Skin in the game: labor shocks and preferences for redistribution in Colombia

Juan David Torres*

Universidad de los Andes

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Abstract

How do labor shocks shape preferences for redistribution and political preferences? I this paper I show how individuals exposed to job loss on their households augment their preferences for a major role of the government in reducing the gap between the rich and the poor and for redistributing income to the latter. However, this shift in redistributive preferences does not imply a change in political preferences, since individuals do not modify their position in the political spectrum. I further find that there seems to be a shock-smoothing mechanism in which rich individuals shield themselves and compensate the shock in a way that their redistributive preferences do not change. On their behalf, the poor seem to change their preferences, at least by shifting towards inequality aversion. This non-complementarity between redistributive and political preferences suggests that the polarization of opinions covers conflicts that are not solved in the ballot box and that governments should aim to expand their safety nets to mitigate exposition to job loss.

Keywords: Redistribution, Social mobility, Labor shocks
JEL Classification: D31, D72, P16

*Faculty of Economics, Universidad de los Andes. Email: jd.torres15@uniandes.edu.co. Calle 19A No. 1-37 Este Bloque W., Bogota (+571) 3394949.

1 Introduction

Social mobility and income inequality are important topics in developing countries. On the other hand, recent electoral outcomes across the globe have led researchers to think how, in the context of globalization, these can be explained by the paths that inequalities have taken during the last decades. In particular, it is discussed how several perturbations of the income distribution and thus, the transit of households from one social position to another, could be guiding their way of thinking and necessarily, of voting. This discussion becomes relevant in a country such as Colombia, in which the political agenda has been combined with growing polarization. The country has been divided between opposed and strongly opinionated political trends with little space for the center of the political spectrum. Recent elections and a peace referendum are an example of it.

In this sense, it is of interest to analyse if the economic landscape faced by individuals could shape these opinions and voting outcomes. Even though there is ample evidence of the relation between shocks –especially violent and conflict-related ones– and migration decisions, economic aspirations and reconciliation preferences (Moya, 2018; Fergusson et al. 2018), little has been said about the role played by shocks on economic conditions explaining political preferences, at least in developing countries.

This way, embracing a framework of winners and losers allows to explore how individuals think and vote as they face what nature holds for them; this is, different shocks that alter the income distribution. These shocks modify the expectations of individual upon their economic future and their relative position in societal hierarchies. Moreover, they give them an idea of how permissive or not are current institutions and social norms in face of these perturbations. In addition, it is of interest how the reaction to these shocks behaves across the income distribution, since the rich can shield themselves and maintain their political positions unchanged. This gives an insight of which part of the income distribution mobilizes its opinions upon shocks, with important implications for the functioning of democracy. Specifically, I will refer to negative labor market shocks, which can be more easily identified.

In this paper, I study how labor shocks affect preferences for redistribution and political preferences. The former account for views on the role that the government should play in the economy to reduce inequality and how much should it redistribute income to the poor. The latter simply accounts for the conventional political spectrum divided by the left, the center and the right. On their behalf, labor shocks are broadly understood as a member of the household losing their job. In particular, I use the Colombian Longitudinal Survey (ELCA) to compare preferences of individuals subject to these shocks with the ones of those not affected in two periods of time using differences in differences. I further propose a shock-smoothing mechanism using wealth quintiles to explore which individuals can dodge –with their preferences– these shocks.

I find that labor shocks increase individual preferences for redistribution and make them more inequality averse; however, these shocks do not seem to shift political preferences to either side of the spectrum. This is, facing unemployment in the household –including their own unemployment– makes individuals more willing for the government to take action to tackle inequality but not more eager that changing their current preferred political party can make it possible. Moreover, I verify the shock-smoothing mechanism showing that preferences for redistribution are mostly modified in the lower part of the wealth distribution, possibly due to their inability to settle themselves after the shock. This results are robust to some proposed checks; however, due to lack of political data in the survey before 2013, there is no possibility of formally testing the parallel trends assumption.

This question becomes important within the framework that studies the determinants of the demand for redistribution and political preferences of individuals. Traditionally, empirical research on this topic has addressed three lines: the one of Meltzer and Richard (1981) argues that preferences for redistribution are reduced as income grows. In this sense, the poor will support redistribution because they are the immediate recipients of it. Nevertheless, this idea assumes that poor individuals only act based on their current income, limiting the possibility of future beliefs. Particularly, Fong (2001) showed the limitations of this view. In this sense, Bénabou and Ok (2001) proposed the upward-mobility hypothesis (POUM), which argued that it is possible that if individuals, even being below average income, expect high social mobility in the future, they can reduce their demands for redistribution. This idea is built in the conceptual framework brought by Piketty (1995) in which beliefs on future income are key to shape preferences in the present. This idea has been backed up by authors such as Siedler and Sonnenberg (2012). Finally, Alesina and Angeletos (2005) introduced a social justice view in which preferences are built according the the weight that individuals give to exogenous elements such as initial endowments and luck relative to endogenous factors such as individual effort. These hypotheses have been exhaustively addressed in Gaviria (2007) and Silva and Figueiredo (2013).

Studying the effects of labor shocks on redistributive and political preferences allows to explore the channels through which these preferences are formed and, thus, to offer causal evidence regarding this relation, a difficult task to achieve while assessing these preferences from social mobility expectations and established positions. This way, exploiting variation on labor market shocks leads to identify their effect on preferences. In this sense, this paper contributes to literature that tries to link social mobility perspectives to preferences for redistribution, such as Londoño (2011). Moreover, the paper frames inside the literature that aims to recover causal effects regarding these preferences from experimental designs (Alesina and FUchs-Schundeln, 2007; Di Tella et al. 2007; Ahok, et al. 2015; Alesina, et al. 2017; Londoño, 2016). Additionally, the paper contributes to lit-

erature that determines the impact of economic shocks on political preferences (Brunner et al. 2011; Guiliano and Spilimbergo, 2009; Schoch, 2017).

This paper is organized as follows. The next section presents data and some descriptive statistics. Section 3 presents my empirical strategy and discusses identification. Section 4 presents my results and account for robustness and mechanisms. Section 5 concludes.

2 Data

I use the Colombian Longitudinal Survey (ELCA), which delivered three follow-ups to approximately 10.400 rural and urban households in Colombia. I use the 2013 and 2016 issues, which account for questions regarding redistributive –inequality, government and personality– and political –political trend– preferences for household leaders and their partners; in this sense, the sample is restricted to individuals between 20 and 65 year for a total of 17.832 observations in a balanced panel.

While analysing preferences for redistribution, I account for their direct dimension, which is captured by the following inequality-related question: *The government should put in practice sound policies to reduce inequality between the rich and the poor?*. The variable that answers this question is coded in an ordered way (totally not agree, not agree, agree, totally agree). However, due to limitations to manipulate ordered data in a two period context, I decide to translate it to whole values –1 to 4, correspondingly– and standardize it. This exercise allows to incorporate this variable with two other questions that capture an implicit dimension of preferences for redistribution. The first asks for agreement with the following sentence: *the government is the first responsible of assuring people's welfare* and *each individual is responsible of its own welfare*. I replicate the manipulation procedure for these variables; however, I build the latter with an inverse scale to align its message with the rest: higher values of this variable imply a larger preference for redistribution. With these variables I build an aggregate index of preferences for redistribution that takes the average of the standardized values of the variables. On its behalf, political trend is coded between 1 and 5 (right, center-right, center, center-left and left). Those who do not identify their political trend receive a missing value.

On the other hand, I gathered data on shocks faced by the household to which each individual belongs. In particular, I focus on shocks that disturb economic conditions of the household, which in fact have a potential to affect political preferences of individuals. In this sense, the paper focuses on labor shocks defined by the survey in the following situations: *(i) The household leader lost their job; (ii) Its partner lost their job; and (iii), another member of the household lost their job.* The survey allows to explore these shocks and extract their impact level (high, medium and low) and the number of times that they were received in a period of time.

Additionally, I use wealth quintiles, which allow a better precision relative to subjective income measures in the survey. Finally, I count with a set of observable individual characteristics, such as age, sex, years of education and civil status.

In this sense, the sample is split between a group of treated individuals and control ones. The first are denominated as *Shocked* and correspond to those who *did not* receive a labor shock in 2013 or before but *did* receive one between 2014 and 2016. The second group, *Not shocked*, gathers those who did not receive a labor shock in *any* period.

Table 1 presents some descriptive statistics of the principal variables of interest for treated and control groups in 2013 (before) and 2016 (after). This way, it shows the behavior and differences in observable characteristics between both groups in two time periods. This is done for general impact shocks and high and medium impact ones. Note that the variable *number of shocks* is only built for the after period –individuals receive, on average, 1,3 and 1,5 general impact and high and medium impact shocks, correspondingly–, because the number of shocks accumulated during the before period is irrelevant for these exercises since individuals that receive shocks during this period are cut from the sample.

It is shown that in the before period, when no group received labor market shocks, there are no statistically significant differences in redistributive, inequality-related and political preferences between both groups for any type of shock. However, once the treated group receives a shock –in the after period– it presents a higher preference for the government closing the gap between the rich and the poor, more support for redistribution –given a larger role of the government, and not individuals, providing welfare– and a major leaning towards the left of the political spectrum[1]. These differences are larger in the case of shocks of medium and high impact. Additionally, there are some statistically significant differences in some characteristics between both groups: the treatment group is, on average, more educated, younger and wealthier. These systematic differences should be accounted for while comparing both groups since they can be related to unobservable characteristics that can shape preferences after a labor market shock.

3 Empirical strategy

As it was mentioned, I will use differences in differences to evaluate the effect of labor shocks on preferences over inequality, redistribution and political trends. This, since only comparing the preferences of individuals that receive a shock, before and after receiving it, using a difference of means test, can be contaminated by other events that can happen around the shock that could alter preferences. On the other hand, comparing the

[1]This is interpreted in these terms since variables are standardized in a way that larger values of them are traduced into a major support for the role of government in redistribution, closing gaps and leaning to the left.

preferences of those that receive a shock with those that did not, using ordinary least squares on *ex post* data can lead to biased estimations since these individuals can differ systematically in a way that affects both their exposure to the shocks and their political preferences. Moreover, it could be that people's preferences make them decide in a way that they are more exposed to labor shocks, inducing a problem of reverse causality.

Additionally, one can not study any type of shock, since these have different levels of exogeneity for individuals, not only related to the exogeneity of the shock itself –e.g., suddenly being a victim of violence, bankruptcy or a flood– but to the exposition or propensity to receive a shock –e.g., people with certain characteristics, systematically different to the ones of other people, are more prone to be victimized or face natural disasters, which limits identification–. In this sense, I focus on labor market shocks related to unemployment of a member of the household, which I consider more exogenous and less predictable than other type of shocks such as violence or natural disasters. This assumption comes along with thinking labor shocks of the household more exogenous than those that accrue to one individual, since they are far beyond control than an individual shock. Controlling for some observables can account for the exposition of the individual and its household of losing their job. Moreover, it should be considered that a change in the preferences of and individual due to these shocks can obey to the hope that the government or a policy change can limit the exposition to them and their impact.

I propose a difference in difference specification in differences (not levels), following Bernal and Peña (2012). This is done by transforming panel data, in which each observation corresponds to and individual in one period, to a "wide" matrix in which each observation corresponds to an individual for which one observes its characteristics in each period. This way, one can build a variable that represents the *change* in the dependent variable, which is estimated as a function of a treatment indicator –which was defined in the previous section– and a series of observable characteristics of the before period in *levels*.

In particular, I estimate the following model

$$\Delta Pref_i = \beta_0 + \beta_1 Shocked_i + \delta X_{i1} + \varepsilon_i$$

in which $\Delta Pref_i$ corresponds to the change in preferences of the individual i between 2016 and 2013, the after and before periods. This could be according to the inequality, redistribution or political preference measures. β_0 is a constant term and X_{i1} is a set of observable characteristics for the before period –1–, such as sex, civil status, years of education and age, which control for differences in the *change* of preferences between treated and control individuals as the shocks are received (e.g., if someone marries, grows up or studies more can modify its positions, independently of receiving a shock) (Bernal and Peña, 2012). As it was mentioned before, controlling for these variables also accounts

for the exposition to a labor market shock that can be explained, for example, by years of education. This builds on the assumption that these variables are not affected by labor market shocks, which can be strong in the case of civil status or years of education. Anyway, it is assumed that if the shocks could affect these variables, this would require more time that the one that is contemplated in the sample (e.g., losing a job does not immediately lead to divorce or to dropout college but to smoothing the shock). I present estimates with and without controls. The variable $Shocked_i$ defines if the individual belongs to treatment –received shocks between 2014 and 2016 but not before– or to control –did not receive a shock in any period–. Finally, ε_i is a term that accounts for the change in errors $u_{i2} - u_{i1}$.

The coefficient of interest is β_1, representing the effect on the change of preferences of the individual that received labor market shocks *only* after 2013 relative to the change in preferences of individuals that never received these shocks. To have an unbiased estimate, I need that the change in the difference of the errors between the before and after period is not related to the fact of receiving the shocks; this is, $E[\varepsilon_i|Shocked_i] \neq 0$.

This assumption can be violated if parallel trends do not hold. This is, that in absence of the shock the preferences of shocked and not shocked individuals would trend differently. In this sense, I need that there is no element in the error terms that leads the natural trend of the preferences of both groups to move towards different directions. A way to verify this assumption is to use past data in which I would compare the evolution of the preferences of both groups before 2013, when the shocks were established. However, the political module of the survey, from which I recover my dependent variables, arrived in 2013, thus impeding the verification of the assumption.

Data presented on Table 1 gives an indication than, in absence of shocks, the preferences of both groups present no statistically significant differences and that any difference seems to activate with shocks. Moreover, since the sample is restricted to individuals that in 2013 *or before* did not receive a shock. this leads to conservative estimates since they only account for individuals that differ in receiving or not a shock after 2013 but that before did not have any shocks (between 2010 and 2013). I add estimations of the intensive margin with shocks according to their impact and the number of times they are faced.

4 Results

Table 2 presents the results of the baseline specification changing two dependent variables –change in standardized inequality or redistribution preferences– and three treatment variables –received labor shocks, received high and medium labor shocks and the number of labor shocks after 2013– with and without control variables. Even though the inclusion of controls reduces the magnitude of the estimates, it is generally observed that receiving

labor shocks augments preferences for the role of the government in reducing inequalities and a major redistribution relative to those that do not receive shocks. Note that this effect is larger for high and medium impact shocks.

Specifically, the effect of a labor shock over the preference for the role of government in reducing inequality relative to those who do not receive shocks is 0,12 standard deviations larger and significant at the 5% level. For high and medium impact shocks this effect is raised to 0,16 standard deviations. If one accounts for the number of shocks, it is observed that an additional high or medium impact shock has an effect of 0,9 standard deviation over the inequality variable. These effects are significant at the 1% level. In the case of preferences for redistribution, the effects are similar, of 0,12, 0,15 and 0,07 standard deviations, correspondingly for each of my shock measures and statistically significant at the 5% and 1% levels. These effects have an important magnitude, specially the one of the additional shock, since the mean dependent variables in the before period for the treated are -0,02 and -0,01, correspondingly. Note that this means that in absence of shocks these individuals seem to be neutral over inequality and redistribution.

Table 3 presents the results of the same specifications changing the dependent variable to the standardized political trend. It is observed that, for every specification, with and without control variables and for each treatment measure, labor shocks do not have a significant effect over political trends relative to individuals that are not shocked. In this sense, labor shocks augment preferences for redistribution and an active government that reduces inequality but do no significantly alter the political trend of individuals.

4.1 Robustness

The limits of data in the survey, even restricting the sample to individuals without labor shocks before 2013, does not allow to formally test for parallel trends. However, the specification is robust to the inclusion of control variables that could alter the trend of the preferences of individuals before shocks and contaminate their effects. There are also no statistically significant differences in the dependent variables before shocks in both groups.

Anyway, and recognizing the limitations of these exercises regarding the parallel trends assumption, I propose a robustness check to verify the relevance of labor shocks to explain the change in preferences. As it was argued before, not every shock is exogenous but also not every shock is valid to explain the change in preferences. Because of this, I run a placebo exercise in which the treatment is changed to one defined by the shock of the arrival of a family member to the household and a cut of remittances. These shocks, included in the survey, are also more exogenous than other shocks –such as violence or natural disasters– in the sense that are less predictable –e.g., the arrival of a family member or cutting remittances obey to external elements not necessarily related to the

characteristics of an individual–. However, one do not expect that these shocks modify the preferences of an individual since it should not aspire that an answer of the government or a policy change could limit the exposition or the impacts of these shocks –e.g. that a government redistributes more should not make less family members to arrive or that more remittances are sent continuously but it can compensate or shield the effects of job loss with instruments such as employment subsidies or public employment–.

Table 4 presents previous specifications using remittance cuts' shocks as a placebo as one would not expect that this shock would alter the preferences of interest. Effectively, this is the case for each dependent variable –inequality, redistribution and political trend–. The only exception seems to be Column 8, in which the effects of an additional remittance cut shock *reduces* preferences for redistribution in 0,03 standard deviations, at the 10% level. On its behalf, Table 5 shows the same placebo exercise with a shock of the arrival of a family member to the household, which shows no effect for inequality and is less robust for the political trend (Column 3 presents positive and significant effects at the 10% level, a shift to the left in the political spectrum). The same happens with preferences for redistribution upon a high or medium impact family member shock. However, in general, it seems that these shocks, which should not produce major hopes that the government play an important role to mitigate them, do not affect preferences.

I also check how much of my results can be a product of chance. I use randomization inference by simulating a placebo distribution. This is done by randomizing the treatment variable –Shocked– and running the baseline specification 800 times. Figure 1 shows that my estimate is close to the tail of this simulated distribution: 0,3% of random treatment draws for inequality preferences as a dependent variable and 0,2% for redistribution are above the actual estimates of 0.123 and 0.120.

4.2 Shock-smoothing mechanism

A possible mechanism behind these results could be the capacity that individuals have to smooth labor market shocks that is given by their accumulated wealth. In this sense, the poorest individuals, with less endowments to smooth out the loss of jobs in the household could change, in a larger extent relative to the rich, their preferences for redistribution and for the role of the government to reduce inequality. Thus, these would be the voters that could drive the demands for more coverage from the government to mitigate these shocks.

This way, I propose an exercise in which I add a series of interactions between the treatment status and an inverse quintile of wealth[2]. To avoid multicolinearity, I exclude the first inverse quintile –the richest–, thus coefficients of interest are interpreted, not

[2]Quintiles are inverted to ease the interpretation of the interactions, where higher quintiles represent *less* wealth.

only relative to those who do not receive a shock but relative to rich individuals. Table 6 presents these results. It is observed that the poorest quintiles that receive labor shocks augment their preferences for a major role of the government in reducing inequality 0,34 standard deviations, significant at the 10% level. This effect augments with high impact shocks. However, it is observed that this is not the case for the poorest quintile regarding the index of preferences for redistribution and is negative and significant for the second most poorest quintile and positive and significant for the richest –in the specification with number of shocks–. This way, the smoothing mechanism is verified for preferences on inequality reduction but not necessarily for the index of preferences for redistribution, in which the poorest do no give the government a major weight in welfare provision after receiving a shock relative to the rich and to those who do not receive shocks.

5 Conclusion

This paper studies the effect of labor market shocks over preferences for redistribution and political trends. Using a longitudinal survey and differences in differences I compare individuals that receive these shocks *only* after 2013 against those that do not receive them in any period covered by the survey. The foremost limitation of this exercise is the lack of data to verify that parallel trends hold. In general, I observe that labor market shocks augment preferences for redistribution and for a major role of the government in reducing inequality but do not alter the political trend of individuals. Moreover, I verify a shock-smoothing mechanism in which poor individuals shift their preferences in face of a shock in a larger extent that the richest ones. This seems to be the case for the role of the government in reducing inequality. I show that these results are robust to randomizing the treatment status and to changing the shock for a placebo one.

These results suggest a paradox in which individuals raise their demands for a major role of the government in closing social gaps and providing welfare when they are exposed to labor market shocks but they do not change their voting behavior. In this sense, it is possible that popular demands that are not addressed by the government lead to major unrest and polarization of political opinions. However, this unrest would not manifest itself in the ballot boxes but in other types of conflicts and social tensions which result much more hard to solve. This way, governments should try to shield citizens, especially the poorest, upon potential labor shocks. This should be done through active labor market policies that expand protection safety nets. However, governments, particularly those in developing countries, should be aware of how they can expand and finance this protection without broadening the incentives to labor informality.

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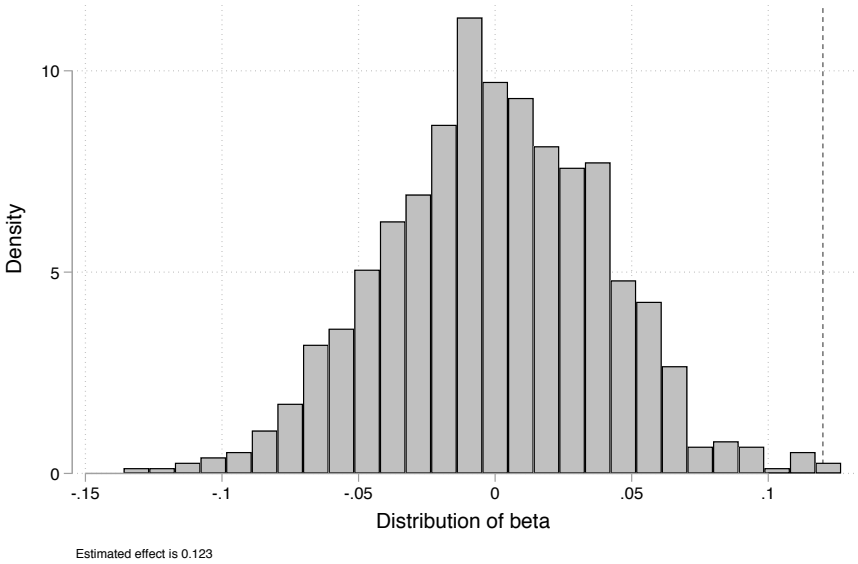
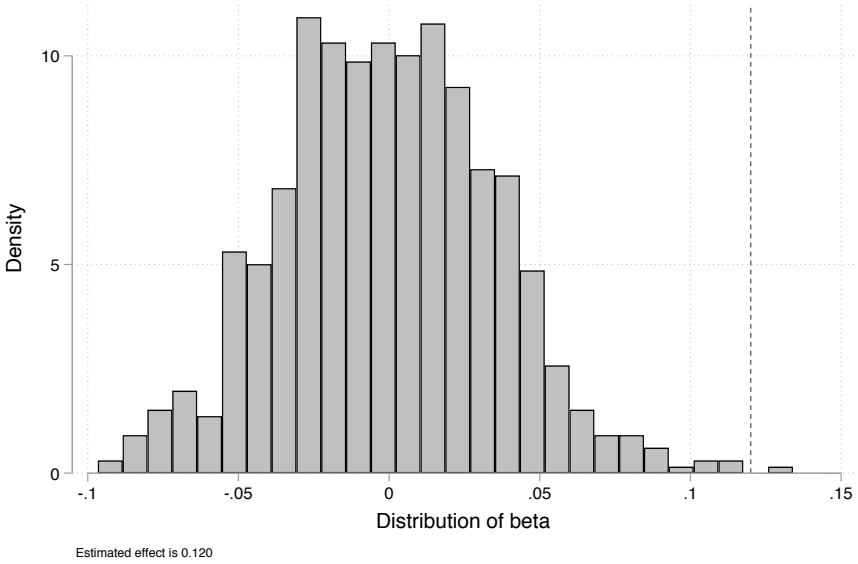
Figures

Figure 1: Randomization inference



(a) Inequality (0.3%)



(b) Redistribution (0.2%)

Notes: The figure plots randomization inference tests at the treatment level (randomizing the shock assigned to individuals) for both inequality and redistribution variables. I run 800 simulations of a placebo distribution of my coefficient of interest, which in my main specification is estimated to be 0.123 and 0.120, correspondingly. For this estimate to be a product of something different from chance, it should be –in absolute value– close to the end of the right tail of the placebo distribution.

Tables

Table 1: Descriptive statistics

	General impact						High and medium impact					
	Before			After			Before			After		
	Shocked	Not Shocked	Difference	Shocked	Not Shocked	Difference	Shocked	Not Shocked	Difference	Shocked	Not Shocked	Difference
Number of shocks				1.266	0	-1.266***				1.526	0	-1.526***
				(1.496)	(0)					(1.517)	(0)	
Inequality	-0.023	-0.011	0.013	0.114	-0.028	-0.142***	-0.041	-0.011	0.03	0.126	-0.028	-0.154***
	(1.057)	(0.991)		(1)	(0.993)		(1.066)	(0.991)		(1.002)	(0.993)	
Redistribution	-0.01	0.003	0.013	0.091	-0.009	-0.101***	-0.006	0.003	0.01	0.107	-0.009	-0.117***
	(0.538)	(0.523)		(0.552)	(0.524)		(0.553)	(0.523)		(0.56)	(0.524)	
Political Trend	1.966	1.894	-0.072	2.123	1.934	-0.189**	1.936	1.894	-0.042	2.153	1.934	-0.219**
	(1.365)	(1.366)		(1.395)	(1.347)		(1.346)	(1.366)		(1.401)	(1.347)	
Quintile	2.965	2.82	-0.145***	3.162	3.099	-0.063*	2.911	2.82	-0.091**	3.101	3.099	-0.002
	(1.435)	(1.403)		(1.429)	(1.383)		(1.435)	(1.403)		(1.429)	(1.383)	
Age	38.662	41.276	2.614***	41.666	44.29	2.624***	38.731	41.276	2.545***	41.733	44.29	2.557***
	(11.862)	(11.719)		(11.861)	(11.725)		(11.737)	(11.719)		(11.733)	(11.725)	
Years of education	11.372	10.102	-1.27***	12.792	10.642	-2.15***	11.39	10.102	-1.287***	12.689	10.642	-2.047***
	(4.085)	(4.188)		(4.811)	(4.588)		(4.07)	(4.188)		(4.604)	(4.588)	
Sex	0.449	0.462	0.013	0.449	0.462	0.013	0.443	0.462	0.018	0.443	0.462	0.018
	(0.498)	(0.499)		(0.498)	(0.499)		(0.497)	(0.499)		(0.497)	(0.499)	
Civil Status	2.402	2.255	-0.147***	2.402	2.255	-0.147***	2.383	2.255	-0.129***	2.383	2.255	-0.129***
	(1.133)	(1.165)		(1.133)	(1.165)		(1.122)	(1.165)		(1.122)	(1.165)	

Notes: *** p<0.01 ** p<0.05 * <0.1. Standard deviations in parentheses. The table presents means differences between the treated group (Shocked) and the control one (Not Shocked) para each variable of interest in the sample. This is done for the before period in 2013 (which involves shocks in 2013 or before) and the after period in 2016 (which involves shocks during 2016, 2015 or 2014). This exercise restricts the sample for individuals who received shocks of low, medium and high impact (general impact) and those with medium and high impact shocks only. Since the number of shocks is only relevant for the after period, it is not reported for the before one (an individual in the before period, treated or not, has no previous shocks in the sample, thus counting its shocks before is irrelevant).

Table 2: Labor market shocks and preferences for redistribution

	Inequality		Redistribution		Inequality		Redistribution		Inequality		Redistribution	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Shocked	0.140**	0.123**	0.133***	0.120**								
	[0.055]	[0.058]	[0.051]	[0.051]								
Shocked (Impact)					0.171***	0.157**	0.162***	0.147***				
					[0.059]	[0.062]	[0.056]	[0.056]				
Shocked (Times)									0.091***	0.094***	0.072**	0.065**
									[0.030]	[0.032]	[0.031]	[0.032]
Age		-0.001		0.002		-0.001		0.002		-0.001		0.002
		[0.002]		[0.001]		[0.002]		[0.001]		[0.002]		[0.001]
Years of education		0.003		0.006		0.003		0.006		0.003		0.006
		[0.005]		[0.004]		[0.005]		[0.004]		[0.005]		[0.004]
Civil Status		-0.019		0.013		-0.023		0.011		-0.019		0.014
		[0.041]		[0.028]		[0.041]		[0.028]		[0.041]		[0.028]
Sex		0.024		0.021		0.025		0.021*		0.023		0.02
		[0.018]		[0.013]		[0.018]		[0.013]		[0.018]		[0.013]
Constant	-0.023	-0.041	-0.028**	-0.215**	-0.023	-0.052	-0.028**	-0.215**	-0.019	-0.046	-0.024*	-0.211**
	[0.020]	[0.120]	[0.014]	[0.084]	[0.020]	[0.121]	[0.014]	[0.085]	[0.020]	[0.120]	[0.014]	[0.084]
R Squared	0.001	0.002	0.003	0.005	0.002	0.002	0.004	0.006	0.002	0.003	0.002	0.005
Observations	4996	4826	2564	2507	4879	4715	2528	2474	4996	4826	2564	2507

Notes: *** p<0.01 ** p<0.05 * <0.1. Robust standard errors in parentheses. The table presents the effects of labor market shocks –defined as job loss of a member of the household– for the treatment group on their preferences for redistribution relative to the ones of the group that does not receive a shock. I present estimates with a dichotomous treatment –shocked with general impact and shock with high and medium impact– and a continuous one –number of shocks between 2014 and 2016–. The sample is restricted to individuals who did not receive a shock in 2013 or before. Dependent variables are presented as standardized changes. An augment over the variable inequality implies a major preference for a government that promotes policies to reduces the gaps between the poor and the rich. For redistribution, an augment in this variable implies a major score in the index built with the inequality variable and two other variables that account for the weight individuals give to the government and the individual in the responsibility for its welfare. Estimates include and exclude control variables.

Table 3: Labor market shocks and political trend

	Political trend					
	[1]	[2]	[3]	[4]	[5]	[6]
Shocked	-0.074	-0.091				
	[0.100]	[0.104]				
Shocked (Impact)			-0.104	-0.116		
			[0.109]	[0.112]		
Shocked (Times)					0.026	0.024
					[0.071]	[0.075]
Age		0.003		0.004		0.003
		[0.004]		[0.004]		[0.004]
Years of education		0.001		0.003		0
		[0.008]		[0.008]		[0.008]
Civil status		0.201***		0.217***		0.204***
		[0.070]		[0.071]		[0.070]
Sex		0.019		0.021		0.018
		[0.031]		[0.031]		[0.031]
Constant	0.038	-0.25	0.038	-0.305	0.023	-0.261
	[0.037]	[0.224]	[0.037]	[0.226]	[0.036]	[0.225]
R Squared	0.001	0.01	0.001	0.013	0	0.01
Observations	1054	1013	1032	993	1054	1013

Notes: *** p<0.01 ** p<0.05 * <0.1. Robust standard errors in parentheses. The table presents the effects of labor market shocks –defined as job loss of a member of the household– for the treatment group on their political preferences relative to the ones of the group that does not receive a shock. I present estimates with a dichotomous treatment –shocked with general impact and shock with high and medium impact– and a continuous one –number of shocks between 2014 and 2016–. The sample is restricted to individuals who did not receive a shock in 2013 or before. Dependent variables are presented as standardized changes. An augment over the variable political trend implies a movement over the political spectrum towards the left. Estimates include and exclude control variables.

Table 4: Remittances shocks, preferences for redistribution and political trend

	Inequality	Redistribution	Political Trend	Inequality	Redistribution	Political Trend	Inequality	Redistribution	Political Trend
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Shocked	-0.11	-0.046	0.095						
	[0.141]	[0.095]	[0.264]						
Shocked (Impact)				-0.128	-0.105	0.211			
				[0.164]	[0.116]	[0.244]			
Shocked (Times)							-0.039	-0.036**	0.035
							[0.044]	[0.015]	[0.024]
Age	-0.001	0.002	0.002	-0.001	0.002*	0.002	-0.001	0.002	0.003
	[0.002]	[0.001]	[0.003]	[0.002]	[0.001]	[0.003]	[0.002]	[0.001]	[0.003]
Years of Education	0.006	0.009**	0.002	0.006	0.009**	0.003	0.006	0.009**	0.002
	[0.004]	[0.004]	[0.007]	[0.005]	[0.004]	[0.007]	[0.005]	[0.004]	[0.007]
Civil Status	-0.013	0.008	0.129**	-0.012	0.008	0.124*	-0.012	0.013	0.124*
	[0.037]	[0.027]	[0.064]	[0.038]	[0.027]	[0.064]	[0.038]	[0.027]	[0.066]
Sex	0.019	0.021*	0.014	0.02	0.022*	0.013	0.016	0.02	0.008
	[0.016]	[0.012]	[0.028]	[0.016]	[0.012]	[0.028]	[0.017]	[0.012]	[0.029]
Constant	-0.077	-0.245***	-0.169	-0.082	-0.249***	-0.16	-0.076	-0.239***	-0.217
	[0.112]	[0.081]	[0.196]	[0.112]	[0.081]	[0.195]	[0.115]	[0.083]	[0.204]
R Squared	0.001	0.004	0.004	0.001	0.004	0.004	0.001	0.004	0.005
Observations	5737	2696	1231	5716	2682	1226	5489	2643	1180

Notes: *** p<0.01 ** p<0.05 * <0.1. Robust standard errors in parentheses. The table presents the effects of remittances shocks –defined as a cut in income remittances– for the treatment group on their preferences for redistribution and political trend relative to the ones of the group that does not receive a shock. I present estimates with a dichotomous treatment –shocked with general impact and shock with high and medium impact– and a continuous one –number of shocks between 2014 and 2016–. The sample is restricted to individuals who did not receive a shock in 2013 or before. Dependent variables are presented as standardized changes. An augment over the variable inequality implies a major preference for a government that promotes policies to reduces the gaps between the poor and the rich. For redistribution, an augment in this variable implies a major score in the index built with the inequality variable and two other variables that account for the weight individuals give to the government and the individual in the responsibility for its welfare. An augment over the variable political trend implies a movement over the political spectrum towards the left. Estimates include and exclude control variables.

Table 5: Family arrival shocks and preferences for redistribution

	Inequality	Redistribution	Political Trend	Inequality	Redistribution	Political Trend	Inequality	Redistribution	Political Trend
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Shocked	0.047	-0.056	0.173*						
	[0.053]	[0.041]	[0.095]						
Shocked (Impact)				0.118	-0.192**	-0.077			
				[0.089]	[0.078]	[0.204]			
Shocked (Times)							0.068	-0.123***	-0.07
							[0.072]	[0.044]	[0.145]
Age	0	0.002	0.003	-0.001	0.002	0.004	0	0.002	0.003
	[0.002]	[0.001]	[0.004]	[0.002]	[0.001]	[0.004]	[0.002]	[0.001]	[0.004]
Years of Education	0.006	0.009**	0.002	0.005	0.010**	-0.002	0.005	0.009**	0.003
	[0.005]	[0.004]	[0.007]	[0.005]	[0.004]	[0.007]	[0.005]	[0.004]	[0.007]
Civil Status	-0.029	0.003	0.144**	-0.021	-0.009	0.166**	-0.03	0.003	0.146**
	[0.039]	[0.028]	[0.067]	[0.041]	[0.029]	[0.072]	[0.039]	[0.028]	[0.067]
Sex	0.019	0.023*	0.024	0.024	0.015	0.014	0.02	0.023*	0.025
	[0.017]	[0.013]	[0.029]	[0.018]	[0.013]	[0.031]	[0.017]	[0.013]	[0.030]
Constant	-0.117	-0.237***	-0.268	-0.1	-0.227**	-0.23	-0.115	-0.236***	-0.261
	[0.115]	[0.086]	[0.207]	[0.121]	[0.090]	[0.220]	[0.115]	[0.085]	[0.208]
R Squared	0.001	0.004	0.009	0.001	0.006	0.007	0.001	0.006	0.006
Observations	5232	2489	1127	4721	2253	1002	5232	2489	1127

Notes: *** p<0.01 ** p<0.05 * <0.1. Robust standard errors in parentheses. The table presents the effects of remittances shocks –defined as a cut in income remittances– for the treatment group on their preferences for redistribution and political trend relative to the ones of the group that does not receive a shock. I present estimates with a dichotomous treatment –shocked with general impact and shock with high and medium impact– and a continuous one –number of shocks between 2014 and 2016–. The sample is restricted to individuals who did not receive a shock in 2013 or before. Dependent variables are presented as standardized changes. An augment over the variable inequality implies a major preference for a government that promotes policies to reduces the gaps between the poor and the rich. For redistribution, an augment in this variable implies a major score in the index built with the inequality variable and two other variables that account for the weight individuals give to the government and the individual in the responsibility for its welfare. An augment over the variable political trend implies a movement over the political spectrum towards the left. Estimates include and exclude control variables.

Table 6: Labor market shocks, preferences for redistribution and political trends by wealth quintiles

	Inequality	Redistribution	Political Trend	Inequality	Redistribution	Political Trend
	[1]	[2]	[3]	[4]	[5]	[6]
Shocked	0.048	0.235	-0.340**			
	[0.151]	[0.157]	[0.153]			
Quintile 4	-0.105	-0.053	0.166	-0.104	-0.053	0.168
	[0.075]	[0.065]	[0.130]	[0.075]	[0.065]	[0.130]
Quintile 3	-0.146**	-0.088	-0.141	-0.144**	-0.088	-0.139
	[0.073]	[0.059]	[0.123]	[0.073]	[0.059]	[0.123]
Quintile 2	-0.116*	-0.086	0.025	-0.114	-0.087	0.029
	[0.070]	[0.056]	[0.131]	[0.070]	[0.056]	[0.131]
Quintile 1	-0.147**	-0.096*	-0.019	-0.144**	-0.097*	-0.014
	[0.071]	[0.057]	[0.130]	[0.071]	[0.057]	[0.130]
Shocked X Quintile 4	0.049	-0.212	0.147			
	[0.199]	[0.214]	[0.270]			
Shocked X Quintile 3	0.012	-0.053	0.434			
	[0.203]	[0.201]	[0.280]			
Shocked X Quintile 2	-0.076	-0.314*	0.425			
	[0.194]	[0.177]	[0.311]			
Shocked X Quintile 1	0.337*	0.05	0.229			
	[0.184]	[0.183]	[0.272]			
Shocked (Impact)				0.041	0.286**	-0.330**
				[0.155]	[0.127]	[0.165]
Shocked (Impact) X Quintile 4				0.059	-0.186	0.19
				[0.211]	[0.208]	[0.313]
Shocked (Impact) X Quintile 3				0.033	-0.082	0.494
				[0.213]	[0.186]	[0.302]
Shocked (Impact) X Quintile 2				0.002	-0.376**	0.35
				[0.204]	[0.153]	[0.343]
Shocked (Impact) X Quintile 1				0.407**	0.043	0.092
				[0.192]	[0.166]	[0.281]
Constant	0.101	-0.119	-0.252	0.085	-0.116	-0.305
	[0.139]	[0.102]	[0.258]	[0.140]	[0.102]	[0.260]
Controls	SI	SI	SI	SI	SI	SI
R Squared	0.004	0.011	0.019	0.005	0.012	0.021
Observations	4826	2507	1013	4715	2474	993

Notes: *** p<0.01 ** p<0.05 * <0.1. Robust standard errors in parentheses. The table presents the effects of labor market shocks –defined as job loss of a member of the household– for the treatment group on their preferences for redistribution and political trend relative to the ones of the group that does not receive a shock interacted with the quintiles of the wealth distribution excluding the richest one. I present estimates with a dichotomous treatment –shocked with general impact and shock with high and medium impact– and a continuous one –number of shocks between 2014 and 2016– interacted by an inverse quintile of wealth. The sample is restricted to individuals who did not receive a shock in 2013 or before. Dependent variables are presented as standardized changes. An augment over the variable inequality implies a major preference for a government that promotes policies to reduces the gaps between the poor and the rich. For redistribution, an augment in this variable implies a major score in the index built with the inequality variable and two other variables that account for the weight individuals give to the government and the individual in the responsibility for its welfare. An augment over the variable political trend implies a movement over the political spectrum towards the left. Estimates include control variables, whose coefficients are not reported.