reparations do not affect any of these observable characteristics: the coefficients are close to zero and not statistically significant at conventional levels. This indicates that the test score gains are not driven by changes in the observable features of the high school children attend nor by changes in the characteristics of their peers. Instead, reparations likely raise test scores by improving children's living and health conditions, as we previously documented. Moreover, the money may have boosted students' aspirations, resulting in greater effort.

5 A Stylized Cost-Benefit Analysis

Scholars argue that reparations may be essential to fostering peace and promoting civic trust, social cohesion, reconciliation (de Greiff, 2009; Firchow, 2013). By recognizing suffering and responsibility, reparations have a powerful symbolic value. Nevertheless, they are fiscally costly. Without prior knowledge of their effects, some detractors deem this cost too hefty a burden to impose on the taxpayer. Our results enable us to assess the cost of reparations in light of their measured benefits on victims' well-being. This section contributes to this discussion by providing a back-of-the-envelope cost-benefit analysis in light of our findings.

We make two main assumptions to assess the cost of reparations relative to measured benefits on victims' well-being. First, we project the medium-term gain based on the event-study coefficients estimated three or four years after the reparations payout plotted in figure XII. Second, we translate these coefficients into a net present value (NPV) money metric (using a 5 percent interest rate) for outcomes that represent a quantifiable benefit to the victim's quality of life, in terms of their work, consumption, health, or the next generation's human capital. Table VII reports the main results. We briefly summarize the steps taken to obtain our cost-benefit calculation in what follows.

Regarding reparations' impacts on earnings and entrepreneurship, we calculate the NPV dollar equivalents for formal earnings and business profits. Since the average age of reparations recipients is 45, we assume they have 15 years left in the labor force. (Colombia's retirement age is 62 for men and 57 for women.) First, we estimate reparations will raise annual formal earnings by 0.31 percentage points (2.05 percent from table II multiplied by the control mean) or US$3.05 per year. This leads to an NPV of US$34.80 per adult, or US$71.60 per household since there are 2.05 adults per household. Second, reparations raise the likelihood of having an active business by 0.28 percentage points (13.7 percent from table III multiplied by the control mean). We assume these businesses perform like the average micro-firm in Colombia, which makes a monthly profit of 94 percent of the legal minimum wage, according to Colombia's national statistics agency (DANE, 2020). This would imply an additional US$7.88 per adult per year, or US$184.80 per household in NPV.

Regarding reparations' impact on consumption, we add the NPV dollar equivalent of the higher inferred credit card consumption and the average purchased value associated with the higher number of real estate purchases. First, reparations increase quarterly inferred consumption by US$3.33 (32.46 percent from table IV multiplied by the control mean), which would imply a US$13.32 gain per year. Assuming this gain persists for 40 years based on Colombia's life expectancy (nearly 78 years), the NPV cash equivalent gain would be US$241.90 per adult, or US$498.30 per household. Second, reparations increase real estate purchases by 3.3 percentage points per household (39.37 percent from table A.4 multiplied by the control mean). Since the average purchase value for the recipient population is US$12,500, this represents an average gain of US$393.70.

Regarding reparations' impact on health, we compute the NPV of the total savings associated with reductions in ED visits, hospitalizations, and number of procedures. First, we estimate that reparations reduce health care utilization by 0.46 percentage points for ED visits and 0.2 percentage points for hospitalizations and induce 0.14 fewer medical procedures (17.2 percent, 17.5 percent, and 14.8 percent from table V multiplied by the respective control mean) four years after receiving reparations. We assume this reduction will persist for 40 additional years. The average cost of a medical procedure in Colombia is US$25, meaning reparations reduce the yearly cost of procedures by US$6.87 per person, or US$506.70 in NPV per household. The average length of a hospitalization is 5.8 days according to RIPS data from 2014, and the average daily cost of a hospitalization is US$401 (MinSalud, 2014). This means reparations reduce the yearly cost of hospitalizations by nearly US$9.44 per person, or US$696.00 in NPV per household. We could not find information about the cost of an ED visit, so we assume it is worth one-fourth of the daily cost of hospitalization, meaning reparations reduce the annual cost of ED visits by US$67.70 in NPV per household.

Regarding reparations' impacts on the next generation's human capital, we compute the expected dollar NPV gain from higher investment in undergraduate attendance and higher standardized test scores. First, we assume the increase in undergraduate attendance will translate into a higher chance of earning a degree. For private colleges, the cost of attendance is the tuition fee plus foregone earnings (which we observe to be one-third of the monthly minimum wage for victims aged 15–25). Public colleges, by contrast, 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 institution for the government, which is 85 percent of the cost for a private institution (Londoño-Vélez et al., 2020). In terms of benefits, we estimate that the attendance gain is 0.29 percentage points for private universities and 0.17 percentage points for public universities (32.3 percent and 10.6 percent from table A.7 relative to the control mean). Colombia's Ministry of Education estimates annual earnings to be US$16,214 for private university graduates and US$14,034 for public university graduates. Because the average victim with no college education earns around 70 percent of the minimum wage, this means a total annual gain of US$57.30, or US$937 in NPV for the household. Second, reparations increase students' performance in the high school exit exam by 2.37 percentiles. Previous work suggests that 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 US$1,160 in NPV.

Aggregating the benefits of reparations along the different dimensions and assuming a 16 percent increase in employer payroll taxes (social security contributions and *parafiscales*) due to higher earnings, the total NPV is US$4,019 excluding the consumption gains or US$4,911 including the consumption gains. Compared to the average household reparations payment of US$4,250, the benefit of reparations exceeds their cost. This result should be viewed as a lower-bound estimate. First, we ignore any (positive or negative) effect of reparations that we cannot measure with our administrative data, like satisfaction or trust in the state; reconciliation and trust are key objectives of reparations programs. Second, we ignore positive externalities from a more educated workforce, like improved aggregate productivity or reduced crime. We also 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. As a result, the benefits of reparations likely outweigh their costs significantly.

We plot these results graphically in figure XIII, where we report the share of reparations' cost recovered by welfare gains along the three main margins studied—work, health, and human capital.[26] We transform the main outcome variables by calculating their associated NPV, generating cash-equivalent indices for each margin. Then, we use the event-study approach to report the cash-equivalent welfare effects of reparations. We divide the coefficients by the average household reparations payment to interpret the effects as the share of reparations' cost recovered by welfare gains. While the welfare effects from work impacts are minuscule, reparations led to substantial welfare gains thanks to their improving victims' health and human capital in the long term. One-half of the reparations cost is recovered four years later thanks to reduced health care spending. An additional 60 percent of the cost of reparations is recovered by the resulting welfare gains from human capital investments. Again, these are lower-bound estimates—the welfare benefits of reparations likely far outweigh their costs.

6 Conclusion

This paper has evaluated the effects of economic reparations on victims of human rights violations by leveraging variation from Colombia's reparations program, which has awarded sizable, lump-sum cash payments to hundreds of thousands of victims of the internal armed conflict. We have used comprehensive linked national administrative panel microdata to estimate the impacts of reparations on various outcomes, capturing different aspects of people's quality of

[26] As explained in section 3.1, the effects on consumption through the credit markets are based on Colombia's nationwide and census-like data set on formal loans. By contrast, the effects on real estate purchases are based on a sample from Antioquia. As a result, we cannot combine the two data sets to aggregate results along these two components. Therefore, we exclude the consumption margin from figure XIII.

life. Consistent with the "reparations as development" model, we have found that reparations have transformative effects, allowing households to make fundamental investments and improve their well-being. While victims have complete discretion when spending the money, they invest it in income-generating activities and their children's human capital. Moreover, they enhance their work and living conditions, and as a result, their health outcomes also improve. As a result, reparations programs like Colombia's can pursue transitional justice and simultaneously be a cost-beneficial development tool to help lift victims out of poverty and reduce the gaps formed due to victimization.

There are three considerations. The first is whether the causal relationship we have found holds over variation in victims, settings, and reparations-policy implementation. In our view, four features of Colombia's economic reparations may have contributed to their "transformative" impact on victims. First, the compensation amount is large enough to enable a meaningful purchase or investment. Second, victims who receive reparations are impoverished, with the payment representing several years of household income. Third, Colombia can attend to victims' needs and manage reparations payments thanks to its relatively high institutional and state capacity. Fourth, Colombia's implementation of the reparations policy promotes economic reparations as seed money for victims to invest and use to transform their lives. Under these conditions, reparations can foster development opportunities. By contrast, reparations may not fulfill their transformative potential if they are small, recipients are not poor, state capacity is weak, or reparations are not seen as a tool to overcome poverty.

Second, we have estimated the effects of reparations on outcomes available in administrative data sets. However, reparations potentially have numerous effects on many valuable peace-building and recovery outcomes that we, unfortunately, cannot capture, like reconciliation, social integration, and civic trust. Whether reparations causally affect these outcomes remains to be explored.

Last, Colombia's reparations program encompasses financial reparations and four additional measures—restitution, rehabilitation, satisfaction, and guarantees of non-repetition—all of which pursue transitional justice and are key to redressing gross human rights violations. Our empirical strategy isolates the effects of financial compensation, but victims may benefit from other forms of social assistance, like public education, health, and housing, as well as symbolic reparations measures, like truth recovery. Although further work remains in examining the causal effects of these various dimensions of victim reparations, especially the importance of non-monetary and symbolic reparations, the results of this study demonstrate that the financial-compensation component of reparations positively transforms victims' lives.

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Tables and Figures

Table I: 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

Note: This table presents descriptive statistics for our data on victims of the Colombian internal armed conflict 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 the RUV. Column (2) restricts the sample to the 7.7 million victims with valid contact information. 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, resulting in 7.4 million victims eligible to receive reparations. Column (4) presents descriptive statistics for the eligible victims who received reparations by June 2019. Column (5) presents summary statistics for the 25.8 million individuals in SISBEN in 2010. Columns (6)–(8) report baseline characteristics for eligible victims in SISBEN in 2010. *As of 2019. §We restrict the sample by dropping direct victims of homicide or forced disappearance.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Table II: Impact of Reparations on Labor Market Outcomes

Outcome	**Average in 2014q1** (1)	**Pre-event** $k \in [-4, -2]$ (2)	**Immediate** $k = 0$ (3)	**1 Year** $k \in [1, 4]$ (4)	**2 Years** $k \in [5, 8]$ (5)	**3 Years** $k \in [9, 12]$ (6)
Employment	16.40%	-0.2 (0.47)	-0.96** (0.45)	-2.4*** (0.54)	-2.18*** (0.66)	-2.03*** (0.73)
Self-employment	1.56%	-3.84* (2.08)	0.09 (1.93)	-1.34 (2.46)	-2.93 (3.15)	-5.43 (3.45)
Wage employment	14.80%	0.19 (0.5)	-1.08** (0.48)	-2.51*** (0.57)	-2.11*** (0.7)	-1.67** (0.77)
Heterogeneity by job risk and pay						
Low-risk	6.72%	-0.01 (0.82)	-0.32 (0.76)	-1.04 (0.97)	-0.69 (1.21)	0.24 (1.35)
High-risk	8.11%	0.35 (0.75)	-1.7** (0.76)	-3.74*** (0.86)	-3.28*** (1.05)	-3.25*** (1.13)
Min. wage	9.76%	-0.48 (0.84)	-2.73*** (0.89)	-3.91*** (0.9)	-3.51*** (1.05)	-3.45*** (1.12)
Above min. wage	5.07%	1.48 (1.08)	2.11* (1.19)	0.19 (1.18)	0.61 (1.43)	1.75 (1.57)
Low-pay and high-risk	5.27%	0.04 (1.21)	-2.97** (1.31)	-4.54*** (1.29)	-3.77*** (1.46)	-3.99*** (1.53)
Low-pay and low-risk	4.49%	-1.09 (1.31)	-2.45* (1.35)	-3.18** (1.42)	-3.21* (1.68)	-2.82 (1.82)
High-pay and high-risk	2.83%	0.94 (1.44)	0.67 (1.58)	-2.24 (1.63)	-2.35 (2)	-1.89 (2.2)
High-pay and low-risk	2.24%	2.15 (1.74)	3.94** (1.9)	3.27* (1.91)	4.35* (2.33)	6.37** (2.56)
Days of salaried work (includes zeros)	11.11	0.15 (0.5)	0.43 (0.47)	-1.83*** (0.58)	-1.66** (0.73)	-1.45* (0.82)
Days of salaried work (excludes zeros)	66.35	0.34 (0.38)	0.65 (0.43)	-0.32 (0.38)	-0.2 (0.4)	-0.08 (0.4)
Daily wage (in min. wages)	1.373	0.21 (0.36)	0.94** (0.39)	1.63*** (0.41)	2.14*** (0.46)	3.11*** (0.54)
Earnings (in min. wages)	0.149	0.46 (0.65)	1.45** (0.64)	-0.38 (0.77)	0.10 (0.97)	2.05 (2.13)
MPE		0.02 (0.03)	0.03 (0.02)	-0.03 (0.03)	-0.02 (0.03)	0.01 (0.04)
Quarters out-of-formal-employment†	6.48	-0.10 (0.07)	0.08 (0.05)	0.38*** (0.09)	0.43*** (0.13)	0.38*** (0.15)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 reparations, respectively. We refer to low- and high-paying jobs as those paying at or above the minimum wage, respectively.
† Capped at eight quarters.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Table III: Impact of Reparations on Entrepreneurial Activity

Variables	**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)
Registered business						
Has an active business	2.04%	-0.44 (2.42)	10.6*** (2.59)	8.72*** (2.93)	11.2*** (3.14)	13.7*** (3.39)
Microcredit						
Has any microcredit	8.86%	-0.05 (0.39)	-4.65*** (0.35)	-2.39*** (0.53)	0.99 (0.74)	3.24*** (0.86)
Outstanding balance	145.6 USD	-0.64 (0.56)	-3.59*** (0.48)	-0.38 (0.76)	5.54*** (1.14)	9.29*** (1.4)
Days delinquent	3.8	0.85 (1.7)	-13.45*** (1.52)	-25.67*** (2.53)	-27.65*** (3.69)	-24.14*** (4.56)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 reparations, respectively.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, Confecámaras, and the Financial Superintendence of Colombia.

Table IV: Impact of Reparations on Consumption through the Credit Market

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)
Credit card debt						
Has any credit card debt	3.59%	-1.02	-5.63***	0.77	7.77***	12.9***
		(0.79)	(0.79)	(1.03)	(1.44)	(1.7)
Outstanding balance	US$19.31	-1.26	-11.11***	1.72	18.26***	32.96***
		(1.57)	(1.37)	(2.02)	(3.18)	(4.27)
Consumption	US$10.26	13.55	-3.79	10.35	26.94**	32.46**
		(14.92)	(13.36)	(11.42)	(12.8)	(14.37)
Auto loans						
Has any automobile loan	0.11%	-9.24	12.27**	26.64***	24.79**	21.6*
		(6.16)	(5.92)	(8.01)	(10.92)	(12.77)
Outstanding balance	US$6.18	-9.94	-1.52	39.56***	42.86**	63.24***
		(8.32)	(6.05)	(13)	(18.15)	(24.46)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 reparations, respectively.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Table V: Impact of Reparations on Health Care Utilization

Variables	**Average in** **k=-1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,2] (4)	**2 Years** k ∈ [3,4] (5)	**3 Years** k ∈ [5,6] (6)	**4 Years** k ∈ [7,8] (7)
Any ED visit	2.67%	-0.12	-1.14	-5.38***	-7.66***	-15.21***	-17.15***
		(1.39)	(1.59)	(1.77)	(2.36)	(2.84)	(3.26)
Any hospitalization	1.16%	0.13	-0.37	-0.72	-8.32***	-15.6***	-17.49***
		(2.11)	(2.47)	(2.51)	(3.16)	(3.73)	(4.21)
Any procedure	18.30%	-0.71	-2.07***	-2.15***	-4.83***	-8.85***	-10.88***
		(0.50)	(0.54)	(0.61)	(0.82)	(1.05)	(1.26)
Number of procedures	0.93	-0.97	-0.37	-1.05	-3.85**	-9.47***	-14.78***
		(0.92)	(1.06)	(1.19)	(1.59)	(1.97)	(2.32)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in $k = -1$, the period immediately before the reparations payout. Columns (2)–(6) report the difference in outcome between treated and not-yet-treated victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 reparations, respectively.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Table VI: Impact of Reparations on High School Graduation and Test Scores

	Takes Saber 11 exam	Conditional on taking Saber 11 exam						
		Age	Test score					
			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	
Counterfactual mean	0.51	17.67	41.23	-0.32	40.52	-0.28	40.52	-0.27
Counterfactual SD	(0.5)	(1.38)	(26.22)	(0.82)	(26.63)	(0.86)	(26.73)	(0.9)
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Reparations-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-of-birth 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

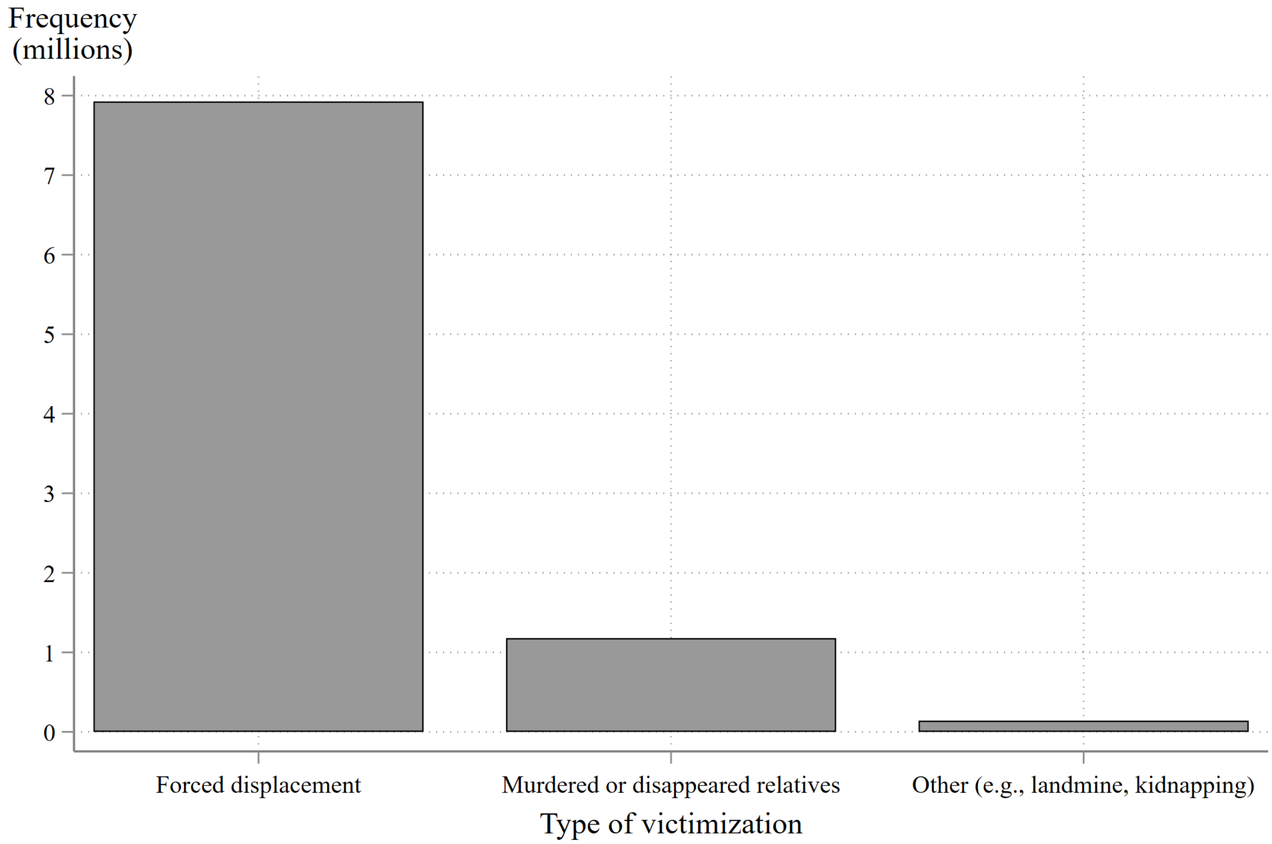
Note: This table presents the effects of reparations on high school graduation and performance in Colombia's national standardized high school exit exam, reporting the β coefficient and robust standard errors using specification (2). The outcome in column (1) is the likelihood of a child's taking the Saber 11 exam. For those who take Saber 11, the outcome is the age when the child took the exam in column (2), the test score percentile in columns (3), (5), and (7), and the standardized test score using the standard deviation of the entire population of test-takers in columns (4), (6), and (8). We also include the following individual and household SISBEN controls: year of SISBEN survey, SISBEN wealth score, household income, household size, female dummy, attending school dummy, attending a public school dummy, living in an urban area dummy, household homeownership status, and indicators for health care regime, female head of household, living in an apartment or household, access to electricity, access to sewerage, access to gas, owns a phone, owns a refrigerator, owns a TV, and has cable. The sample is restricted to children aged 10–15 in 2010 whose households received reparations when the child was aged 13–19.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using data from the SRNI, SISBEN, and ICFES.

Table VII: Stylized Cost-Benefit Analysis

	% effect	pp effect	Annual Dollars	Years Proj.	NPV	HH NPV
Income						
Total formal wage earnings per adult	2.05%	0.0031	3.05	15	34.8	71.6
Profit from having an active business	13.7%	0.0028	7.88	15	89.7	184.8
Health						
ED visits	17.2%	0.5%	0.92	40	16.7	67.7
Hospitalizations	17.5%	0.2%	9.44	40	171.4	696.0
Procedures	14.8%	13.7%	6.87	40	124.8	506.7
Human Capital						
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
Consumption						
Credit card consumption	32.46%	3.3304	13.32	40	241.9	498.3
Real estate purchases	39.37%	0.0315				393.7
Taxes and SS contributions					335.3	372.5
			Total NPV with consumption			$ 4,911
			Total NPV without consumption			$ 4,019
			Reparations			$ 4,250

Note: We use an annual interest rate of 5 percent. 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 15–25 years old and 1.23 children younger than 15.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, PILA, Confecámaras, DANE, RIPS, SPADIES, ICFES, the Financial Superintendence of Colombia, and Catastro Antioquia.

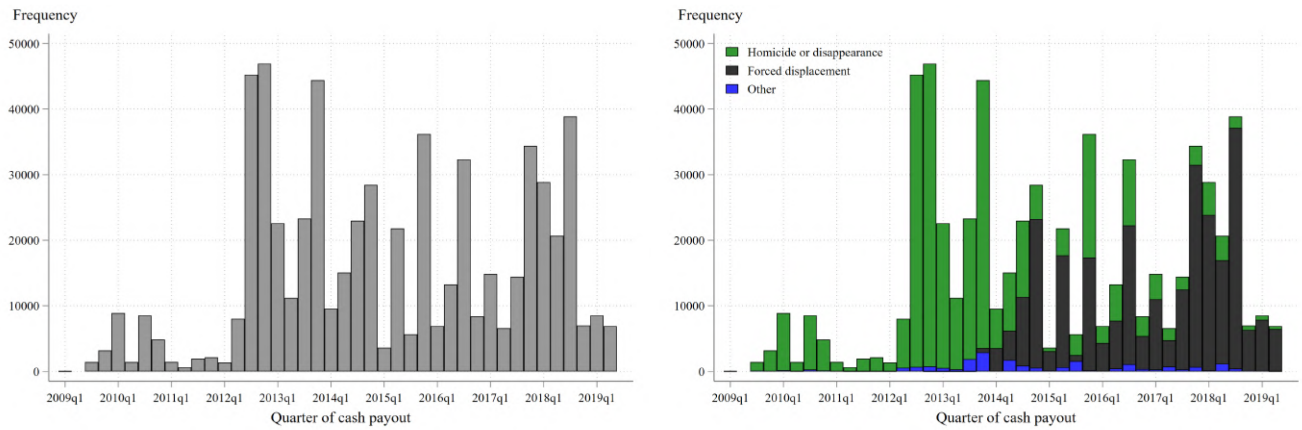
Figure I: Number of Victims by Type of Victimization



Note: 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 who were victims of homicide or forced disappearance. Victims that have suffered neither type of victimization are included in the category "Other." This category includes, for instance, victims of torture, rape, or kidnapping. The figure shows there were 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 the SRNI.

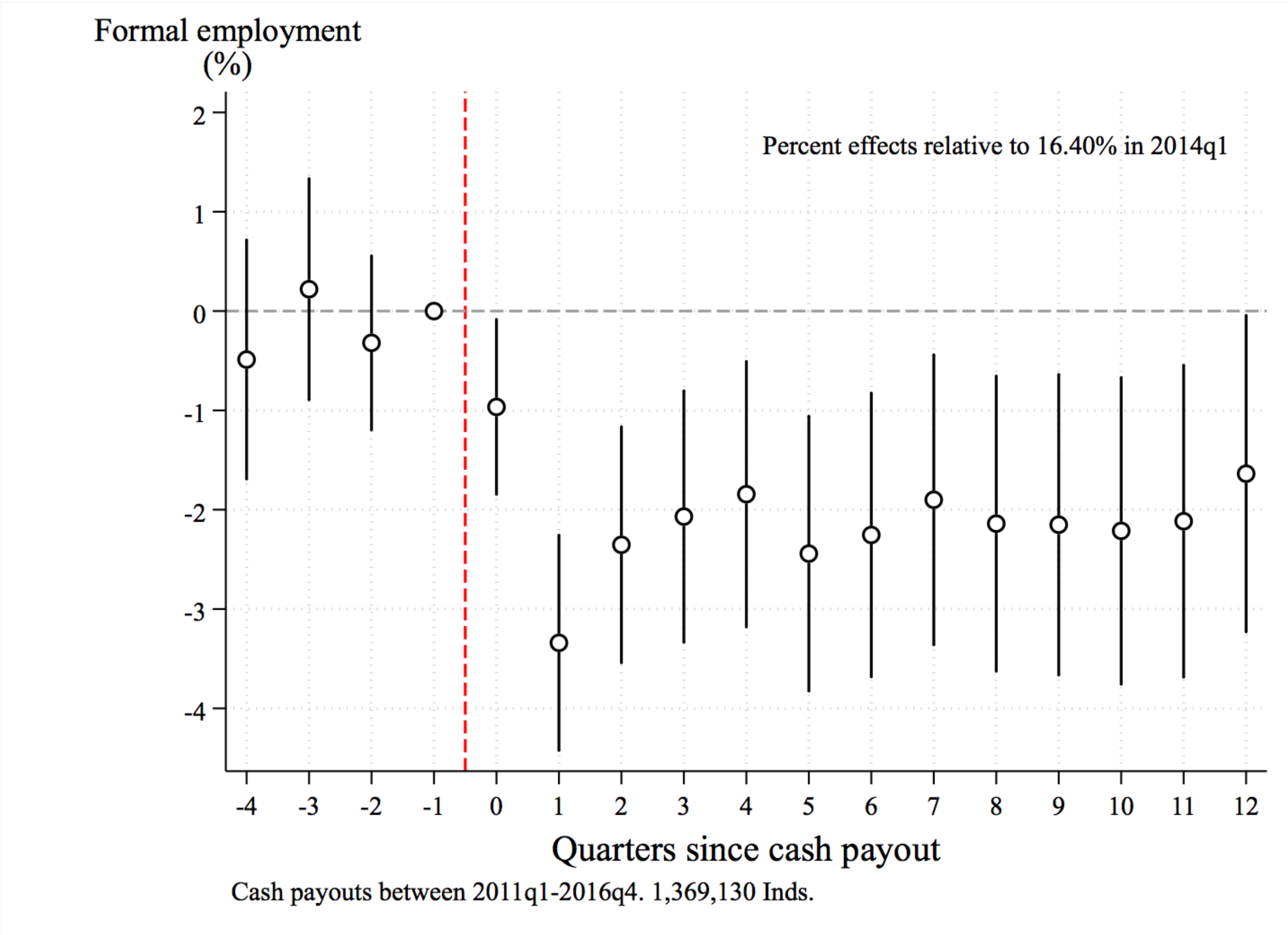
Figure II: Reparations by Quarter

(a) All victimizations (b) By victimization type



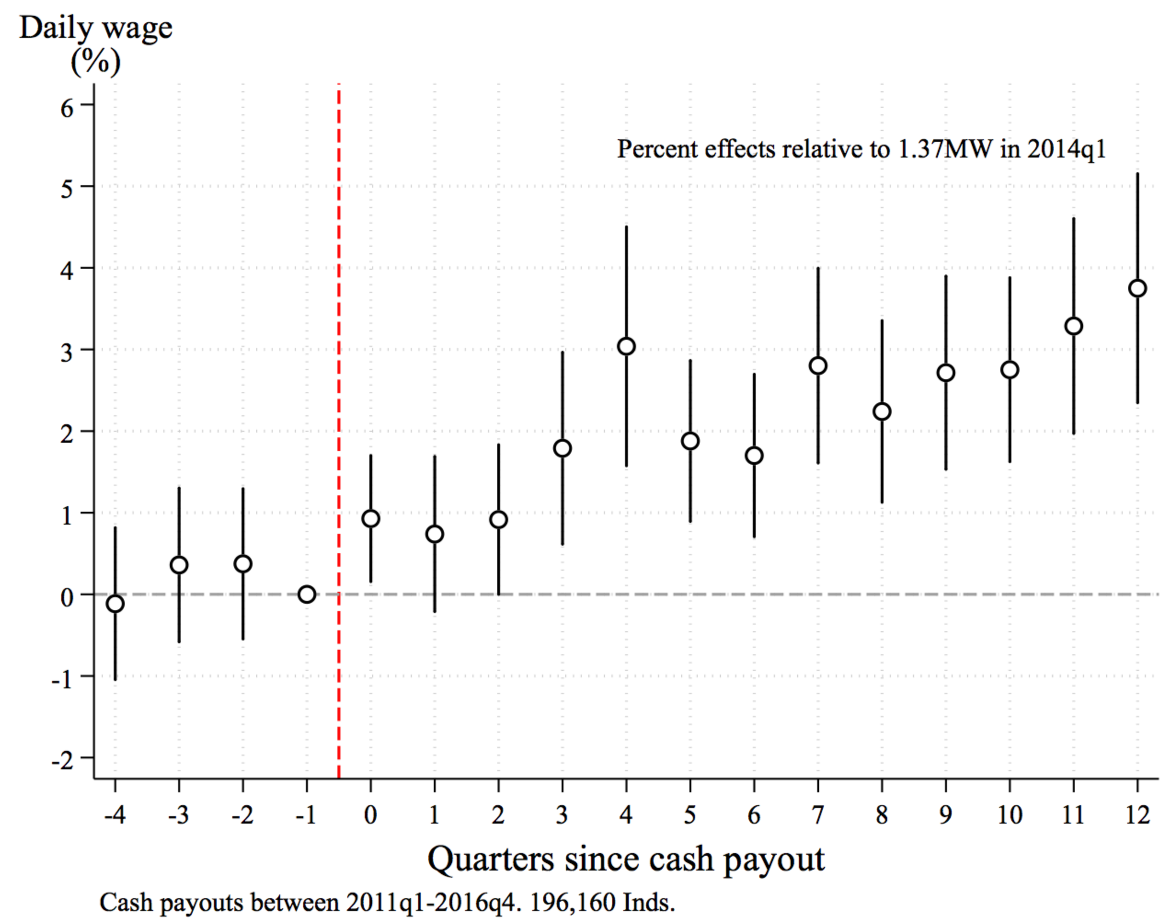
Note: This figure plots the frequency of reparations by the quarter of the reparations payout. Panel (a) plots the series for all types of victimization. Panel (b) plots the series by victimization type: homicide or forced disappearance (in green), forced displacement (in black), and all other victimizations (in blue). A victim can receive more than one reparations payment; in fact, 64 percent of victims with murdered or forcibly disappeared relatives were also forcibly displaced. Reparations for homicide and forced disappearance were prioritized and distributed earlier than others. By contrast, reparations for forced displacement began only after the Constitutional Court ruled to compensate these households.
Source: Authors' calculation using RUV data from the SRNI.

Figure III: Formal Employment



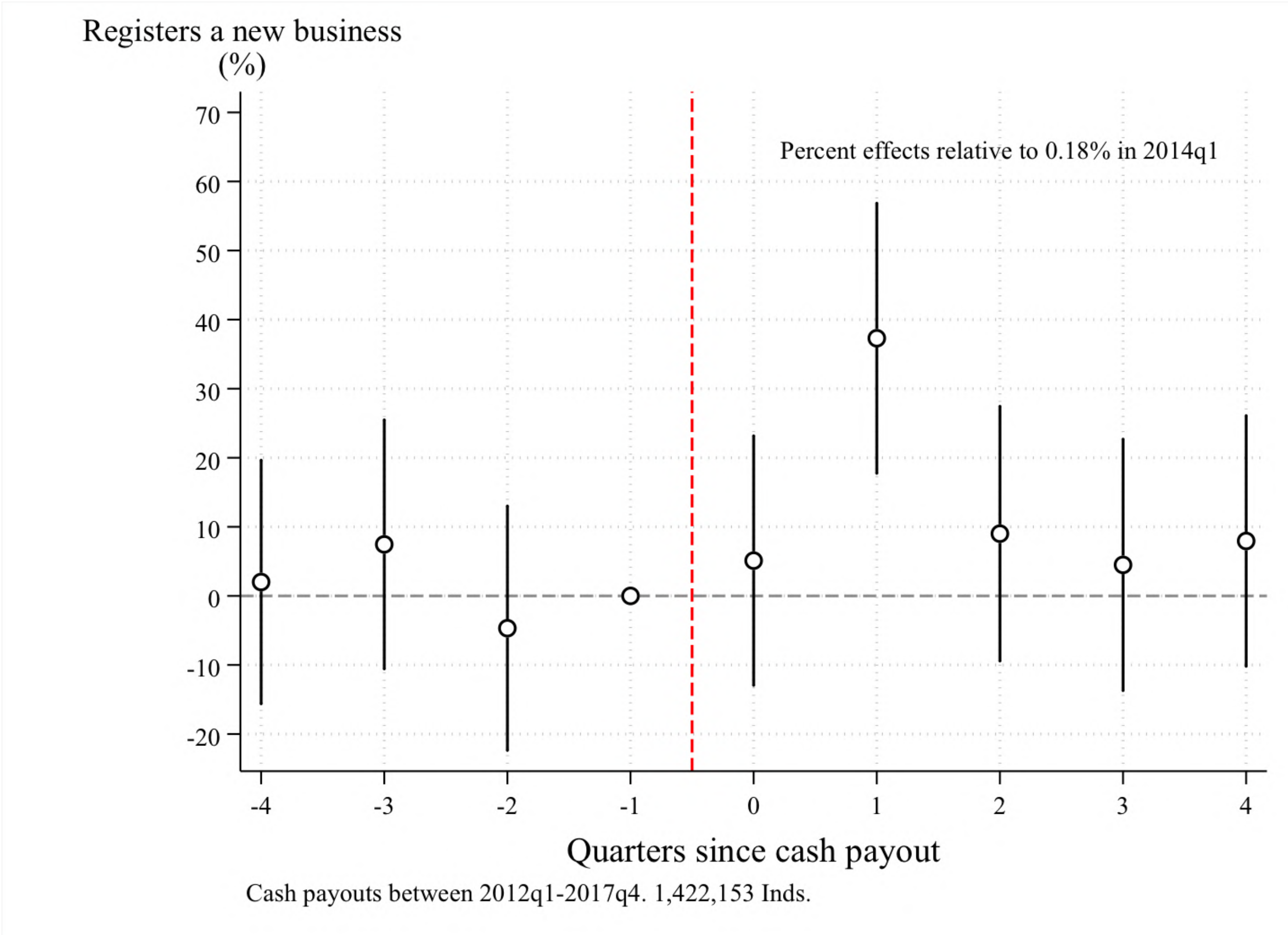
Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual works in the formal sector (i.e., contributes to social security)—is plotted against the time from the reparations payout (in quarters). The figure shows that reparations reduce formal employment by 3.3 percent relative to a baseline mean of 16.4 percent. 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 reparations, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.8 shows similar results excluding never-treated victims and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Figure IV: Formal Daily Wages



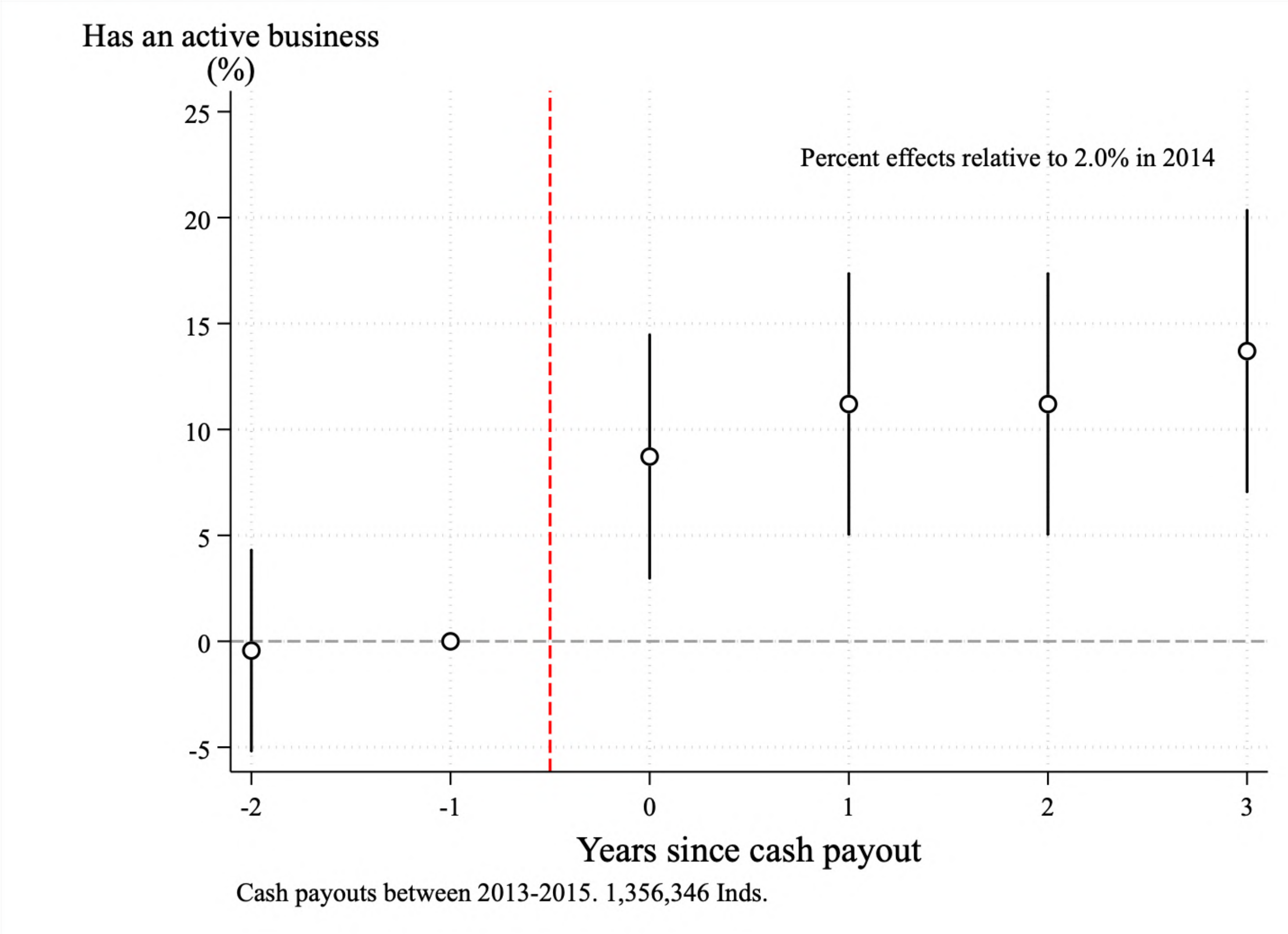
Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—daily wages for salaried workers (expressed relative to the minimum wage)—is plotted against the time from the reparations payout (in quarters). The figure shows that reparations significantly increase 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.11 shows similar results when excluding never-treated individuals (though the effects are less precisely estimated) and when using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Figure V: Formal Business Creation



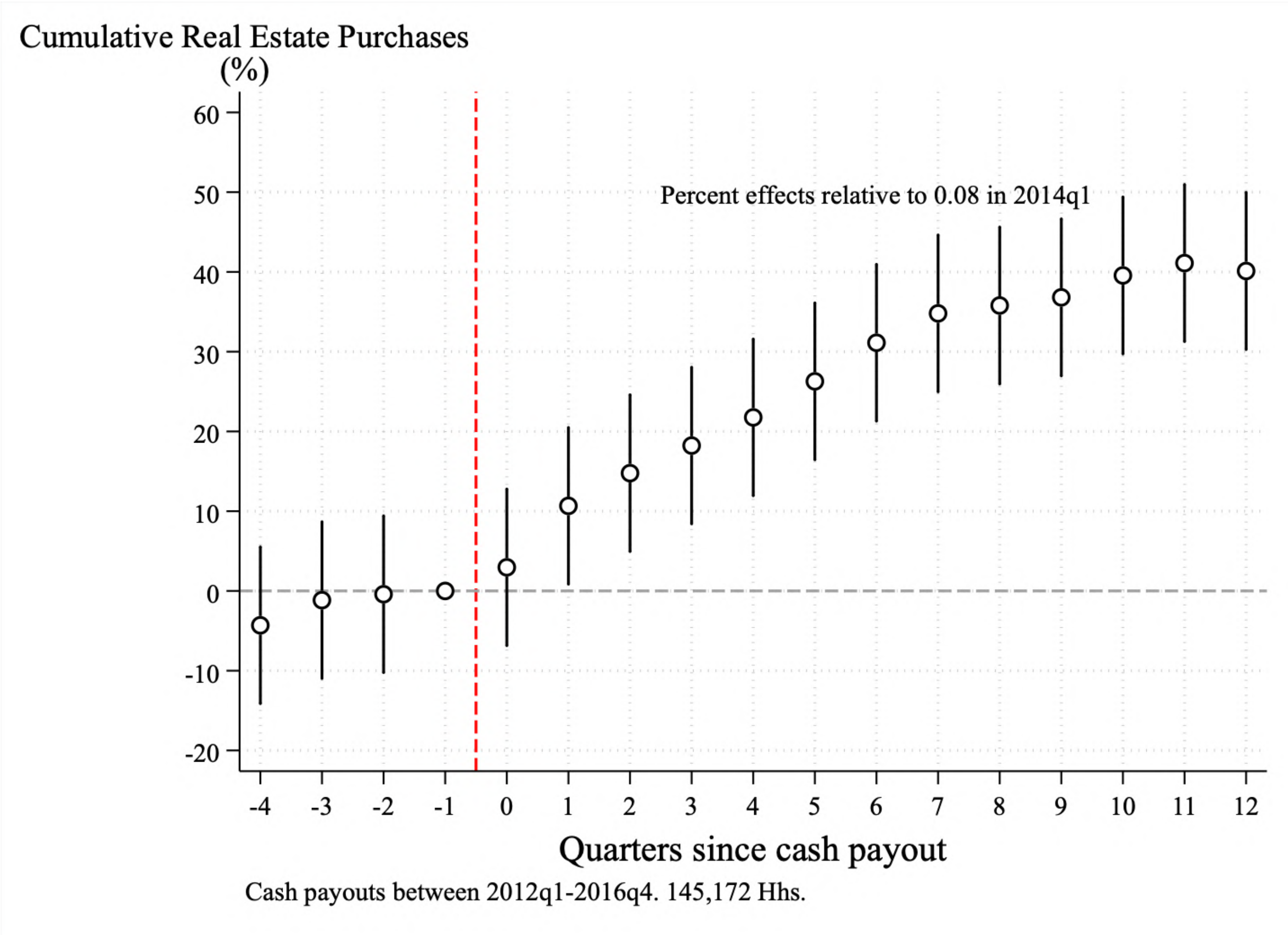
Note: This figure presents the event-study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—whether an individual registered a new business that quarter—is plotted against the time from the reparations payout (in quarters). The likelihood of registering a new business one quarter after the reparations payout ($k = 1$) increases 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 reparations, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.14 shows similar results when excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and Confecámaras.

Figure VI: Business Survival



Note: 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 time from the reparations payout (in years). We define active businesses as those with a valid license, which firms are required to renew every year. The likelihood of having an active business increases by 14.7 percent in the year of the reparations payout ($k = 0$) from a base of 1.6 percent, and the coefficient is significant at the 1 percent level. The effect persists for at least three years after the reparations payout. The treatment is defined at the individual level, and the sample is balanced in event time and includes victims that did not receive reparations, i.e., never-treated individuals. Standard errors are clustered at the household level. Figure A.15 shows similar results when excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and Confecámaras.

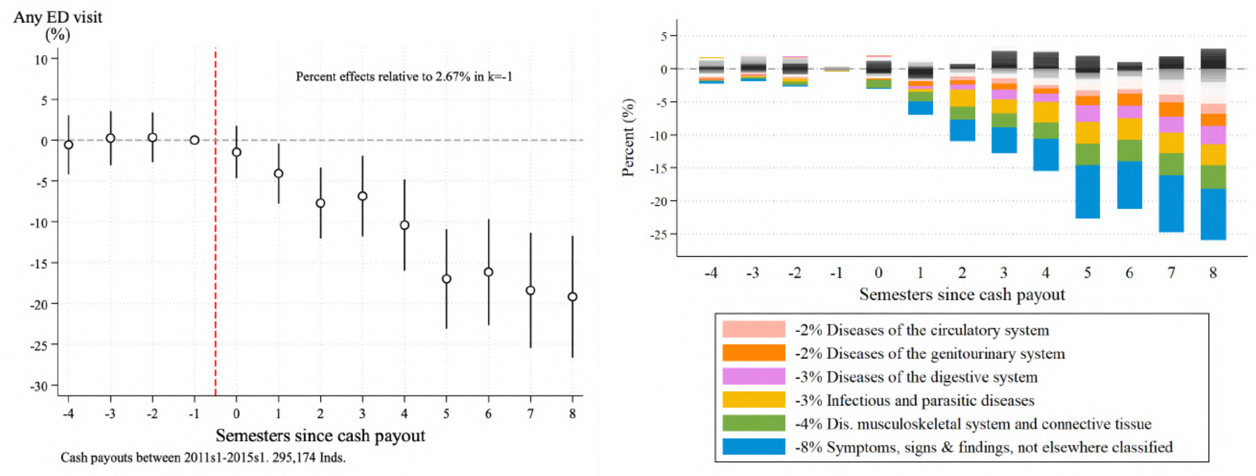
Figure VII: Cumulative Real Estate Purchases



Note: 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 time from the reparations payout (in quarters). The figure shows that reparations significantly increase real estate purchases. The effect is statistically significant at the 1 percent level and increases over time, reaching 40.1 percent by $k = 12$. The treatment is defined at the household level, and the sample is balanced in event time. Standard errors are clustered at the household level. Figure A.25 shows similar results when excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Catastro Antioquia.

Figure VIII: Emergency Department (ED) Visits

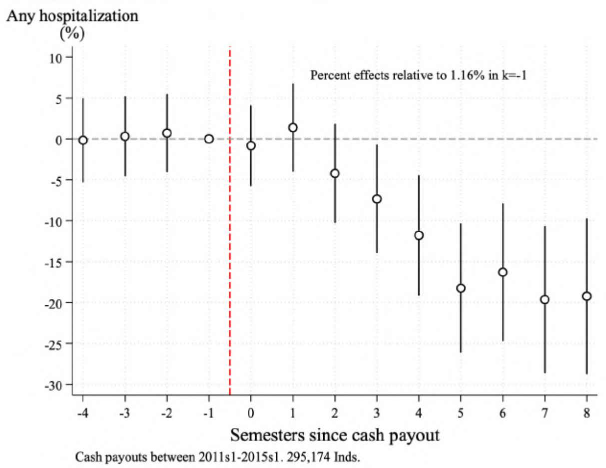
(a) Any ED visit (b) Decomposed by diagnosis



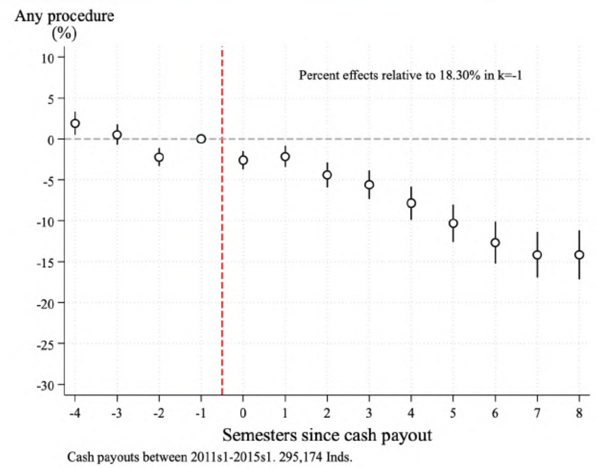
Note: This figure presents the effect of reparations on ED visits. Panel (a) plots the event-study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome—the likelihood of an emergency department visit in a given semester—is plotted against the time from the reparations payout (in semesters). Reparations reduce ED visits, with compounding effects over time: by $k = 8$, ED visits are 19.19 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 reduce ED visits associated with diseases from external causes (e.g., various symptoms, musculoskeletal illnesses, and infectious and parasitic diseases). Over time, reparations also lower ED visits due to diseases associated with dietary and other habits that affect the circulatory, genitourinary, and digestive systems. The treatment is defined at the household level, and the event time is defined as the first date when any household member received reparations. The sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level. Figure A.28 shows similar results when including never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Figure IX: Other Measures of Health Care Utilization

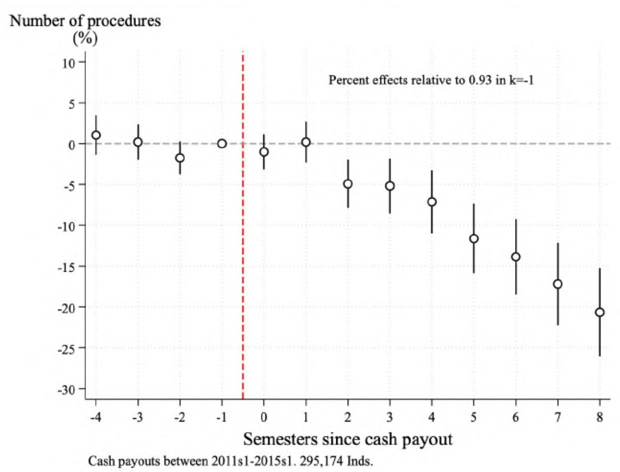
(a) Any hospitalization



(b) Any medical procedure

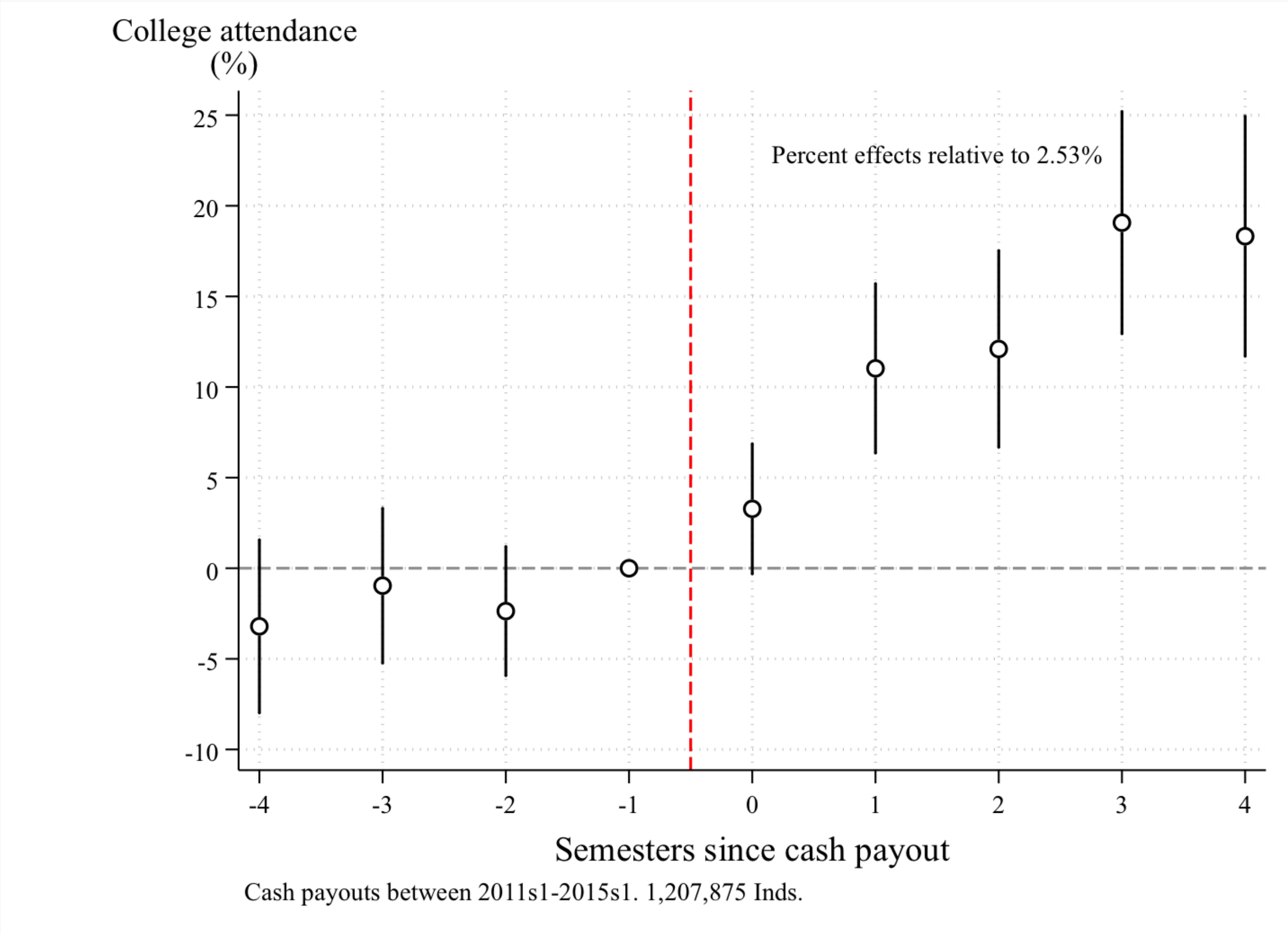


(c) Number of medical procedures



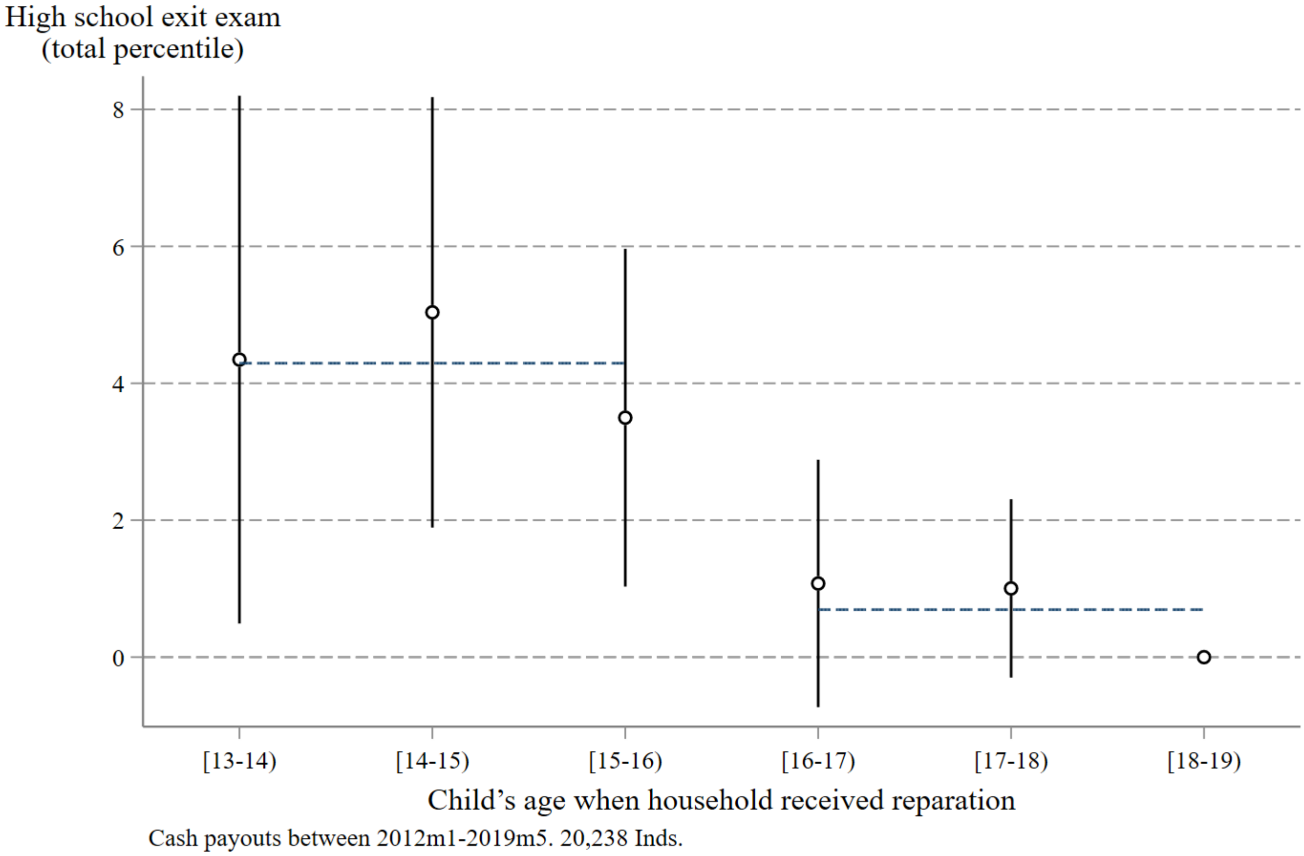
Note: This figure presents the event-study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the time from the reparations payout (in semesters). The outcome is an indicator for hospitalization in panel (a), undergoing a medical procedure in panel (b), and the number of medical procedures in panel (c). Reparations reduce hospitalizations and medical procedures, with compounding effects over time. The treatment is defined at the household level, and the event time is defined as the first date when any household member received reparations. The sample is balanced in event time and excludes never-treated individuals. Standard errors are clustered at the household level. Figures A.29, A.30, and A.31 report the results when including never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

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



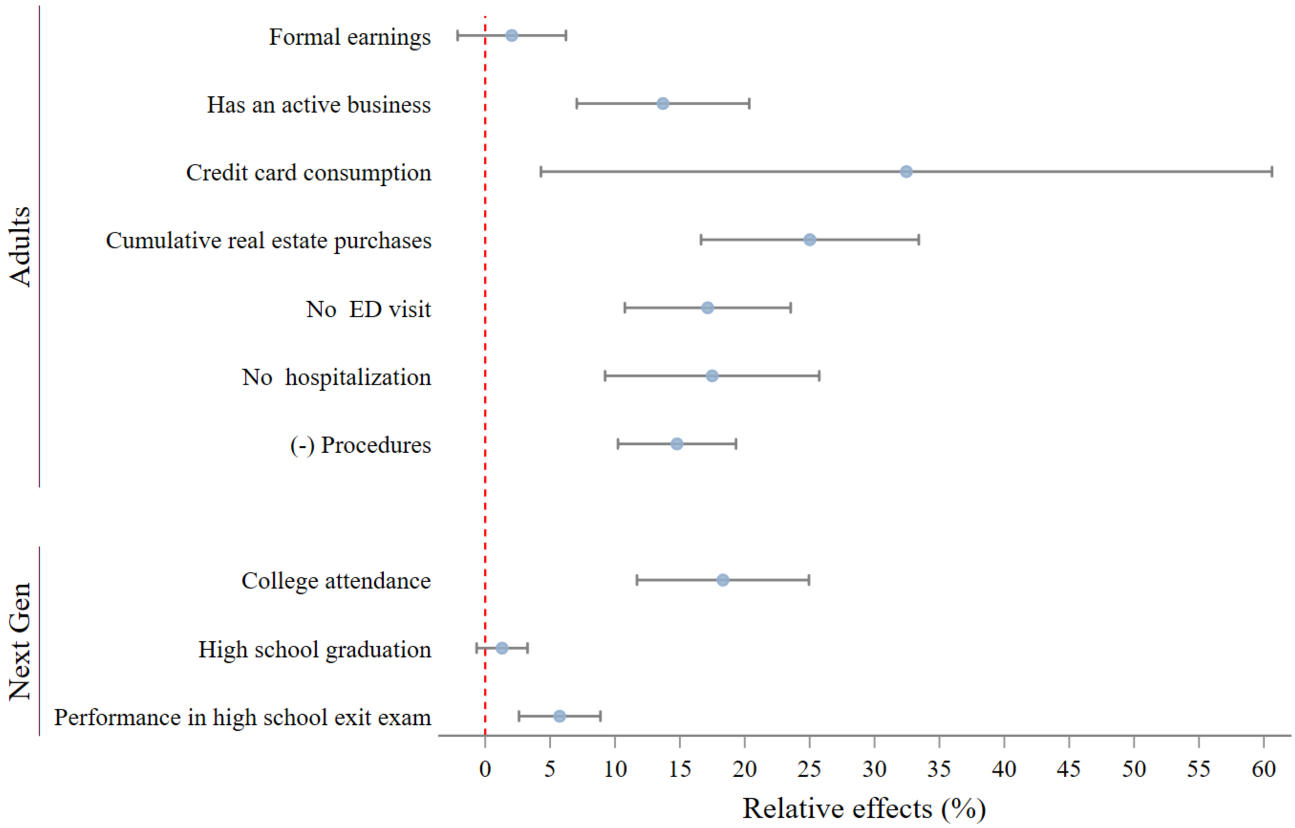
Note: This figure presents the event-study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a four- or five-year undergraduate program in a given semester—is plotted against the time from the reparations payout (in semesters). The treatment is defined at the household level, and the event time is defined as the first date when any household member received reparations. The sample (i) is balanced in event time, (ii) is restricted to individuals aged 15–25 at $k = -1$, and (iii) includes never-treated individuals. Standard errors are clustered at the household level. The figure suggests reparations boost undergraduate attendance, with compounding effects over time: by $k = 4$, attendance is 18.3 percent higher. Figure A.32 shows that part of the effect at $k = 1$ is driven by increased access: the likelihood of attending an undergraduate program for the first time increases by 24 percent. Figure A.33 compares enrollment in private and public colleges. Figure A.34 presents enrollment in short-cycle programs and overall postsecondary enrollment.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Figure XI: Effect of Reparations on Performance in High School Exit Exam



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

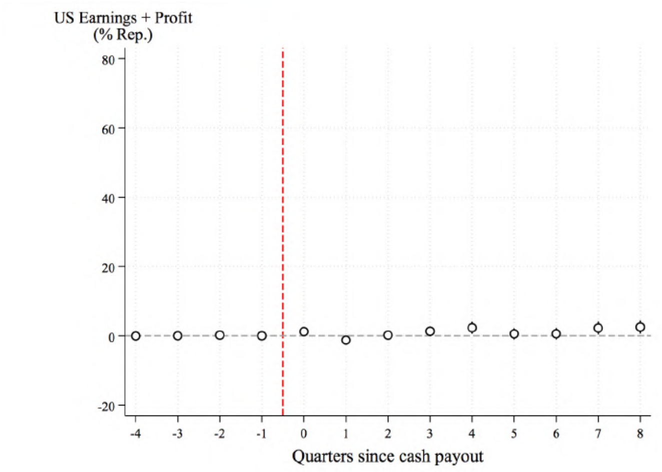
Figure XII: Summary of the Effects of Reparations on Adult Victims and Children Three or Four Years Later



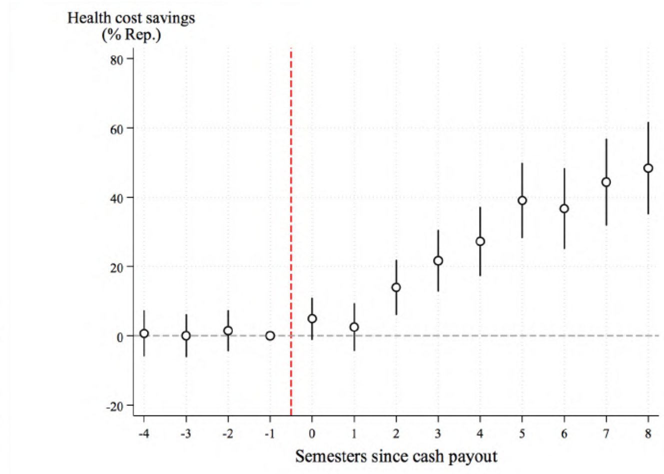
Note: This figure summarizes the relative effects of reparations on adult victims and their children three or four years after the reparations payout. The effects are reported in percentage terms (i.e., relative to the mean of the comparison group). Each row reports the event-study coefficient and associated 95 percent confidence interval on the latest available period after the reparations payout. For high school performance, this is the β coefficient from specification (2).
Sources: Tables II, III, IV, V, VI, A.4, and A.7.

Figure XIII: Cash-Equivalent Gains from Reparations on Victims' Well-Being

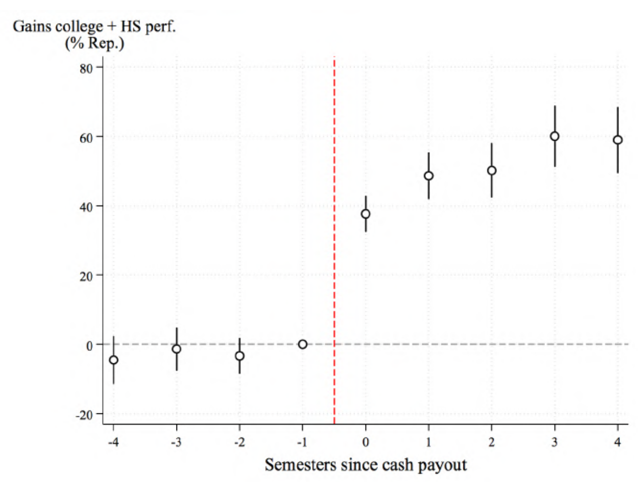
(a) Work



(b) Health



(c) Human capital



Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—a cash-equivalent index—is plotted against the time from the reparations payout (in quarters or semesters). The largest welfare gains from reparations are driven by improvements in health and human capital.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, PILA, Confecámaras, RIPS, SPADIES, and ICFES.

Appendices

A Online Tables and Figures

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

Type of victimization	Reparations 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
Kidnapping		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

* Resolution 7381/2004.
** Resolution 00552/2015 reduced this amount from 30 times the monthly minimum wage to 10.
*** The Victims Law reduced the amount of the reparations from 27 to 17 times the monthly minimum wage for people who had been forcibly displaced after April 22, 2008, or who had not registered this victimization in the RUV (and therefore were not included in the waiting list 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, or grandparents, with sibling(s)	Closest relative	Siblings	No cash award to sibling(s)
5	Single without children or parents, with sibling(s) and grandparent(s)	Closest relative	Sibling(s)	Grandparent(s)
6	Single without children or 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 or parents	Partner	Partner	Partner
10	Single without children, parents, or sibling(s)	Closest relative	Closest relative	Grandparent(s)

Note: Two individuals are considered to be cohabitating 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) he/she provided, financially, for the victim before his/her death or disappearance. A Constitutional Court ruling expanded the definition of relatives eligible to receive these reparations to include 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)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. 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 reparations, respectively.

$*p < 0.1; **p < 0.05; ***p < 0.01$.

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

Table A.4: Impact of Reparations on Land- and Homeownership

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)
Real estate purchases						
Cumulative real estate purchases	0.08	-1.94 (4.09)	2.97 (5.01)	16.36*** (3.96)	31.98*** (3.97)	39.37*** (3.98)
Mortgage loans						
Has any mortgage debt	0.31%	-4.39** (1.93)	-2.4 (1.7)	1.88 (2.76)	9.34** (4.3)	17.25*** (5.44)
Outstanding balance	US$28.22	-9.09** (4.52)	-1.16 (2.32)	7.09 (4.56)	24.73*** (7.63)	45.96*** (10.28)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 reparations, respectively.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Table A.5: Impact of Reparations on Health Care Utilization (Including Never-Treated)

Variables	**Average in 2014s1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,2] (4)	**2 Years** k ∈ [3,4] (5)	**3 Years** k ∈ [5,6] (6)	**4 Years** k ∈ [7,8] (7)
Any ED visit	2.89%	-1.33 (1.14)	1.69 (1.38)	-1.95 (1.28)	-2.12 (1.35)	-7.62*** (1.35)	-7.05*** (1.36)
Any hospitalization	1.16%	10.27*** (1.91)	-4.78** (2.38)	-15.76*** (2.15)	-23.4*** (2.16)	-28.15*** (2.13)	-21.05*** (2.16)
Any procedure	19.00%	-0.55 (0.43)	-1.56*** (0.49)	-1.47*** (0.48)	-3.82*** (0.51)	-6.33*** (0.52)	-5.69*** (0.53)
Number of procedures	0.99	-2.71*** (0.78)	0.87 (0.95)	0.74 (0.93)	-0.78 (0.98)	-4.53*** (0.99)	-4.48*** (1.02)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014q1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 reparations, respectively.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Table A.6: Impact of Reparations on Health Care Utilization by Diagnosis

Variables	**Average in k=-1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Year** k ∈ [1,2] (4)	**2 Years** k ∈ [3,4] (5)	**3 Years** k ∈ [5,6] (6)	**4 Years** k ∈ [7,8] (7)
Any ED visit	**2.67%**	-0.11 (1.39)	-1.15 (1.59)	-5.39*** (1.77)	-7.68*** (2.36)	-15.21*** (2.84)	-17.15*** (3.26)
Symptoms, signs, and abnormal clinical and laboratory findings	30.26%	-0.35 (0.77)	-0.11 (0.92)	-2.64*** (0.94)	-4.42*** (1.22)	-7.68*** (1.46)	-8.31*** (1.67)
Diseases of the musculoskeletal system	8.39%	-0.36 (0.39)	-1.09** (0.48)	-1.62*** (0.48)	-2.13*** (0.61)	-3.04*** (0.72)	-3.18*** (0.81)
Certain infectious and parasitic diseases	10.52%	-0.40 (0.46)	0.27 (0.57)	-1.15** (0.55)	-2.01*** (0.71)	-2.48*** (0.82)	-2.16** (0.91)
Diseases of the digestive system	7.08%	0.07 (0.38)	0.15 (0.45)	-0.67 (0.44)	-1.48*** (0.55)	-2.39*** (0.64)	-2.79*** (0.74)
Diseases of the genitourinary system	9.06%	-0.07 (0.41)	-0.27 (0.49)	-0.76 (0.51)	-1.06* (0.63)	-1.82** (0.75)	-2.25*** (0.86)
Diseases of the circulatory system	4.04%	-0.25 (0.27)	0.15 (0.34)	-0.32 (0.33)	-0.65 (0.41)	-0.72 (0.49)	-1.31** (0.58)
Diseases of the ear and mastoid process	2.17%	0.12 (0.21)	-0.14 (0.25)	-0.15 (0.24)	-0.40 (0.29)	-0.91*** (0.34)	-1.06*** (0.39)
Diseases of the respiratory system	12.47%	-0.30 (0.51)	0.51 (0.6)	0.41 (0.6)	0.48 (0.76)	-0.32 (0.9)	-0.81 (1.01)
Neoplasms	0.88%	0.08 (0.13)	-0.17 (0.15)	0.04 (0.15)	-0.20 (0.19)	-0.25 (0.23)	-0.45* (0.26)
Pregnancy, childbirth, and the puerperium	5.58%	0.44 (0.34)	-0.22 (0.39)	0.14 (0.39)	-0.34 (0.47)	0.10 (0.55)	-0.05 (0.62)
Diseases of the skin and subcutaneous tissue	3.37%	0.27 (0.26)	-0.06 (0.32)	-0.02 (0.31)	-0.03 (0.37)	-0.22 (0.43)	-0.43 (0.49)
Endocrine, nutritional, and metabolic diseases	1.60%	-0.07 (0.18)	0.03 (0.21)	-0.14 (0.20)	-0.25 (0.26)	-0.26 (0.30)	-0.54 (0.35)
Diseases of the eye and adnexa	1.22%	0.08 (0.16)	0.02 (0.19)	-0.03 (0.18)	-0.19 (0.21)	-0.16 (0.25)	-0.37 (0.29)
Congenital malformations, deformations	0.05%	0.06 (0.04)	0.03 (0.04)	0.00 (0.04)	0.01 (0.06)	-0.05 (0.06)	-0.07 (0.07)
Diseases of the blood and blood-forming organs	0.51%	-0.18* (0.1)	-0.08 (0.11)	0.03 (0.11)	0.02 (0.13)	-0.05 (0.15)	-0.05 (0.18)
Certain conditions originating in the perinatal period	0.05%	0.04 (0.04)	0.15** (0.06)	0.04 (0.05)	0.06 (0.06)	0.04 (0.06)	0.04 (0.07)
Mental, behavioral, and neurodevelopmental disorders	0.94%	0.04 (0.14)	0.18 (0.17)	0.11 (0.16)	0.28 (0.20)	0.11 (0.23)	0.13 (0.27)
Diseases of the nervous system	3.17%	-0.16 (0.25)	0.14 (0.30)	0.19 (0.30)	0.76** (0.38)	0.61 (0.43)	0.42 (0.49)
Injury, poisoning, and certain other consequences of external causes	14.87%	0.19 (0.56)	0.39 (0.67)	-0.17 (0.65)	0.81 (0.80)	0.14 (0.95)	0.14 (1.09)
External causes of morbidity	1.05%	-0.23* (0.14)	0.26 (0.18)	0.04 (0.16)	0.20 (0.21)	0.38 (0.25)	0.72** (0.29)
Factors influencing health status and contact with health services	5.21%	0.46 (0.32)	-0.64* (0.38)	-0.51 (0.40)	0.24 (0.52)	-0.60 (0.61)	0.54 (0.73)

Note: This table presents the event-study coefficients from specification (1) but collapses the event-time dummies into bins where all the percentage-point estimates are divided by 2.67 percent to be expressed as a share of the overall ED visit probability. Column (1) shows the average outcome in $k = -1$, the period immediately before the reparations payout. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the period immediately before the reparations payout. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 1–2, 3–4, 5–6, and 7–8 semesters after reparations, respectively.
$*p < 0.1; **p < 0.05; ***p < 0.01.$
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Table A.7: Impact of Reparations on Postsecondary Enrollment

Variables	**Average in 2014s1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Semester** k=1 (4)	**2 Semesters** k=2 (5)	**3 Semesters** k=3 (6)	**4 Semesters** k=4 (7)
Bachelor's programs	2.53%	-2.17	3.28*	11.03***	12.11***	19.07***	18.32***
		(1.89)	(1.83)	(2.39)	(2.77)	(3.13)	(3.38)
– Private universities	0.90%	-5.43*	3.01	16.4***	17.84***	30.79***	32.29***
		(3.26)	(3.06)	(4.2)	(4.91)	(5.55)	(6.08)
– Public universities	1.63%	-0.36	3.42	8.05***	8.91***	12.55***	10.55***
		(2.32)	(2.28)	(2.9)	(3.34)	(3.77)	(4.07)
Associate's programs	0.75%	-7.41	-1.26	0.20	-4.78	-13.08*	-15.00**
		(4.85)	(4.68)	(5.74)	(6.58)	(7.09)	(7.5)
Any program	3.39%	-2.93	2.34	8.54***	8.00***	11.61***	10.30***
		(1.79)	(1.73)	(2.21)	(2.55)	(2.83)	(3.04)

Note: This table presents the event-study coefficients from specification (1), but includes age fixed effects and collapses the event-time dummies into bins. Column (1) shows the average outcome in 2014s1 for victims who did not receive reparations during the study period. Columns (2)–(6) report the difference in outcome between treated and control victims relative to the first semester of 2014. The effects are expressed in percentage terms relative to column (1). Column (2) shows the effects before reparations; 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 1–2, 3–4, 5–6, and 7–8 semesters after reparations, respectively. The sample includes never-treated victims.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Table A.8: Impact of Reparations on Postsecondary Enrollment (Eventually Treated)

Variables	**Average in k=-1** (1)	**Pre-event** k ∈ [-4,-2] (2)	**Immediate** k=0 (3)	**1 Semester** k=1 (4)	**2 Semesters** k=2 (5)	**3 Semesters** k=3 (6)	**4 Semesters** k=4 (7)
Bachelor's programs	3.11%	0.64	3.48*	9.73***	11.73***	17.24***	19.39***
		(2.25)	(1.91)	(3.02)	(4.19)	(5.43)	(6.6)
– Private universities	1.12%	0.08	2.34	10.93**	10.63	20.17**	22.54**
		(3.78)	(3.21)	(5.22)	(7.17)	(9.31)	(11.36)
– Public universities	1.99%	0.96	4.12*	9.05**	12.35**	15.59**	17.63**
		(2.79)	(2.39)	(3.73)	(5.18)	(6.67)	(8.11)
Associate's programs	1.27%	-2.17	-3.74	-3.47	-8.18	-16.15*	-18.05*
		(3.97)	(3.39)	(5.09)	(6.94)	(8.67)	(10.2)
Any program	4.51%	0.30	1.30	5.43**	5.13	6.64	7.23
		(1.95)	(1.64)	(2.56)	(3.53)	(4.53)	(5.46)

Note: This table reproduces the estimates from table A.7 but excludes never-treated individuals from the estimation sample.
$*p < 0.1; **p < 0.05; ***p < 0.01.$
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Table A.9: Effect of Reparations 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
Counterfactual mean	17.67	0.19	0.16	0.09	0.04	0.02	0.01
Counterfactual SD	(1.38)	(0.39)	(0.36)	(0.29)	(0.2)	(0.14)	(0.12)
Municipality FE	Y	Y	Y	Y	Y	Y	Y
Reparations-year FE	Y	Y	Y	Y	Y	Y	Y
Year-of-birth 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

Note: This table presents the effect of reparations on the age at which a 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 specification (2). The outcome is the average age in column (1). Columns (2)–(7) report the effects using dummies for different age bins. We also include the following individual and household SISBEN controls: year of SISBEN survey, SISBEN wealth score, household income, household size, female dummy, attending school dummy, attending a public school dummy, living in an urban area dummy, household homeownership status, and indicators for health care regime, female head of household, living in an apartment or household, access to electricity, access to sewerage, access to gas, owns a phone, owns a refrigerator, owns a TV, and has cable. The sample is restricted to children aged 10–15 in 2010 according to SISBEN whose households received reparations when they were aged 13–19.

$*p < 0.1; **p < 0.05; ***p < 0.01.$

Source: Authors' calculation using data from the SRNI, SISBEN, and ICFES.

Table A.10: Impact of Reparations on High School Graduation and Test Scores—Excluding 16–17

	Takes Saber 11 exam	Conditional on taking Saber 11 exam						
		Age	Test score					
			Total		Math		Reading	
			Percentile	SD	Percentile	SD	Percentile	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficient	0.014	-0.288***	1.933**	0.059*	1.889*	0.053*	1.324	0.041
Standard error	(0.01)	(0.04)	(0.95)	(0.03)	(0.98)	(0.03)	(1.00)	(0.03)
Observations	31,416	16,012	16,011		16,011		16,011	
Counterfactual mean	0.51	17.68	41.47	-0.32	40.77	-0.27	40.47	-0.27
Counterfactual SD	(0.5)	(1.4)	(26.32)	(0.83)	(26.67)	(0.86)	(26.74)	(0.9)
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Reparations-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-of-birth 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

Note: This table presents the effect of reparations on high school graduation and performance in Colombia's national standardized high school exit exam, reporting the β coefficient and robust standard errors using specification (2). The outcome is the likelihood of a child's taking the Saber 11 exam. For those who take Saber 11, the outcome is the age when the child took the exam in column (2), the test score percentile in columns (3), (5), and (7), and the standardized test score using the standard deviation of the entire population of test-takers in columns (4), (6), and (8). We also include the following individual and household SISBEN controls: year of SISBEN survey, SISBEN wealth score, household income, household size, female dummy, attending school dummy, attending a public school dummy, living in an urban area dummy, household homeownership status, and indicators for health care regime, female head of household, living in an apartment or household, access to electricity, access to sewerage, access to gas, owns a phone, owns a refrigerator, owns a TV, and has cable. The sample is restricted to children aged 10–15 in 2010 whose households received reparations when the child was aged 13–19, excluding the 16-year-old population, which is partially treated.
$*p < 0.1; **p < 0.05; ***p < 0.01.$
Source: Authors' calculation using data from the SRNI, SISBEN, and ICFES.

Table A.11: Impact of Reparations on Total Test Scores—Robustness

	Total score percentile						
	OLS 1	OLS 2	OLS 3	OLS 4	OLS 5	OLS 6	*Nearest neighbor matching estimation*
Coefficient	1.25**	1.33**	1.91**	1.61*	1.62*	2.37**	3.31***
Standard error	(0.38)	(0.39)	(0.43)	(0.68)	(0.68)	(0.66)	(0.78)
Observations				20,238			
Counterfactual mean				0.01			
Counterfactual SD				(0.12)			
Municipality FE		Y	Y	Y	Y	Y	
Reparations-year FE			Y	Y	Y	Y	
Year-of-birth FE				Y	Y	Y	
Victimization-type FE					Y	Y	Y
Controls SISBEN (2010)						Y	Y
Exact-match variables							
Municipality							Y
Reparations year							Y
Year of birth							Y
Education by 2010							Y

Note: This table presents the effect of reparations on high school graduation and performance in Colombia's national standardized high school exit exam, reporting the β coefficient and robust standard errors using specification (2). The outcome is the test score percentile. We also include the following individual and household SISBEN controls: year of SISBEN survey, SISBEN wealth score, household income, household size, female dummy, attending school dummy, attending a public school dummy, living in an urban area dummy, household homeownership status, and indicators for health care regime, female head of household, living in an apartment or household, access to electricity, access to sewerage, access to gas, owns a phone, owns a refrigerator, owns a TV, and has cable. The sample is restricted to children aged 10–15 in 2010 whose households received reparations when the child was aged 13–19, excluding the 16-year-old population, which is partially treated.
$*p < 0.1; **p < 0.05; ***p < 0.01$.
Source: Authors' calculation using data from the SRNI, SISBEN, and ICFES.

Table A.12: Effect of Reparations on High School Characteristics

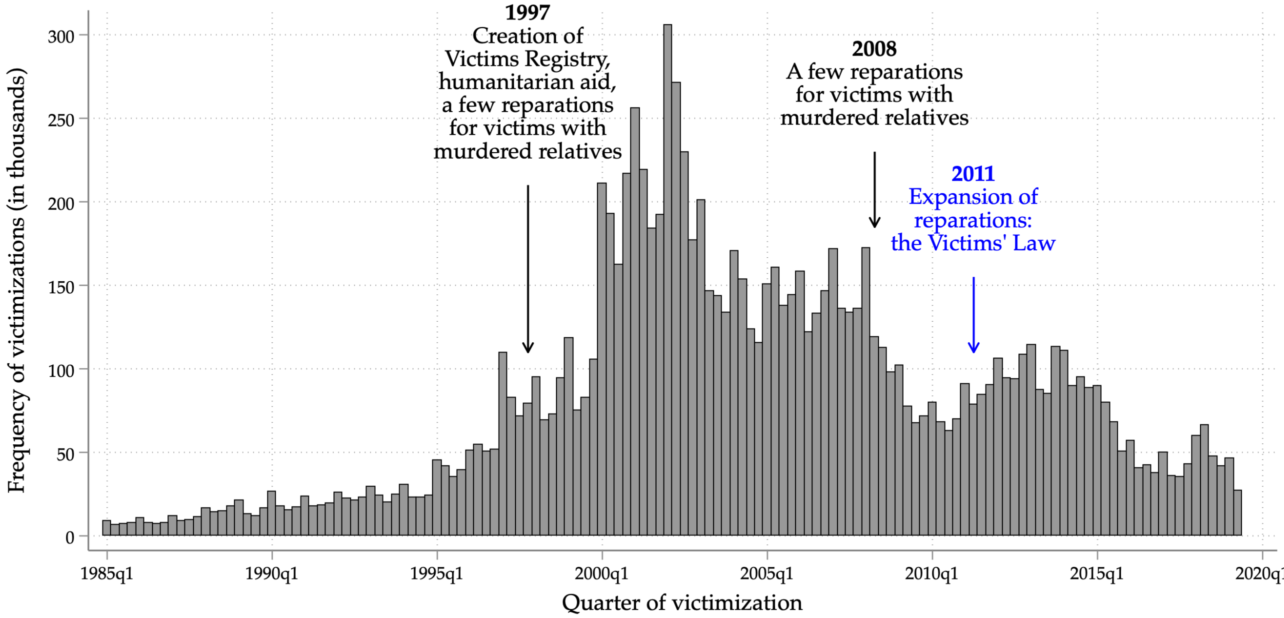
	Calendar A HS	Migration	Socioecon. stratum 1	Socioecon. stratum 2	Socioecon. stratum >2	Private HS	HS cohort size	HS LOM Total percentile	HS LOM Math percentile	HS LOM Reading percentile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Coefficient	0.008	-0.001	0.000	0.001	0.000	-0.012	2.172	-0.276	-0.277	-0.277
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
Counterfactual mean	0.94	0.18	0.62	0.25	0.04	0.12	110.54	12.09	12.13	12.12
Counterfactual SD	(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
Reparations-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

Note: This table presents the effect of reparations on the age at which a 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 specification (2). The outcome is the average age in column (1). We also include the following individual and household SISBEN controls: year of SISBEN survey, SISBEN wealth score, household income, household size, female dummy, attending school dummy, attending a public school dummy, living in an urban area dummy, household homeownership status, and indicators for health care regime, female head of household, living in an apartment or household, access to electricity, access to sewerage, access to gas, owns a phone, owns a refrigerator, owns a TV, and has cable. The sample is restricted to children aged 10–15 in 2010 according to SISBEN whose households received reparations when they were aged 13–19.

$*p < 0.1; **p < 0.05; ***p < 0.01$.

Source: Authors' calculation using data from the SRNI, SISBEN, and ICFES.

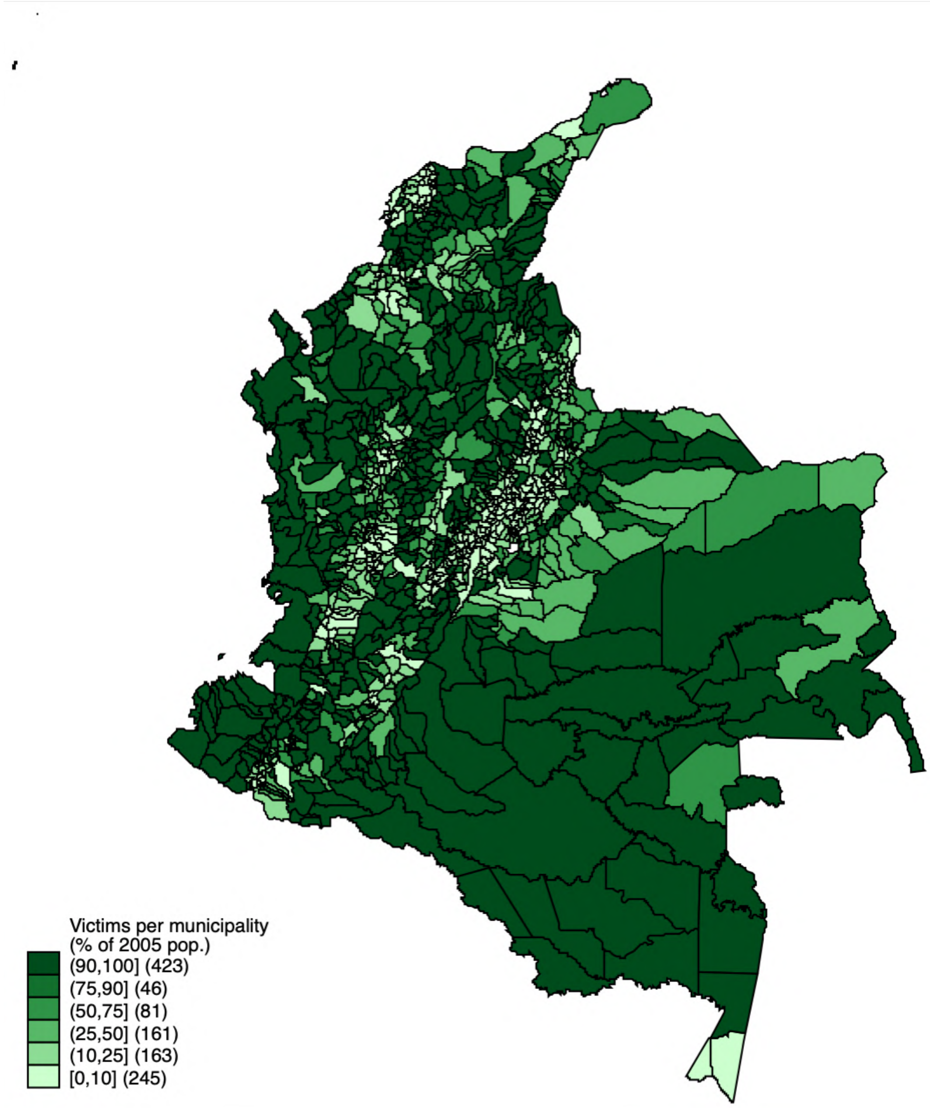
Figure A.1: Frequency of Victimizations between 1985 and 2019



Note: This figure plots the frequency of victimizations from the Colombian internal armed conflict by the quarter in which the victimization occurred. 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 II.

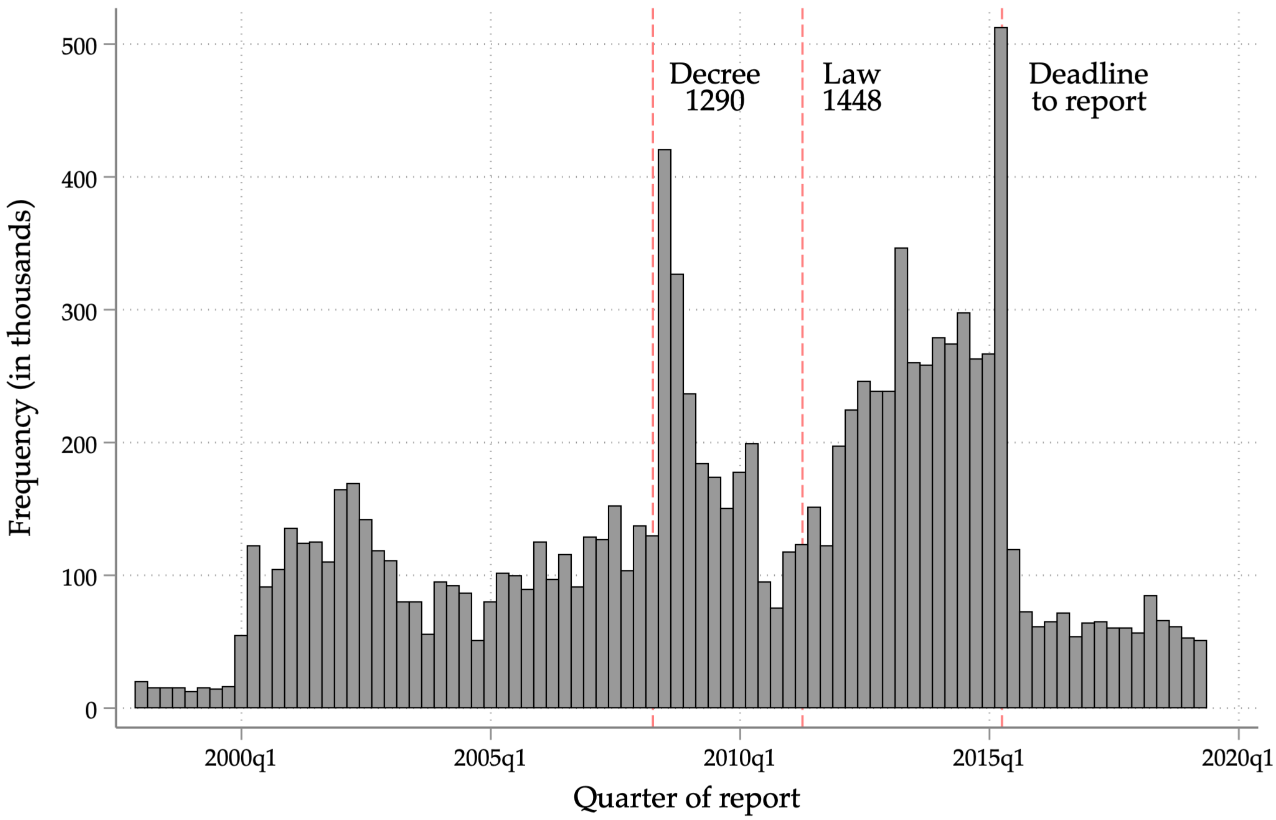
Source: Authors' calculation using RUV data from the SRNI.

Figure A.2: Number of Victims by Location



Note: This figure plots the total number of victims by geographic location, expressed as a share of the 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 the SRNI and SISBEN.

Figure A.3: Victimization Self-Reporting by Date of Report

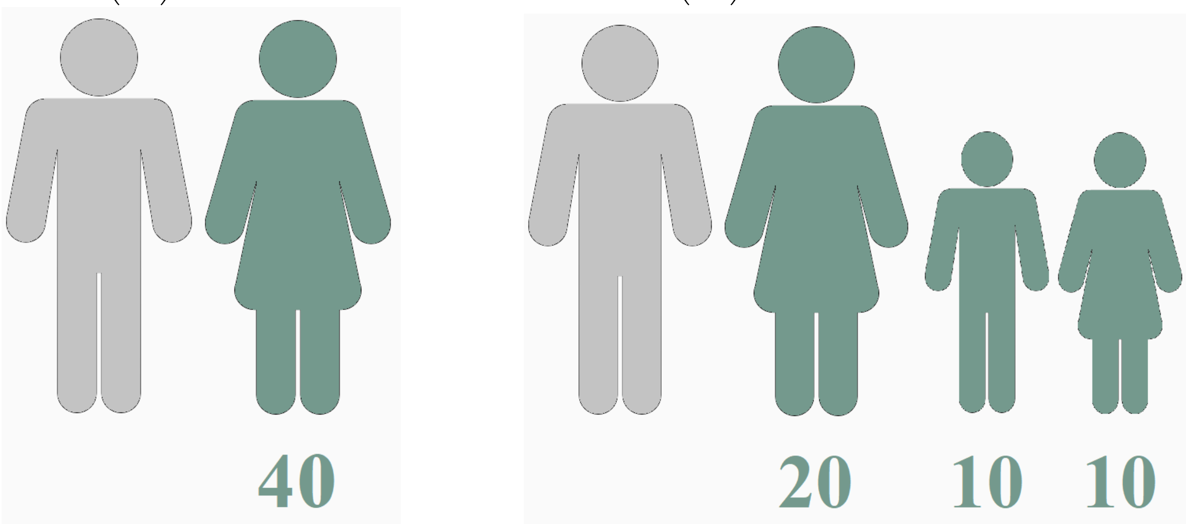


Note: This figure plots the frequency of victimization self-reporting by the quarter of the report. The evolution of reported victimizations is influenced by (i) the definition of who is a victim, (ii) the length of time victims have to report the victimization after it occurred, and (iii) the requirement that people report their victimization to be eligible for reparations. In particular, Decree 1290/2008 established that, to be eligible for reparations, the victimization must have taken place before April 22, 2008, and have been 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 reparations. All of this led to an increase in the number of reported victimizations after the adoption of Law 1448/2011.
Source: Authors' calculation using RUV data from the SRNI.

Figure A.4: Examples of Reparations Distribution

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

(a.1): Household without children (a.2): Household with children

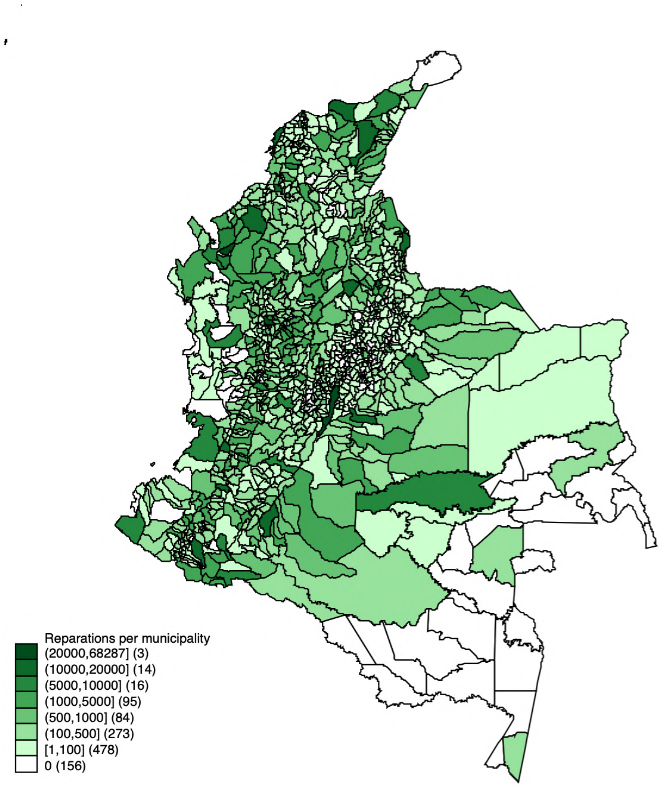


(b) Forcibly displaced household (27 minimum wages)

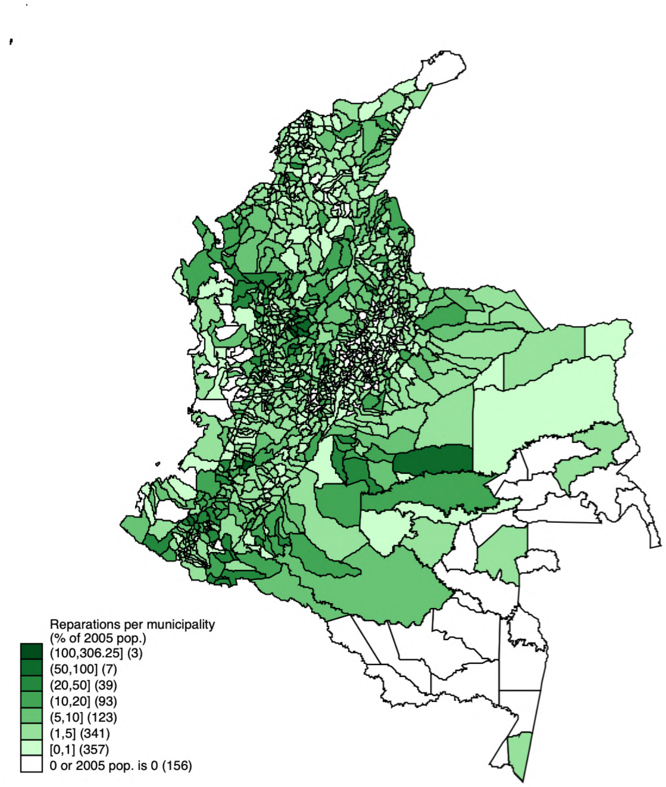
Note: This figure illustrates several examples of the intra-household distribution of victim reparations, in multiples of the legal monthly minimum wage. Panel (a) presents two examples of how the 40 minimum wages awarded to victims of homicide or forced disappearance could be distributed, depending on household composition. If the murdered or disappeared victim does not have children, 100 percent of the reparations payment is awarded to the spouse or civil partner (a.1). If, instead, the murdered or disappeared victim has two children, 50 percent of the reparations payment is awarded to the spouse or civil partner, and the remainder is split equally among the children (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.)

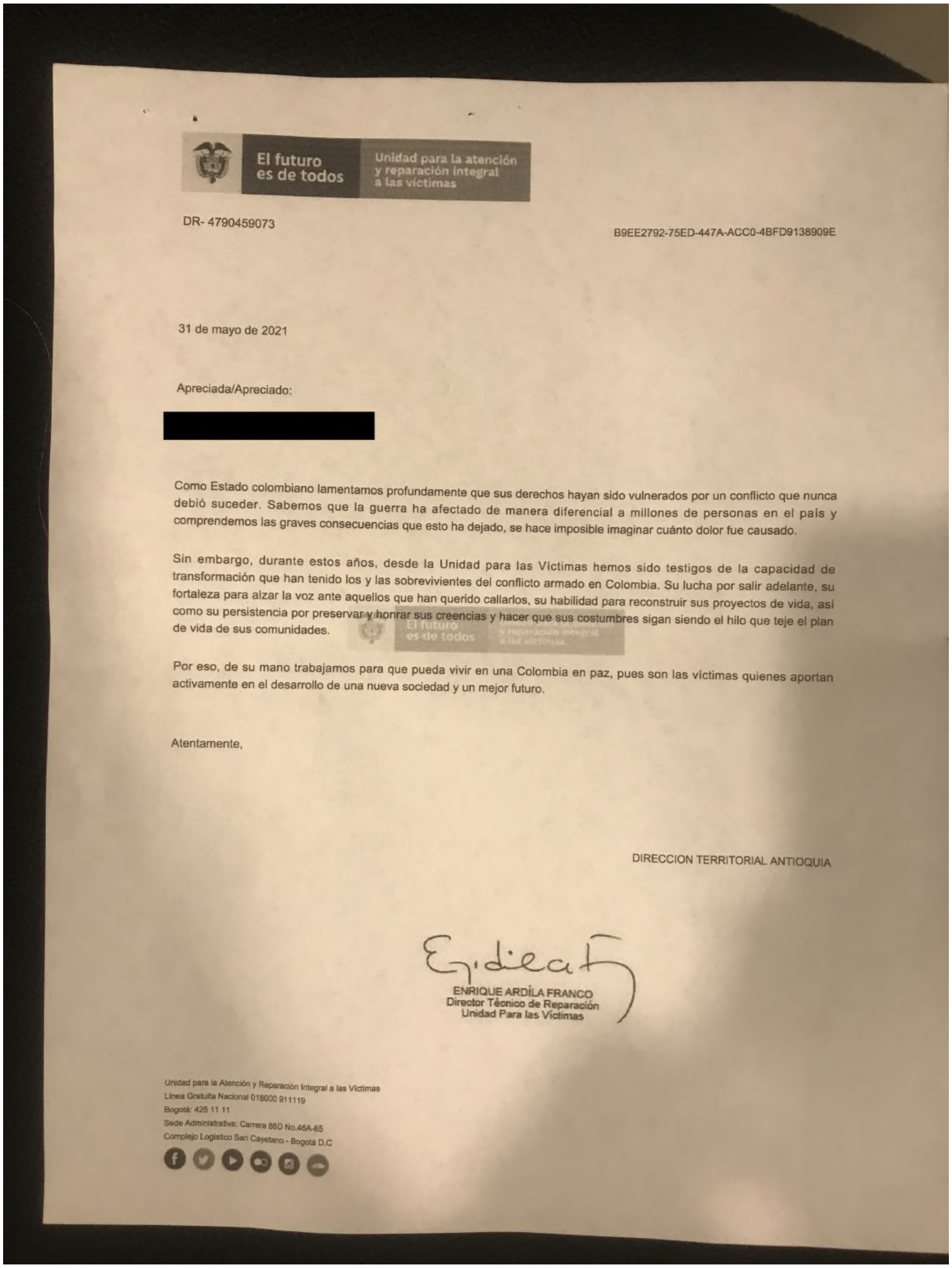


Note: This figure plots reparations by municipality. There are around 1,120 municipalities in Colombia. Panel (a) presents the frequency of reparations and shows most municipalities received at least one reparations payment by June 2019. Panel (b) expresses this number as a share of the 2005 population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

Figure A.6: Victims Are Informed They Will Receive Reparations

Note: This picture was taken in one of the victim reparations meetings between the UARIV and beneficiaries in Medellin, Colombia. The UARIV informs victims they will receive a reparations check in the following days.
Source: Photo taken by Arlen Guarin.

Figure A.7: Dignifying Letter



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

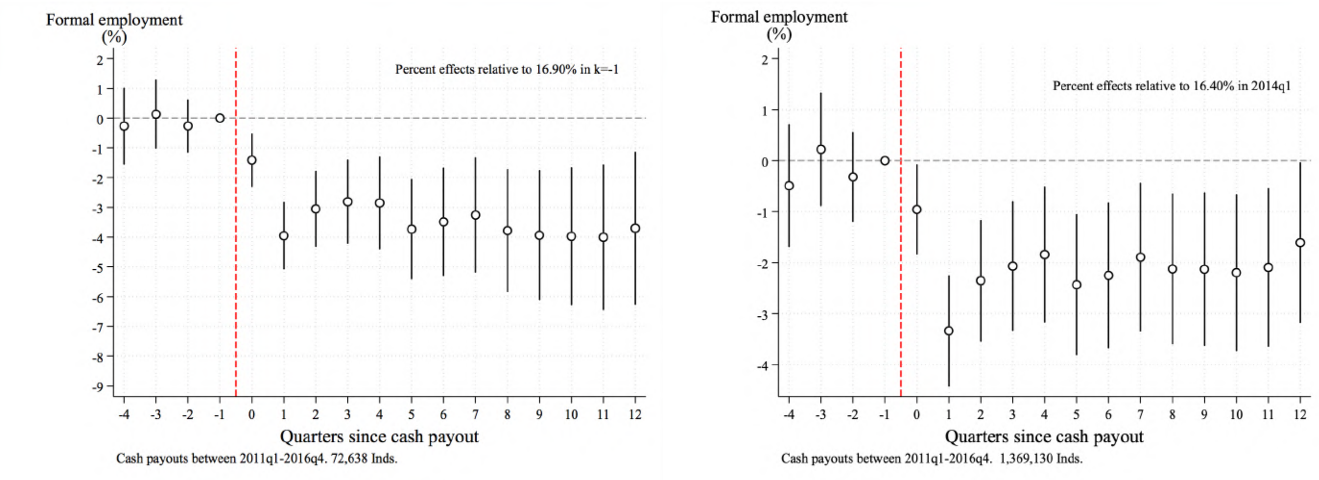
ENRIQUE ARDÍLA FRANCO
Director Técnico de Reparación
Unidad Para las Víctimas

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

Note: This picture was taken in one of the victim reparations meetings between the UARIV and beneficiaries in Medellin, Colombia.
Source: Photo taken by Arlen Guarin.

Figure A.8: Formal Employment: Robustness

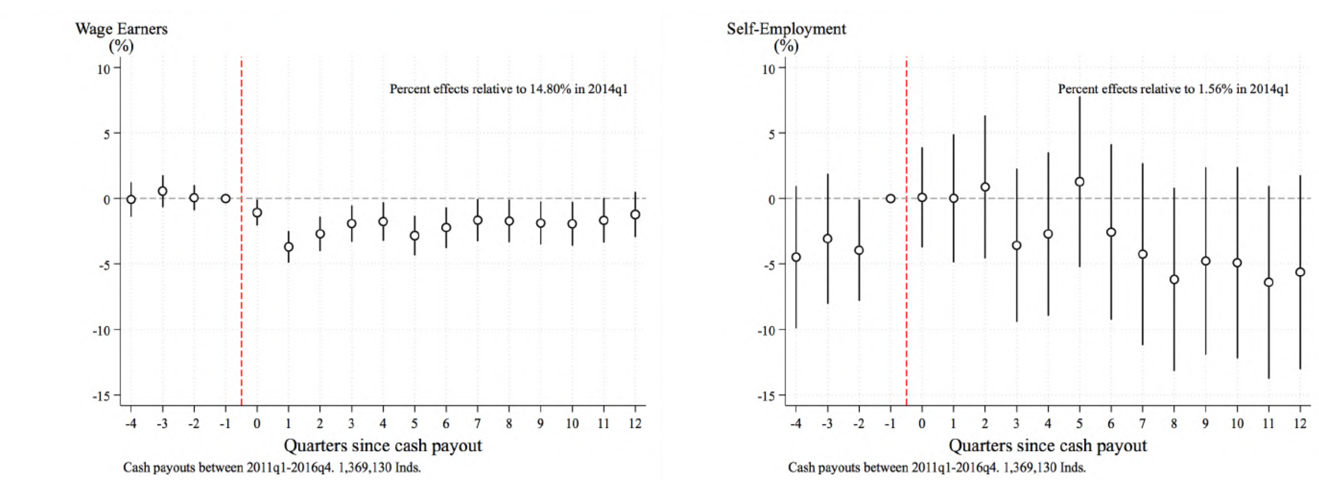
(a) Excluding never-treated victims (b) Callaway and Sant'Anna (2021) estimator



Note: This figure shows that the results on formal employment from figure III are robust. Panel (a) excludes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Figure A.9: Formal Wage Employment versus Self-Employment

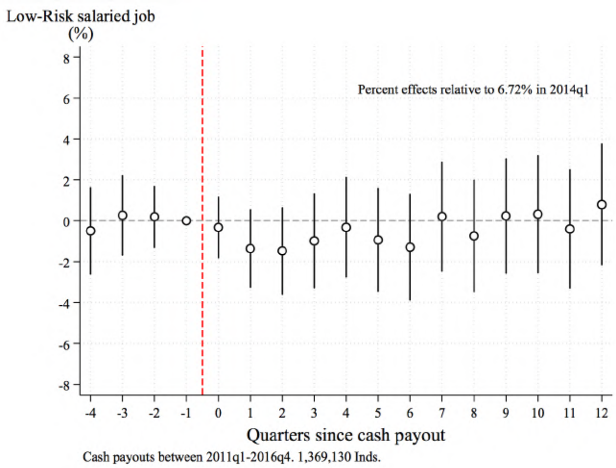
(a) Wage employment (b) Self-Employment



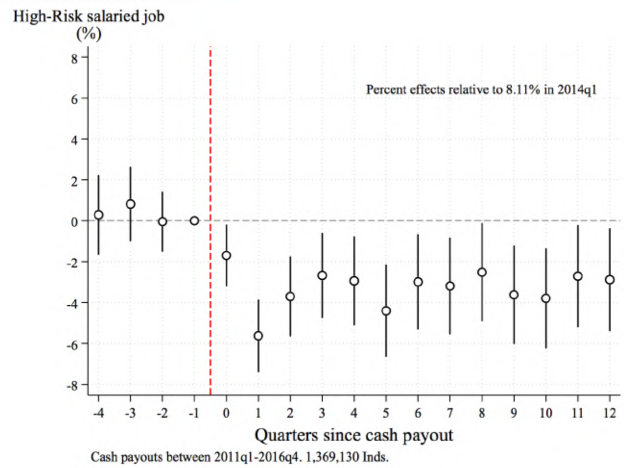
Note: These figures decompose the negative employment result from figure III between wage and self-employment in panels (a) and (b), respectively. The employment drop is driven by wage employment, which collapses the quarter after the reparations payout and remains constant and significant even ten quarters afterward. Figure A.10 further decomposes the wage-employment effect by job risk and pay.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Figure A.10: Wage Employment by Job Risk and Pay

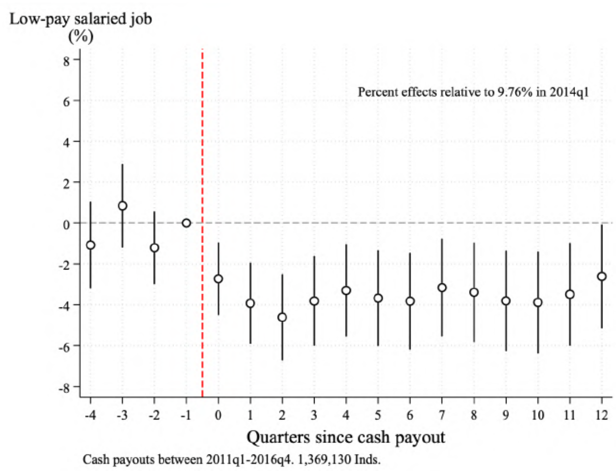
(a) Low-risk



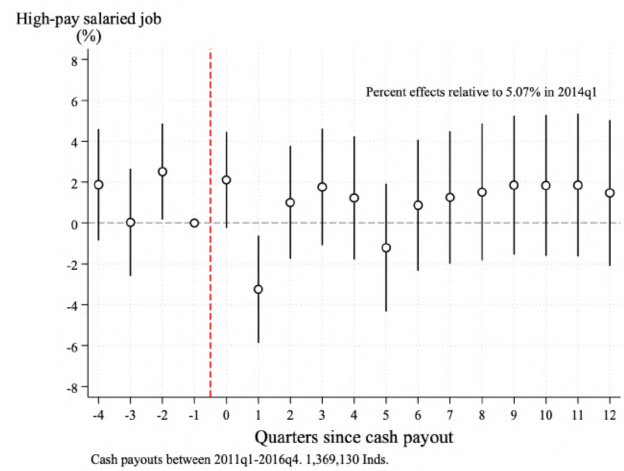
(b) High-risk



(c) Low-pay



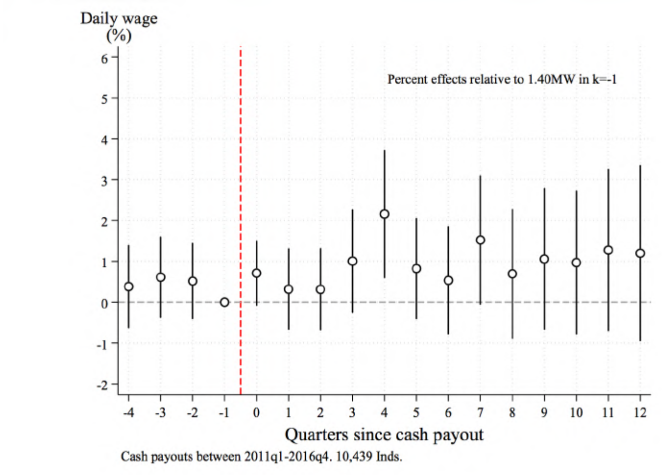
(d) High-pay



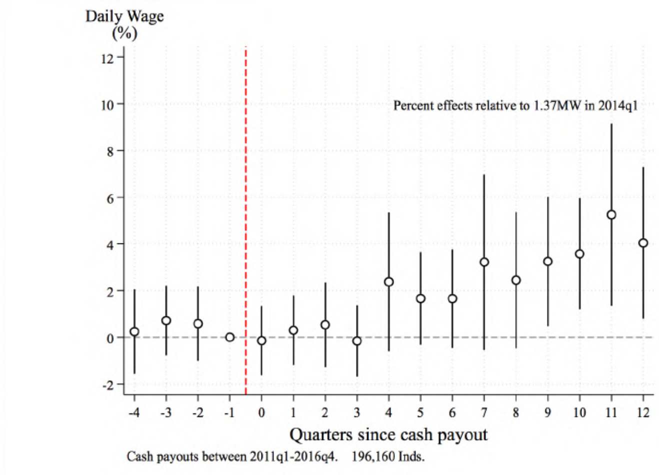
Note: These figures decompose the negative wage-employment result from panel (a) of figure A.9 between low- and high-risk salaried jobs in panels (a) and (b) and minimum- and higher-wage salaried jobs in panels (c) and (d). The risk category is based on employers' contribution rate for the workers' compensation system; occupations with an above-median contribution rate are coded as "high risk." Low- and high-pay jobs refer to those paying at or above the minimum wage, respectively. The employment drop is driven by high-risk and low-pay salaried jobs.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Figure A.11: Daily Wages: Robustness

(a) Excluding never-treated victims

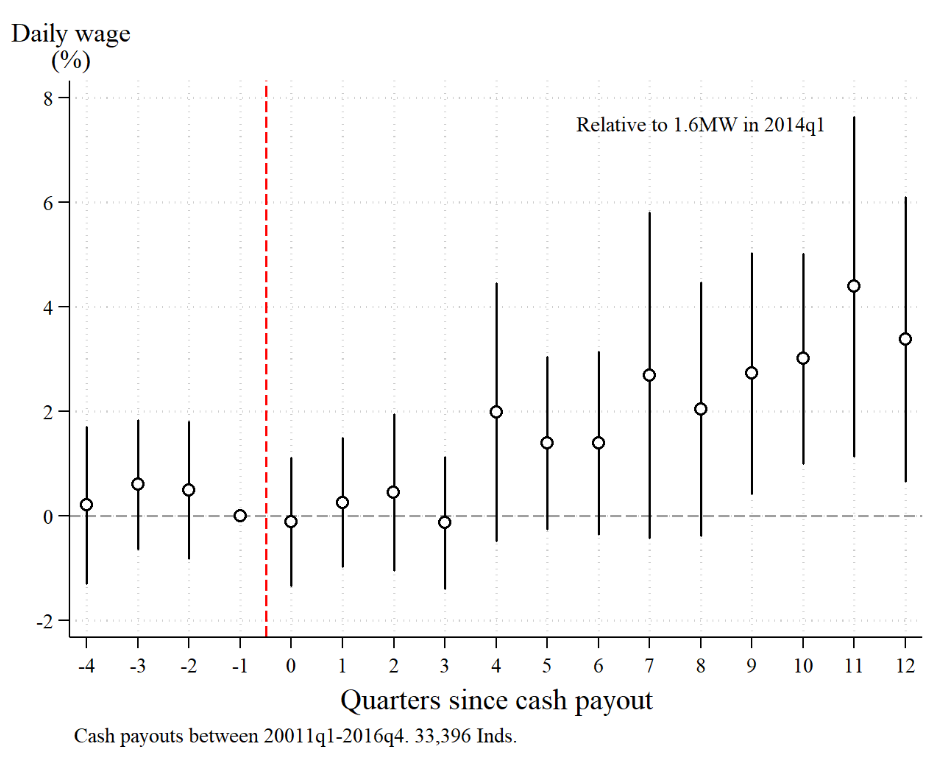


(b) Callaway and Sant'Anna (2021) estimator



Note: This figure shows that the results on daily wages from figure IV are robust. Panel (a) excludes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

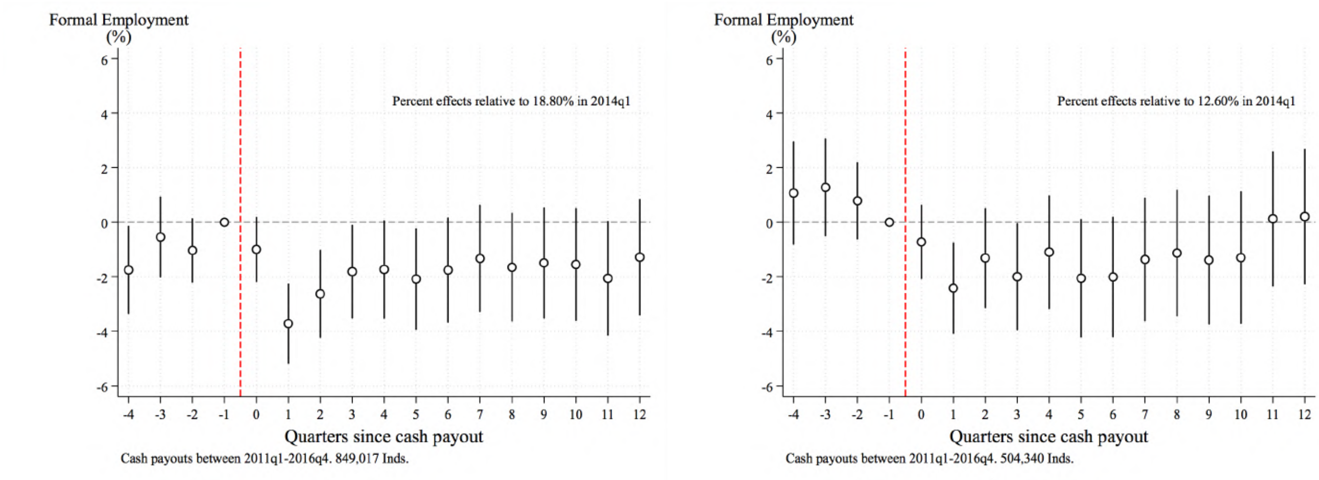
Figure A.12: Daily Wages for Workers Employed Every Quarter



Note: This figure reproduces the results from figure IV when restricting the estimation sample to individuals employed every quarter during the period of analysis.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and PILA.

Figure A.13: Employment by Age Group

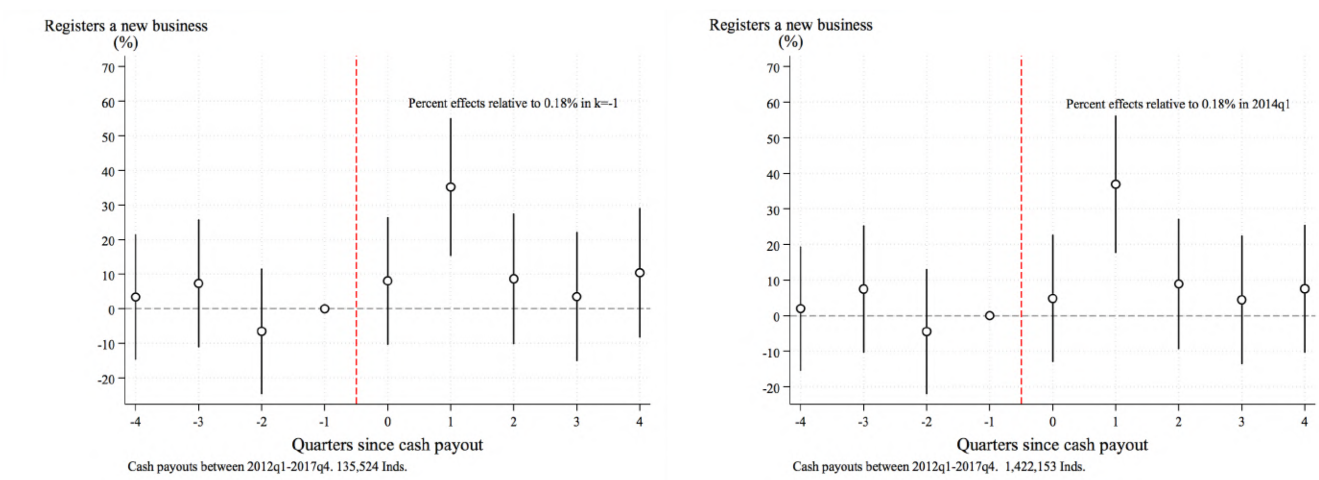
(a) Ages 18–39 (b) Ages 40–55



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

Figure A.14: Formal Business Creation: Robustness

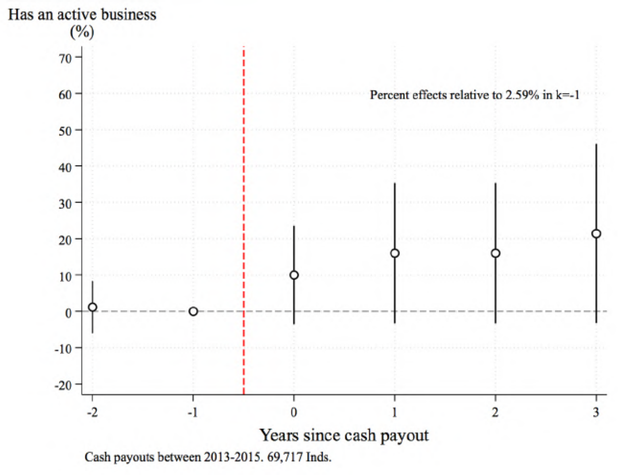
(a) Excluding never-treated victims (b) Callaway and Sant'Anna (2021) estimator



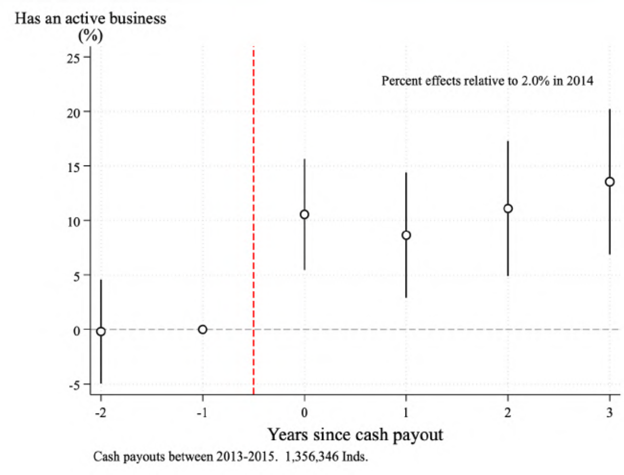
Note: This figure shows that the results on formal business creation from figure V are robust. Panel (a) excludes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and Confecámaras.

Figure A.15: Business Survival: Robustness

(a) Excluding never-treated victims



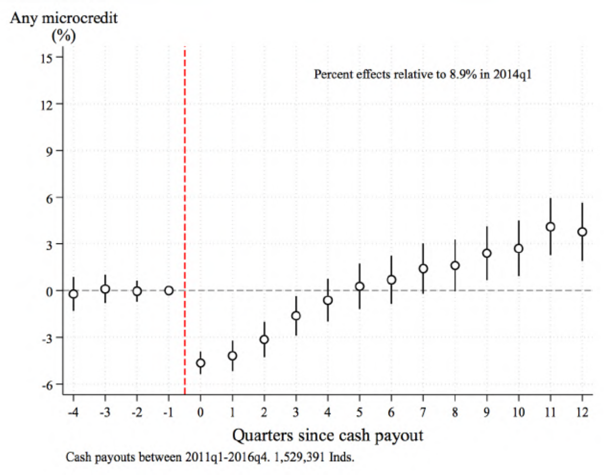
(b) Callaway and Sant'Anna (2021) estimator



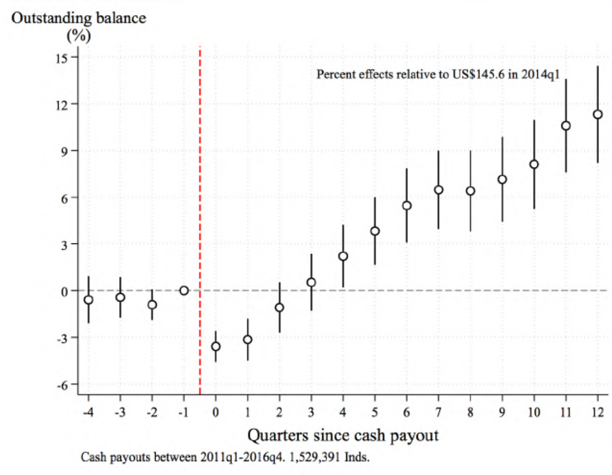
Note: This figure shows that the results on firm survival from figure VI are robust. Panel (a) excludes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and Confecámaras.

Figure A.16: Microcredit

(a) Any microcredit

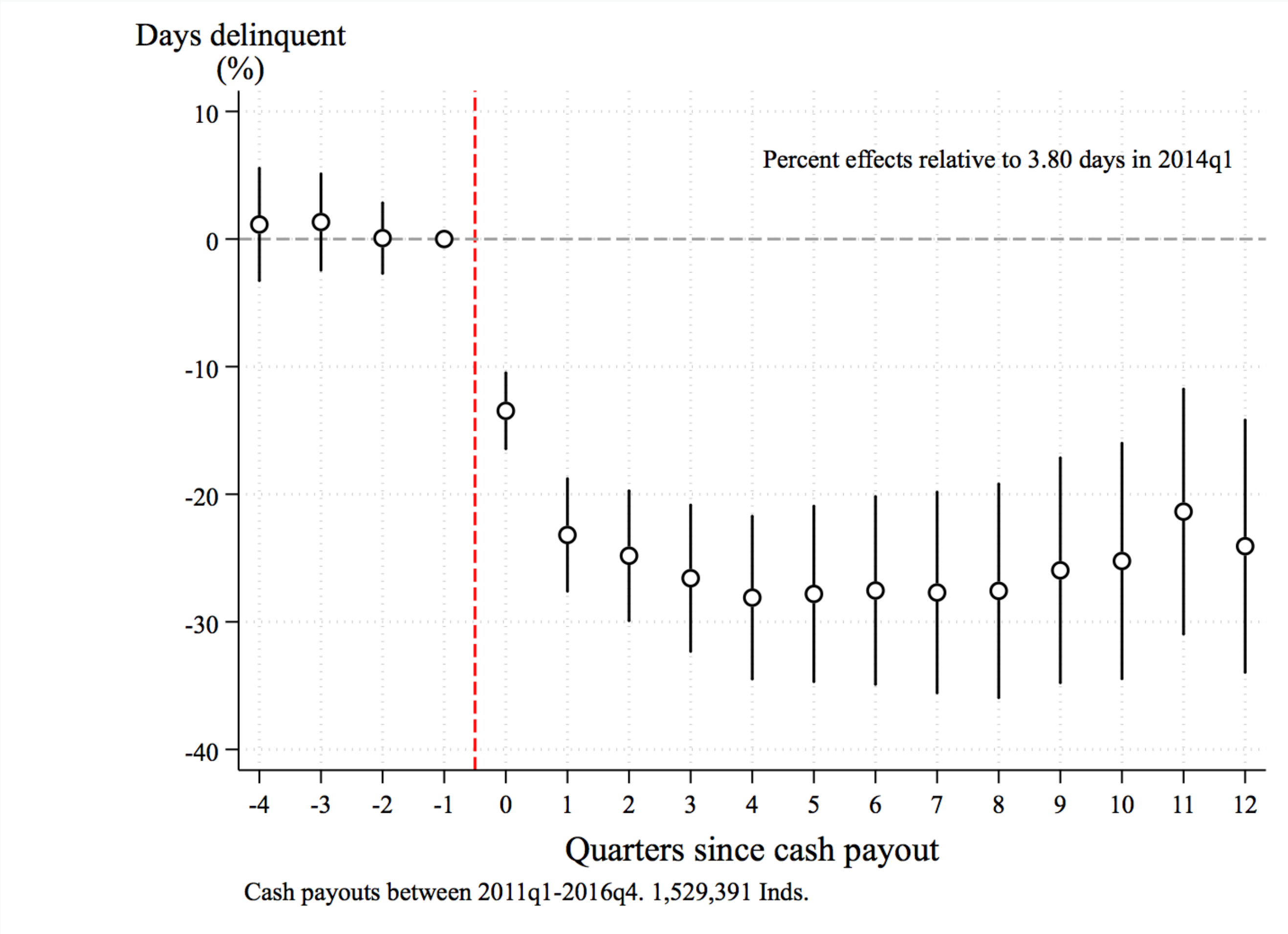


(b) Outstanding balance



Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the time from the reparations 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 the outstanding balance, measured in constant US dollars and including zeros for those who do not owe any microcredit. Panel (a) shows that reparations induce victims to pay off their microloans: the probability of owing microcredit drops in $k = 0$. This effect is short lived: three years afterward, reparations raise the likelihood of owing any microcredit. Similarly, panel (b) shows that the amount of money owed increases three years after receiving the money, suggesting reparations increase victims' intensive use of microcredit for funding 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.18 shows that these results are robust to excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

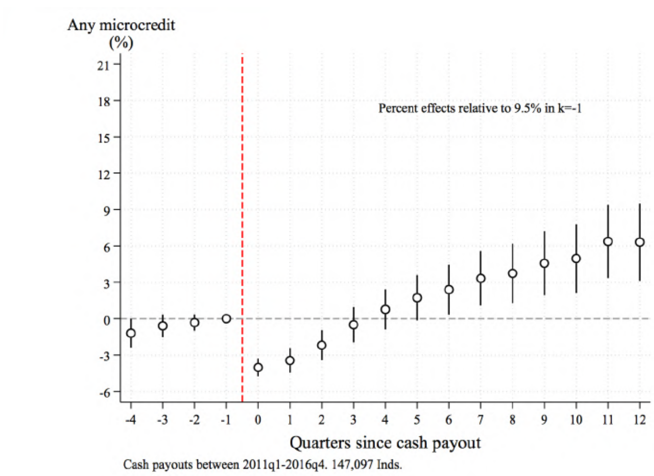
Figure A.17: Number of Days Delinquent in Microcredit



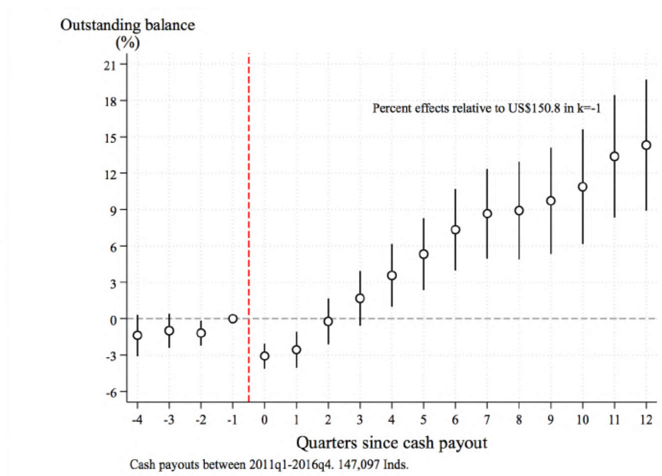
Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome—the number of days delinquent for a microloan—is plotted against the time from the reparations payout (in quarters). On average, control victims are 3.8 days delinquent on a microloan in 2014q1. Despite the more intensive use of microcredit shown in figure A.16, delinquency falls significantly and remains lower three years after reparations. 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.19 shows that these results are robust to excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.18: Microcredit: Robustness

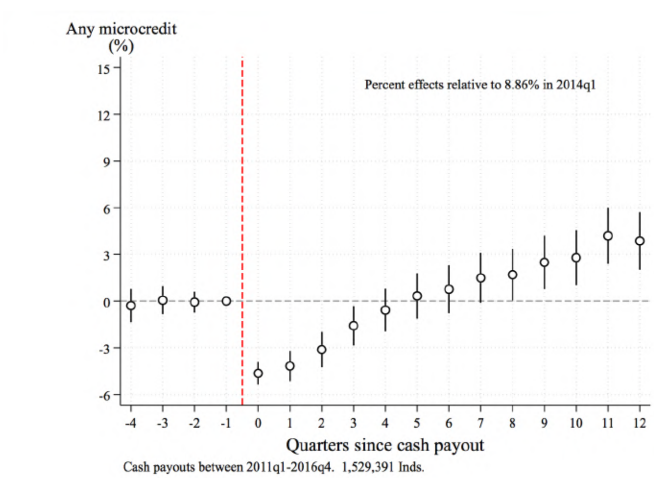
(a) Excluding never-treated victims:
Any microcredit



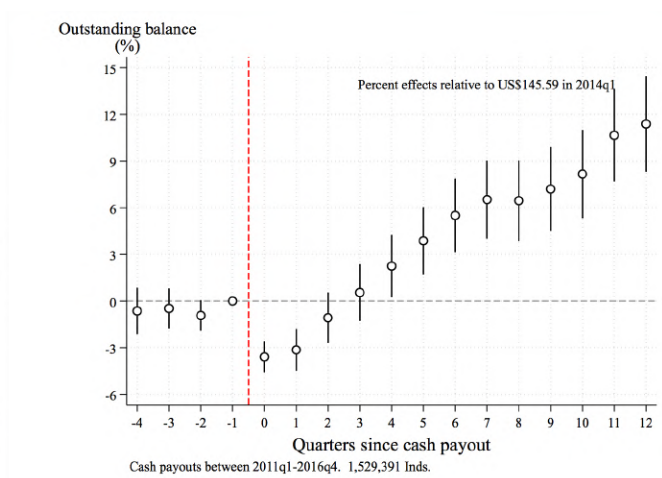
(b) Excluding never-treated victims:
Outstanding balance



(c) Callaway and Sant'Anna (2021) estimator:
Any microcredit



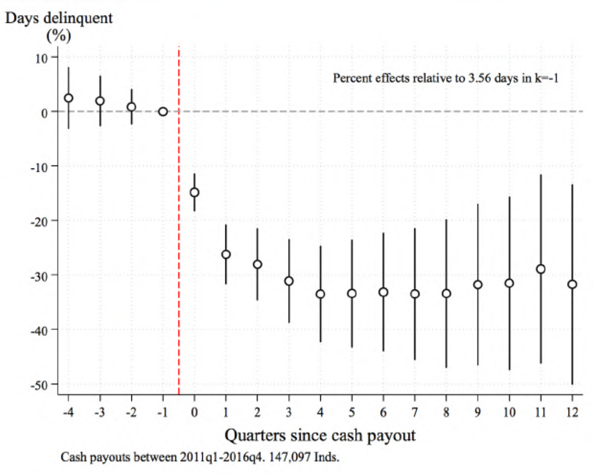
(d) Callaway and Sant'Anna (2021) estimator:
Outstanding balance



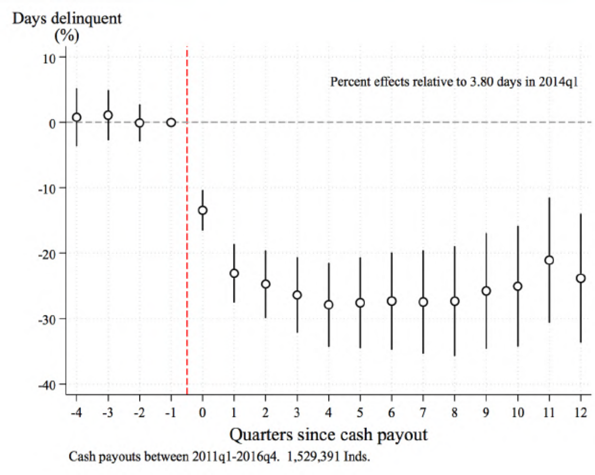
Note: This figure shows that the results on microcredit from figure A.16 are robust. Panels (a) and (b) exclude never-treated victims from the estimation sample. Panels (c) and (d) use the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.19: Number of Days Delinquent in Microcredit: Robustness

(a) Excluding never-treated victims



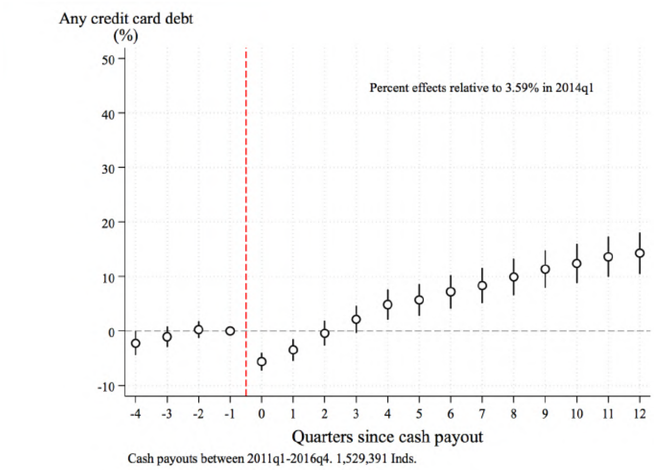
(b) Callaway and Sant'Anna (2021) estimator



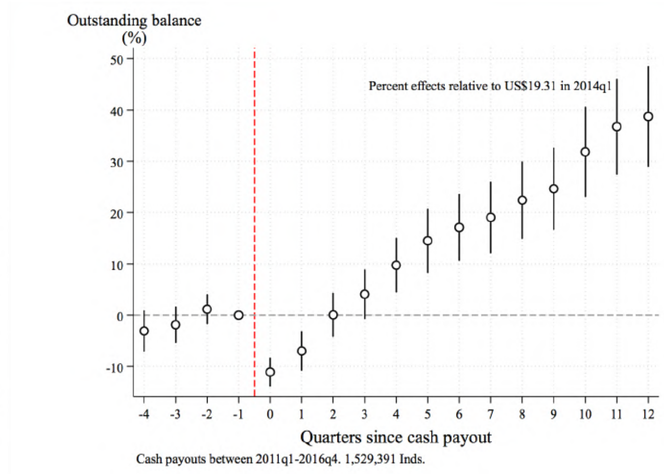
Note: This figure shows that the results on firm survival from figure A.17 are robust. Panel (a) excludes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.20: Credit Card Debt

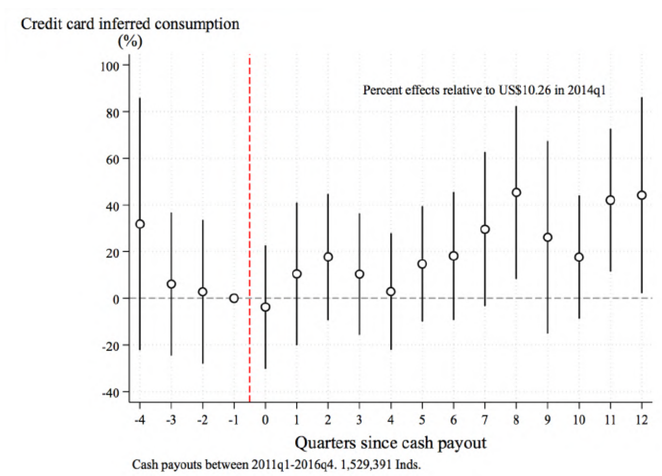
(a) Any credit card debt



(b) Outstanding balance on credit card debt



(c) Inferred consumption

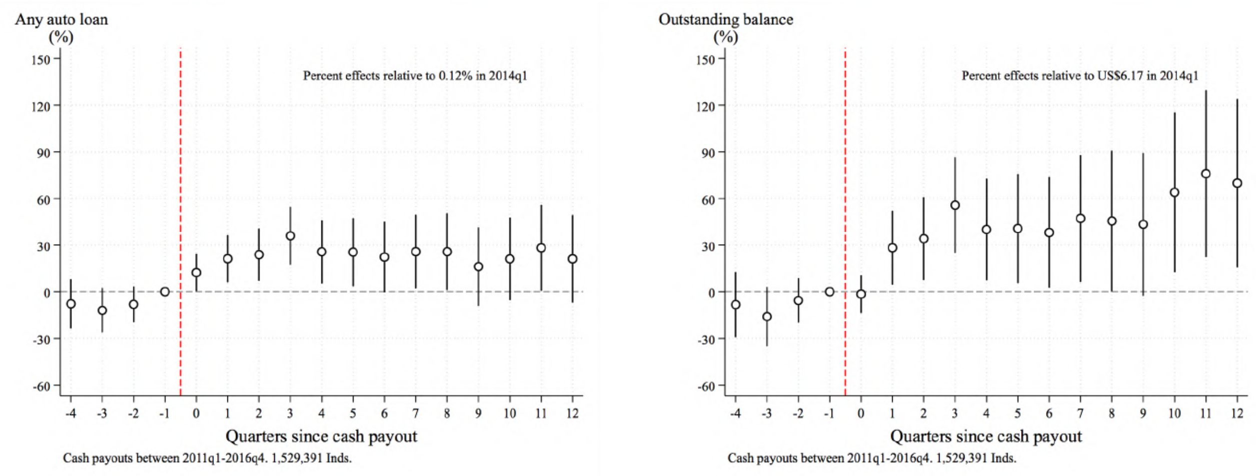


Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the time from the reparations payout (in quarters). The outcome is owing any credit card debt in panel (a) and the outstanding balance on 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 with no credit card debt. Panel (a) shows the likelihood of owing any credit card debt decreases from a base of 3.59 percent for never-treated victims in 2014q1. The coefficient increases in magnitude over time, becoming zero and non-significant two quarters after the payout and then positive and significant. Similarly, panel (b) shows outstanding credit card debt increases by nearly 40 percent. Panel (c) shows that consumption increases over time such that, three years after the reparations payout, credit card activity has increased around 40 percent. 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. Figures A.22 and A.23 show that these results are generally robust to excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.21: Auto Loans

(a) Any auto loan

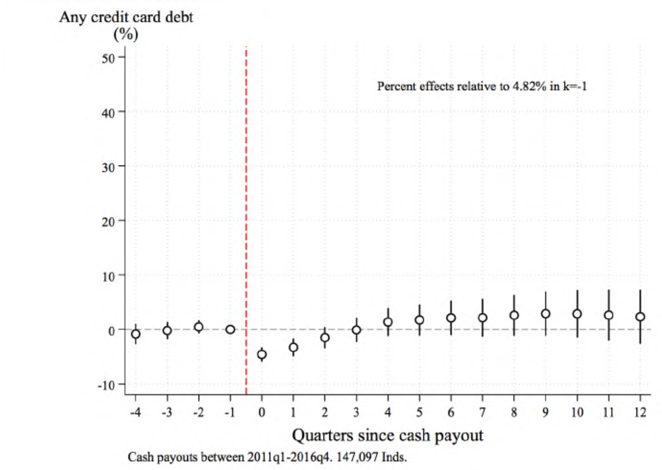
(b) Outstanding balance on auto loan debt



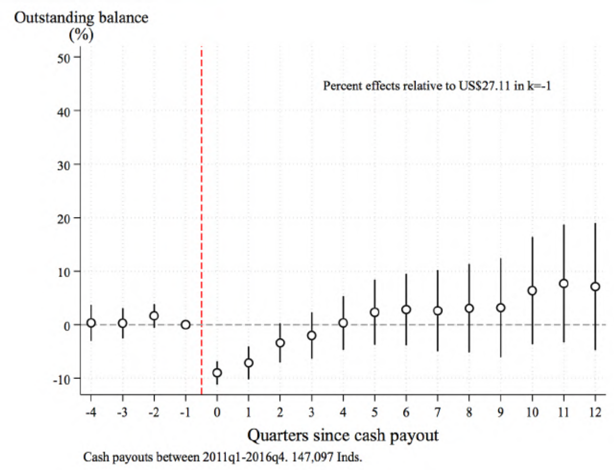
Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the time from the reparations payout (in quarters). The outcome is owing debt on any auto loan in panel (a) and the amount of outstanding balance on the auto loan debt in panel (b). The outcomes are measured in constant US dollars and include zeros for people who do not have auto loans. Auto loans increase after reparations such that, three years after the reparations payout, victims have over 60 percent more auto loan debt. 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.24 shows that these results are robust to excluding never-treated individuals and using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.22: Credit Card Debt: Robustness to Excluding Never-Treated Victims

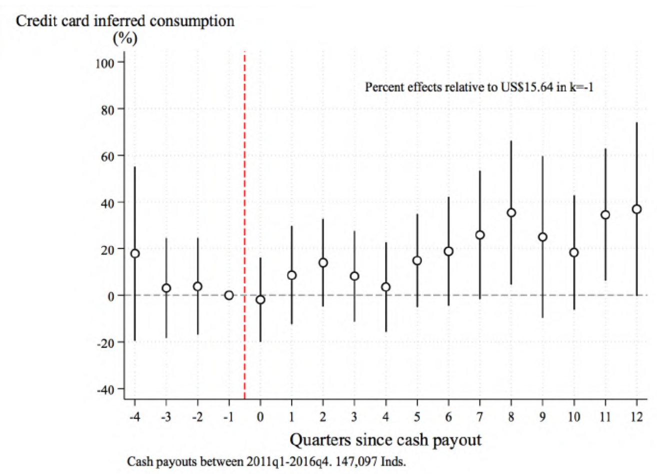
(a) Any credit card debt



(b) Outstanding balance on credit card debt



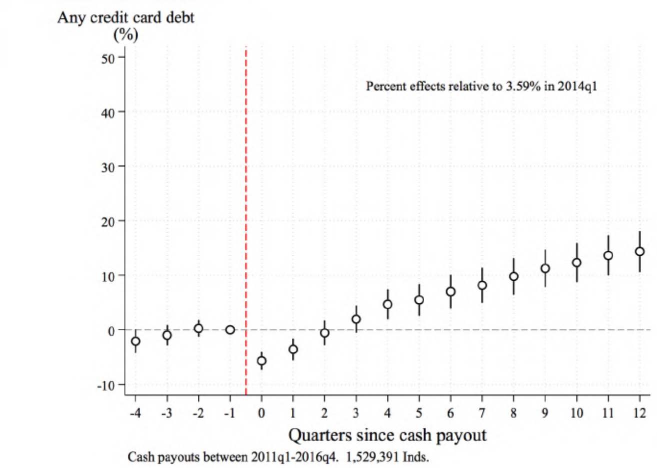
(c) Inferred consumption



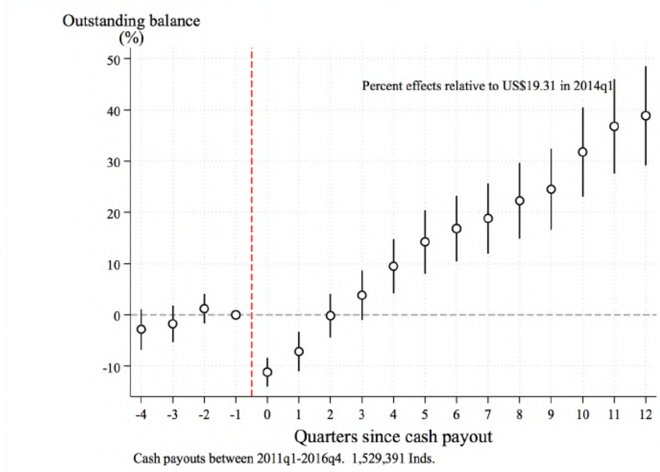
Note: This figure shows that the results on credit card debt from figure A.20 are robust to excluding never-treated victims from the estimation sample.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.23: Credit Card Debt: Robustness to Using Callaway and Sant'Anna (2021) Estimator

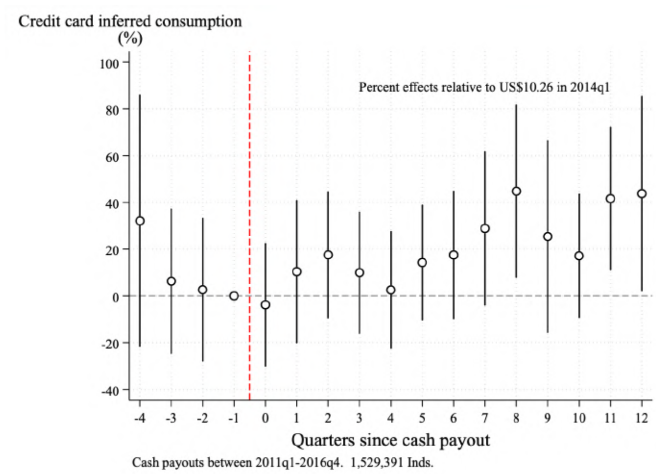
(a) Any credit card debt



(b) Outstanding balance on credit card debt



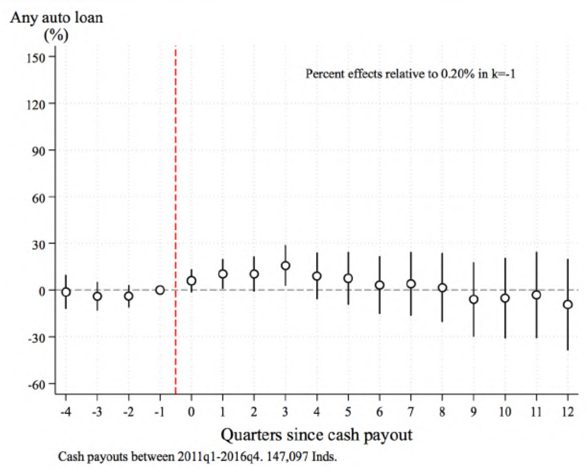
(c) Inferred consumption



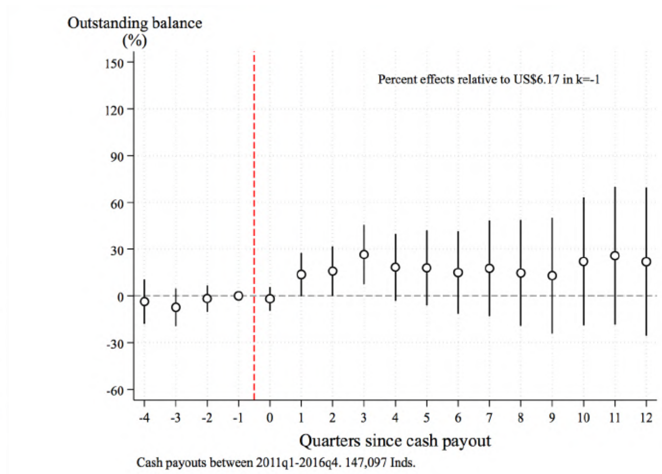
Note: This figure shows that the results on credit card debt from figure A.20 are robust to using the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.24: Auto Loans: Robustness

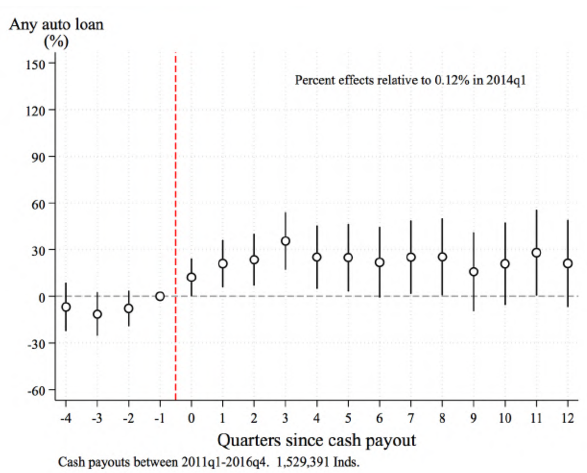
(a) Excluding never-treated victims: Any auto loan



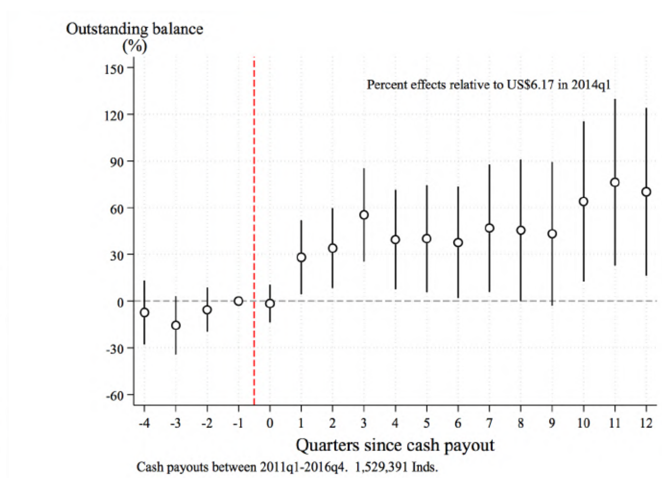
(b) Excluding never-treated victims: Outstanding balance



(c) Callaway and Sant'Anna (2021) estimator: Any auto loan



(d) Callaway and Sant'Anna (2021) estimator: Outstanding balance

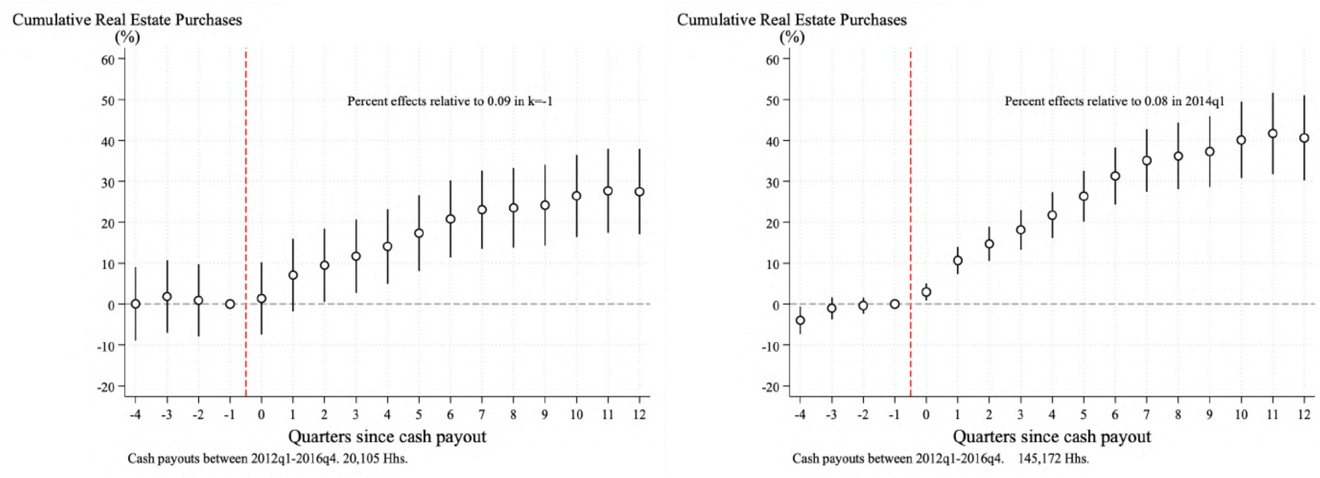


Note: This figure shows that the results on auto loans from figure A.21 are robust. Panels (a) and (b) exclude never-treated victims from the estimation sample. Panels (c) and (d) use the Callaway and Sant'Anna (2021) estimator. *Source:* Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.25: Cumulative Real Estate Purchases: Robustness

(a) Excluding never-treated victims

(b) Callaway and Sant'Anna (2021) estimator

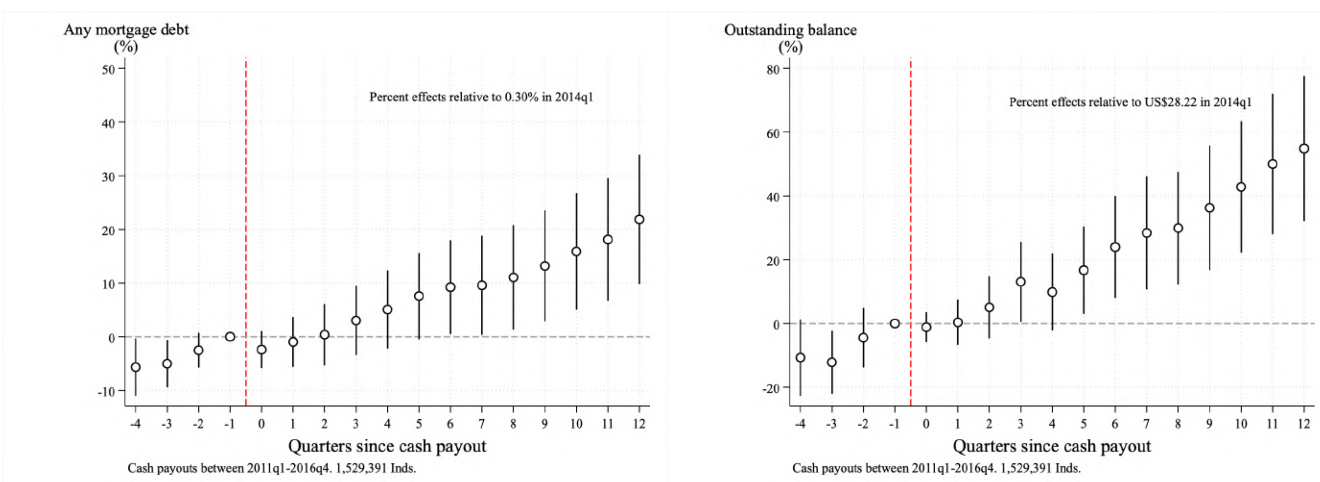


Note: This figure shows that the results on cumulative real estate purchases from figure VII are robust. Panel (a) excludes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Catastro Antioquia.

Figure A.26: Mortgage Loans

(a) Any mortgage loan

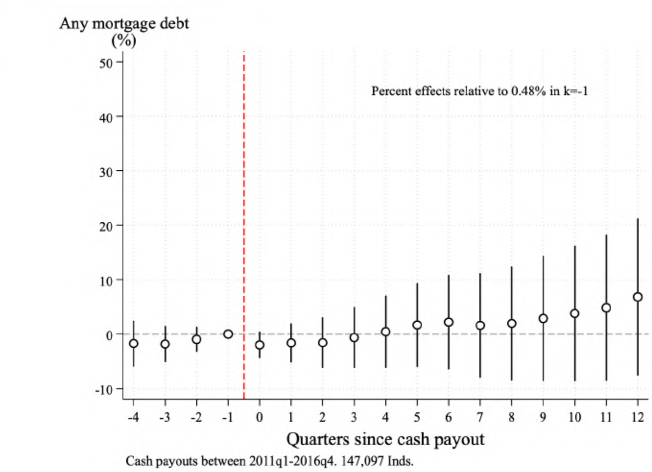
(b) Outstanding balance



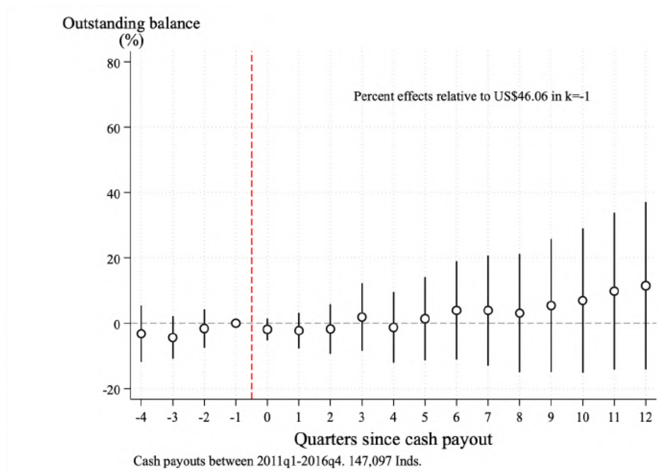
Note: This figure presents the event-study coefficients and associated 95 percent confidence intervals using specification (1), where the outcome is plotted against the time from the reparations payout (in quarters). The outcome is owing any mortgage loan in panel (a) and the amount of outstanding balance on mortgage loans in panel (b). The outcomes are measured in constant US dollars and include zeros for people who do not have mortgage loans. Mortgage loans increase after reparations such that, three years after the reparations payout, victims have over 60 percent more mortgage loan debt. 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 the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.27: Mortgage Loans: Robustness

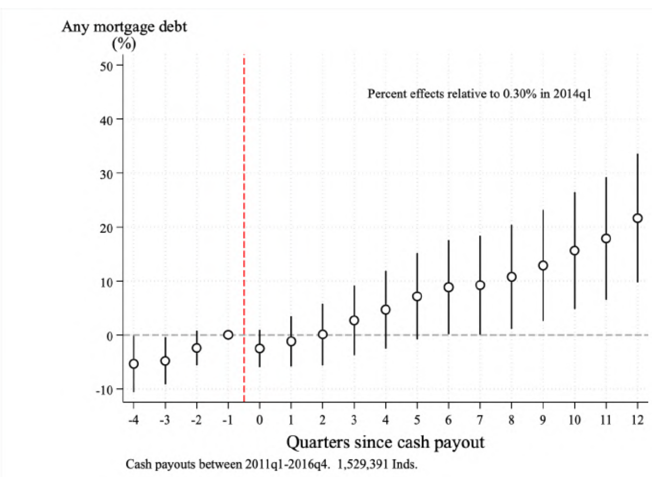
(a) Excluding never-treated victims:
Any mortgage loan



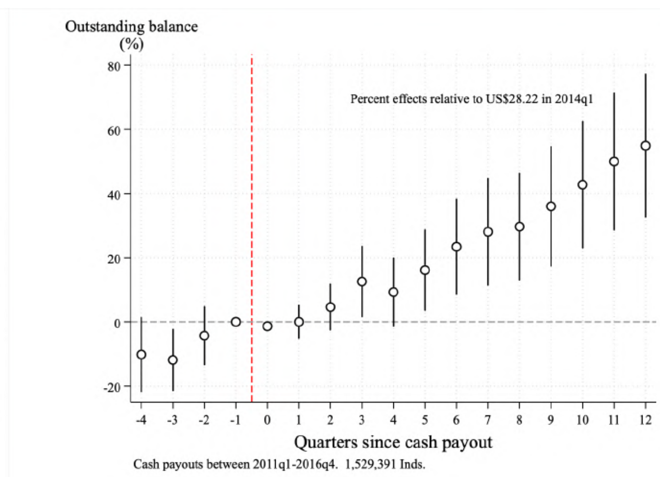
(b) Excluding never-treated victims:
Outstanding balance



(c) Callaway and Sant'Anna (2021) estimator:
Any mortgage loan



(d) Callaway and Sant'Anna (2021) estimator:
Outstanding balance

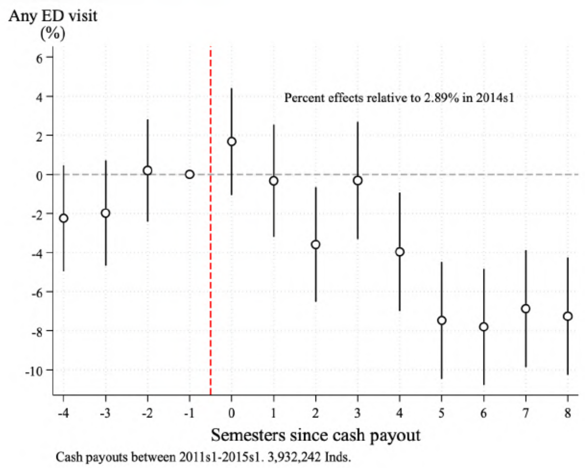


Note: This figure shows that the results on mortgage loans from figure A.26 are robust. Panels (a) and (b) exclude never-treated victims from the estimation sample. Panels (c) and (d) use the Callaway and Sant'Anna (2021) estimator.

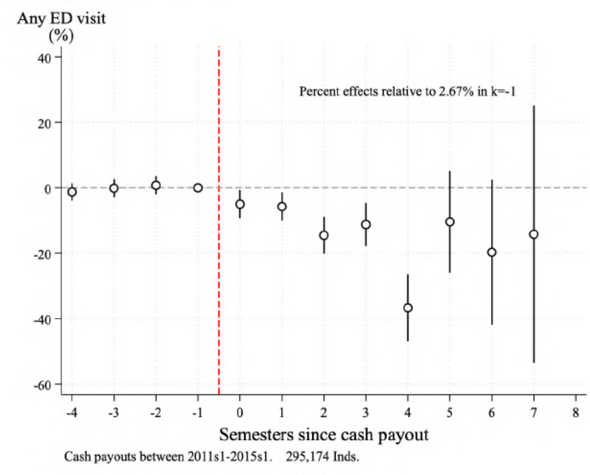
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and the Financial Superintendence of Colombia.

Figure A.28: ED Visits: Robustness

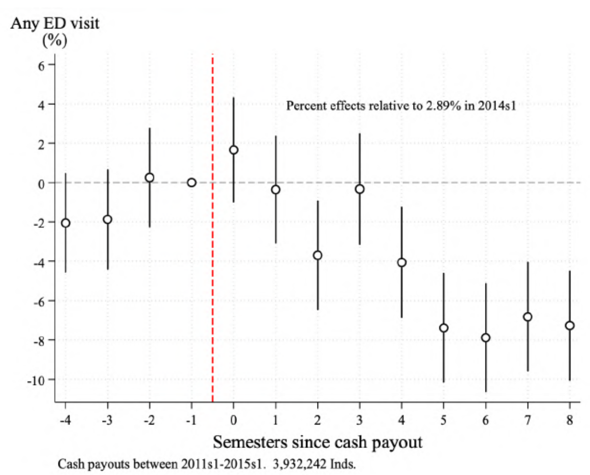
(a) Including never-treated victims



(b) Using Callaway and Sant'Anna (2021) estimator



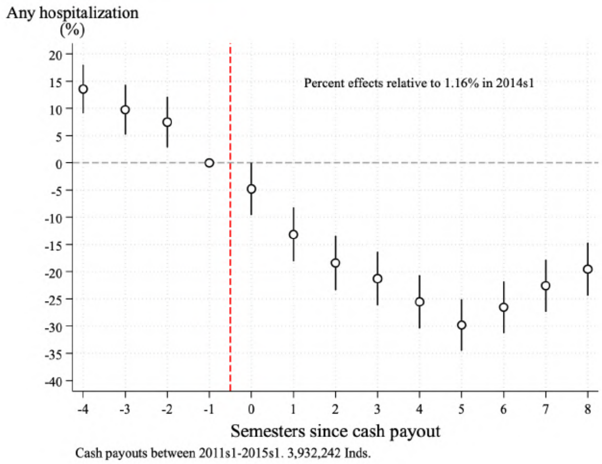
(c) Using Callaway and Sant'Anna (2021) estimator
and including never-treated victims



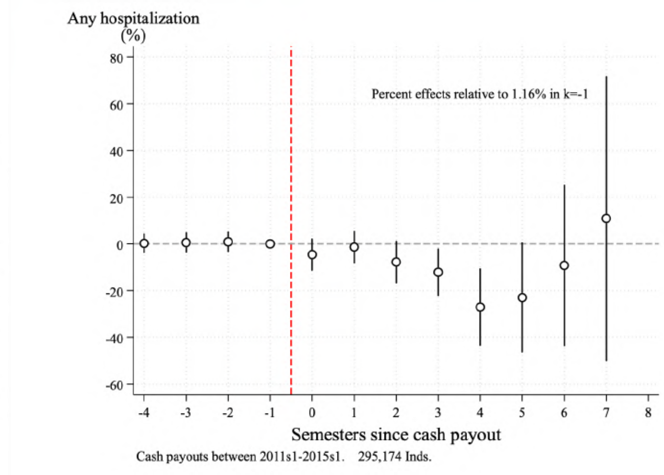
Note: This figure shows that the results on ED visits from figure VIII are robust. Panel (a) includes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator. Panel (c) includes never-treated victims and uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Figure A.29: Hospitalization: Robustness

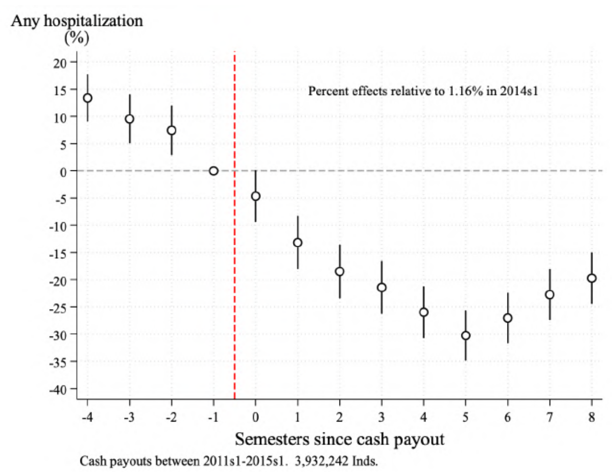
(a) Including never-treated victims



(b) Using Callaway and Sant'Anna (2021) estimator



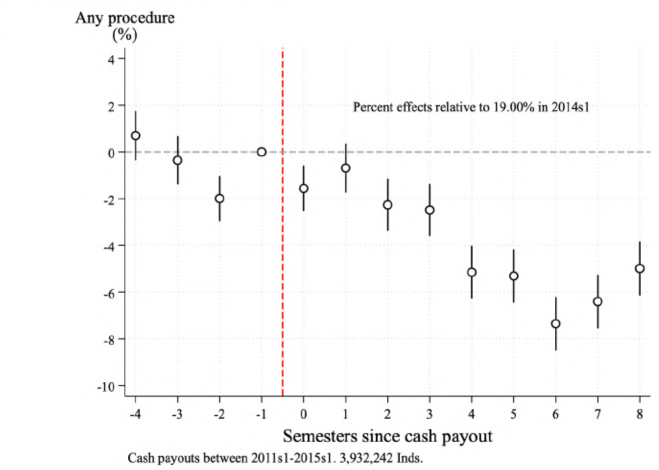
(c) Using Callaway and Sant'Anna (2021) estimator
and including never-treated victims



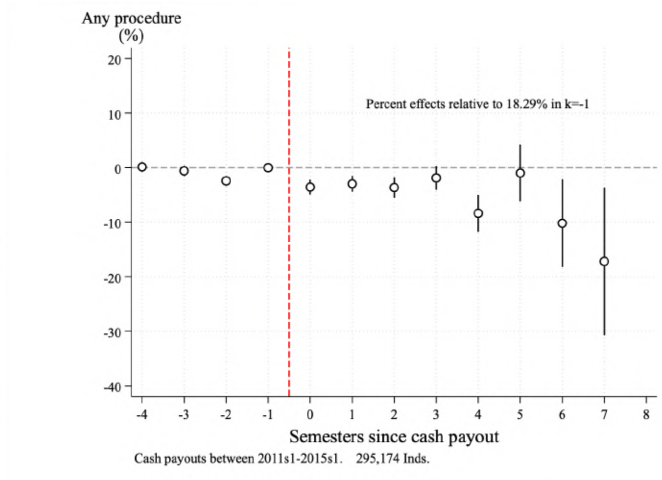
Note: This figure shows the results on the likelihood of hospitalization from figure IX. Panel (a) includes never-treated victims from the estimation sample. We do not have complete hospitalization records for never-treated victims before 2015, which causes a pre-trend. Panel (b) uses the Callaway and Sant'Anna (2021) estimator. Panel (c) includes never-treated victims and uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Figure A.30: Likelihood of a Medical Procedure: Robustness

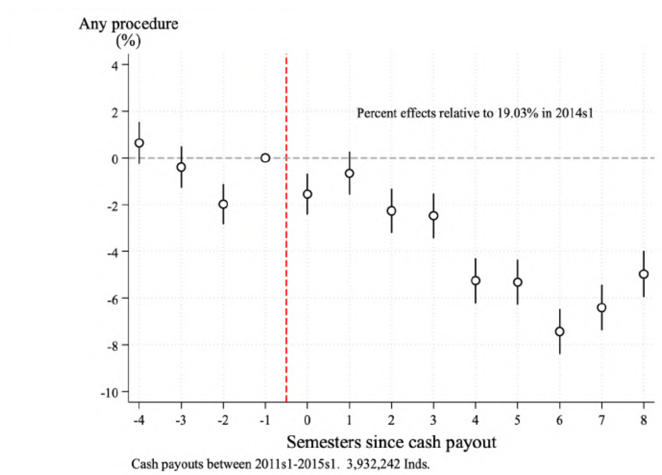
(a) Including never-treated victims



(b) Using Callaway and Sant'Anna (2021) estimator



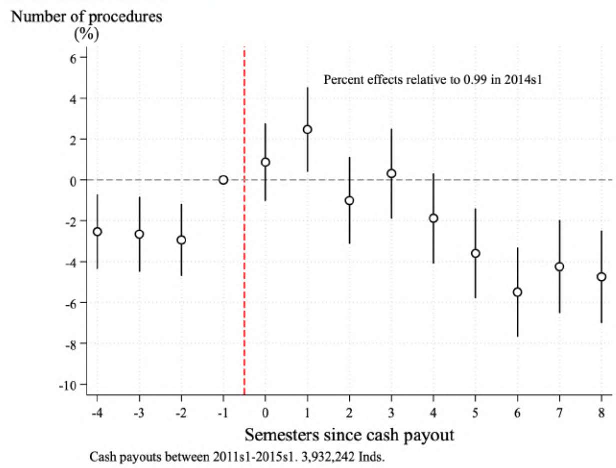
(c) Using Callaway and Sant'Anna (2021) estimator
and including never-treated victims



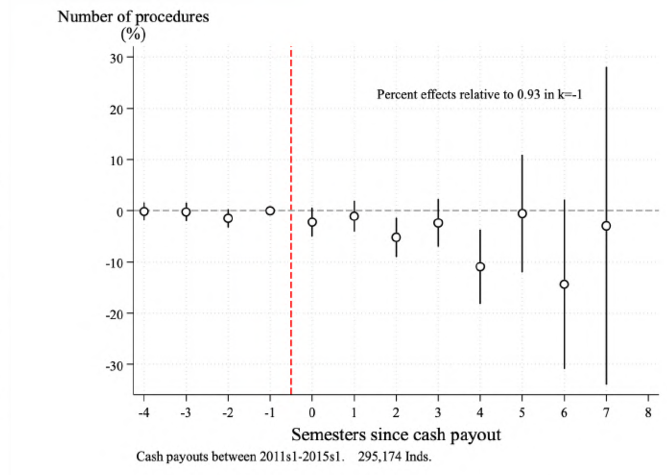
Note: This figure shows that the results on the likelihood of undergoing a medical procedure from figure IX are robust. Panel (a) includes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator. Panel (c) includes never-treated victims and uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

Figure A.31: Number of Medical Procedures: Robustness

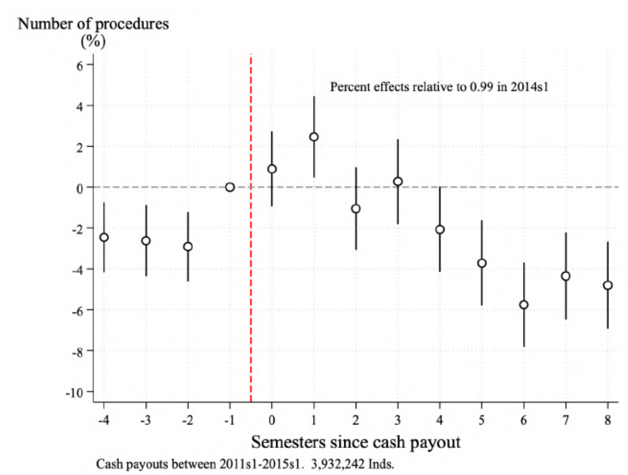
(a) Including never-treated victims



(b) Using Callaway and Sant'Anna (2021) estimator

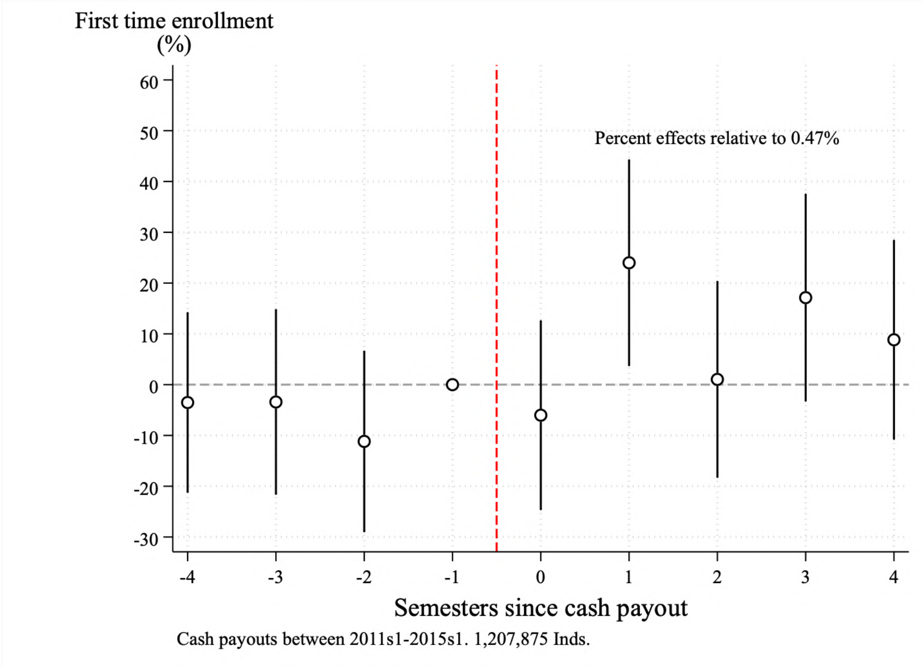


(c) Using Callaway and Sant'Anna (2021) estimator
and including never-treated victims



Note: This figure reports the robustness checks for the number of medical procedures from figure IX. Panel (a) includes never-treated victims from the estimation sample. Panel (b) uses the Callaway and Sant'Anna (2021) estimator. Panel (c) includes never-treated victims and uses the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and RIPS.

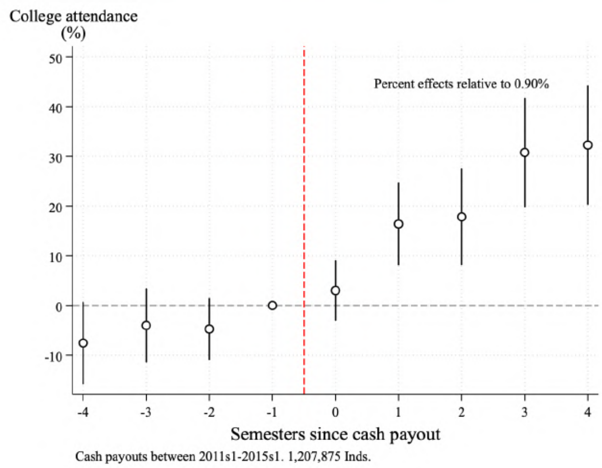
Figure A.32: First Enrollment in a Four- or Five-Year Undergraduate Program



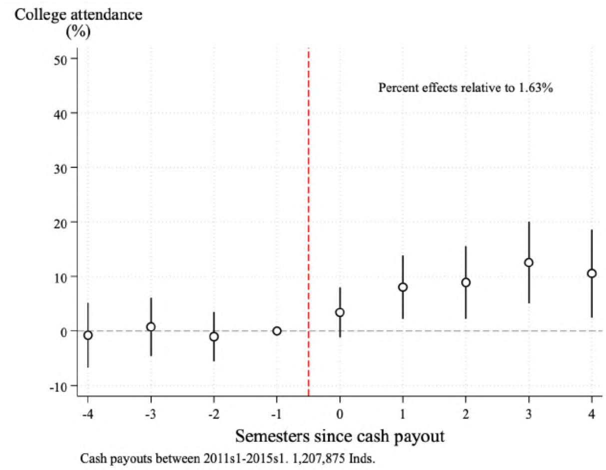
Note: This figure presents the event-study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a four- or five-year undergraduate program for the first time—is plotted against the time from the reparations payout (in semesters). The treatment is defined at the household level, and event time is defined as the first date when any household member received reparations. The sample (i) is balanced in event time, (ii) is restricted to individuals aged 15–25 at $k = -1$, and (iii) includes never-treated individuals. Standard errors are clustered at the household level. The figure suggests 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 15–25.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Figure A.33: Undergraduate Enrollment: Private versus Public Institutions

(a) Private institutions



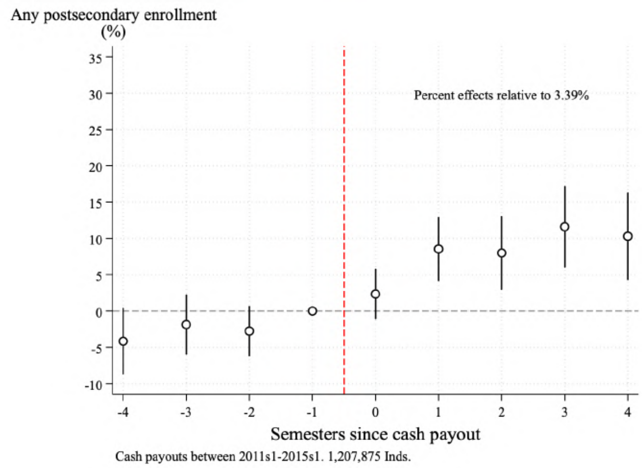
(b) Public institutions



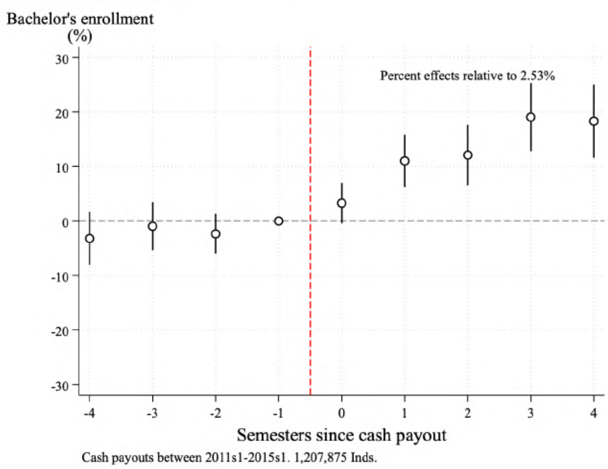
Note: This figure decomposes the undergraduate-enrollment result from figure X by private versus public institutions. Relative to the baseline mean, reparations particularly encouraged enrollment at private institutions.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Figure A.34: Postsecondary Enrollment

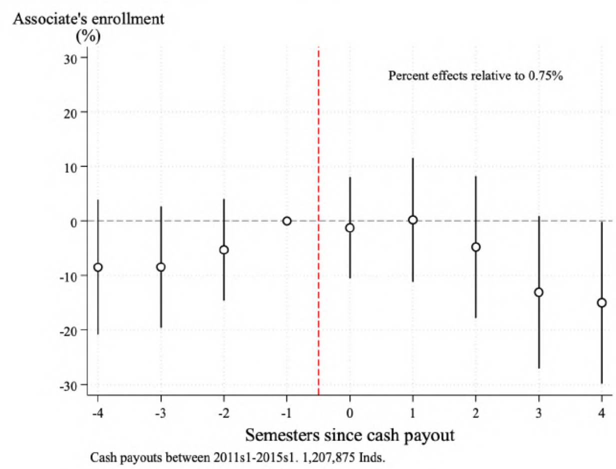
(a) Any



(b) Bachelor's



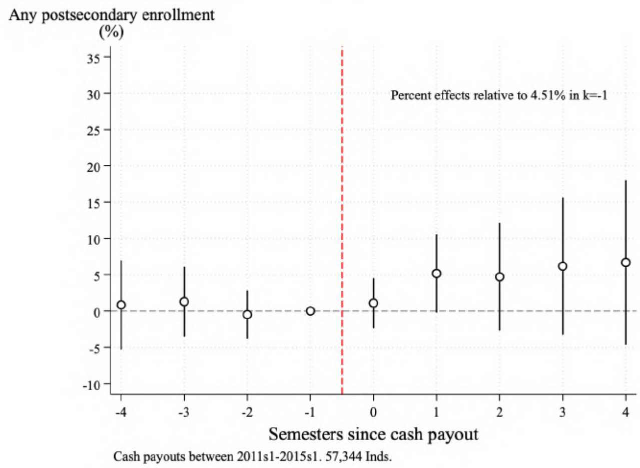
(c) Associate's



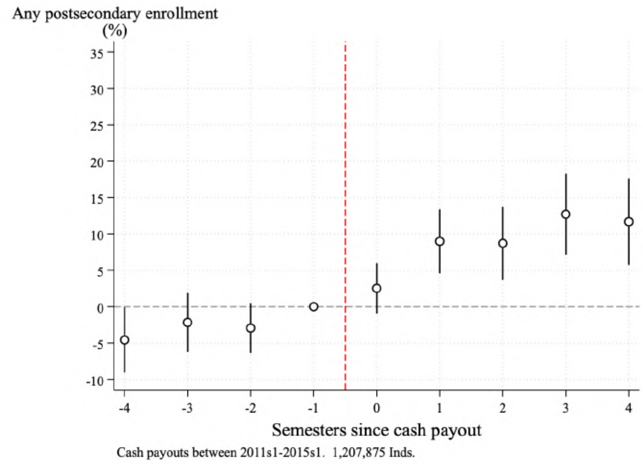
Note: This figure presents the event-study coefficients (in percentage terms) and associated 95 percent confidence intervals using specification (1) with age fixed effects, where the outcome—an indicator for attending a postsecondary education institution in a given semester—is plotted against the time from the reparations payout (in semesters). Panel (b) restricts the outcome to enrollment at four- or five-year bachelor's programs, while panel (c) does the same for enrollment at two- or three-year associate's programs. The treatment is defined at the household level, and event time is defined as the first date when any household member received reparations. The sample (i) is balanced in event time, (ii) is restricted to individuals aged 15–25 at $k = -1$, and (iii) includes never-treated individuals. Standard errors are clustered at the household level. Reparations encouraged postsecondary enrollment, with compounding effects over time. Panel (a) shows postsecondary enrollment increases by 10.3 percent by $k = 4$ on a control mean of 3.4 percent. Panel (b) shows the rise is particularly sizable for bachelor's programs, where the increase at $k = 4$ is 18.3 percent. Panel (c) shows enrollment in associate's programs *decreases* by 15 percent, suggesting individuals are also switching across program types. Figure **??** presents these results excluding never-treated individuals and shows quantitatively similar effects.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Figure A.35: Postsecondary Enrollment: Robustness

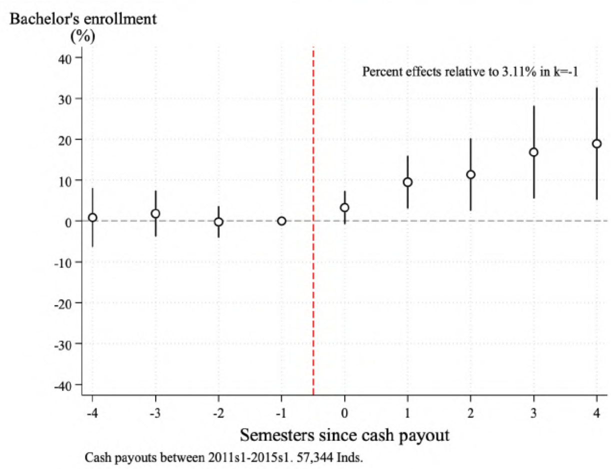
(a) Any: Excluding never-treated



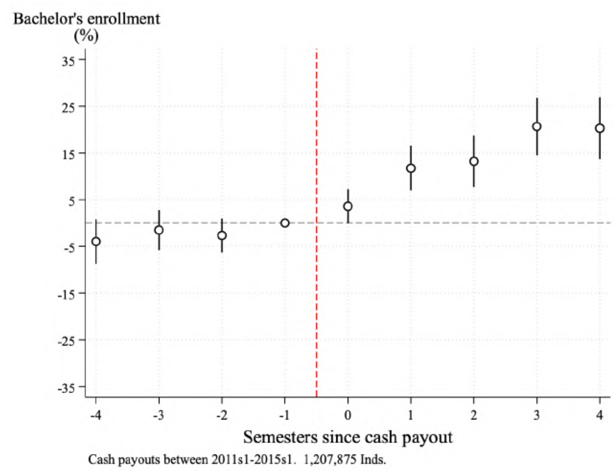
(b) Any: Callaway and Sant'Anna (2021)



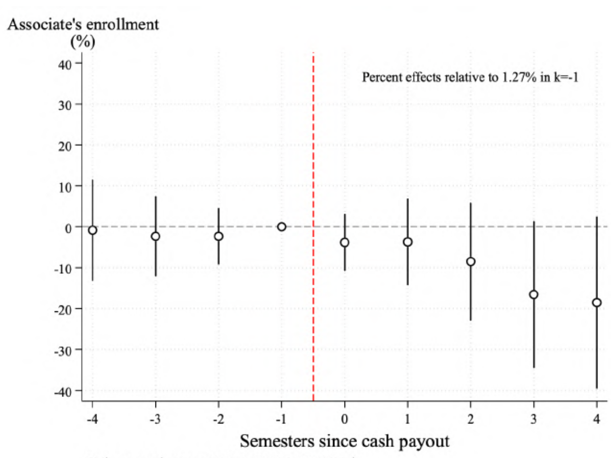
(c) Bachelor's: Excluding never-treated



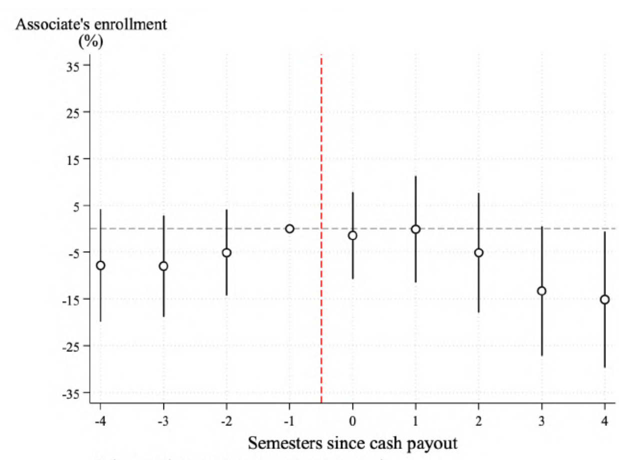
(d) Bachelor's: Callaway and Sant'Anna (2021)



(e) Associate: Excluding never-treated

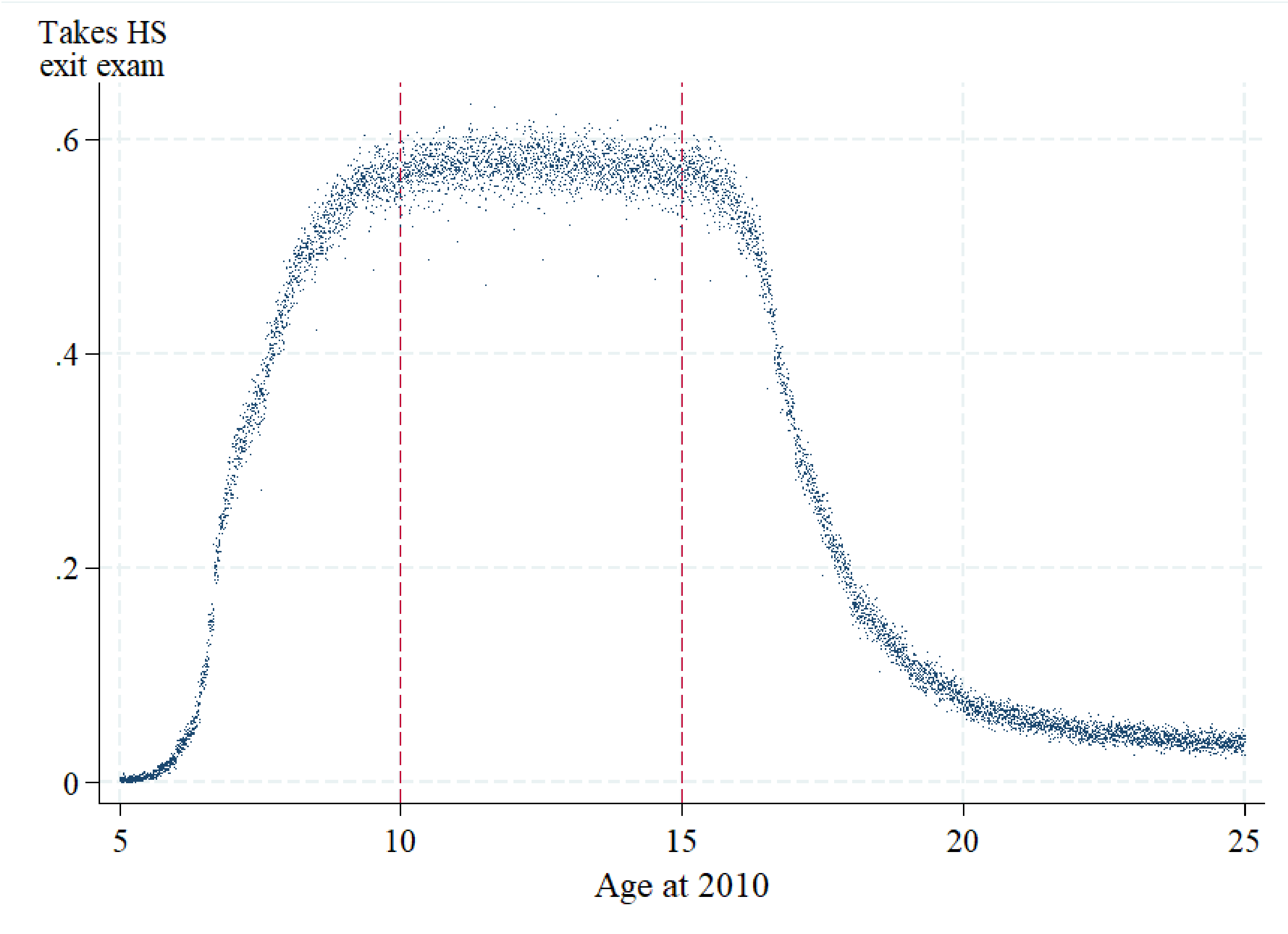


(f) Associate's: Callaway and Sant'Anna (2021)



Note: The panels on the left present the enrollment results from figure A.34 excluding never-treated households, while the figures on the right use the Callaway and Sant'Anna (2021) estimator.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, and SPADIES.

Figure A.36: Likelihood of Taking High School Exit Exam by Age in 2010



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

B Random Forest Analysis

We use a random forest model to predict reparations status and the timing of a reparations payment. We also examine how the prediction changes for reparations before and after Colombia introduced de jure prioritization rules in 2013.

Data. We start with the population of victims in the RUV, which covers all individuals reporting a victimization in the context of Colombia's internal armed conflict between January 1985 and August 2019. Because reparations are often awarded to the household—not the individual—we perform the random forest analysis at the household level.

However, the RUV does not include information about households' socioeconomic status (SES) and sociodemographic conditions. Moreover, it does not always contain accurate household-level information, like information about who other household members are. Therefore, we merge these records with SISBEN to shed light on household composition and conditions (e.g., size and poverty index) in 2010, the year before Colombia adopted the Victims Law. We construct household-level variables as follows. First, we generate measures of victimization and declaration dates using any household member's first date of victimization and declaration, respectively. Second, we construct an indicator variable equaling one if any household member has ever received financial compensation ("treated") and zero otherwise. For treated households, we take the quarter of the first reparations payment received by any member to mark the "event."

Next, we assign households to municipalities based on where members lived according to the RUV and SISBEN. We obtain information about municipality characteristics (e.g., population size and poverty level) from DANE, Colombia's department of statistics. We identify the most vulnerable territories and conflict-affected areas from the municipalities included in Colombia's Development Plans with a Territorial Focus (PDET), which are the municipalities most affected by violence in the context of the armed conflict. In addition, we obtain municipality-level information on voting outcomes from the National Civil Registry. Last, we generate a random variable using a uniform distribution within a municipality-by-victimization type cell. The full list of variables is summarized in table B.1.

We base the analysis on two data sets: first, a random sample of 500,000 households, including treated and untreated households, and second, a data set with all treated households and no untreated households.

Prioritization rules. Colombia has not awarded compensation on a first-come, first-served basis. Following efforts to demobilize paramilitary groups and reintegrate them into civilian life (the Peace and Justice Law, or Law 975/2005), the Colombian government standardized and broadened the victim reparations program in 2008. Victims registered in the judicial process of the Peace and Justice Law could obtain reparations for homicide and forced disappearance, as well as kidnapping, torture, sexual violence, and forced recruitment (Decree 1290/2008). However, indirect victims—people whose relatives had been murdered or forcibly disappeared—were prioritized.

Subsequently, victims and human rights organizations pressured the state to expand the reparations program to other victims, including victims of state forces and forcibly displaced persons. Following the adoption of the Victims Law in 2011, the government established a prioritization scheme in 2013 (Resolution 0223, April 8, 2013). We generate variables to capture these prioritization rules based on the RUV and SISBEN. Specifically, we generate indicators equaling one if any household member meets the following priority conditions:

1. A victim covered by Decree 1290/2008 and Law 418/1997; mainly people whose relatives had been murdered or forcibly disappeared ("indirect" victims),
2. A victim with a disability,
3. A female head of household with more than two children and a SISBEN score below 63,
4. A female head of household with disabled children,
5. A victim of sexual violence,
6. A victim older than 60 with a SISBEN score below 63, or
7. A victim of forced recruitment.[1]

We cannot observe the following four priority conditions from Resolution 0223/2013: victims remitted by courts (these are excluded from our data), victims with a terminal illness, victims under collective reparations, and LGBTI victims.

This prioritization scheme was modified after the Constitutional Court ruled to compensate forcibly displaced households (Decree 1377/2014, Decree 1084/2015, and Resolution 090/2015). Instead, prioritization depended on whether the victim was forcibly displaced. Forcibly displaced victims were prioritized if they had a disability, were aged 70 and above, had already fulfilled their basic needs, or wished to return to their place of origin (Decrees 1377/2014 and 1084/2015). We can observe the first two conditions (disability and age) but not the last two conditions. All other victims——namely, those who were not forcibly displaced——were prioritized if they were disabled, a female head of household with a disabled or ill child, aged 70 or above, a victim of sexual abuse or a child born as a product of rape, a victim of forced recruitment, or a landmine victim (Resolution 090/2015).[2] There are five additional conditions

[1]In table B.1, these variables are labeled Resolution 0223 p2, p5, p6, p7, p8, and p9, respectively.
[2]In table B.1, these variables are labeled Resolution 0090 p3, p5, p7, p6, p8, and p4, respectively.

for non-forcibly displaced victims that we cannot observe: having a costly illness, being part of collective reparations, LGBTI victims, living abroad, and indirect victims whose relatives' bodies were returned to them.

Estimation and performance measures. We evaluate the random forest model using the randomForest and varImp R packages. We use 1,000 decision trees and 25 hyper-parameters, independent and randomly selected variables over which the algorithm may split. We rescale the variables to the same scale by standardizing them using the min-max scaler.

We use mean decrease accuracy (MDA) to compute the feature importance on permuted out-of-bag (OOB) samples based on the mean decrease in accuracy. MDA expresses how much accuracy the model loses by excluding each variable. A higher value indicates the importance of that variable for successful classification (e.g., treated versus untreated households). We plot the variables in descending importance.

For each tree, the prediction error on the OOB portion of the data is recorded (error rate for classification). Then the same is done after permuting each predictor variable. The difference between the two is then averaged over all trees.

Using the predicted values based on the OOB portion of the data, we calculate the model's accuracy as:

$$\frac{\text{True positive} + \text{True negative}}{\text{True positive} + \text{True negative} + \text{False positive} + \text{False negative}}$$

Moreover, using the predicted values based on the OOB portion of the data, we calculate the model's precision as:

$$\frac{\text{True positive}}{\text{True positive} + \text{False positive}}$$

For example, in section B.1, "positive" and "negative" refer to whether the household received reparations or not. The model's accuracy calculation measures how accurate the model is at categorizing households that receive and do not receive reparations, while the model's precision calculation measures how precise the model is at categorizing households that receive reparations.

Section B.2 predicts when a household receives reparations. In that case, we only produce measures of accuracy, not precision, because the dependent variable is categorical.

B.1 Predicting Who Receives Reparations

This analysis is based on a random sample of 500,000 households, including treated and untreated units, for computational purposes. To match the treatment period used in the main analyses, we define an indicator variable that equals one if any household member received a reparations payment between 2011q1 and 2016q4 and zero otherwise. While the RUV data also includes reparations between 2017q1 and 2019q2, our event-study estimates are based on reparations taking place between 2011q1 and 2016q4 to examine outcomes up to four years after the event.

Figure B.1 presents the random forest variable importance plot. We assess the performance using MDA. The model's accuracy is 0.925, meaning we can correctly classify treated and untreated households. Indeed, the model's accuracy is high because more than 90 percent of eligible households are untreated, facilitating an accurate prediction of negatives. Since only a minority of households have received reparations, the model is substantially less precise in predicting positives: the model's precision is 0.67. Since the precision for a random variable would be 0.5, a precision score of 0.67 means the model's ability to precisely categorize positives is quite low. In other words, most eligible victims have not received compensation and, as a result, the model is more accurate in classifying untreated than treated households.

Five additional results emerge from figure B.1. First, the most important variables for classifying treated households are the poverty rate of the victims' municipality and its population size (where a household's municipality is based on the RUV), for which the MDA is 0.019 and 0.018, respectively. The number of victims in the municipality is also an important variable. Second, the most important household-level variable is being an indirect victim. Indeed, indirect victims are most likely to receive compensation as they were first prioritized in 2008 after the Peace and Justice Law. Furthermore, table I shows that 50.7 percent of treated individuals are indirect victims. Third, the year of declaring victimization is also important; table I shows that treated victims tend to have registered their victimization earlier. Moreover, most indirect victims reported their victimization following the Peace and Justice Law in 2005. Fourth, households' SES is not an important predictor: the SISBEN score's MDA is only 0.006, below the MDA of the random variable. Other measures of SES, like household income, do not even show up among the 20 most important variables plotted in the figure. This suggests that Colombia awarded reparations based mainly on victimization type and not need. Last, a skeptical reader might think the government may have rewarded victims who voted for the incumbent party. In practice, this is extremely unlikely. Moreover, there is no evidence that households are more likely to receive reparations if members live in municipalities that voted 50 percent or more for the government party in the second round of the presidential elections in 2010 (based on data from the RUV and the National Civil Registry).

B.2 Predicting When a Household Receives Reparations

The analysis sample uses all treated households to predict when a household receives reparations. The dependent variable is the date of the reparations payout, a categorical variable beginning in 2011q1 and ending in 2016q4.

Figure B.2 presents the random forest variable importance plot for the date of the reparations payout. The model's accuracy is 0.320; compared to classifying treated households, we predict less accurately when a household is treated. The most important variable is being an indirect victim: the MDA for homicide is 0.054. Indeed, indirect victims were prioritized and received reparations first. The second most important variable is the year of declaration: the sooner a treated household registers its victimization, the sooner they receive reparations. This is, in large part, because most indirect victims registered their victimization very early on since the Peace and Justice Law prioritized them. Once again, SES is not important for predicting the date of the reparations payout, meaning Colombia's reparations program did not prioritize victims based on need.

B.3 How the Prioritization Rules Affect Who Receives Reparations

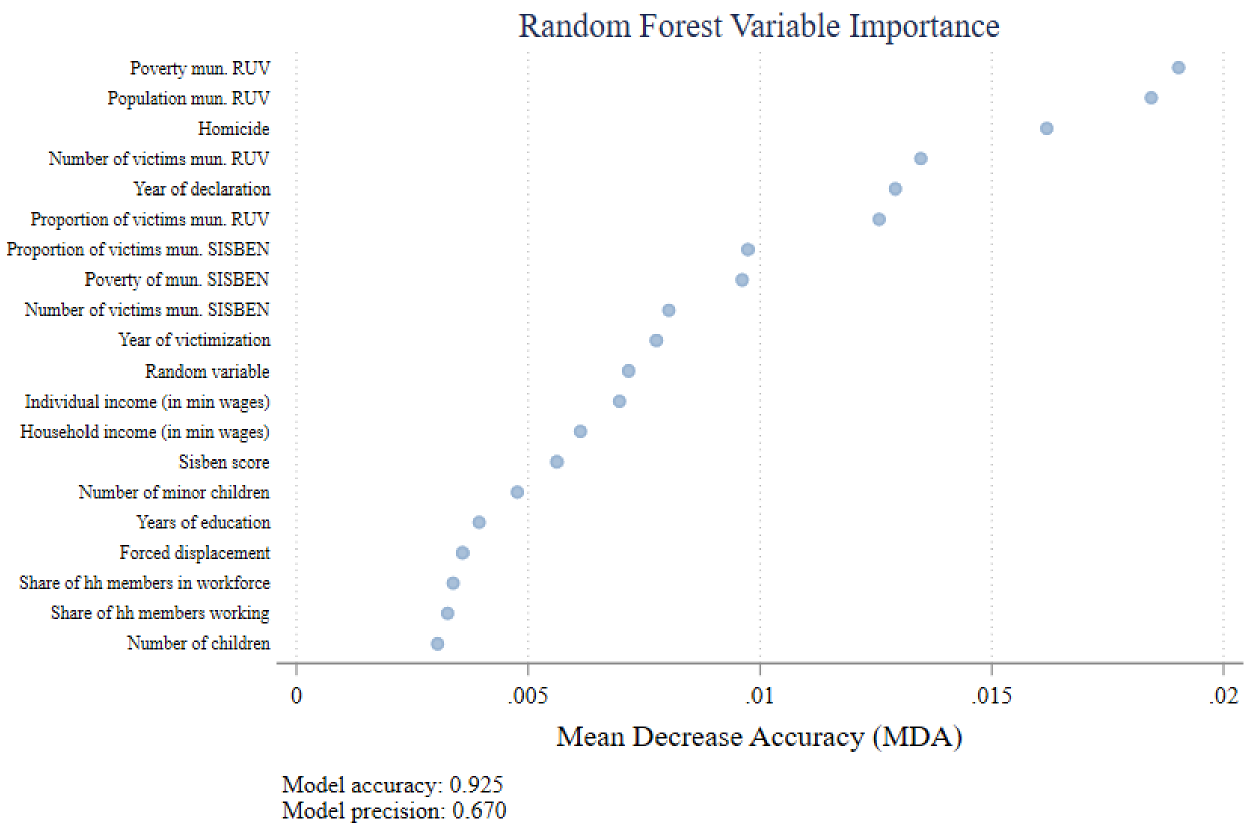
This analysis is based on the full sample of treated households. First, we generate a dummy variable that equals one if the household received reparations before July 2013 and zero otherwise. Of treated households, 30 percent received compensation before July 2013. Next, we generate a dummy variable that equals one if the household received reparations after July 2013 but before the last 40 percent of households that received reparations. This guarantees the same number of households having received reparations before and after July 2013. Moreover, according to government reports to Congress (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2018), the government sought to spread reparations across Colombia's 32 geographic departments. Therefore, we generate variables for the share of treated and untreated eligible victims in each department by July 2013. Figure B.3 presents the results from this analysis.

Several striking results emerge from figure B.3. First, the model can predict who does and does not receive reparations more accurately before July 2013 than afterward: the model's accuracy is 0.830 before July 2013 and 0.770 afterward. However, the model predicts who receives reparations more precisely after July 2013: the model's precision is 0.575 in panel (a) compared to 0.705 in panel (b). This suggests the model is more accurate at predicting who does *not* receive reparations before July 2013. Namely, households whose members are not indirect victims are unlikely to receive reparations before July 2013. Indeed, panel (a) shows that the most important variable in predicting treatment is having a murdered relative since, as explained above, indirect victims had been prioritized since 2008. Victims of forced displacement are unlikely to have received reparations before July 2013 (unless they are also indirect victims), which explains why the MDA is high for these households too. Victims receiving reparations before July 2013 are likelier to have declared their victimization sooner because, again, most indirect victims reported their victimization following the Peace and Justice Law in 2005. Last, the MDA is low for the remaining variables: the observable characteristics are no better predictors of reparations payouts before July 2013 than a random variable.

Second, panel (b) shows the model is more precise in predicting who receives reparations after July 2013, after the prioritization schemes were established following the Victims Law. For instance, the number of people who already received reparations in a department features as an important variable, consistent with the anecdotal evidence that the state sought to spread reparations across departments (Comisión de Seguimiento y Monitoreo al Cumplimiento de la Ley 1448 de 2011, 2018). However, unlike in panel (a), no variable stands out as being especially important after July 2013 in panel (b): the MDA is below 0.02 for all variables. Indeed, the importance of homicide—which featured as the most important variable before July 2013—shrinks by over 80 percent. Arguably, after the Constitutional Court ruling expanded the sample of eligible victims to include all victimization types—notably, including forcibly displaced households—the prioritization rules were broad and made too many victims eligible for reparations at once. The waiting list was long, leading to substantial uncertainty about when an eligible victim would receive reparations—before Resolution 1958/2018 and Resolution 1049/2019 subsequently modified the prioritization rules.

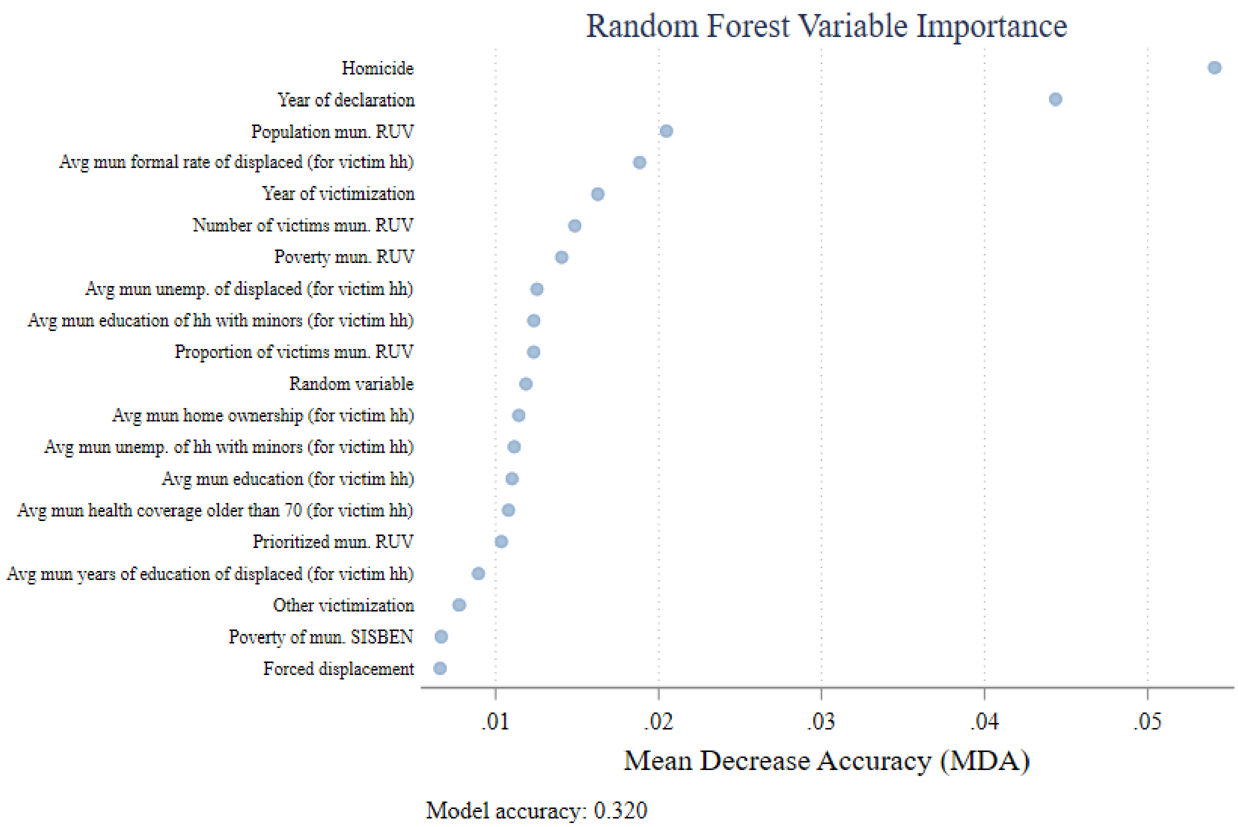
In sum, the results from the random forest model support the argument that precisely predicting who receives reparations is rare despite the reparations program's established prioritization rules. The chance of a precise prediction is low even for an econometrician using random forest models, ex-post data of thousands of reparations over many years, and more than 90 household- and municipality-level variables. Therefore, it is extremely unlikely that victims themselves could predict who would receive reparations or when they would receive the money.

Figure B.1: Predicting Who Receives Reparations



Note: This figure plots the MDA of the 20 most important variables for reparations status in descending order of importance.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, DANE, PDET, and the National Civil Registry.

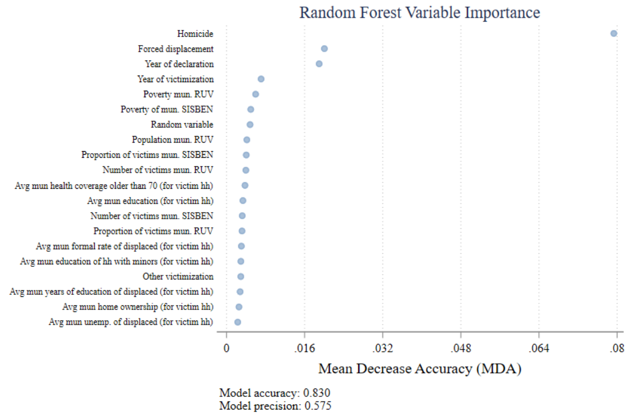
Figure B.2: Predicting When a Household Receives Reparations



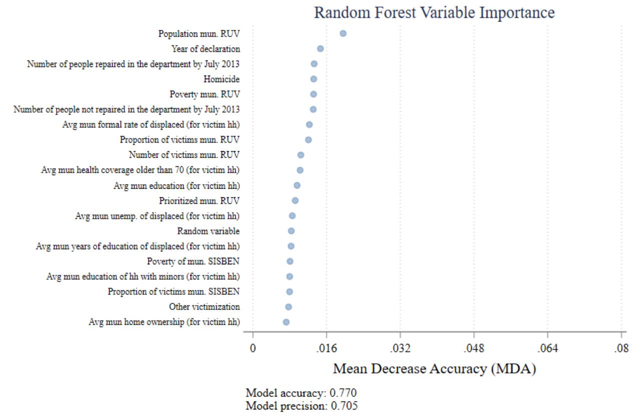
Note: This figure plots the MDA of the 20 most important variables for the quarter of the reparations payout in descending order of importance.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, DANE, PDET, and the National Civil Registry.

Figure B.3: Predicting Who Receives Reparations before versus after July 2013

(a) Before July 2013



(b) After July 2013



Note: This figure plots the MDA of the 20 most important variables for reparations status (before versus after July 2013) in descending order of importance.
Source: Authors' calculation using RUV data from the SRNI, SISBEN, DANE, PDET, and the National Civil Registry.

Table B.1: Variables Used in Random Forest Model

Unit of observation	Data source	Variable description
Household	RUV	Forced displacement
		Forced disappearance
		Other victimization
		Homicide
		Disabled
		Year of victimization
		Year of declaration
		Resolution 0223 - p2
		Resolution 0223 - p5
		Resolution 0223 - p6
		Resolution 0223 - p7
		Resolution 0223 - p8
		Resolution 0223 - p9
		Resolution 0090 - p1
		Resolution 0090 - p3
		Resolution 0090 - p4
		Resolution 0090 - p5
		Resolution 0090 - p7
		Resolution 0090 - p12
	SISBEN	Has a child below age 5

Has a child between ages 6 and 10
Has a child between ages 11 and 17
Has a child below age 5 and mother head of HH
Has a child between ages 6 and 10 and mother head of HH
Has a child between ages 11 and 17 and mother head of HH
Lives with a senior between ages 50 and 60
Lives with a senior between ages 61 and 70
Lives with a senior above age 70
Mother head of household
Dummy = 1, Disabled child(ren)
Number of children
Mother head of household and 2 children
Number of minor children
Household size
Proportion of women
Individual income (in min. wages)
Household income (in min. wages)
SISBEN score
Dummy = 1, HH member in workforce
Dummy = 1, HH member works
Dummy = 1, HH member earns minimum wage or more
Dummy = 1, HH member unemployed
Dummy = 1, HH member has health
Dummy = 1, HH member is formal
Years of education
Stratum 0
Stratum 1
Stratum 2
Stratum 3 or more
Owns home
Refrigerator
Washing machine
TV
Cable
Heater
Microwave
Air conditioner
Electricity
Sewer
Gas

		Telephone
		Share of HH members in workforce
		Share of HH members working
		Share of HH members unemployed
		Share of HH members with health access
		Share of HH members in formality
		Share of HH members who earn minimum wage or more
Municipality	SISBEN	Missing mun. SISBEN
		Dummy = 1, Municipal header
		Dummy = 1, Town center
		Dummy = 1, mun. SISBEN capital city
		Number of victims mun. SISBEN
		Proportion of victims mun. SISBEN
		Avg. mun. home ownership (for victim HH)
		Avg. mun. education (for victim HH)
		Avg. mun. education of HH with minors (for victim HH)
		Avg. mun. unemp. of HH with minors (for victim HH)
		Avg. mun. years of education of displaced (for victim HH)
		Avg. mun. unemp. of displaced (for victim HH)
		Avg. mun. formal rate of displaced (for victim HH)
		Avg. mun. health coverage older than 70 (for victim HH)
	DANE	Poverty mun. RUV
		Population mun. RUV
		Poverty of mun. SISBEN
	RUV	Number of victims mun. RUV
		Proportion of victims mun. RUV
		Number of people who have received reparations in the departmen
		Number of people who have not received reparations in the depart
	PDET	Prioritized mun. SISBEN
		Prioritized mun. RUV
	National Civil Registry	Dummy = 1, if government obtained more than 50 percent of the v
		Random variable