

# The Promised Land:

## The Effects of the Land Restitution Program in Colombia

FRANCESCO BOGLIACINO, CHRISTIAN POSSO, MARTA JUANITA VILLAVECES<sup>1</sup>

AUGUST 10, 2021

### Abstract

In cases of internal conflicts, international law recommends restituting properties to repair the victims of land dispossession. While righting the tort, this process will also— in the spirit of the law – shape the beliefs of the legitimacy of the property rights system and increase the willingness to participate in the economy. To assess whether this is the case, we estimate the causal effect of the Colombian land restitution program on access to credit. We use the timing of the restitution as the source of identification in an event study approach. Since farmers typically recur to small-to-medium loans with small-to-none guarantees, and this corresponds to the legal definition of microcredit in Colombia, we use access to this source of credit as the outcome variable. Using administrative data from the program and the census of credit transactions, we show a significant increase in access to credit in both the intensive and the extensive margin. The effects are stronger two years after the restitution when individuals acquire full property rights.

**JEL Code:** G14; H81; O15; Q15

**Keywords:** Land restitution; microcredit; property rights; agriculture; event study

**Word counts:** 5110

**Exhibits:** 5

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<sup>1</sup> Bogliacino: *Universidad Nacional de Colombia and Centro de Investigaciones para el Desarrollo (Corresponding author, Facultad de Ciencias Económicas, Universidad Nacional de Colombia, Kr 30, No 45-03, Bogotá (Colombia), [fbogliacino@unal.edu.co](mailto:fbogliacino@unal.edu.co))*, Posso: *Banco de la República ([cpossosu@banrep.gov.co](mailto:cpossosu@banrep.gov.co))*, Villaveces: *Universidad Nacional de Colombia and Centro de Investigaciones para el Desarrollo ([mjvillavecesn@unal.edu.co](mailto:mjvillavecesn@unal.edu.co))*. Funding: *Universidad Nacional, “Convocatoria Nacional de Proyectos para el fortalecimiento de la Investigación, creación e innovación de la Universidad Nacional de Colombia 2016-2018” (Project 35685) and Universidad Nacional, “Convocatoria Jesus Antonio Bejarano 2020” (Project 51668)*. We are grateful to Camilo Sánchez, Julian Salamanca, and Laura Gutiérrez for discussions regarding the *Ley de Víctimas* and the international principles. Participants to internal seminars at Banco de la República, CEER, Universidad Javeriana, Universidad de Antioquia, UMASS Amherst, Universidad del Rosario, Universidad de los Andes, Masaryk University provide useful comments. We also thank *Superfinanciera* for providing access to the credit data. We thank Christian Palencia, Camilo Gómez, Douglas Rodriguez, Camila Lara, Jose Daniel Velasco which worked in data collection. Manuela Cardona and Sara Londoño provided excellent research assistance. The opinions expressed herein belong to the authors and do not necessarily reflect the views of *Banco de la República* or its Board of Directors.

## 1. Introduction

The international judicial principles to address land dispossession in the context of internal conflicts (Deng and Pinheiro Principles; UN 2005a, 2005b; Paglione, 2008) recommend land restitution. Restituting a property right to those that were illegally dispossessed or forced to abandon it is righting the wrong and is desirable *per se*. There is an additional aim of the law on land restitution, though, which concerns the principle of restorative justice. According to this doctrine, beneficiaries of land restitution are given back asset-based agency, which should make them perceive the system of property rights as legitimate, help them rebuild their project of life, and increase their willingness to participate in the economy (Athuahene, 2007). Evidence of whether this works is scarce.

The laboratory for the Deng and Pinheiro principle is Colombia. In 2011, the president in charge and Nobel Prize winner, JM Santos, passed Bill 1448 or *Ley de Víctimas* (2011) that promoted land restitution for the victims of land dispossession or forced abandonment. This is the policy we analyze here. We assess to what extent beneficiaries of land restitution increase their access to credit. Why credit? Essentially for two reasons. The first argument is that measuring the reestablishment of the life project and the willingness to participate poses a tremendous challenge, as surveys are limited both in terms of coverage and longitudinal dimension, and as investment in rural areas is arduous to monitor due to poor administrative records. Instead, in Colombia, information on the formal credit market is systematic, longitudinal, and at the census level since 2004.

The second argument is that small farmers typically recur to small-to-medium loans oriented towards productive activities, and potentially without real guarantee (Banerjee and Duflo, 2010; Bose, 1998). This ends up being the *legal* definition of *microcredit* in Colombia, which we can monitor thanks to the administrative nature of our data. Hereafter, this is our main outcome variable.

In this article, we use an event study approach to estimate the impact of land restitution on access to credit. Our identification strategy leverages on the temporal variation in the date of restitution. Since the full property of the restituted land (including the right to sell) is acquired after an embargo period of two years, this clause of the law provides us with a double exploitable variation: the sentence that grants the restitution and the acquisition of the full property. Therefore, we follow the households 16 quarters after the restitution and 16 quarters before the restitution.

To perform this analysis, we combine granular administrative records of all formal credit transactions in Colombia and collect data on all sentences of land restitution. We construct a novel dataset from over eight years of archival judicial data. We digitize thousands of sentences documenting

the period of the restitution, the perpetrators, the plot object of the sentence, demographic characteristics, and any additional relevant information, between 2011 and 2018. Our credit records are from the “Individual Debtor Report and Active Credit Operations” (or Format 341) from *Superfinanciera*, the Colombian government’s agency responsible for overseeing financial regulation. *Superfinanciera* requires all financial institutions to classify all active credit operations granted to micro enterprises as microcredit (Financial Superintendence of Colombia, 2019). In addition, *Superfinanciera* obliges all financial institutions to exclude credit cards or other consumption-related debt from the report.

Our empirical strategy mirrors other research on the effect of exposure to violence (Bindler and Ketel, 2020; Guarín et al. 2021) or unanticipated shocks (Dobkin et al., 2018; Kuziemko et al., 2018). Making a causal claim requires that, conditional on being restituted during our observation window, the timing of the restitution is uncorrelated with the credit outcomes. For instance, an act of restitution that is preceded by anticipated information by the victims would violate the assumption. Nonetheless, this is not plausible in our context, since the exact date of the restitution is the result of a two-step process that involves an administrative discussion in a governmental agency and a judicial phase in court, where the average victim lacks concrete opportunities to speed up or delay the procedure (CCJ, 2019). By the same token, we argue that heterogeneity of the effect across cohorts is also unlikely, which would pose a potential problem to our two-way fixed effect estimator.

To convince the reader of the causal interpretation, we also looked at potential confounds. After the initial two quarters, the sentences are rolled out mimicking a uniform distribution. This suggests a lack of strategic manipulation. In terms of observables, when we assess the balancing of covariates across different cohorts of beneficiaries, the null hypothesis of lack of confounds cannot be rejected. In addition, estimated effects before the restitution are consistent with the assumption of parallel trends. To wrap up, the institutional design of the policy and the indirect evidence from our data argue in favor of the plausibility of the identification.

Now, a glimpse into our main results. We show that the land restitution program increases access to microcredit in both the extensive and the intensive margin, i.e., the probability of access and the size of the loan, respectively. Whereas the mean outcome in the quarter before the restitution is 1.3%, according to the point estimate, two years after the restitution access to credit increases by 2.7%, with a huge relative effect of 208%. Notably, the effect increases until the acquisition of the full property right, stabilizing and remaining statistically significant after that. On average, the effects after the acquisition of the full property right are almost twice as large as those of the first two years of the

program and the difference is statistically significant (the cumulated effect is respectively 19.90% and 10.52%). Things are even more striking when this credit is put in perspective, considering the rural domain of our study. On average in Colombia, loans classified as microcredit are worth 3.6 million COP (Estrada and Hernández, 2019). In our data, the average loan issued to a restituted is about one and a half times larger (5.6 million COP).

Having established this main result, we investigate whether the effect corresponds to an increase in the demand for credit or whether it is externally driven by the design of the law. We ask this additional question, as the law makes it possible, for the government, to further support beneficiaries encouraging the undertaking of a sort of business plan. According to the Victims' law in Colombia, the sentences may require the State to convey technical, financial, and operational resources to benefitted households, following an approved plan to exploit productively the plot. When we restrict the analysis to those that did not benefit from such mandate from the sentence, the impact of the restitution is maintained.

We also looked at whether the effect is driven by a standard collateral effect, i.e., having acquired the land, the complementarities between land and capital increase the demand for credit, and land acts as a guarantee for the bank. Notice that the embargo period prohibits to use the land as collateral within the first two years from the restitution, but not afterwards. As expected, in the first two years, these loans do not come with a real guarantee (the restituted plot), but neither they do after the eighth quarter. The category of real collateral and other assets explains at most 11.07% of the loans in the first seven quarters, and at most 8.44% in the following two years.

In fact, when we delve into the guarantees, we found that most of the loans come with a backing up from a public program called the *Fondo Agropecuario de Garantías -AGF-*, a government agency created in 2010 that provides insurance to financial institutions for agricultural investments. More precisely, this class of guarantee explains 61.64% of the effect in the first two years and 44.66% in the following two. This suggests the existence of a complementarity of the restitution with this additional policy instrument. Notice that the AGF is established before and does not belong to Bill 1448, as it covers all small agricultural producers independently of the land restitution program (it is not constrained to victims of land dispossession), and it is unrelated to the agency managing the restitution (*Unidad de Restitución de Tierras -URT-*).

We can analyze heterogeneity (or lack thereof) along different dimensions. While most of these margins are informed by theory or policy, a caveat is in order. In fact, we do not claim causality in these further results. When we introduce interactions, it is more likely that there exist variables that

confound the empirical relationship found in our data; moreover, the requirements in terms of power are more demanding. The effect is not concentrated on large plots, which is something that is often visible in studies of securing property rights in the rural context, as in the well-studied case of Peru (Kerekes and Williamson, 2010). Additionally, we found that the effect is essentially the same when the perpetrator is the guerrilla or the paramilitary (together they represent 71% of total land dispossessed): given that the armed groups were controlling different areas, this stylized fact suggests the effectiveness of the program across the different regions of the country.

This article is intended to make two contributions to the literature. Our first contribution is to provide the first impact evaluation of land restitution. Some descriptive evidence is available from South Africa (Dikgang and Muchapondwa, 2015; Hall, 2004; 2010). Previous quantitative evidence for the Colombian case is either descriptive or limited to the analysis of the productive projects (Bogliacino et al. 2021; Moreno Sánchez et al. 2020). Our second contribution is to the literature that analyzes investment by small scale actors (in development economies): it has been argued that small farmers forgo productive investment due to conflict (DeRoux and Martinez, 2021) or other forms of shocks (Rosenzweig and Wolpin, 1993; Fafchamps, 1992; Burke et al., 2019). We contribute by showing that positive asset shocks may slack this constraint.

This article proceeds as follows. Section 2 describes the data and the research design, Section 3 presents the results, and Section 4 concludes.

## **2. Data and Analysis**

### **2.1. Institutional context**

Colombia represents a laboratory “test tube” case for the application of the Deng and Pinheiro doctrine (UN, 2005a; 2005b). The result of this process is Bill 1448/2011 (Ley 1448 10/6/2011, *Ley de Víctimas*) aimed at repairing the victims of the internal conflict through restituting the land which was forcibly abandoned or dispossessed after 1991 (around 5.5 million hectares according to *Contraloría General de la República*, 2015). Colombia partially deviates from the international recommendations to establish land restitution as a purely administrative procedure, as it includes both an administrative phase and a judiciary one. The inclusion of a judiciary phase counterbalances the decision-making power of the government, as the armed force was a perpetrator in some cases (GMH, 2013). Moreover, the judicial phase is deemed necessary to establish property rights in the context of

poor cadastral information and to settle the disputes that may emerge due to the involvement of multiple parties that may have owned or exploited the plots in the time (sometimes decades) that elapsed between the original dispossession (or forced abandonment) and the present (Sánchez León, 2017).

During the administrative phase, the Land Restitution Agency checks the existence of the three conditions necessary to include the victims on the registry of dispossessed land: first, being a victim (and victimized after 1991); second, being dispossessed or forced to abandon their land (formally owned or de facto possessed, if private, or authorized to exploit, in case of public land, the so called *baldío*); and third, the existence of a connection between the violent act and the internal conflict. The judicial phase is fully regulated by the law: the trial does not follow the standard procedure of the criminal law and is supervised by a court only when there are opponents (the default is a single judge). As a result, there is limited room for any manipulation of the timing by the restituted. The resulting decision of the two phases is the sentence of restitution and in case the court (or the judge) rules favorably to the victims (94% of the times according to our census), the mandatory land titling.

The second feature of the Colombian implementation is the design of a “carrot and stick” system to induce the return of the victims. The “carrot” is the potential inclusion of a “productive project” or other reparations, partially financed by governmental institutions, whereas the “stick” is an embargo period of two years during which the land cannot be sold. This carrot and stick provision by the law represents a local solution to implement the recommendation by the Pinheiro’s doctrine to spur the return of farmers to their land. Given the conditions of underdevelopment within most of rural Colombia, the return was thought to be impractical unless the restitution came with these further conditions.

## **2.2. Data**

We construct a novel dataset from 2011 to 2020 using public archival judicial data from *Unidad de Restitución de Tierras* (2021), the agency in charge of the supervision of the administrative part of the process. We digitized 4,396 sentences documenting the beneficiary (name and ID, household size), the time of the restitution, the perpetrators, the plot object of the sentence, demographic characteristics, the inclusion of a productive project, plus any additional relevant information. In this paper, we consider the time window 2011-2014 (last quarter) to harmonize the data with our credit

data. In particular, we need to be able to observe 16 quarters before and after the restitution, in order to ensure that we have at least two years' data after the acquisition of the full property. Data provide information for 1,574 households (6,036 individuals from 768 sentences) and for a total land area of 16,621.41 hectares.

In addition, we use data from the “Individual Debtor Report and Active Credit Operations” or “Form 341” from the Colombian government agency responsible for overseeing financial regulation, the Financial Superintendence of Colombia (2019). These quarterly data contain formal credit information for more than 700 million credit transactions for all types of credit, including microcredit, consumer, commercial, and mortgage credits in the country from the first quarter of 2004 until the last quarter of 2019. Format 341 provides the identification of the individuals who have credits (which allows our matching with the sentences) and a detailed list of specific information about the loans. The dataset includes information on the amount of the credit, the bank that issued the credit, the guarantee, the interest rate, the number of days in default, the quality of the credit, and so on.

### **2.3. Econometric strategy**

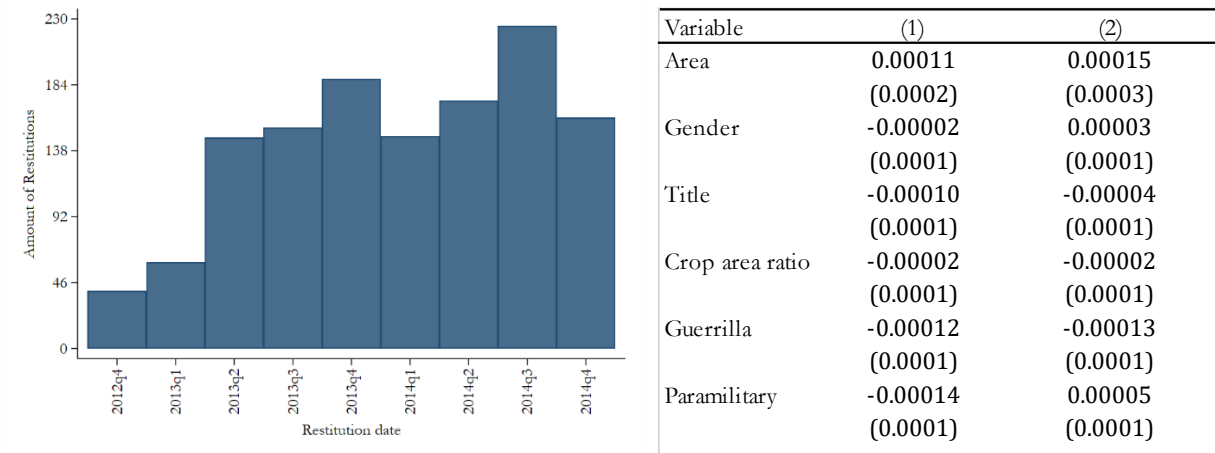
Our aim is to study the evolution of the access to microcredit (and the size of the loan) before and after the land restitution, identifying the change in the outcomes around the quarter of the sentence. Since the program includes an embargo period of two years during which the land cannot be sold, we analyze the effects of the program up to 16 quarters (four years) after the restitution (that is, eight quarters before and after the embargo). We also analyze the access to microcredit up to 16 quarters before the start of the program.

Our identification strategy is based on the event study design introduced by Jacobson, LaLonde and Sullivan (1993). We follow recent applications, such as Bindler and Ketel (2020), Dobkin et al. (2018) and Kuziemko et al. (2018). We define the “event” as the quarter in which the individual is restituted the land (Bill 1448/2011). To identify the effect of the land restitution program, we leverage on the variation from the unanticipated timing of the restitution to isolate the effects of the program on the extensive and intensive margins. Our identification assumption does not hold if a restitution is preceded by anticipated information about the timing of the restitution. In the previous subsections, an illustrative presentation of the institutional procedure has shown why the end date of

each process cannot be strategically manipulated by the victims, thus this is not plausible in the current setting. In practice, land restitution was rolled out over time.

Figure 1 supports the previous arguments. In panel A, it shows that from the second quarter of 2013 until the end of our time window, the distribution of sentences across the quarters is approximately a uniform distribution. Notice that the presence of a take-off period of two quarters is plausible given the implementation of a new law. In addition, panel B reports a test of the balancing of the observable variables extracted from the sentences, across the quarters. We regress several characteristics against the specific date of the restitution. In the Table, the coefficients are standardized subtracting the mean and dividing by the standard deviation. While the test does not exclude differences across unobservable characteristics, the lack of significant effect is consistent with a lack of strategic manipulation.

Figure 1. Robustness tests



Notes: The left-hand panel shows the rolling up of the sentences from the last quarter of 2012 to the last quarter of 2014. The right-hand panel shows the results of a set of regressions of the specific date of the restitution against several covariates available in the sentence. In column (1), we control for department fixed effects, while in column (2) we further control for the year of the dispossession. All the coefficients are standardized in z-score.

The other threat to identification is the possibility that the timing of the land restitution may be correlated with an individual’s probability of access to microcredit before the event; for instance, access to credit may be more common for those who apply early to the land restitution program. We show that the data are not consistent with this hypothesis.

We use the following event study model:



$$(1) \quad \text{Microcredit}_{i,t} = \alpha_i + \gamma_t + \sum_{-16 \leq k \leq 16, k \neq -1} \delta_k * \text{Restitution}_{i,t+k} + \varepsilon_{i,t}$$

where  $\text{Microcredit}_{i,t}$  is an indicator recording whether individual  $i$  got a loan at time  $t$  (but we also use as the outcome the size of the loan),  $\text{Restitution}_{i,t+k}$  is an indicator for whether the individual received a land restitution  $k$  quarters ago,  $\alpha_i$  is an individual fixed effect,  $\gamma_t$  is a calendar time fixed effect and  $\varepsilon_{i,t}$  is the error term. The parameters of interest are  $\delta_k$  which measure the effect of the land restitution before, during, and after the event, conditional on the individual- and time-specific effects.

The term  $k$  indexes the set of time indicator variables starting 16 quarters before the restitution and up to 16 quarters after the event. Since our specification includes a constant and the parameter  $\delta_{-1}$  is excluded, then the estimated  $\delta_k$  parameters are relative to the probability of access to microcredit the quarter before the restitution. We interpret these estimated parameters as the causal relationship between the land restitution program and microcredit. Additionally, the estimated coefficients prior to the event, i.e.,  $k = -2, -3, \dots, -16$ , test whether (or not) the program events are correlated with the probability of accessing microcredits before the restitution. The statistical significance of such coefficients is evidence of the dynamic selectivity of the land restitution program: in the jargon, this would be a violation of parallel trends. In the next section, we show that the estimated effects before the restitution are not consistent with such violation of parallel trends. Finally, since we use a panel of individuals, we cluster standard errors at the individual level for inference.

### 3. Results

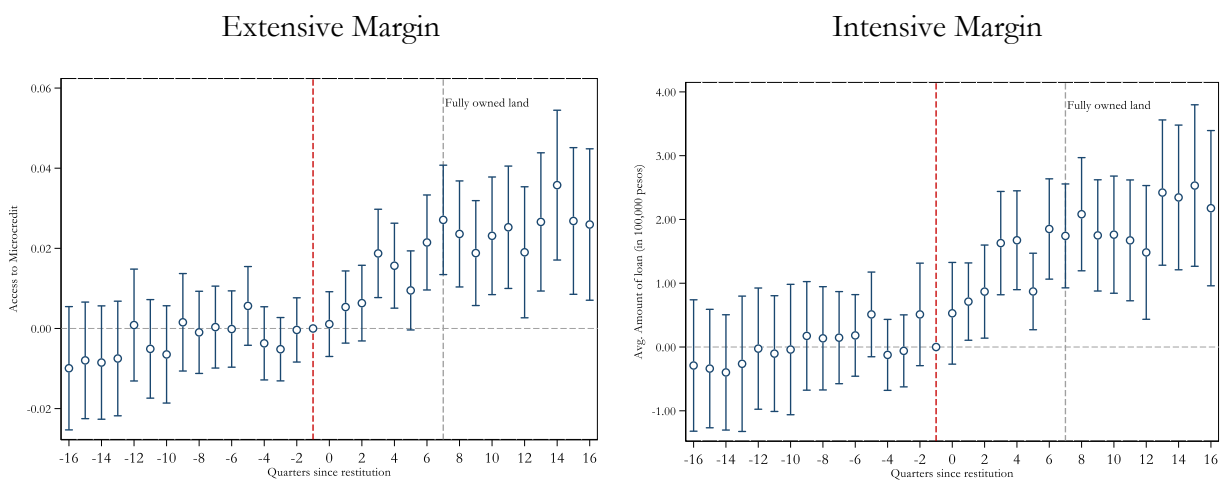
#### 3.1. Main results

This section describes the causal effects of the land restitution program on our main outcomes. In Figure 1, we show the results of the main estimation from the event study. On the x-axis, we report the quarters, centered on the moment of the restitution and covering 16 periods before and after. In the estimation, the omitted category is the quarter leading up to the restitution. In the graph, we plot the separate dummies for each quarter (i.e., the  $\delta_k$  in equation (1)), together with the 95% confidence interval. We also plot two vertical dashed lines to indicate the quarter before the restitution and the quarter before acquiring the full property. The outcome variable is the access to new microcredit in

the left-hand panel (the extensive margin), and the amount of the new loans in the right-hand panel (the intensive margin).

The main finding is that land restitution increases the access to microcredit in both the intensive margin and the extensive margin. While there is no ongoing trend before the restitution (as the entire set of the dummies for the 16 quarters preceding the restitution is not statistically significant), consistent with parallel trends and lack of dynamic selectivity, an effect materializes three quarters after the restitution. The effect increases throughout the embargo period, at which point, it stabilizes and remains statistically significant. Averaging the point estimates over the interval of interest, the effect can be quantified as follows: after acquiring the property, access to formal microcredit increases on average, in the first two years after the restitution, by 1.32 percentage points, and, in the third and fourth year after the restitution, by 2.49 percentage points. Since the mean access to new microcredit in the period before the event was 1.3%, the relative effects are 102% in the first two years and 192% afterwards. Similarly, the average size of the loan of the new microcredit increases by 250% in the first two years and 406% in the subsequent two. This effect size is substantial. The difference in the cumulative effects between the two years after the restitution and the two years after the acquisition of the full property are statistically significant at the 5 per cent level in both the extensive and the intensive margins (respectively  $F=5.64$ ,  $p=0.018$ ;  $F=6.00$ ,  $p=0.014$ ). As we already mentioned in the introduction, the average loan classified as microcredit is worth 3.6 million COP (Estrada and Hernández, 2019), while in our data the average loan issued to a restituted is about one and a half times larger (5.6 million COP).

Figure 2. Main Results: Causal Effects of Restitution on the Access to Microcredit

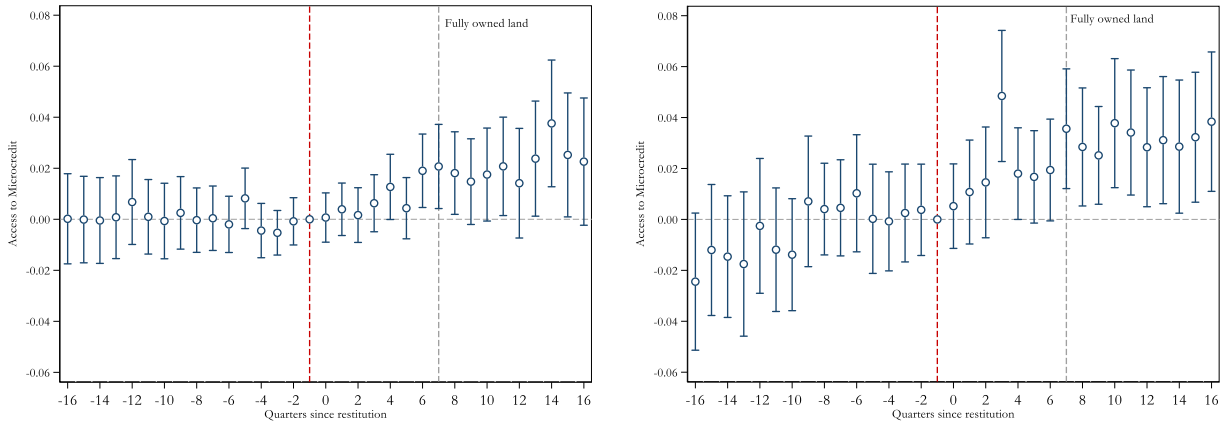


Notes: The left-hand panel shows the probability of obtaining a new microcredit in quarter  $-16 \leq t \leq 16$  relative to the restitution date. The reference period is  $t = -1$ , which has an average access to credit of 0.013. The right-hand panel shows the average real loan amount in 100,000 Colombian pesos issued in quarter  $-16 \leq t \leq 16$ . The reference period is  $t = -1$ , which has an average of 49,400 pesos, considering an amount of 0 for those who do not get any credit.

### 3.2. Productive project

Having established our main result, we investigate whether the effect is mechanically driven by State support. The Restitution program makes it possible to mandate the State to further help the beneficiaries by providing the financial and operational resources linked to a specific productive project. This is indeed the case for a sizable share of the sentences (75.5% of the sample). In Figure 3, we plot the result discriminating between those sentences that include an obligation for the State to provide a productive project. When we restrict the analysis to those that did not benefit from such provision, the impact of restitution is maintained.

Figure 3. With Productive Project vs Without Productive Project



Notes: The left-hand side shows the probability of getting a new microcredit in quarter  $-16 \leq t \leq 16$  when the sentence includes a productive project. The average microcredit access for the reference period is 0.014. The right-hand side shows the microcredit access when the sentence does not include a productive project. The average microcredit access for the reference period is 0.010.

### 3.3. Collateral

We now explore the information over the guarantee for the loan. To begin with, we check for a standard collateral effect. Since the property may act as a guarantee for the bank, there are potential complementarities between land and capital. The restitution program imposes an embargo period that

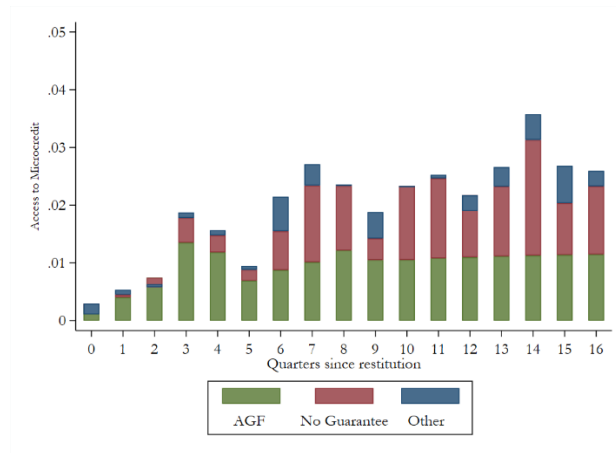
prohibits the use of the land as collateral within the first two years of the restitution. After that, the victim acquires the full property of the restituted land, including the right to sell. We exploit this additional variation to test this hypothesis. In addition, we look for potential complementarities between the restitution program and a public program called the *Fondo Agropecuario de Garantías* -AGF, a government agency created in 2010 that provides insurance to financial institutions for agricultural investments. The AGF covers all small agricultural producers in Colombia, and it is an independent institution unrelated to the land restitution program.

We report the results of these analyses in Figure 4. Once we defined a set of subcategories that are mutually exclusive and exhaustive (in this case, the AGF, *other assets* including the real guarantees, personal properties or other assets; and third, no guarantee), we can estimate equation (1) on an indicator variable that takes the value of one for each sub-category and zero otherwise. The sum of these coefficients from the auxiliary regressions adds to the main effect for each period by construction and provides us with an estimation of the relative contribution of each subcategory to the coefficient. Then, we can plot the coefficient from the main regression, but decomposed it in each component. In Figure 4, we present the decomposition by type of guarantee associated with the loan. On the x-axis, we consider the 0-16 quarters; that is, the post-restitution period. Notice that most loans do not come with a real guarantee, neither in the first two years nor after the eighth quarter. In fact, the category of other assets, which include the real collateral, explain at most 11.07% of the loans in the first seven quarters, and at most 8.44% in the following two years.

On the contrary, in the decomposition exercise in Figure 4, we found that most of the loans come with support from AGF. This class of guarantee explains 61.64% of the effect in the first two years, and 44.66% in the following two. This decomposition suggests the existence of a complementarity of restitution with this additional policy instrument.

Notice that this line of guarantee via the AGF is associated with standardized conditions in terms of interest rate (it is capped at two hundred basis points above the reference interest rate in the credit market). Unsurprisingly, when we estimate the impact of restitution on the interest rate negotiated on the loan, we did not detect any effect.

Figure 4. Decomposition by Source of Guarantee



Notes: The figure shows the decomposition of the probability of obtaining a new microcredit in quarter  $0 \leq t \leq 16$  by type of guarantee of the credit. The first category corresponds to the Fondo Agropecuario de Garantías (AGF). The second category includes credits without guarantees or no valid guarantees. Other types of guarantees include personal assets, deposits, letters of credit, among others.

### 3.4. Other heterogeneous effects

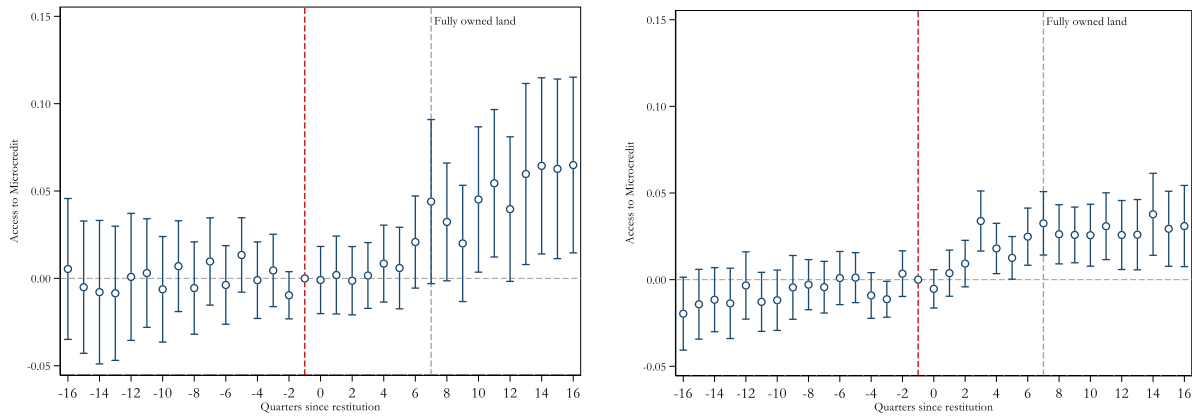
We further explore the implications of the results, exploiting some of the heterogeneity of the rich administrative data of this project. To begin with, every sentence includes a description of the context that has paved the way to the dispossession or forced abandonment and indicate the name of the illegal actor(s). In our codification, we distinguish between paramilitary groups (the largest promoter of displacement, in our sample), and guerrilla groups (largely FARC-EP). We consider guerrilla and paramilitary groups as they represent 70.6% of total dispossessions (23.3% are ascribed to the FARC-EP and other illegal guerrillas and 47.3% to paramilitaries). Results are plotted in Figure 5, Panel A. Again, these graphs follow the same format as previous exhibits. Notice on the left graph in Panel A, how the confidence intervals expand, for the case of the guerrilla. This suggests some caution in making inferences as we may be lacking the necessary power. Nevertheless, at least qualitatively, the effect holds the same when we consider the two actors separately. These two graphs document that the growing access to microcredit from the moment of restitution onwards is independent of the original perpetrating actor. This is important as these illegal armies were in competition for territorial control, each with its own basin of influence. This finding suggests that effectiveness is not restricted to a particular region.

We also consider heterogeneity according to the size of the plot. The evidence from Panel B of Figure 4 shows that access to credit does not discriminate by the size of the restituted plot. We split

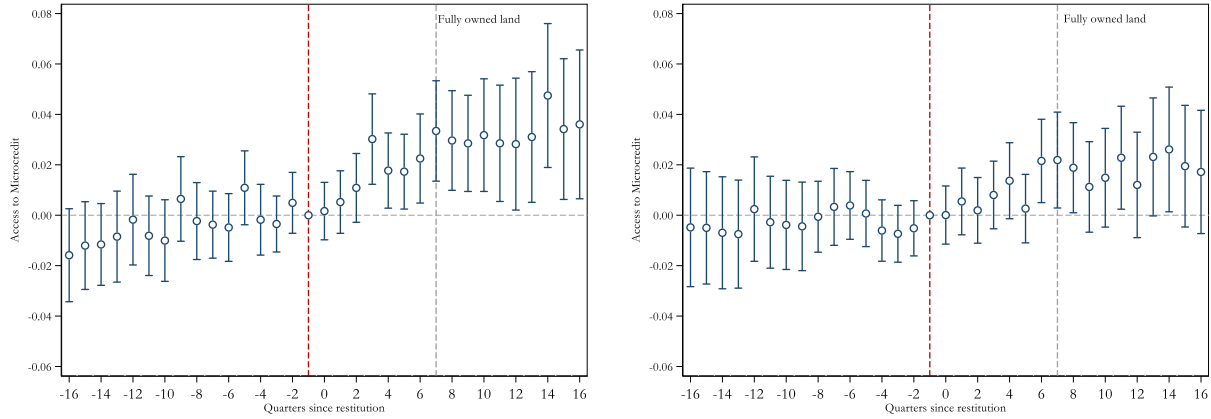
the sample between large and small using the median of the distribution of the parcels restituted. The results for the small parcels are slightly less stable but qualitatively the same. As we mentioned in the introduction, the literature on land titling always points to the existence of a threshold effect, as the banking system tends to be biased against the small producers. The fact that this does not hold in our setting is important and deserves further investigation. Notice that 5 Ha is not a meaningful definition of a large plot, but again, this has to do with the targeted population of this law, and *a fortiori* strengthens our argument that size does not drive the results in this case. It is worth mentioning that for both Panel A and Panel B, the intensive margin (average size of the loan) produces the same pattern.

Estimating a gradient of the effect along other observable dimensions does not produce differences either. The effect is similar for women and men and for different regions. Not even the nature of the possession, whether it is a formal title or a de facto possession seems to matter. This last variable refers to the title that is used to prove the relationship with the plot, which could be a formal register or could be a durable occupation of the land.

Figure 5. Heterogenous effects on perpetrator group and plot size  
 Panel A. Victims of Guerrilla versus Victims of Paramilitary Groups



Panel B. Plot Size over 5 Ha vs equal or less than 5 Ha



Notes: The left-hand side of Panel A shows the probability of getting a new microcredit in quarter  $-16 \leq t \leq 16$  when the perpetrator is defined only as guerrilla. The average microcredit access for the reference period is 0.016. The right-hand side of Panel A shows the probability of getting a new microcredit when the perpetrator is defined as paramilitary groups. The average microcredit access for the reference period is 0.015. The left-hand side of Panel B shows the probability of getting a new microcredit when the total area of the land is greater than the median (5 Ha). The average microcredit access for the reference period is 0.012. The right-hand side of Panel B shows the probability of getting a new microcredit when the total area of the land is smaller or equal to the median (5 Ha). The average microcredit access for the reference period is 0.015.

#### 4. Concluding remarks

Over the last decade, Colombia has undergone significant efforts to recognize and repair victims of the large internal conflict. An important milestone has been the approval of Bill 1448/2011 (*Ley de Víctimas*), which has adopted the international principles for the protection of IDPs, recommending land restitution. Restituting and securing property rights for the dispossessed aims at overcoming the trauma and invisibility of the victims and helps to restore their life's projects. From an economic point of view, victims can now be the residual claimants of their effort, and this should help to motivate the decision to invest and capitalize on their assets.

Using rich administrative data, we show that this is the case, victims are more likely to have access to credit and are more likely to receive larger loans. Although the effects are evident in the semester following the restitution, once two years have passed, when the restituted persons acquire full property rights, the effects become almost twice as large.

Our findings also imply that complementarities exist between restorative justice and specifically designed financial institutions for small farmers.

This work may have interesting implications for both titling and microcredit. There has been a large body of work studying land titling in rural areas (Besley and Ghatak, 2010). Restoring disposed and forcedly abandoned rural properties establishes and enforces rural property rights. In fact, given that formal titles represent no more than 36 per cent of rural households (Varela, 2019), land restitution changes the former condition of tenure and weak protection (Meertens, 2016). The literature on security tenure and investment in rural Africa has not provided convincing evidence, since documented positive effects (Besley, 1995; Goldstein and Udry, 2008; Ali et al., 2014; Goldstein et al., 2018) coexist with null results (Fenske, 2011; Huntington and Shenoy, 2021). In contrast, the evidence in the urban area is quite compelling (Galiani and Schargrotsky, 2010).

Finally, our article contributes to the literature on microcredit. Most of the existing research in microfinance in developing countries has shown that microentrepreneurs are reluctant to take out microcredits despite the high rates of return (De Mel, McKenzie, & Woodruff, 2008). The behavioral economics' understanding is that present bias and cognitive load determined by the exposure to poverty may be the candidate causal explanation (Mani et al., 2013; Haushofer and Fehr, 2014). This should hold *a fortiori* for IDPs, who are both poor and victims of violence, whose cognitive effect is like poverty's (Bogliacino et al., 2017). By causing an increase in uptake by beneficiaries, the restoration of property rights and a positive wealth shock counteract some of the negative bandwidth effects.



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