Long-Run Effects of Democracy on Income Inequality: Evidence from Repeated Cross-Sections

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Abstract

In this paper I assess the link between democracy and inequality. I measure inequality at the cohort level by using pseudo-panel data built from nine Latin American countries' household surveys (1995-2009, biannual), and I measure democracy as a stock during long periods of time both before and after each cohort's year of birth. I present evidence that long-run patterns in the degree of democracy relate to income inequality. However, this relationship appears to be non-monotonic: inequality first increases with the stock of democracy before falling. I also present evidence that education may be one mechanism explaining this result.

Keywords: Inequality, democracy, Latin America, pseudo panel

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1 Introduction

This paper assesses the relationship between democracy and income inequality. It is reasonable to think that democracies reduce income inequality by allowing citizens to vote for political parties that privilege redistributive platforms. Thus, societies with a more egalitarian distribution of political power may have lower income inequality. Nonetheless, Asian countries, for example, display low income inequality and low-rating democratic institutions while Latin America, a region with higher-rating democratic institutions, displays extreme income inequality (Savoia et al., 2009).

The lack of a clear relationship between democracy and income inequality has two popular explanations. The first one is the political Kuznets curve. This hypothesis states that democratization first produces an increase in inequality before it produces an improvement in the distribution of income. However, Palma (2011) states that inequality does not decline after democratization in countries with historically high levels of inequality. Acemoglu and Robinson (2002) clarify that there are exceptions to the political Kuznets curve, for example, inequality might fluctuate if countries switch between less and more democratic regimes (e.g., Latin America). A second explanation suggests that democracies that begin with high inequality may develop poor economic and political institutions that perpetuate the unequal distribution of income. Indeed, recent research attributes high and persistent inequality to the presence of "bad" economic and political institutions, which are in turn a result of high initial inequality (Savoia et al., 2009).[1]

Gerring et al. (2005; 2012) argue that that the best way to think about the relationship between democracy and social outcomes is as a time-dependent, historical phenomenon. Most recent empirical studies assess the link between democracy and inequality using cross-country panel data sets, traditionally: the Deninger and Squire data base; UNU-WIDER's World Income Inequality data base; and the World Bank's World Development Indicators.[2] In general they conceptualize this link as the effect of regime type on inequality at time t plus some specified period. However, regimes are historically informed phenomena rather than contemporary variables. Gerring et al. find, for example, that a country's level of democracy in a given year is not likely to affect its level of human development, but its stock of democracy over a very long-period of time does.

On top of the previous problem "traditional" cross-country studies have other considerable problems: i) Some surveys report data on income and some on expenditure, making cross-country comparisons difficult, ii) The degree of accuracy is still a problem; e.g., some surveys undertaken in the midst of civil wars in sub-Saharan Africa have "national" coverage, but polities were fragmented

[1] See Acemoglu et al. (2013) for further discussion.

[2] For example: Li et al. (1998), Gradstein et al. (2001), Reuveny and Li (2003), Chong (2004), Hamanaka (2008) and Timmons (2010), to name a few.

during those events, iii) None of the data sets provide relevant distributional information given that they are composed of country-level aggregates, iv) Democracy is endogenous –it might be a result of many socioeconomic phenomena (omitted variables bias) and may vary between groups within countries (measurement error), v) Although many studies resort to instrumental variables to solve the problem of endogeneity, the variables related to the quality of democratic institutions relate to either a prior or subsequent redistribution of income, violating the exclusion restriction.[3]

I assess the link between democracy and inequality using pseudo-panel data built from nine harmonized Latin American countries' household surveys (1995-2009, biannual), conceiving within-cohort inequality as a function of the accumulated level of democracy (or its stock) over long-periods of time around the cohort's year of birth. My approach has some advantages over cross-country studies, in that: i) I explore a relatively homogenous region, ii) surveys report the same sources of income, making surveys comparable between countries and over time, iii) I do not rely upon country-level measures of income inequality but upon cohort-level inequality (somewhat accounting for heterogeneity between groups within countries), iv) I measure democracy over long periods of time; thus, I explore the accumulated effect of historical legacies on the contemporary level of inequality.

The biggest empirical challenge to overcome in this study is endogeneity in the proxy for democracy. To address endogeneity I include a set of fixed-effects that capture gender differences, long-run and short-run country-level economic differences, long-lasting effects of colonial institutions, and changes in survey lifting. Therefore, my identification strategy, although it does not completely solve the problem of endogeneity, attenuates it considerably. Furthermore, to reassure the reader about the validity of my results I perform various robustness checks.

A number of messages emerge from this paper. First, cohorts that "experienced" long periods of quality democratic institutions have lower inequality. I find that 20 years of high quality democratic institutions could reduce the dispersion in labor income (on average) by around 17%. Second, I find suggestive evidence that this relationship is non-monotonic: The level of inequality increases with the democracy stock and then starts decreasing with long periods of quality democratic institutions. Third, cohorts that "experienced" long periods of quality democratic institutions show lower dispersion in educational attainment, and higher educational attainment. I find that 20 years of high quality democratic institutions could reduce the dispersion in the years of education (on average) by around 9%, while average educational attainment increases (on average) by 5%. These relationships also appear to be non-monotonic. These results give credence to the hypothesis that

[3]See Atkinson and Brandolini (1999), Timmons (2010), Palma (2011), Niemeyer et al. (2008), Wejnert (2005), Savoia et al. (2009), Bardhan (2005), Nikoloski (2009, 2010), Engermann and Sokoloff (1997, 2000) and Acemoglu et al. (2013) for further discussion on each of these problems.

education maps democracy onto long-run changes in the distribution of human capital and then onto contemporary changes in income inequality.

The rest of the paper proceeds as follows. The next section briefly presents a review of four mechanisms that link democracy and inequality, their limitations, and presents an alternative approach. Section three describes the data. Section four describes the econometric approach and addresses the methodological challenges. Section five presents the main results and shows suggestive evidence that education may be one mechanism explaining the link between democracy and inequality. The last section concludes.

2 Do democracies breed more egalitarian societies?

Meltzer and Richard (1981), Alesina and Rodrik (1994), and Persson and Tabellini (1994) propose that utility-maximizing individuals redistribute based on rational choices; if the median income lies below the mean income, the median voter chooses redistribution and higher taxation for rich people. This model predicts that democracies have lower levels of inequality than non-democracies (Acemoglu and Robinson, 1998).

A second mechanism alleges that democracy lowers the costs of political participation of organized labor, allowing labor unions to obtain a privileged position in the policy process (Schumpeter, 1942; Rodrik, 1999). As a result, democracy encourages unionization, centralized wage bargaining, and minimum wages, which reduce wage dispersion (Katz and Autor, 1999; Rodrik, 1999).

Democracies should also guarantee broad access to property rights. With well-defined property rights and broad access to them, the poor have the possibility to gain access over improved or produced assets by facilitating the development of efficient market-based economies and opening up markets and institutions, which also prevent the elite from erecting entry barriers and enjoying markets with monopoly power (Gerring et al., 2005; Acemoglu, 2008).

A fourth mechanism suggests that democracy increases competition among politicians for citizen support. This causes governments to invest more in public services, such as education (Saint-Paul and Verdier, 1993). Education, in turn, acts as a redistribution channel reducing the dispersion of human capital and increasing a generation's human capital relative to the previous generation.

I find these four mechanisms the most relevant.[4] However, recent empirical findings reveal caveats in most of these mechanisms. In a well regarded paper, Milanovic (2000), using micro-level data from the Luxembourg Income Studies, finds weak evidence for redistribution through

[4]See Thorbecke and Charumilind (2002) and Savoia et al. (2009) for further discussion.

the median voter channel. Indeed, he finds that the middle classes are not net beneficiaries from redistributive transfers. Timmons (2010), using the University of Texas Inequality Project and the United Nations Industrial Development Organization (UTIP-UNIDO) data set, finds no evidence for the existence of the second channel. He shows that although democracies may pay higher average wages in manufacturing, democracy does not dampen wage dispersion between industries. Amendola et al. (2013), using the UNU-WIDER Income Inequality data set, find evidence that democracy is not a sufficient condition to reduce income inequality in the presence of strong property rights. They find that in presence of weak democratic institutions, strong property rights actually lead to an increase in the level of inequality.

Education presents an interesting alternative. The historical record offers many cases where the birth of democracies opened up new possibilities for the scaling up of public education (Sachs, 2012). Engerman et al., (2000), Baum and Lake (2001), Lindert (2004) and Glaeser et al. (2004) suggest that democracy increases the education output, mapping democracy onto long-run changes in the distribution of human capital and then onto contemporary changes in income inequality. However, if the quality of education is low for the poor or the social payoff from education diminishes as a consequence of deeply ingrained patterns of social exclusion and discrimination, educational attainment may not reduce inequality (Perry et al., 2003).

A considerable issue that democracy faces to achieve redistribution is inequality itself: If democracies display high initial levels of inequality, it is difficult for the poor to hold the rich and powerful accountable. Such a state of affairs affects the social norms about the legitimacy of rules and institutions. On the one hand, the wealthy minority may be inclined and able to establish a legal framework to ensure themselves a disproportionate share of political power, and use it to establish rules, laws, and government policies to give themselves broader access to economic opportunities. On the other hand, citizens may feel discouraged to press for redistributive changes that may not ultimately benefit them. Hence, property rights, legal systems, and fiscal and economic institutions may perpetuate the unequal distribution of income. (Engerman and Sokoloff, 2000; You and Kagram, 2004; Savoia et al., 2009; Acemoglu and Dell, 2010.) In other words, the effects of democracy on inequality may be endogenous to the initial levels of inequality.

2.1 Conceptualizing democracy over long periods of time

The quality of democratic institutions might have long-lasting impacts on social outcomes. On the one hand, democracies are more likely to increase the allocation of resources to social programs. These social programs might benefit people during their early childhood: Kudmatsu (2011), for example, shows that democratization was associated with an increase in the use of some health

inputs on children in Sub-Sharan Africa. Or during their school years: For example, Brown and Hunter (2004), Ansell (2008), Huber and Stephens (2012), and Harding and Stasavage (2014), show that more democratic regimes are more likely to spend more on school education. On the other hand, democratic institutions might also benefit people during their work years: Workers might enjoy more bargaining power as a result of the political power of unions (Rodrik, 1999), or they might have access to job opportunities that were previously available only to the elite.

Institutional effects unfold over time, sometimes a great deal of time, and are cumulative. Think for example of health and education. If our parent's generation benefited from high public spending on health during their early childhood, they are more likely to have grown healthier and have acquired competitive cognitive and social skills (Heckman, 2011). Add onto that high public spending on education, which may had provided them also with good educational resources to develop their cognitive skills. Now, our generation would have benefited as well from the benefits our parents enjoyed, as it is well known (for example) that more educated parents are more likely to invest more time and money in the education of their children. Furthermore, we may also benefit from high public spending as our parent's did, or from the capacity that was built previously. It is precise to explore the long-term effects of these legacies; democracy is likely to have intergenerational impacts on social outcomes.

3 Data

The data come from several harmonized household surveys of nine Latin American countries: Argentina, Brazil, Bolivia, Colombia, Honduras, Peru, Panama, Paraguay and Uruguay. To maximize the number of countries and periods I use surveys between 1995 and 2009, biannual, and restrict myself to urban areas in Bolivia due to the lack of rural data for the 1995 and 2009 surveys. Peru and Honduras surveys for 1995 are not harmonizable, but the countries represent sources of variation too important to exclude them; therefore, my pseudo-panel is unbalanced. Table A1, in the appendix, reports the data sources.

I restrict myself to individuals born between 1936 and 1977. However, I drop individuals between 18 and 24 years of age in 1995 and between 21 and 24 years of age in 1997 to consider that by 2009 life expectancy in the region was about 74 years –to keep a representative sample of "old" individuals- and that a person's educational attainment is likely to remain unchanged after age 25 –to avoid truncation in the distribution of educational attainment.

The source of income for every observation is monthly monetary labor income (constant prices of 2005, US$, adjusted by purchasing power parity), which is the sum of earnings from wages,

tips (as applicable), over-time payments, commissions and bonuses. I drop monthly monetary labor income outliers.[5] The previous procedure comprises 2% of the sample with positive labor income.[6]

I measure educational attainment as the number of years of education attained in the year previous to the survey. Missing values for the variable of educational attainment comprise an additional 3% of the sample.

The pseudo panel

When using repeated cross-sections ordinary least squares estimators are likely to be biased, however we can use cohort aggregates to obtain consistent estimators (Deaton, 1985). In other words, we can use a pseudo-panel. A pseudo-panel consists of cohorts (synthetic individuals) that we are able to follow over time. However, to obtain consistent estimators from a pseudo-panel, grouping variables –those that aggregate individuals into cohorts- must i) not have missing values for any individual in the sample, ii) not vary over time, and iii) be exogenous and relevant (Verbeek, 2008). It is also important that the number of cohorts is large enough to avoid small sample size problems, that cohort-sizes are large enough to avoid measurement error problems, and that cohorts somehow minimize within-cohort heterogeneity and maximize heterogeneity between cohorts (Verbeek and Nijman, 1993; McKenzie, 2000).

I use gender, country and birth year in three-year spans as grouping variables –which should be exogenous and relevant (Verbeek, 2008). I group individuals in three-year-span birth-cohorts to reduce within cohort heterogeneity and obtain high heterogeneity between cohorts. This also provides me a relatively large number of cohorts to mitigate sample size problems and cohorts that, due to their large sizes, should not present considerable measurement error problems.[7] Table A2, in the Appendix, shows the distribution of individuals and of cohorts.

Income inequality estimates consist of Gini indices at the cohort level using survey weights. However, one potential problem of using labor income to compute inequality is that of self-selection into the labor markets, which leads to self-selection bias. However, if grouping variables are exogenous and relevant, they should act as instruments for self-selection into the labor markets (Moffitt, 1983; Moscarini and Vella, 2003).[8] Therefore self-selection is not a concern. I proxy educational

[5] I detect outliers using the blocked adaptive computationally efficient outlier nominators algorithm (Billor et al., 2000), using the first percentile of the chi-squared distribution as a threshold to separate outliers from non-outliers.

[6] The sample with positive labor income comprises 59% of the individuals born between 1936 and 1977.

[7] I check the robustness of my results using also five-year-span birth-cohorts and seven-year-span birth-cohorts, with the concomitant of smaller samples.

[8] The identification assumption is that selection into labor has to do exclusively with cohort membership, while

attainment as the average number of years of education attained and its dispersion as the standard deviation of the number of years of education attained.

Measuring democracy

I measure democracy as *stock* rather than a level variable. This implies conceptualizing democracy as an asset. It implies that democracy is a fungible resource that may accumulate over time, promising increased returns in the future. Along these lines, if a democratic regime endures it is likely to yield some return, for example: social-welfare-augmenting economic policies, market-augmenting economic policies, political stability, rule of law, and efficient public bureaucracies (Gerring et al., 2005). But, why should we expect better results from a long-term democracy?

Paraphrasing Gerrin et al. (2005): In democratic regimes the policy-making process, by definition, involves a considerable number of players. Policy-making is a continual back and forth between different interest groups (e.g., legislators, citizens, economic organizations, etc.). Each of these players must learn to anticipate the goals, interests, and special sensitivities of the other players. Hence, the process of defining a "good policy" is likely to take considerable time. Not only must governing politicians learn what constitutes good policy; voters must also learn to recognize good policies. In authoritarian regimes, in contrast, this process is generally monopolized by a small number of elite actors and has few mechanisms of accountability. Naturally, the longer these elites are in power, the greater their opportunities for gaining experience in the diverse tasks of governance. But, since the political environment is highly constrained, the development of legal-bureaucratic authority is virtually impossible. Therefore, it is less likely that "good" policies institutionalize.

To measure democracy I make use of the Polity2 democracy index, which I draw from the Polity IV data set. This variable measures the extent to which "authority patterns are institutionalized" in a given country. It takes into account how the executive is selected, the degree of checks on executive power, and the form of political competition (Marshall et al., 2010). This indicator employs a twenty-one-point scale (-10 to 10) from more autocratic to more democratic, and offers good historical coverage (1800 onwards).

To create a stock measurement of democracy I first add up each person's Polity2 score from 30 years before their year of birth to 20 years after, and between their year of birth and 20 years after. I assume that the stock of democracy depreciates at a rate of r per year, with $r \in [0,1]$. This captures the possibility that the democratic stock does not accumulate linearly over time. Now, given that for every individual born in any given year (t) belonging to any given country (p) the value of the

cohort-time-varying components play little to no role.

stock would be the same, I compute the simple average of the proxy for the three birth years that compose the cohort (c) to obtain the cohort average.

To estimate r it is precise to specify how a particular historical path in country p up to year t maps into a value of the stock of democracy $S_{p,t}$. The simplest way to do this might be to assume that democratic capital accumulates and is discounted geometrically at the rate $(1-r)$:

$$S_{p,t} = D_{p,t} + (1-r) \times S_{p,t-1},$$

where $D_{p,t}$ denotes the value of Polity2 for the year t and country p.

We can solve backwards to obtain

$$S(r)_{p,t} = \sum_{s=t_0}^{t} D_{p,s}(1-r)^{t-s},$$

where t_0 denotes the initial period. Thus, democratic experience is more valuable the closer to the present it is. I use several values of $r \in [0.01, 0.1]$ for my main exercises.[9]

Summarizing, my main variables of interest are the depreciated democracy stock of country p in the birth-year t, averaged for cohort c, defined by:

$$\overline{D}_{c,p} = \frac{1}{3} \sum_{t \in c} \left(\sum_{s=t-30}^{t+20} (1-r)^{t-s} D_{p,s} \right), \tag{a}$$

$$\overline{D}_{c,p} = \frac{1}{3} \sum_{t \in c} \left(\sum_{s=t}^{t+20} (1-r)^{t-s} D_{p,s} \right). \tag{b}$$

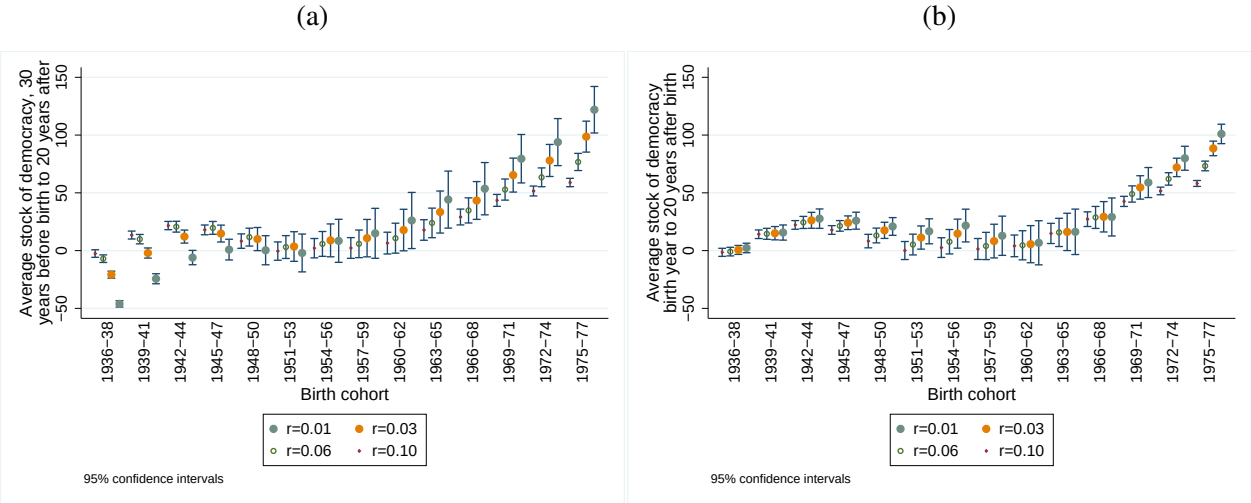
Figure 1 illustrates the distribution of these variables for different levels of r. There are a some particularities worth highlighting: i) Younger cohorts have higher stocks of democracy, ii) The lower r, the lower the variation of the stock of democracy in the entire sample; however, variation between cohorts remains much more stable, iii) The longer the time span, the greater the variation of the stock of democracy.

It is important to note that I define the upper limit to be at most 20 years after the year of birth to consider all the years of individual formation (childhood and teenage years), and to avoid dropping the youngest cohorts from the sample. Defining the lower limit of the span "30 years before the year of birth" allows me to avoid considering the *one thousand days war*, when Panama was not

[9] Person and Tabellini (2009) find that (globally) r lies between 0.01 and 0.06, thus the values of r I use are not unreasonable.

yet an independent country,[10] and therefore allows me avoid loosing my oldest cohorts. These time spans also permit me to explore whether democracy has intergenerational effects or not; that is, the variable defined by Equation (a) is statistically significant and it is statistically bigger than that defined by Equation (b) –and as such, historical legacies are important contemporaneously.

Figure 1: Stock of democracy by birth cohort



Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual and Polity IV Project.

4 Approach

Given the panel settings of my data (panel of successive cross-sections), we can model the relationship between democracy and inequality as follows:

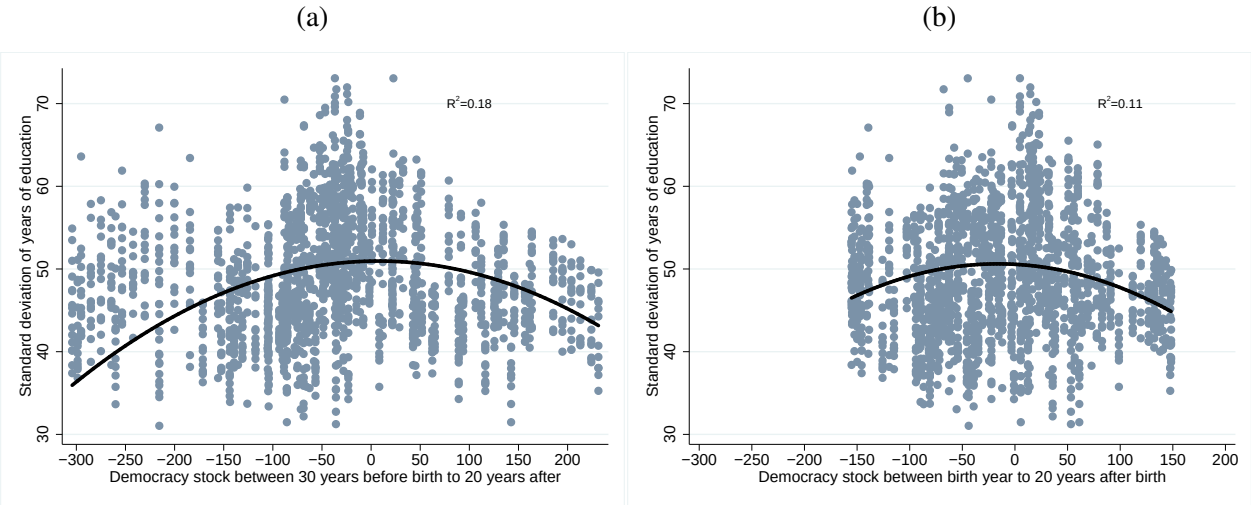
$$Y_{c,g,p,e} = \alpha + \beta \overline{D}_{c,p} + \overline{\varepsilon}_{c,g,p,e}, \tag{1}$$

where $Y_{c,g,p,e}$ denotes the Gini index of birth-cohort c, of gender g, of country p and survey e; $\overline{D}_{c,p}$ is a proxy for the accumulated quality of democratic institutions; and $\overline{\varepsilon}_{c,g,p,e}$ denotes the idiosyncratic error. Given that my interest lies on the historically determined component of institutions, this regression does not (cannot) control for a full set of cohort dummies. Since I have more than one observation per cohort, but my key regressor only varies by cohort, I cluster the standard errors by birth-cohort and country. This equation, however, is misspecified.

[10] The one thousand days war was a civil armed conflict between the liberal and conservative parties in the newly created Republic of Colombia. After the peace treaty Panama separated from Colombia.

The relationship between democracy and inequality is likely to be non-monotonic (Figure 2) –it shows the form of an inverted U. Acemoglu and Robinson (2002) suggest that, "economic development tends to increase inequality, but this inequality contains the seeds of its own destruction, because it induces a change in the political regime towards a more redistributive system." In this setup, where I measure contemporary inequality, this is a plausible mechanism if it acts through an intergenerational channel –a channel that can be influenced by long-periods of democracy, for example education (Saint-Paul and Verdier, 1993). Indeed, the historical evidence suggests that democratization led to a surge in redistribution: The increased supply of educated workers that came to be as a result, and the direct impacts of these redistributive efforts, are key factors in the non-monotonic pattern of inequality (Acemoglu and Robinson, 2002).

Figure 2: Stock of democracy vs. labor income Gini



Note: The figures above assume $r = 0.01$, however the U-shape also appears for values higher than 0.01 (figures available upon request to the author).
Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual and Polity IV Project.

$\overline{\mathbf{D}}$ is endogenous. Indeed, some researchers attribute the persistently high level of inequality in Latin America to the long lasting effects of colonial institutions. The encomienda (which gave Spanish conquistadors the right to Amerindian labor), the mita (a system of forced labor used in the mines), the repartimiento (the forced sale of goods to Indians, typically at highly inflated prices), the resguardo (which organized the Amerindian labor to be provided in the *Haciendas* and mines) and slavery, established important sources of economic and political inequality that endured after the region gained independence from their colonial masters and despite significant social, economic and political changes during the 20th century (Perry et al., 2003). This would likely lead to omitted variable bias, $E(\overline{D}_{c,p}, \overline{\varepsilon}_{c,g,p,e}) \neq 0$.

To address the potential misspecification problems I rewrite Equation (1) as follows:

$$Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \beta\overline{D}_{c,p} + \delta\overline{D}^2_{c,p} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}, \quad (2)$$

where η, ϕ, φ and ψ denote fixed effects by gender, country, survey, and an interaction between survey and country fixed effects respectively. The gender fixed effects capture the differences in socioeconomic conditions between men and women, for example gender roles and other gender differences which are prevalent in the region (Ñopo, 2012). The country fixed effects, survey fixed effects, and their interaction allow to control for long-run and short-run country-level economic changes, such as GDP growth, social public spending, inflation, etc., changes in survey lifting, and other possible long-lasting effects of colonial institutions. Also, note that I multiply both sides of the equation by $w_{c,g,p,e}$, which is equal to $\sqrt{N_{c,g,p,e}}$, with N the cohort size. The reason for this is to correct for underlying heteroscedasticity due to variation between periods in cohort sizes. Finally, $\overline{D}^2_{c,p}$ captures the non-monotonic relationship between democracy and inequality.

All in all, I exploit the variation coming from differences in the stock of democracy between gender-specific-cohorts, within-country, in a given survey period.

Given that I have a number of time invariant variables I use ordinary least squares (OLS) to estimate my main regression. On this regard, if grouping variables are exogenous and relevant and cohort sizes are large enough (as they indeed are) I can estimate Equation (2) using OLS (Moffitt, 1983; Verbeek and Vella, 2005).[11]

5 Results

Table 1 shows the estimates of democracy on inequality for several specifications of the pseudo-panel model. Each duplet of columns corresponds to computing Equation (2) using 0.01, 0.03, 0.06 and 0.10 as values for r respectively. The first column of each duplet shows the results obtained after estimating Equation (2) not including gender, country, survey and survey-country fixed effects; the second column adds the complete set of fixed effects.

In general columns (1), (3), (5) and (7) suggest a non-linear relationship between democracy and inequality ($\delta < 0$ and statistically significant), but our coefficient of interest (β) is not statistically significant. Nonetheless, by controlling for the set of fixed effects I obtain a lower value for β, and statistically significant. Note also that coefficients in panel A are larger than those in panel

[11] Girma (2000) and Collado (1997) provide other methods to estimate pseudo-panel models that are useful in the presence of time-invariant variables. However, these do not offer any gain in terms of consistency, and provide less efficient estimators (Verbeek and Vella, 2005). Thus, I do not consider these approaches.

B and that the higher r, the bigger β. These results suggest that the "older" the institutions, the smaller their effect on contemporary outcomes (β increases with r), however legacies are still very important –democracy has intergenerational impacts (β is bigger for a longer time span). These results also provide a sense about the direction of the bias due to non-observable covariates: β is likely to be biased upwards. It is likely that the region's democratic institutions are failing as a commitment device that prevents ruling minorities from erecting barriers to the excluded, likely due to the effects of long-lasting legacies; e.g., high initial inequality. (I test for the robustness of all these specifications in section 5.2.)

Table 1: Estimates of the pseudo-panel model, income inequality

A. Assuming $\overline{D}_{c,p} = \frac{1}{3}\sum_{t\in c}\left(\sum_{s=t-30}^{t+20}(1-r)^{t-s}D_{p,s}\right)$

Equation: $Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \beta\overline{D}_{c,p} + \delta\overline{D}^2_{c,p} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}$

	Dependent variable: Income inequality (Gini, 0-100 scale)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stock of democracy								
From 30 years before	0.0022	-0.0367***	0.0025	-0.0425***	-0.0045	-0.0495***	-0.0205	-0.0591***
birth to 20 years after it	(0.005)	(0.009)	(0.008)	(0.012)	(0.012)	(0.014)	(0.017)	(0.016)
Squared value	-0.0002***	-0.0000	-0.0003***	-0.0001**	-0.0005***	-0.0004***	-0.0008**	-0.0010***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	50.9524***	41.3353***	50.8682***	46.2987***	50.8253***	47.0358***	50.9335***	43.5713***
	(0.844)	(1.665)	(0.892)	(1.615)	(0.933)	(1.540)	(0.952)	(1.522)
Full set of fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.178	0.684	0.163	0.676	0.146	0.671	0.141	0.670
Clusters	126	126	126	126	126	126	126	126
Observations	1850	1850	1850	1850	1850	1850	1850	1850

B. Assuming $\overline{D}_{c,p} = \frac{1}{3}\sum_{t\in c}(\sum_{s=t}^{t+20}(1-r)^{t-s}D_{p,s})$

Equation: $Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \beta\overline{D}_{c,p} + \delta\overline{D}^2_{c,p} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}$

	Dependent variable: Income inequality (Gini, 0-100 scale)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stock of democracy								
From the year of birth to	-0.0069	-0.0221*	-0.0103	-0.0288**	-0.0170	-0.0393***	-0.0281*	-0.0526***
20 years after it	(0.009)	(0.011)	(0.011)	(0.012)	(0.013)	(0.014)	(0.017)	(0.015)
Squared value	-0.0002**	-0.0002***	-0.0003**	-0.0003***	-0.0004**	-0.0006***	-0.0008*	-0.0011***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	50.5659***	48.0631***	50.6058***	47.9851***	50.7030***	47.9669***	50.8865***	43.9699***
	(1.002)	(1.437)	(0.995)	(1.462)	(0.989)	(1.483)	(0.991)	(1.588)
Full set of fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.112	0.625	0.117	0.633	0.124	0.644	0.134	0.655
Clusters	126	126	126	126	126	126	126	126
Observations	1850	1850	1850	1850	1850	1850	1850	1850

Note: Robust standard errors clustered by country and birth-cohort in parentheses. * Significant at ten percent; ** significant at five percent; *** significant at one percent. Specifications (1), (3), (5) and (7) do not include fixed effects. Specifications (2), (4), (6) and (8) include the following fixed effects: gender (η), country (φ), survey (ϕ), and an interaction between survey and country fixed effects (ψ).

Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual and Polity IV Project.

Now I focus on columns (2), (4), (6) and (8). Within the parameters of these specifications, a

cohort with no existing stock of democratic capital experiences the following democratic inequality effect: for each full decade of high-quality democracy (Polity2 $= 10$), the democracy stock increases by approximately 100. To estimate the predicted effect of this change on inequality, I simply multiply this change by the coefficient on democracy stock, for example (in panel A) -0.0367 –column (2)- or -0.0591 –column (8). Focusing on panel A, the predicted impact on inequality of a decade of high-quality democracy is then approximately between -3.7 and -5.9. On panel B, in contrast, is approximately between -2.2 and -5.2. Considering that the average labor income Gini is around 48, this means that by having 20 years of high quality democratic institutions we could reduce (on average) labor income inequality by 17% –a sizeable improvement considering the high and persistent level of inequality in the region (Perry et al., 2003; Lopez-Calva and Lustig, 2010).

5.1 The role of education

It is plausible to regard democracy as an important institutional factor in the development of education. Political elites in a democracy have electoral incentives to improve the quality of life for the least advantaged; these incentives are present to a much smaller degree in authoritarian systems. Indeed, studies have shown that democratic rule translates into improvements on education (Baum and Lake, 2003; Brown and Hunter, 2004; Ansell, 2008; Huber and Stephens, 2012; Harding and Stasavage, 2014).

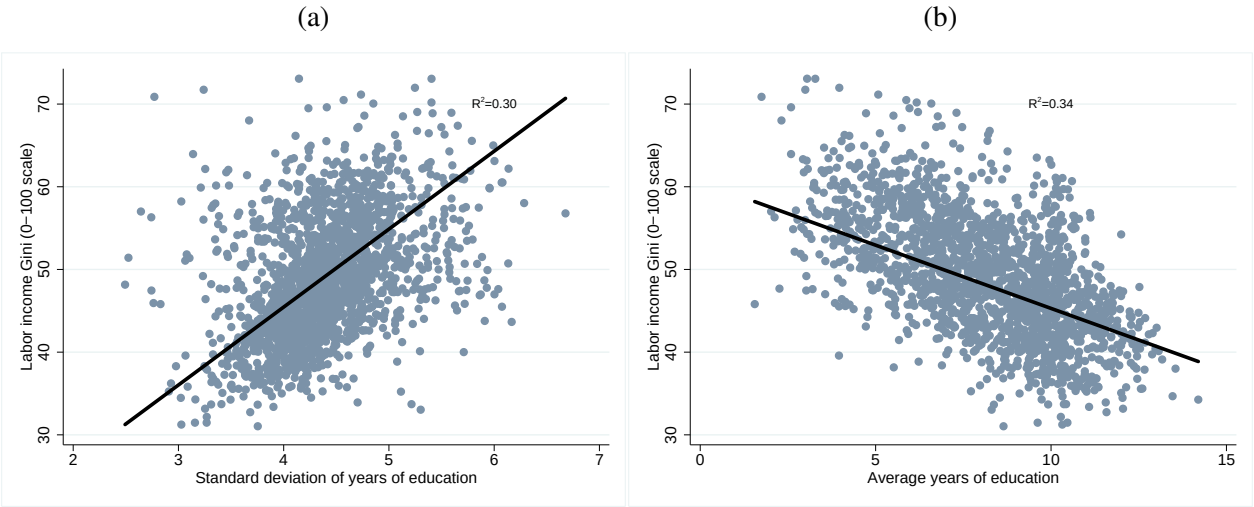
Because of the long-lived nature of human capital, the longer a democracy is in place, the more pronounced we can expect its impact on human capital to be (Gerring et al., 2012). Democracy might change the education output, mapping democracy onto long-run changes in the distribution of human capital and then onto contemporary changes in income inequality. Education, therefore, might be a suitable mechanism to explain the link between democracy and income inequality. This is indeed plausible, the level and the dispersion of educational attainment and income inequality are highly correlated (Figure 3).

The pseudo-panel models for the level and dispersion of educational attainment follow that of Equation (2). Although for these equations $\overline{\mathbf{D}}$ is also endogenous,[12] the set of fixed effects should deal with endogeneity. It is also important to note the number of years of education has serial correlation. Although I restrict myself to individuals ages 24 years and older to avoid truncation in the distribution of educational attainment, individuals might still attain more education with time

[12]Governments in Latin America, particularly democratic governments, conducted substantial efforts to expand primary education during the 20th century, however the elite was able to procure schooling services for their own children and resist subsidizing services for others (Bethell, 1997; Perry et al., 2003; Schiefelbein, 2007). Indeed, governments did not provide adequate access to education to most people until the second half of the 20th century. Furthermore, the differences in the quality of education between the wealthy and the poor of the population prevailed. This would likely lead to omitted variables bias.

(e.g., graduate studies). Therefore, I also introduce a lag of the dependent variable as a regressor to control for serial correlation when analyzing the level of educational attainment.[13]

Figure 3: Educational attainment vs. labor income Gini



Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual and Polity IV Project.

Similar to Table 1, in Table 2 each duplet of columns corresponds to computing Equation (2) using 0.01, 0.03, 0.06 and 0.10 as values for r respectively. Our results are consistent with our prior: democracy is related to the level of educational attainment. (Note that β is biased downwards.) Furthermore, in columns (2), (4), (6) and (8), democracy and educational attainment show a slight non-monotonic relationship.

Now, let us consider the results of columns (2), (4), (6) and (8). A cohort with no existing stock of democratic capital experiences the following democratic educational effect: for each full decade of high-quality democracy (Polity2 = 10), democracy stock increases by approximately 100 points. Considering panels A and B, I find that the predicted impact on educational attainment of a decade of high-quality democracy is approximately between 0.09 and 0.19 years of education. Considering that the average educational attainment is 5.6 years, having 20 years of high quality democratic institutions would increase educational attainment (on average) by 5%.

[13] Verbeek and Vella (2005) show that if grouping variables are exogenous and relevant and cohorts sizes are large enough, it is possible to obtain and appropriate measure of $\overline{Y}_{c,g,p,e-1}$, equivalent to the lagged value of $\overline{Y}_{c,g,p,e}$.

Table 2: Estimates of the pseudo-panel model, level of educational attainment

A. Assuming $\overline{D}_{c,p} = \frac{1}{3}\sum_{t\in c}\left(\sum_{s=t-30}^{t+20}(1-r)^{t-s}D_{p,s}\right)$

Equation: $Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \theta Y_{c,g,p,e-1} + \beta\overline{D}_{c,p} + \delta\overline{D}_{c,p}^{2} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}$

	Dependent variable: Average years of education							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stock of democracy								
From 30 years before	-0.0008***	0.0017***	-0.0011***	0.0018***	-0.0015***	0.0019***	-0.0016***	0.0019**
birth to 20 years after it	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Squared value	0.0000	0.0000***	0.0000	0.0000***	0.0000	0.0000***	-0.0000	0.0001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.1418	2.1783***	0.1474	2.0949***	0.1365	1.9990***	0.1273	1.8085***
	(0.122)	(0.316)	(0.119)	(0.312)	(0.118)	(0.314)	(0.123)	(0.314)
Full set of fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.918	0.955	0.918	0.955	0.918	0.954	0.918	0.954
Clusters	126	126	126	126	126	126	126	126
Observations	1598	1598	1598	1598	1598	1598	1598	1598

B. Assuming $\overline{D}_{c,p} = \frac{1}{3}\sum_{t\in c}\left(\sum_{s=t}^{t+20}(1-r)^{t-s}D_{p,s}\right)$

Equation: $Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \theta Y_{c,g,p,e-1} + \beta\overline{D}_{c,p} + \delta\overline{D}_{c,p}^{2} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}$

	Dependent variable: Average years of education							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stock of democracy								
From the year of birth to	-0.0010***	0.0009**	-0.0012***	0.0011**	-0.0013***	0.0013**	-0.0015***	0.0014**
20 years after it	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Squared value	0.0000	0.0000***	0.0000	0.0000***	-0.0000	0.0001***	-0.0000	0.0001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.1327	0.4144	0.1291	0.4379	0.1258	0.4314	0.1260	1.5764***
	(0.122)	(0.283)	(0.124)	(0.300)	(0.126)	(0.320)	(0.130)	(0.294)
Full set of fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.918	0.953	0.918	0.953	0.918	0.953	0.917	0.953
Clusters	126	126	126	126	126	126	126	126
Observations	1598	1598	1598	1598	1598	1598	1598	1598

Note: Robust standard errors clustered by country and birth-cohort in parentheses. * Significant at ten percent; ** significant at five percent; *** significant at one percent. Specifications (1), (3), (5) and (7) do not include fixed effects. Specifications (2), (4), (6) and (8) include the following fixed effects: gender (η), country (φ), survey (ϕ), and an interaction between survey and country fixed effects (ψ).
Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual and Polity IV Project.

Let us consider now dispersion in educational attainment. Focusing on coulmns (2), (4), (6) and (8), and both panel A and panel B, I find that the predicted impact on educational attainment of a decade of high-quality democracy is approximately between 0.02 and 0.4 years of education. Considering that the average dispersion is almost 4.7 years, this means that by having 20 years of high quality democratic institutions we could reduce the dispersion in the years of education (on

average) by 9%.

Table 3: Estimates of the pseudo-panel model, dispersion in educational attainment

A. Equation: Assuming $\overline{D}_{c,p} = \frac{1}{3}\sum_{t\in c}\left(\sum_{s=t-30}^{t+20}(1-r)^{t-s}D_{p,s}\right)$

Equation: $Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \beta\overline{D}_{c,p} + \delta\overline{D}^2_{c,p} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}$

	Dependent variable: Standard deviation of years of education							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stock of democracy								
From 30 years before	-0.0010**	-0.0014*	-0.0014**	-0.0019**	-0.0022***	-0.0029***	-0.0034***	-0.0043***
birth to 20 years after it	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Squared value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	4.3260***	4.1864***	4.3202***	4.1644***	4.3120***	4.1361***	4.3026***	4.3012***
	(0.071)	(0.149)	(0.069)	(0.153)	(0.067)	(0.157)	(0.066)	(0.120)
Full set of fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.039	0.240	0.051	0.255	0.071	0.278	0.097	0.304
Clusters	126	126	126	126	126	126	126	126
Observations	1850	1850	1850	1850	1850	1850	1850	1850

B. Assuming $\overline{D}_{c,p} = \frac{1}{3}\sum_{t\in c}\left(\sum_{s=t}^{t+20}(1-r)^{t-s}D_{p,s}\right)$

Equation: $Y_{c,g,p,e}w_{c,g,p,e} = [\alpha + \beta\overline{D}_{c,p} + \delta\overline{D}^2_{c,p} + \eta_g + \phi_p + \varphi_e + \psi_{p,e} + \overline{\varepsilon}_{c,g,p,e}]w_{c,g,p,e}$

	Dependent variable: Standard deviation of years of education							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stock of democracy								
From the year of birth to	0.0001	-0.0002*	0.0001	-0.0003**	0.0001	-0.0004**	-0.0000	-0.0005***
20 years after it	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Squared value	-0.0000*	-0.0000***	-0.0000*	-0.0000***	-0.0000	-0.0000***	-0.0000	-0.0000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.2991***	0.2049***	0.2986***	0.2052***	0.2980***	0.2064***	0.2974***	0.2290***
	(0.014)	(0.010)	(0.014)	(0.010)	(0.014)	(0.011)	(0.015)	(0.014)
Full set of fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.050	0.616	0.043	0.624	0.036	0.632	0.031	0.632
Clusters	126	126	126	126	126	126	126	126
Observations	1850	1850	1850	1850	1850	1850	1850	1850

Note: Robust standard errors clustered by country and birth-cohort in parentheses. * Significant at ten percent; ** significant at five percent; *** significant at one percent. Specifications (1), (3), (5) and (7) do not include fixed effects. Specifications (2), (4), (6) and (8) include the following fixed effects: gender (η), country (φ), survey (ϕ), and an interaction between survey and country fixed effects (ψ).

Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual and Polity IV Project.

5.2 Robustness

In this section I verify if my results survive statistical scrutiny. As a first robustness check I test if my results hold when using five-year-span birth-cohorts and seven-year-span birth-cohorts, with the concomitant of smaller samples. I include the results of these regressions in an online appendix in

order to save space.[14] I find that the signs and statistical significance of my coefficients of interest (β and δ) hold for these specifications. Although the coefficients are larger, we have to keep in mind that the related pseudo-panel samples are smaller; hence, they have to be considered with a grain of salt.

I also check whether my findings hold after controlling for periods of wars and economics crises. Both wars and economic crises can lead to regime transitions and affect socioeconomic outcomes. They can destroy or deplete capital assets, reducing top capital incomes (Piketty and Saez, 2003). They can also raise the income of the poor, for example, by employing them in the arms industry (as in the case of the United States during the Second World War) or reduce it, by leaving poor people jobless.

These phenomena can also have effects on human capital. For example, UNESCO (2010) reported that conflict often results in smaller shares of the population with formal schooling, fewer average years of education, and decreased literacy rates, which persist over time and might affect particularly marginalized groups. Economic crises might also affect human capital. In Latin America, for example, the economic crisis and structural adjustment during the 1980s economic crisis led to declines in family income, which may have resulted in growing inequality of educational opportunity (Fernandez and Lopez-Calva, 2010; Torche, 2010).

Wars and economic crises can also hasten regime transitions by destabilizing political regimes. In Latin America, in many opportunities regime changes were result of a war that replaced the incumbent head of government with another. In Brazil, for example, after the end of World War II, Getúlio Vargas regime became unsustainable because during the period of the war the threat of a German attack did not materialize, leading him to be swiftly overthrown in a military coup that "restated" democracy. In the Argentine *Revolución Libertadora* (1955), the Peron regime was deposed for a military dictatorship in a coup d'état by military forces.

Economic crisis also preceded regime transitions. The economic depression of the 1930s weakened the legitimacy of emerging democratic institutions, which led to social and political unrest between liberal movements and oligarchs and eventually to a military dictatorship in most countries backed up by the right-wing (which sought to protect the *status quo*). The economic crisis of the 1980s brought important economic problems for dictatorships to deal with: high unemployment, inflation and growth stagnation, which had deligitimazing effects. Eventually the economic crisis of the 1980s led non-democratic regimes to an end and to the birth of representative democracies in the region. (Bethell, 1997; Hagopian and Mainwaring, 2005.)

The effects of wars and economic crises can be far reaching, and although we can consider

[14] See: https://sites.google.com/site/cfbalcazars/misc.

them as shocks, they might have long-lasting consequences. Given that these two phenomena can precede regime transitions, I test for the stability of my proxy of democracy by including proxies that account for these events in my main regressions. However, given their transitory nature it would not be wise to measure wars and economic crisis as stock variables. Instead, I compute several proxies as level variables at different periods around a person's year of birth. For both wars and economic crisis I compute the percentage of years of war or crisis that an individual born in year t experienced between $t-30$ and $t-1$; $t-20$ and $t-1$; $t-10$ and $t-1$; t and $t+10$; t and $t+20$; $t-30$ and $t+20$; $t-20$ and $t+20$, and $t-10$ and $t+10$. These comprise a comprehensive set of proxies. Table A3, in the appendix, presents the periods of wars and economic crisis that took place in the countries of interest.

Given that for every individual born in any given year (t) belonging to any given country (p), and for a given time interval, the percentage of years of war or crisis would be the same, I compute the simple average of the proxy for the three birth years that compose the cohort to obtain the cohort average. That is

$$\overline{H}_{c,p} = \frac{1}{3}\sum_{t \in c} H_{t,c,p}, \tag{3}$$

where $H_{t,c,p}$ denotes the percentage of years of war or economic crisis for the birth year t belonging to cohort c and country p, and for a given time interval.

I run 648 different regressions, which come from including each proxy in my main regressions and first set of robustness tests with replacement. These regressions can be also found in the online appendix. Overall, my main results hold for the relationship between inequality in labor income and educational attainment and democracy; that is, $\beta < 0$ and statistically significant. Furthermore, I also find $\beta > 0$ in most specifications when analyzing the relationship between democracy and the level of educational attainment.

6 Conclusions

Despite many research documents assessing the link between democracy and inequality, empirical research is still not conclusive on whether and how democracy and inequality relate. This relationship is a complex and multidimensional process that involves many socioeconomic phenomena. Empirical research usually resorts to cross-country panel data sets, but these impose several measurement error and endogeneity problems that are hard to solve. This paper tries to overcome many of these difficulties using pseudo-panel data built from several household surveys of nine Latin American countries, exploiting panel data techniques to control for endogeneity due to unobserved factors, and proposing a renewed approach which seeks to determine if long-run patterns in the

degree of democracy relate to contemporary income inequality.

My findings show that democracy appears to be non-monotonically related to income inequality, and that it also appears to be non-monotonically related to the level of and dispersion in educational attainment. In particular, long periods of high-quality democratic institutions can lead to substantial decreases in inequality on income and education. My interpretation of these results is: It seems plausible to regard democracy as an important institutional factor in the development of education. Political elites in a democracy have electoral incentives to improve the quality of life for the least advantaged; incentives that are present to a much smaller degree in authoritarian systems. By changing the education output, political regimes map democracy onto long-run changes in the distribution of human capital and then onto contemporary changes in income inequality.

One of the concrete implications of these results is that if democratic institutions contribute to secular-historical changes in policy outcomes, then it is inappropriate to judge the results of institutional reforms on the basis of immediate policy gains. The immediate effects of institutional change are often negative since such change introduces uncertainties and information costs in the short run. Positive changes are likely to take longer to materialize, since they depend upon the establishment of a new equilibrium. (Gerring et al., 2005.) It is unrealistic to expect such reforms to show instantaneous results.

Many important points remain to be considered, I focus on a few important ones. First, what would happen if we consider younger cohorts by having access to recent household survey data –those born after the third democratic wave (Mainwaring and Hagopian, 2005). These cohorts should show larger democracy stocks. Using such data would allow us to further test the validity of the results herein. Second, I did not spend much time analyzing the non-monotonic relationship between the stock of democracy and inequality from the theoretical perspective. It is precise to rethink Acemoglu and Robinson's theory of the political Kuznets curve from the inter-generational perspective, introducing the role of political change (Person and Tabellini, 2009). However, this is a task that I defer to future work.

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Table A1: Data Sources

Country	Name of survey	Year	Coverage	Country	Name of survey	Year	Coverage
Argentina	Encuesta Permanente de Hogares	1995	Urban	**Panama**	Encuesta de Hogares	1995	National
		1997	Urban			1997	National
		1999	Urban			1999	National
		2001	Urban			2001	National
		2003	Urban			2003	National
	Encuesta Permanente de	2005	Urban			2005	National
	Hogares-Continua	2007	Urban			2007	National
		2009	Urban			2009	National
Bolivia	Encuesta Integrada de Hogares	1995	Urban	**Paraguay**	Encuesta de Hogares (Mano de Obra)	1995	National
	Encuesta Nacional de Empleo	1997	National		Encuesta Integrada de Hogares	1997	National
	Encuesta Continua de Hogares	1999	National		Encuesta Permanente de Hogares	1999	National
		2003	National		Encuesta Integrada de Hogares	2001	National
		2001	National		Encuesta Permanente de Hogares	2003	National
		2005	National			2005	National
		2007	National			2007	National
	Encuesta de Empleo	2009	Urban			2009	National
Brazil	Pesquisa Nacional por Amostra de	1995	National	**Peru**	Encuesta Nacional de Hogares	1997	National
	Domicilios	1997	National			1999	National
		1999	National			2001	National
		2001	National			2003	National
		2003	National			2005	National
		2005	National			2007	National
		2007	National			2009	National
		2009	National				

(*Continues on next page*)

Country	Name of survey	Year	Coverage	Country	Name of survey	Year	Coverage
Colombia	Encuesta Nacional de Hogares-Fuerza	1995	National	**Uruguay**	Encuesta Continua de Hogares	1995	Urban
	de Trabajo	1997	National			1997	Urban
		1999	National			1999	Urban
	Encuesta Continua de Hogares	2001	National			2001	Urban
		2003	National			2003	Urban
		2005	National			2005	Urban
	Gran Encuesta Integrada de Hogares	2007	National			2007	Urban
		2009	National			2009	Urban
Honduras	Encuesta Permanente de Hogares de	1997	National				
	Propósitos Múltiples	1999	National				
		2001	National				
		2003	National				
		2005	National				
		2007	National				
		2009	National				

Source: Author's compilations based on data from Latin American household surveys, 1995-2009, biannual.

Table A2: Sample Size

Birth cohort	Number of cohorts by year								Number of individuals	
(years)	1995	1997	1999	2001	2003	2005	2007	2009	Unweighted	Weighted
1936-38	14	18	18	18	18	18	18	18	29237	10034004
1939-41	14	18	18	18	18	18	18	18	42596	14354638
1942-44	14	18	18	18	18	18	18	18	54768	18299629
1945-47	14	18	18	18	18	18	18	18	75225	25036860
1948-50	14	18	18	18	18	18	18	18	95509	32201858
1951-53	14	18	18	18	18	18	18	18	117188	39522084
1954-56	14	18	18	18	18	18	18	18	139507	47440640
1957-59	14	18	18	18	18	18	18	18	159606	54468349
1960-62	14	18	18	18	18	18	18	18	175293	60275779
1963-65	14	18	18	18	18	18	18	18	191295	66839428
1966-68	14	18	18	18	18	18	18	18	188610	66279462
1969-71	14	18	18	18	18	18	18	18	198280	68779300
1972-74	0	18	18	18	18	18	18	18	178815	62720401
1975-77	0	0	18	18	18	18	18	18	165060	57798437
Total	168	216	216	216	216	216	216	216	1467114	503532031

Source: Author's calculations based on data from Latin American household surveys, 1995-2009, biannual.

Table A3: Periods of Economic Crisis and Wars (1905-2013)

a. Periods of war

Country	Years	Type of war
Argentina	1955	Intra-state
	1975-1977	Intra-state
	1982	Inter-state
Bolivia	1932-1935	Inter-state
Brazil	1912-1916	Intra-state
	1932	Intra-state
	1944-1945	Inter-state
Colombia	1948-1958	Intra-state
	1951-1953	Inter-state
	1989-2013	Intra-state
Honduras	1906	Inter-state
	1907	Inter-state
	1924	Intra-state
	1969	Inter-state
Panama	n.a.	n.a.
Paraguay	1911-1912	Intra-state
	1932-1935	Inter-state
	1947	Intra-state
Peru	1932	Intra-state
	1982-1992	Intra-state
	1995	Inter-state
Uruguay	n.a.	n.a.

b. Periods of economic instability

Name of the crisis	Years
Economic recession	1928-1939
Latin American debt crisis	1982-1992
Latin American financial crisis	1998-1999
Global financial crisis	2008-2013

Note: Intra-state wars encompass wars that predominantly take place within the recognized territory of a state. Inter-state wars encompass wars that take place between or among the recognized states.

Source: Author's compilations and Correlates of War.