Electoral Cycles in Inequality

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March 29, 2021[*]
Most recent version is here.

Abstract

Economic policies play a fundamental role in reducing inequality within countries, but such policies are subject to electoral cycles. This raises the question whether there exist electoral cycles in inequality. In this paper, we focus on this phenomenon. We show comprehensive evidence that income inequality decreases in the years of elections. This pattern is concentrated on close elections before which incumbent governments have stronger incentives to engage in policy actions due to higher uncertainty about the election outcome. We also examine whether elections can mitigate inequality of opportunity in regard to access to education. The results illustrate that governments allocate more resources to education in the years of elections. These findings point out that well-functioning democracies and increased political competition can be key in tackling inequality within countries.

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1 Introduction

Rising income inequality is one of the central issues in the ongoing economic debate all around the world, and the strong relationship between economic policies and inequality is well-known. On the other hand, a long literature on political business cycles shows that governments tend to manipulate various types of economic policies before elections -thereby generating electoral cycles in those policies- with the belief that this will help them get reelected. This raises the question of to what extent these pre-election policy changes by incumbent governments affect inequality. To the best of our knowledge, however, there is no evidence on electoral cycles in inequality.

In this paper, we shed light on this phenomenon, and show that inequality exhibits electoral cycles. Using panel data from 55 countries in the period 1986-2017,[1] we find that income inequality (a form of inequality of outcomes) decreases in the election years. The size of the effect is economically important. The decrease in inequality in the years of elections is around two times the average change in the sample, or alternatively, 22% of one standard deviation of the change across the sample. This result is not likely to be driven by omitted variables or reverse causality; and is robust to a large battery of robustness checks. Furthermore, the effect of elections on income inequality is more concentrated in close elections, where incumbent politicians face higher uncertainty about the election outcome, and hence policy changes by incumbents are supposed to be more paramount. This points to the role of pre-election policy choices in our result.

Next, we focus on electoral cycles in inequality of opportunity, namely in access to education. We find that governments channel more resources to education in the election years,

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[1]We adopt the Gini index obtained from the World Bank's PovcalNet database -a unique primary source that calculates inequality measures directly from countries' nationally representative household income (or expenditure) surveys- as our baseline proxy for income inequality. The data to study income inequality in a cross-country setting over time requires a careful and consistent approach to ensure comparability and reliability. Section 2 provides a detailed description of our approach.

which can be interpreted as declining inequality in access to education (a form of inequality of opportunities). This finding implies that, besides the direct and immediate effect on income inequality (as found by the first part), electoral cycles can also have positive and inter-generational effects on inequality through more wide-spread access to education. As pointed out in the literature (e.g. De Gregio and Lee 2002, Sylwester 2002, Brandolini and Smeeding 2009, IMF 2012, Dabla-Norris et al. 2015), a better and broader access to education can reduce income inequality by increasing the income share of the poor and the middle class, thereby acting as a circuit breaker for inter-generational transmission of poverty.[2] Moreover, it can promote longer term economic growth by spurring human capital. Therefore, an improvement in access to education is a win-win situation for equity and growth.

These results on the strong presence of electoral cycles in inequality suggest that electoral pressures can force governments -that are in quest of political dividend- to implement policies that mitigate inequality. They also imply that well-functioning democracies and increased political competition can be key factors in reducing inequality within countries.

Our findings extend the knowledge on "opportunistic political business cycles (PBCs)" (otherwise known as "opportunistic electoral cycles"), by adding new evidence on electoral cycles in inequality to the huge literature on PBCs.[3] This literature aims to answer one of the most popular questions of electoral politics: Do reelection concerns lead incumbent governments to manipulate economic policy and boost the economic activity right before elections?[4] As the answer to this question, the literature finds evidence of electoral cycles in fiscal policy (Remmer 1993, Akhmedov and Zuravskaya 2004, Khemani 2004, Brender and

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[2]In this regard, Rajan (2015) states that "prosperity seems increasingly unreachable for many, because a good education, which seems to be today's passport to riches, is unaffordable for many in the middle class".

[3]See Dubois (2016) for a comprehensive survey of this literature.

[4]One would expect office-motivated candidates to do so if they believe this will help them get reelected. Theoretical literature on electoral cycles shows that pre-election policy manipulations can help the incumbent under certain circumstances, e.g. when voters are myopic or when voters cannot directly observe the competence level of elected officials (Nordhaus 1975, Rogoff and Sibert 1988, Rogoff 1990). Whether these circumstances hold in the real world, thereby leading to policy manipulation by incumbent governments before elections and generating electoral cycles in economic policy and economic activity, is an empirical question.

Drazen 2005, 2007, 2013 among many others), monetary policy (Alesina et al. 1992, Clark and Hallerberg 2000, Leertouwer and Maier 2001, Block 2002, Alpanda and Honig 2010), and more recently, macroprudential policy (Müller 2019, Sever and Yücel 2020).

We are interested in finding out in which direction these electoral cycles in various economic policies affect inequality. The answer is far from clear ex-ante, with potentially competing and conflicting forces at play. In terms of fiscal policy, the literature documents changes in both the level and the composition of government spending before elections, as well as changes in the government budget deficit. Even though more government involvement in the economy is typically thought to lower inequality, the composition of the extra spending matters in terms of how inequality will ultimately change. Moreover, if higher government spending is coupled with reduced taxes and financed through deficits, this may even increase inequality, particularly if taxation is steeply progressive.[5]

In the case of monetary policy, a similarly long literature on electoral cycles almost exclusively finds evidence expansionary policies prior to elections. However, the evidence on the relationship between monetary policy and inequality is mixed. While a wave of recent studies finds that contractionary monetary policy leads to higher inequality (Coibion et al. 2017, Mumtaz and Theophilopoulou 2017, Furceri et al. 2018), there is also the argument that inflation arising from low policy rates disproportionately hurts the poor, who have limited access to financial instruments that hedge against inflation (Easterly and Fischer 2001, Erosa and Ventura 2002, Albanesi 2007).[6]

With regard to macroprudential policy, few recent studies (e.g. Müller 2019 and Sever and Yücel 2020) find evidence on loosened policies before elections. Inequality consequences

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[5]For a detailed discussion on how different fiscal tools can affect income inequality, see IMF (2014).

[6]See Doepke and Schneider (2006) for the counterargument that inflation hurts lenders more, who are typically wealthier. Also see Ghossoub and Reed (2017) on the effect of financial development in determining the relationship between monetary policy and inequality.

of those electoral cycles in macroprudential tools are not clear though. For instance, tighter capital requirements and taxes on financial institutions may limit the growth in financial services and reduce financial sector jobs, which typically pay more than other sectors. This might lower income inequality. On the other hand, higher limits on household credit and leverage can lower consumption by poorer households, possibly exacerbating inequality based on expenditure.[7] Hence, competing channels might be at play in deciding the effect of elections on inequality through the macroprudential policy channel.

Putting all these together, how inequality is affected by upcoming elections is definitely an open and empirical question. To the best of our knowledge, this is the first paper that addresses this question. Given the large literature on electoral cycles on policies and economic outcomes, we are rather surprised that this is the case, as inequality is such an important issue with significant consequences on welfare. First, under the diminishing marginal utility principle, for a given level of total income, higher inequality directly implies lower social welfare: Transferring a dollar from the poor to the rich decreases the utility of the poor by more than it increases the utility of the rich. Second, high inequality can hurt economic growth by causing social and political instability, as well as underinvestment in human capital. Indeed, a large empirical literature on the issue finds that inequality jeopardizes economic growth (Alesina and Rodrik 1994, Persson and Tabellini 1994, Easterly 2007, Halter et al. 2014, Ostry et al. 2014, Berg and Ostry 2017, Berg et al. 2018).

Given these significant welfare consequences and policy relevance of inequality, understanding which factors can mitigate the problem of inequality is a major phenomenon. That brings us to another strand of literature to which we contribute: the vast literature on the determinants of inequality. In addition to the economic policies we described earlier, many different factors are found to affect inequality, including trade (Feenstra and Hanson 1996,

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[7]The evidence is limited, with the exception of Frost and van Stralen (2018), who show that tighter stance on several macroprudential tools, such as loan-to-value, are associated with higher income inequality.

Munch and Skaksen 2008, Helpman et al. 2017), financial development (Greenwood and Jovanovic 1990, Beck et al. 2007, Claessens and Perotti 2007, de Haan and Sturm 2017), financial globalization (Phillipon and Reshef 2012, Furceri and Loungani 2018, Eichengreen et al. 2021), and technological change (Acemoglu 1998, Card and Dinardo 2002, Jaumotte et al. 2013, Prettner and Strulik 2020). In this paper, we add to this literature another important determinant of inequality: Political pressure of the upcoming elections on incumbent governments.

Last but not least, the findings in this paper are even more important for the post-pandemic world. The COVID-19 shock has exacerbated existing inequality within countries by affecting the most vulnerable disproportionately (Adams-Prassl et al. 2020, IMF 2020a). This effect is likely to be long-lasting (Furceri et al. 2020) with inter-generational consequences (Caselli et al. 2020). Such deterioration in inequality can make the economic recovery more challenging and less stable by fueling social unrest which would drag economic output further (Alesina and Perotti 1996, IMF 2020b). The COVID-19 pandemic is also affecting the access to education by poor students disproportionately (Collis and Vegas 2020, UNICEF 2020). Therefore, going forward, education spending remains a priority to curb the scarring in equity and human capital accumulation (IMF 2021). These imply that strengthening the institutional foundations of democracies and ensuring a high degree of political competition are particularly crucial to set the stage for a resilient growth path in the recovery from the pandemic.

The rest of this paper is organized as follows. Section 2 introduces the data sources and the sample. Section 3 illustrates the stylized facts on inequality and elections. Section 4 introduces the methodology. Section 5 represents the results. Section 6 concludes.

2 Data

This section provides a detailed explanation of the variables and the sample. For short description of variables, see Table A1 in Appendix.

2.1 Income inequality variables

Studying income inequality in a cross-country setting over time requires a careful consideration and a consistent approach in terms of the data, generally at the cost of lower number of observations. Otherwise, while having more observations, the results can become less reliable, since the data would not be comparable across countries and/or within countries across years. We will address the trade-off between ensuring reliability and increasing the coverage below. Before that, we go over the details of the dataset used in this study, the reasons why we prefer it, and the steps taken before using the raw data.

To proxy for income inequality, we adopt the Gini index[8] from the World Bank's Povcal-Net database. The main reason why we prefer the PovcalNet database is the following. It is a unique primary data source that generates the inequality measures directly from countries' nationally representative household income or expenditure surveys for a large set of countries over a long time period.[9]

Therefore, the PovcalNet database does not rely on functional form assumptions, model-based imputations, regression-based estimates or interpolations to calculate the Gini in-

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[8]Gini index, or coefficient, is a standard measure of inequality of a distribution. In our context, it is a summary measure of the extent to which the distribution of income (or consumption expenditure) among individuals (or households) in a country deviates from a perfectly equal distribution. It is calculated based on a Lorenz curve plotting the cumulative percentages of income received versus the cumulative number of recipients, starting from the recipient with the lowest income. The Gini index is the area between the Lorenz curve and a hypothetical line of perfect equality. It is expressed as a percentage of the maximum area under the line, ranging from 0 to 100. A Gini index at 0 represents perfect equality, and an index of 100 means perfect inequality.

[9]To be more precise, the PovcalNet uses microdata directly for the majority of calculations, whereas sometimes, the reliance of PovcalNet on household survey microdata is indirect. In those cases, it is based on the best available grouped data (such as, at the percentile or decile levels). The information is still obtained from the underlying microdata, as provided by, for instance, national statistical offices. For more details, see Ferreira et al. (2015).

dexes. When such approaches are followed, data becomes noisy with errors that are hard to assess, and in turn, reliability cannot be easily established. Although the sole reliance of microdata lowers the coverage relative to other sources (to be mentioned below), it comes with less measurement errors, higher quality, higher precision and higher comparability across countries and within countries across time, as discussed by the literature (e.g. Erauskin and Turnovsky, 2019).

Moreover, the PovcalNet database follows a rigorous approach to further improve the comparability by analyzing the underlying surveys. For instance, the distribution is adjusted for household size, providing a consistent measure of per capita consumption (or income). In addition, the PovcalNet does not calculate Gini indexes based on surveys whose sample is a sub-group in the population, such as wage earners. Otherwise, a bias with an ambiguous direction would emerge (Deininger and Squire 1996).

However, using the Gini indexes downloaded from the PovcalNet without any further consideration would still be problematic, since they are calculated using surveys on either income or consumption expenditure. Mixing income- vs consumption-based Ginis together without no correction is misleading, since consumption-based Gini indexes tend to be lower.[10] To cope with this issue, we assess the details of country-year surveys as provided by the PovcalNet website, and construct a dummy variable to identify whether the survey is based on incomes or consumption expenditures. We assign the dummy variable 1, if the Gini is constructed from an income survey, and 0 otherwise.[11]

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[10] For instance, lower consumption-based Gini can be a result of consumption smoothing across years, as discussed by Deininger and Squire (1996), Ravallion and Chen (1996), Deaton (2005) and Jaumotte et al. (2013).

[11] All the Ginis (ATG) database, a secondary data source which combines various inequality data, compiled by Branko Milanovic does a similar assessment for the PovcalNet. It has limited data points compared to the 2020 version of the PovcalNet, but whenever possible, we cross-check our own evaluation with the assessment by ATG, and we observe that it is mostly consistent.

Once we identify whether the underlying survey is based on incomes or expenditures, the question is how to eliminate a potential influence from this difference in the estimation. We follow the standard approach in the literature (e.g. Li et al. 1998, Chambers 2007, Teulings and Van Rens 2008, Erauskin and Turnovsky 2019), and include this dummy variable (indicating income-based Gini) to account for such differences in surveys. As we show in the results, this actually turns out to be important, and income-based Ginis are indeed higher than the consumption-based indexes, as with the literature.[12]

We, however, are aware that this fixed adjustment using the dummy variable approach is not perfect, since it ignores the possibility that the difference between consumption- and income-based indexes can change within countries over time and across countries. We address this concern as well. In robustness, we run the regression using data only from countries for which the Gini is calculated based on income surveys across all years.

We now turn to the trade-off between data reliability and coverage. By choosing the PovcalNet, we privilege the former in our main analysis, although we still show that results remain similar if we prioritize the coverage. For this purpose, in a robustness test, we use another data source: Standardized World Income Inequality Database (SWIID) by Solt (2020). Gini indexes in the SWIID are constructed through model-based imputations and interpolations based on various sources. This database substantially increases the coverage, but this is at the cost of potential measurement errors and precision issues (Jenkins 2015).[13]

There is another point to mention in regard to reliability of income inequality measures. Reliability would require comparability, i.e. using harmonized data from a single source, or at least, data based on consistent calculations. To ensure this, we do not prefer to mix differ-

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[12]Another difference is that the Gini can be per household or per person. We check this as well, and conclude that the Gini indexes are calculated for per person in our sample. Thus, this is not a problem in our case.

[13]We drop five extremely small economies from this sample with SWIID database for potential data quality issues, but the result is not affected much.

ent data sources within countries for different years. The estimation would be problematic in that case, since it would create a bias whose direction is not possible to predict. Empirically, there is no clear way to correct for that. Similarly, we do not prefer to mix different data sources across countries, since it also decreases the comparability across countries.[14]

We acknowledge that, although it is the most-widely used measure of income inequality, the Gini index has several shortcomings. First, the Gini index may be more sensitive to the income of the middle than that of the extremes. Second, there is no unique mapping between the Gini and the underlying income distribution, meaning that countries with different income distributions (and different Lorenz curve shapes) can have the same Gini index. This is true for any summary measure of a distribution though, since it is not feasible to extract the all information from a distribution using a single measure (Deaton and Case 2020). For our case, it means that the Gini index cannot draw the full picture of the underlying income inequality. Considering these two points, as an alternative measure to the Gini, we adopt the ratio of the income share of the top 10 percentile to the income share of the bottom 10 percentile from the PovcalNet (and also the top and bottom 20 percentiles). A higher value of this income ratio means higher income inequality. For summary statistics and correlations across different inequality measures, see Table A2 and Table A3 in Appendix. Third, demographic factors, such as population aging, can affect the Gini index, since younger people earn less in general. However, we attempt to address this by controlling for country-specific year trends.

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[14]In this regard, we are aware of such misleading uses in the literature. In some cases, secondary data sources putting several individual data sources together, such as the ATG database, confused economists. For instance, the earlier version of the ATG database included a variable called "Giniall" that uses all the available data and inserts only one Gini value per country/year, based on a preference order. Thus, in cases where there were multiple Gini observations coming from different datasets, the Giniall variable still has a unique country-year observation. Although this variable has the largest coverage, it is not consistent. In particular, a country may have several values from source 1, followed by several values from source 2, and then from source 3 etc. Researchers tended to simply use this Giniall variable to increase the coverage, but as explained, it is not a comparable variable (neither within countries across years, nor across countries). In order to avoid such confusion, the ATG database drops Giniall variable in the most recent version, and only provides the Gini indexes from various sources separately.

Lastly, although this is not a caveat per se, it is important to note that lower income inequality as measured by the Gini does not necessarily mean that everyone is better off. The Gini index is a relative measure of inequality (but not an absolute measure of incomes), and violates Pareto improvement principle. It is possible for the Gini to decrease while the income of all recipients is decreasing, as long as the new income distribution is less unequal. This implies, for instance, that the Gini can decrease while the number of people living in absolute poverty is increasing. Hence, in a study using the Gini index to proxy for income inequality, the result should be interpreted carefully.

There are other points which can be cause for concern. In case of the PovcalNet, despite the rigorous efforts made by the World Bank experts to ensure comparability,[15] cross-country and within-country comparisons may still not be without problems, since in some cases it may not feasible to correct for all differences in survey details. Few issues that may be important, but not feasible to account for, are as follows. First, exact definition of incomes in the surveys may differ across countries or across time. Second, although household surveys are well-accepted as the best way to capture income variability, they are not without problems, since, for instance, the upper part of the distribution may be truncated, either because extremely rich people may refuse to respond, or since they may simply understate their incomes (Milanovic 2011a).

These are all important points to acknowledge while using any cross-country dataset relying on microdata. Among other available cross-country datasets, we consider that the PovcalNet data has a much higher quality thanks to the rigorous effort by World Bank experts, has the widest and longest coverage relying on microdata (rather than imputations or interpolations), and hence is the best for our purpose.

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[15]For further details on the methodology, see Ravallion and Chen (1996).

2.2 Political and institutional variables

Election years are obtained from the World Bank's Database of Political Institutions (DPI) by Beck et al. (2001) with the update by Scartascini et al. (2018). This dataset has been commonly used in the political economy literature (e.g. Brender and Drazen 2005, Faye and Niehaus 2012). It compiles detailed information from a wide range of sources and provides a comprehensive data on the political environment for a large set of countries until 2017. We assign a dummy variable 1, if there is an election for the chief executive in a given year, and 0 otherwise.

Political orientation of incumbent politicians may shape policy choices, and hence can affect outcomes (e.g. inequality). In a robustness test, we control for the political orientation of the incumbent executives. We obtain this information from the DPI, and construct a variable which takes 1, 2 and 3 if the political orientation of the existing executives is defined as right, center, or left, respectively.

We also test if the result survives when we control for other variables regarding the political environment. In particular, we add the number of years during which the chief political executive has been in the office, and a variable to control for the political system (which takes 1 for presidential, 2 for assembly-elected president, and 3 for parliamentary systems). Both variables are from the DPI.

In a separate test, we identify relatively close elections (for which incumbents face a higher uncertainty for outcomes), to examine if the effect of the election year on inequality is more concentrated in such elections relative to not-so-close (i.e. non-close) elections. If we have reliable poll data prior to elections for a large set of countries over a long time period, that would be ideal to use in order to evaluate the level of closeness and related uncertainty faced by incumbents. This is not feasible though, given the lack of data. Thus, we follow the

literature (e.g. Canes-Wrone and Park 2012, Julio and Yook 2012), and categorize elections as close versus non-close based on the election outcomes. We adopt the gap in vote shares for this purpose. The data on vote shares are from the DPI.[16]

Institutional quality may also affect inequality. For instance, institutions that ensure same political rights for all citizens can impose pressures on politicians for redistributive policies. In robustness, we control for a proxy for institutional quality, by adopting the index on political rights from the database by Freedom House (Freedom in the World). This index summarizes information from country expert surveys based on standardized questions in various sub-categories and provides a summary of the obtained answers of those questions. It ranges from 1 to 7, higher values meaning less political rights.[17]

2.3 Macroeconomic variables

Throughout all regressions, we control for a set of macroeconomic variables that are found to affect inequality by the previous literature. We include the lagged values of the logarithm of real GDP per capita constant in 2010 USD (as a proxy for economic development), trade to GDP ratio (as a proxy for trade openness) and credit to GDP ratio (as a proxy for financial development). In robustness checks, we also include the growth rate of real GDP (constant in 2010 USD), a measure for financial openness, a measure of human capital and a measure of aggregate productivity. In a test with weighted regression, we use the logarithm of population as weights.

All macroeconomic variables, except measures for financial openness, human capital and productivity are from the World Bank's World Development Indicators (WDI) database. We obtain the index on capital account openness from the well-known dataset by Chinn and

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[16]One caveat of this analysis is that the vote shares are available for 122 elections in our sample. We identify close elections among those for which data is available.

[17]There are 10 political rights indicators. The political rights questions are categorized into 3 sub-categories: (i) electoral process (3 questions), (ii) political pluralism and participation (4 questions), and (iii) functioning of government (3 questions). More details of the methodology is available online.

Ito (2006). The authors assess the restrictions on cross-border financial transactions as reported by the IMF Annual Report on Exchange Arrangements and Exchange Restrictions. Therefore, it is a proxy for financial openness in regulatory perspective (a de jure measure). We use the recent version which has been updated by the authors until 2017. It is coded between 0 and 1, higher values meaning financially more open economies.

We adopt measures of human capital and productivity from Penn World Tables (PWT) 10.0. Human capital index in the PWT is based on average years of schooling and rate of return to education. Aggregate productivity is a measure of total factor productivity (TFP). More details on PWT are provided by Feenstra et al. (2015).

In robustness, we add the lagged value of the dummy for the years of banking crises as well, since those events are damaging for the economy and there can be some association between crises and inequality (Baldacci et al. 2002). We obtain the dates for crises from Laeven and Valencia (2018). They define an event as a crisis when the following conditions are met: (1) signs of financial distress in the banking system (e.g. bank runs, losses in the banking system, and/or bank liquidations), and (2) significant banking policy interventions as a reaction to banking losses.

In the last part of the paper, we focus on the government's expenditure on education. We use government expenditure on education as share of total government expenditure for this purpose. For this part, we also obtain total government expenditure to GDP ratio. These variables are from the WDI dataset.

2.4 Sample

We use all countries for which the information is available, with few restrictions. First, we require each country in the sample to have at least one election, since otherwise country fixed effects would absorb the time-invariant dummy variable for elections. Second,

we drop countries that had less than five years of information, since countries with few observations are not likely to provide enough variation for the estimation, particularly because (1) the Gini coefficient is a slow-moving variable, and (2) the empirical specification includes both country-specific trends and country fixed effects. Note that only few observations are dropped through these steps, and none of these restrictions affects our results (see Section 5.2). The main sample consists of 55 countries with 153 elections over the period of 1986-2017. The sample has countries from different income groups and regions. The list of countries is provided in Appendix.

3 Facts on Inequality and Elections

Before the empirical analysis, we provide an insight on whether the Gini index moves differently in election years relative to other years. Left panel in Figure 1 documents the mean change in the overall sample (first bar), in the election years (second bar), and in the years without election (third bar). The mean change in the Gini across the full sample is -0.16. However, in the years of elections, the average drop in the Gini becomes much larger in magnitude, -0.39. In the years with no elections, the change has much smaller size, -0.11.

The right panel accounts for country and year specific features to make sure that trends, or few countries/years do not drive this pattern. It reports the residual change in the Gini index in the years of elections (first bar) and in the years with no elections (second bar). The residual change is estimated by regressing the change in the index on country-specific year trends, country and year fixed effects, as well as the dummy indicating income-based Gini. Hence, the mean change in the overall sample is zero in this case. The chart points to a similar fact: The change in the index in the election years is -0.24, whereas it is 0.05 in the non-election years. Thus, election years appear to be associated with a large(r) decline in inequality, relative to the other years in the sample. Motivated by these facts, we next introduce our empirical methodology to investigate this phenomenon in a more formal way.

Figure 1: *Change in income inequality and elections*
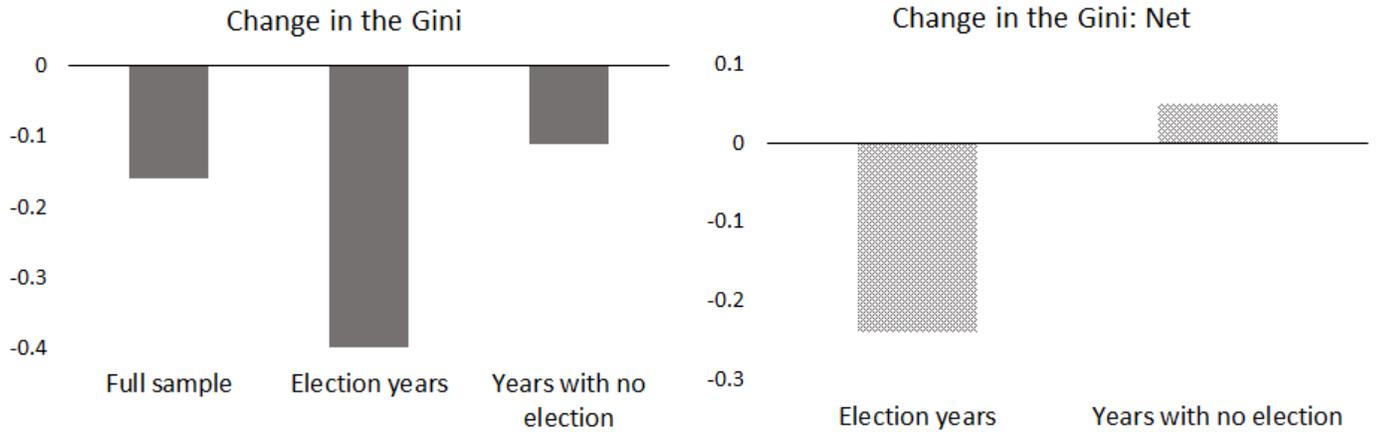
Notes: The figure reports the change in the Gini index. Left panel documents the mean change in the overall sample, in the years of elections, and in the years without an election. The right panel reports the mean change in the Gini index in the election years and in the years without elections, but uses the residual changes. Residuals are obtained by regressing the change in the index on country-specific year trends, country and year fixed effects, and the dummy indicating the income-based Gini calculations. Y axes represent percentage points.

4 Methodology

Our goal is to examine electoral cycles in inequality. The empirical specification is as follows:

$$\Delta\, Gini_{c,t} = \alpha_1\, Election_{c,t}\, +\, \alpha_2\, D_{c,t}^i\, +\, \alpha_3\, Controls_{c,t-1} + \theta_{c,t} + \mu_c\, +\, \mu_t\, +\, \epsilon_{c,t} \qquad (1)$$

where c stands for country and t stands for year. $\Delta\, Gini_{c,t}$ is the percentage point change in the Gini index, our baseline proxy for income inequality. $D_{c,t}^i$ is the dummy variable which takes the value 1 if the Gini is calculated based on income, and 0 otherwise. $Election_{c,t}$ is the dummy variable indicating the years of elections for the chief executive. We expect α_1 to be negative, if election years are associated with a decrease in income inequality.

$Controls_{c,t-1}$ includes real GDP per capita, trade to GDP ratio and credit to GDP ratio, aforementioned. We prefer one-year lagged values of macroeconomic variables to alleviate the reverse causality concern. We also show that the results do not change if these control variables are dropped, or additional control variables are included.

We include country (μ_c) and year (μ_t) fixed effects to account for any effect of time-invariant country-specific features (e.g. geographical characteristics) and common year shocks (e.g. global economic growth, or global financial cycle) on inequality.

Country-specific year trends ($\theta_{c,t}$) allow us to control for pre-existing inequality trends in each country. This is important, particularly given that inequality is likely to show a trend (e.g. Li et al. 1998). If we omit country-specific trends in changes in inequality, the results may be misleading capturing the effect of (potentially) different longer term trends (even before when the data becomes available) rather than the actual effect of the elections.

Moreover, the inclusion of country-specific year trends helps us account for the role of slow-moving factors that can affect inequality, such as (1) rate of technological change (Jau-

motte et al. 2013), (2) demographic factors (e.g. population aging), or (3) declining share of labor (Piketty 2014). If we drop country trends, whenever changes in inequality correlates with, or driven by, such slow-moving factors, our specification would not be able to truly disentangle the effect of elections from their potential effects. Therefore, country-specific trends in our specification address the problem of omitted variables as well. In sum, the inclusion of trends plays an important role in our identification, and they indeed turn out to be statistically significant (to be shown below).

We note that our results are not sensitive to the inclusion of trends or fixed effects, and indeed stay the same with different combinations of those. Standard errors are clustered at the country-level, but we also note that results are virtually the same if we cluster standard errors at the year-level, or do not cluster at all.

5   Results

5.1   Baseline results

Table 1 represents the results on the effect of election years on income inequality based on equation 1 with different combinations of trends, fixed effects and control variables. The first column includes all control variables and country-specific year trends, as well as country and year fixed effects, as shown by equation 1. The coefficient estimate of the dummy for election years is negative and statistically significant at the 5% level. Hence, income inequality decreases in the election years. The size of the impact is economically important. The coefficient estimate suggests that the Gini index decreases by 0.32 points in the election year. This is two times the mean change (-0.16), or alternatively 21.8% of one standard deviation change (1.47) in the sample. Looking at standard deviation of changes in the Gini index within countries, magnitude of the coefficient estimate (0.32) ranges from 10% to 100% of (within-country) standard deviations across the sample.

The first column also reports the coefficient estimates of control variables. Macroeconomic variables do not appear to have much effect on inequality, when we account for trends and fixed effects. Importantly, though, the coefficient estimate of the income-based dummy for the Gini shows that income-based Gini indexes tend to be higher than the consumption-based indexes. This is consistent with the consensus in the literature. We do not report the coefficient estimates of the control variables for the rest of the paper, but they remain similar, particularly the income-based dummy being statistically significant across all tests throughout the paper. Moreover, the coefficient estimates of country-specific year trends are found to be statistically significant (at least at the 10% level) for 20 countries (out of 55). Thus, country-specific year trends turns out to be important ex-post.

Table 1: *Elections and income inequality: Main results*

Variable	Baseline model	No country-year trends	Common trend	Region-year trends	No F.E.s, no trends	No macro controls
Election	-0.32**	-0.30**	-0.31***	-0.30**	-0.29***	-0.31**
	(0.13)	(0.12)	(0.11)	(0.12)	(0.11)	(0.13)
GDP per capita	0.09	0.39	0.06	0.35	0.13***	
	(1.13)	(0.44)	(0.39)	(0.42)	(0.04)	
Trade	-0.01	-0.00	-0.00	-0.00	0.00	
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	
Credit	0.01*	0.01**	0.00	0.01**	0.00	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Income − based	0.85***	0.09	-0.02	0.08	-0.13	0.88***
	(0.25)	(0.41)	(0.37)	(0.41)	(0.11)	(0.25)
Control variables	Yes	Yes	Yes	Yes	Yes	No
Country-specific year trends	Yes	No	No	No	No	Yes
Country fixed effects	Yes	Yes	Yes	Yes	No	Yes
Year fixed effects	Yes	Yes	No	Yes	No	Yes
R^2	0.17	0.13	0.05	0.13	0.02	0.16
Countries	55	55	55	55	55	55
Observations	815	815	815	815	815	815

*Notes: The results are based on equation 1 with different combinations of year trends and fixed effects. The first column includes country-specific year trends, as well as country and year fixed effects. The second column drops country-specific year trends, but keeps country and year fixed effects. The third column replaces country-specific year trends and year fixed effects with a common year trend, and includes country fixed effects. The fourth column replaces country-specific year trends with region-specific year trends (based on continents), and includes country and year fixed effects. The fifth column includes neither country-specific year trends nor country and year fixed effects. The last column includes country-specific year trends, as well as country and year fixed effects, but drops macroeconomic control variables. The dependent variable is the change in the Gini index. Election is a dummy variable indicating the years of elections. Control variables are the logarithm of real GDP per capita, trade to GDP ratio and credit to GDP ratio. The dummy indicating whether the Gini index is based on income is also included. Standard errors in parentheses are clustered at the country-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

The rest of the results in Table 1 shows that the finding in this paper is not sensitive to the inclusion of trends, fixed effects or control variables. The second column drops country-specific year trends, since although conceptually being important, this approach is very stringent and demanding. Country trends may be "over-controlling" and absorbing part of the effect of elections as well.

The third column replaces country-specific year trends with a common year trend, since rather than country-specific trends, global trends (e.g. globalization) might be a common driving force for inequality within countries. The fourth column accounts for region-specific year trends as a potential driver of inequality. Neither common trend nor region-specific trends have statistically significant coefficient estimates in these regressions, suggesting that country-specific trends are indeed more important for inequality. The fifth column drops country and year fixed effects, and country-specific year trends. The last column tests the relationship between elections and inequality when macroeconomic control variables are dropped. The result on the relationship between inequality and election years, both qualitatively and quantitatively, remains very similar across these tests.

Before going into the robustness, we note that one should be careful in interpreting this result. Lower income inequality does not necessarily mean lower wealth inequality. Our result is that income inequality decreases in the years of elections. Whether wealth distribution likewise becomes more equal in election years is still an open question, and cannot be evaluated from this finding.

5.2 Robustness

5.2.1 Income inequality measures

This section tests whether the result is robust to using different definitions or data sources to proxy changes in income inequality. Table 2 documents the results. We start by replacing the change in the Gini index with dummies for large fluctuations.

First, we replace the change in the Gini with a dummy variable which takes 1 whenever the change is below the 25th percentile within a country, and 0 otherwise. This aims to explore whether election years increase the probability of a relatively larger decline in inequality within that country. Second, we use a dummy variable which is assigned 1 whenever the change in the Gini index is above the 75th percentile within a country, and 0 otherwise. The goal here is to examine if the probability of a large deterioration in income inequality becomes less likely in the election years. We use probit models to estimate these regressions. Results show that election years makes a larger decline in inequality more likely (column 1), whereas larger increases are less likely in the election years (column 2). These are consistent with the previous results.

Next, we calculate the change in the Gini index in percent (column 3), rather than percentage points, by normalizing the change with the initial level of inequality to account for any heterogeneous effects arising from the initial level. The result suggests that income inequality decreases by 0.72% relative to its initial level. This is more than two times the mean change in the sample (-0.30%).

It is important to check whether the previous result stays similar when we take the average level of inequality into account. In countries where inequality tends to be higher, governments might have more incentives to reduce it considering the large room for improvement and potential political dividend. To test whether countries with high inequality

can drive the result, column 4 runs a weighted regression, where weights are designed to suppress the influence of countries with high inequality, i.e. weights are the inverse value of (100 minus) the average Gini index for each country. The result does not change much.

Now, we switch to different proxies for inequality rather than the Gini index. We focus on specific percentiles of the income distribution. In column 5, we adopt the change in the ratio of the income share of the top 10 percentile to the income share of the bottom 10 percentile. The result shows that election years decrease this ratio, meaning a lower inequality between those percentiles in the years of elections. The size of the impact is economically important: In the election years, this ratio decreases almost by 17.3% of one standard deviation of the change across the sample.

To dig deeper into this finding, we also run regressions by using the changes in the top and bottom 10 percentile income shares separately, instead of using the ratio of the two. We find weaker evidence on the upper part of the income distribution (column 6). Election years lead to a decrease in the top 10 percentile income share, but the coefficient estimate is statistically significant at the 10% level. On the other side, when we focus on the lower part of the distribution (column 7), the coefficient is positive and statistically significant at the 1% level, meaning a more significant increase in the income of the bottom 10 percentile.

In column 8, we use the change in the ratio of the income share of the highest 20 percentile to the income share of the lowest 20 percentile, rather than the share of top and bottom 10 percentiles. The result is the same. Finally, we adopt the Gini index provided by the SWIID in column 9. Although the sample is much larger in this test, the result stays similar. We conclude that the result is robust to using different definitions or sources for income inequality.

Table 2: *Alternative income inequality measures*

Variable	Large decline	Large increase	Percent change	Weighted regression	Top/bottom 10 ptile	Top 10 ptile	Bottom 10 ptile	Top/bottom 20 ptile	SWIID Gini
Election	0.05**	-0.12**	-0.72**	-0.28**	-3.37**	-0.21*	0.06***	-0.61**	-0.02**
	(0.02)	(0.05)	(0.31)	(0.12)	(1.42)	(0.12)	(0.02)	(0.26)	(0.01)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific year trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 / pseudo R^2	0.13	0.13	0.15	0.16	0.12	0.14	0.16	0.16	0.50
Countries	55	55	55	55	55	55	55	55	135
Observations	798	804	815	815	815	815	815	815	3279

Notes: The results are based on equation 1. First two columns run probit models where the dependent variables are (1) a dummy variable which takes 1 whenever the change in the Gini coefficient is below the 25th percentile within a country (column 1), and (2) a dummy variable which takes 1 whenever the change in the Gini coefficient is above the 75th percentile within a country (column 2). The third column calculates the change in the Gini coefficient normalized by the initial value (i.e. in percent). The fourth column runs a weighted regression where weights are the inverse (100 minus) average Gini index for each country. The fifth column uses the income share of the highest 10 percentile normalized by the income share of the lowest 10 percentile from the PovcalNet database. The sixth (seventh) column uses the income share of top (bottom) 10 percentile. The eighth column adopts the income share of the highest 20 percentile normalized by the income share of the lowest 20 percentile from the PovcalNet database. The last column uses the change in the Gini index based on the SWIID. Election is a dummy variable indicating the years of elections. Control variables are the logarithm of real GDP per capita, trade to GDP ratio and credit to GDP ratio. The dummy indicating whether the Gini index is based on income is also included. Standard errors in parentheses are clustered at the country-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2.2 Other controls

In this section, we include additional control variables, on top of existing ones (namely, GDP per capita, trade, credit and income-based dummy). In columns 1-9 in Table 3, we add the lagged values of a measure of financial openness (Chinn-Ito index), growth rate of real GDP, a measure of aggregate productivity (TFP), human capital index, a proxy for institutional quality (the index on political rights), political orientation of existing executives (left, center or right), the number of years during which the incumbent executive has been in the office, political system (presidential, assembly-elected president or parliamentary) and a dummy for banking crises, respectively. Although sample size substantially shrinks in some of those tests, the main result remains the same. We also note that these additional control variables have statistically insignificant coefficient estimates. The last column adds all controls together, and the result still remains similar.

In unreported results, we also check the result after controlling for other variables which may be associated with inequality, such as inflation rate, exchange rate depreciation, broad money to GDP ratio, age dependency ratios (overall, old, young), or square of GDP per capita. The main result stays virtually the same and the coefficient estimates of those additional variables are statistically insignificant generally with large p-values. Although we cannot rule out that possibility fully, having tested our main result with a large set of additional control variables, we conclude that it is not likely to be driven by omitted variables.

Table 3: *Other controls*

Variable	Financial openness	Growth	TFP	Human capital	Institution quality	Political orientation	Years in office	Political system	Bank crises	All controls
Election	-0.31**	-0.32**	-0.32**	-0.35***	-0.33**	-0.47**	-0.35**	-0.32**	-0.32**	-0.45**
	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.18)	(0.14)	(0.13)	(0.13)	(0.19)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific year trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.17	0.17	0.18	0.18	0.17	0.21	0.17	0.17	0.17	0.24
Countries	54	55	51	52	54	46	55	55	55	42
Observations	799	815	746	767	801	588	813	815	815	531

Notes: The results are based on equation 1. Column 1-9 include a measure of financial openness (Chinn-Ito index), growth rate of real GDP, a measure of aggregate productivity (TFP), human capital index, a proxy for institutional quality (Freedom House political rights index), political orientation of incumbent executives (left, center, right), the number of years during which the incumbent executive has been in the office, political system (presidential, assembly-elected president, parliamentary) and a dummy for banking crises, on top of the control variables in equation 1, respectively. The last column includes all those variables. Control variables are the logarithm of real GDP per capita, trade to GDP ratio and credit to GDP ratio. The dummy indicating whether the Gini index is based on income is also included. Standard errors in parentheses are clustered at the country-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2.3 Sample and elections

In the last set of robustness, we focus on the sample and election dates. In the first column in Table 4, we test the relationship using data only from EMDEs, since it seems like inequality is a problem to a larger extent in EMDEs relative to advanced economies. In the data, the average Gini index for EMDEs is 41.9, whereas it is 30.8 for advanced economies. The result stays similar in this subsample consisting of EMDEs.

The second column is a weighted regression where weights are the lagged values of logarithm of population. This help us get a more complete picture of the change in inequality across the globe, with more weights to larger populations representing country sizes and hence relevance for the world (see Milanovic 2011b). The result is the same. The result also stays the same if we use the logarithm of the lagged GDP as weights, instead of population, to rule out the possibility that smaller economies drive the result. We further note that the results throughout this paper are not affected, if we drop China from the sample.

The third column aims to address the concern whether few large swings in inequality may drive the result. We winsorize the change in the Gini index at the 1-99% levels to suppress the effect of larger changes. The coefficient estimate of the election dummy is not much affected. In unreported results, we run other tests for outliers. The result does not change if we adopt more conservative approaches, namely by (1) winsorizing the change in the Gini at the 5-95% levels, or (2) restricting the change between -2 and 2 points.

Until now, we do a fixed adjustment for income- and consumption-based Gini indexes by including the income-based dummy into regressions. As mentioned before, however, this may not be ideal, if the relationship between the income- versus consumption-based surveys and the Gini changes across time and/or across countries. To mitigate that concern, we now use data only from countries for which the Gini index is calculated based on income

surveys for all years in the sample. The majority of our sample (41 countries) falls into this category, which makes this test feasible to run. The fourth column shows that the result is robust.

Next, we impose different criteria to include countries in the sample. In column 5, we skip the restriction on the number of observations, and include few countries with less than five observations as well. This adds only four countries. The sixth column follows a more conservative restriction and drops countries that have less than ten observations, instead of five. We lose only four countries. In column 7, we now restrict the sample to countries with at least two elections, instead of one. This drops eight countries. The result is similar throughout these tests.

The last test is to rule out the role of a concern on reverse causality, in cases of irregular elections. Economic developments, such as a sudden and large increase in inequality may force incumbent governments to hold an election, which can make the timing of elections endogenous to changes in inequality. On the other hand, in the case of such elections, ruling parties may not have enough time to take policy actions leading to less inequality, which potentially works against our findings. There is another potential channel that works against our result during irregular elections. Incumbent governments, when they have enough power, may prefer to hold elections earlier than scheduled if they believe that the probability of re-election becomes high at that point in time due to some factors which are not related to economic variables. If this is the case, they may not need to implement policies to mitigate inequality, since they think that they are likely to win anyway.

It is testable whether these actually affect our result. For this purpose, we identify irregular elections from the DPI, and assign the dummy for election years 0 for those elections. Thus, we only consider regular elections whose timing are truly exogenous. The last

column illustrates the result. The coefficient estimate remains similar, when we focus on regular elections. Hence, reverse causality is not a very relevant explanation for our result. This finding further suggests that electoral cycles in policies play a role in the findings, since incumbents may have more time and incentives for taking policy actions during regular elections as discussed above.[18]

Table 4: *Sample and elections*

Variable	EMDEs	Population weighted	Winsorized	Income-based Gini	Include few obs.	At least 10 obs.	At least 2 elections	Regular elections
Election	-0.46**	-0.31**	-0.28**	-0.39**	-0.31**	-0.33**	-0.35***	-0.34**
	(0.19)	(0.13)	(0.12)	(0.16)	(0.15)	(0.13)	(0.13)	(0.13)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific year trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.19	0.17	0.18	0.19	0.19	0.16	0.17	0.17
Countries	32	55	55	41	59	51	47	55
Observations	517	815	815	601	828	786	732	815

*Notes: The results are based on equation 1. The first column drops advanced economies as defined by the IMF, and run the analysis for EMDEs. The second column uses weighted regressions where weights are the logarithm of population. The third column shows the result when the change in the Gini coefficient is winsorized at the 1-99% levels. The fourth column uses data only from countries for which the Gini is income-based for all years in the sample. The fifth column extends the sample by including countries with only few observations. The sixth column uses data only from countries which have at least 10 observations in the sample. The seventh column uses data only from countries which had at least 2 elections in the sample. The last column revises the dummy for election years assigning zero for irregular elections. Control variables are the logarithm of real GDP per capita, trade to GDP ratio and credit to GDP ratio. The dummy indicating whether the Gini index is based on income is also included. Standard errors in parentheses are clustered at the country-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

5.3 Close elections

If political cycles in economic policies serve as a channel in the findings above, one would expect their effects to be stronger in close elections. Whenever incumbent governments feel more uncertain about electoral outcomes (i.e. during close elections), they have more incentives to lean towards policies to spur their popularity. Thus, the effect of such policies must be stronger in economic outcomes, in inequality for our case. We test whether this is

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[18]Another point on election dates is about elections that took place in first few months of a year, since once the elections pass, governments may not have much incentives to lean towards policies to eradicate inequality. This may actually work against us. In line with this reasoning, once we exclude those elections (taking place during early months), we find that the effect of elections on inequality remains similar.

the case, focusing on close versus non-close elections in our sample. Table 5 represents the results.

We rely on the election outcomes as a proxy for closeness of elections, as mentioned before. Since there is no unique way to define close elections, we adopt the simplest and most widely-used definition in the literature by taking the gap in the vote shares of the largest two political parties in each election. We assign the dummy for close (non-close) elections 1 if the gap is below (above) the sample median, and 0 otherwise. Thus, an election is defined as close, if the winner achieves a margin of victory that is less than half of the elections.

In each subsample, we run the test for countries that have at least one close and one non-close election. Results show that the effect of election years on inequality is concentrated on close elections. The coefficient estimate is statistically insignificant for non-close elections, although it is still negative. We conclude that electoral cycles in inequality are stronger during the elections where incumbents face higher uncertainty, meaning that increased political competition (and relatedly more active economic policies by governments) is a key channel for our findings in this paper.

Table 5: *Close versus non-close elections*

Variable	Close elections	Non-close elections
Election	-0.47**	-0.20
	(0.23)	(0.18)
Control variables	Yes	Yes
Country-specific year trends	Yes	Yes
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
R^2	0.24	0.16
Countries	34	34
Observations	504	516

Notes: The results are based on equation 1. The dummy for election years is assigned 1 only for close (non-close) elections in column 1 (column 2). An election is categorized as close (non-close) whenever the vote gap between the largest 2 parties is below (above) the sample median. Control variables are the logarithm of real GDP per capita, trade to GDP ratio and credit to GDP ratio. The dummy indicating whether the Gini index is based on income is also included. Standard errors in parentheses are clustered at the country-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.4 Elections and education expenditure

Until now, we have established the evidence on the electoral cycles in income inequality (a form of inequality of outcomes). Now, we explore whether electoral cycles also exist in the case of inequality of opportunity, namely in access to education. Since we do not have a direct and comprehensive proxy for inequality in access to education in a cross-country setting dating back in time, we focus on education expenditure by the government. We adopt the government expenditure on education as share of total government expenditure as the dependent variable.

While testing the election year effect on education spending in the government budget, we control for total government expenditure as share of GDP, GDP per capita (in logarithm, constant in 2010 USD) and institutional quality. Country-specific trends, country and year fixed effects are also included. Standard errors are clustered at the country-level.

The first column in Table 6 illustrates that the share of education expenditure in the government expenditure increases by 0.29 percentage points in the election years. This suggests that governments channel more resources into education in the years of elections. The coefficient estimates of control variables do not appear to be significant. Columns 2-4 show that the result is not sensitive to the inclusion of fixed effects, trends or control variables.

These findings suggest that electoral cycles also exist in the case of inequality of opportunity, i.e. access to education. However, there are several crucial points that limit our interpretation. First, it is not possible to comment whether higher public education expenditure always means a better and wide-spread access to education by the disadvantaged groups. Higher expenditure would mitigate inequality if, for instance, those resources are allocated to build schools in poorer areas, or as in-kind transfers to poorer households for their children's education. Second, the quality of education of the poor is also a key to achieve better

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jobs and higher income. Our result on higher education expenditure does not necessarily imply a higher quality of education.

Table 6: *Elections and education expenditure*

Variable	Baseline model	No country-year trends	No F.E.s, no trends	No macro controls
Election	0.29**	0.31**	0.31**	0.30**
	(0.13)	(0.13)	(0.12)	(0.13)
Govt. expenditure	0.07	0.02	-0.00	
	(0.05)	(0.02)	(0.00)	
GDP per capita	0.92	0.95	-0.00	
	(0.74)	(0.67)	(0.03)	
Institutional quality	0.02	-0.00	-0.00	
	(0.13)	(0.08)	(0.03)	
Control variables	Yes	Yes	Yes	No
Country-specific year trends	Yes	No	No	Yes
Country fixed effects	Yes	Yes	No	Yes
Year fixed effects	Yes	Yes	No	Yes
R^2	0.14	0.07	0.00	0.14
Countries	128	128	128	128
Observations	1693	1693	1693	1693

Notes: The dependent variable is government expenditure on education as share of government expenditure. Election is a dummy variable indicating the years of elections. Control variables include the logarithm of GDP per capita (constant in 2010 USD), institutional quality and government expenditure as share of GDP. Standard errors in parentheses are clustered at the country-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6 Conclusion

Inequality is a major issue for policy makers across the globe. It is crucial to understand what drives it. Economic policies play a fundamental role in tackling the problem of inequality within countries, but those policies are subject to electoral cycles. This raises an important question whether inequality tends to exhibit electoral cycles. To the best of our knowledge, our paper is the first showing a strong link between elections and inequality for a large set of countries over years. We show evidence that income inequality decreases in the years of elections. This relationship is not likely to be driven by omitted variables or reverse causality.

The effect of election years on inequality is concentrated on close elections, further strengthening the reasoning that economic policies around elections are the driver, since the incumbent politicians have more incentives to rely on policies whenever they face more uncertainty about the election outcomes. Finally, we show that elections may mitigate inequality of opportunity, namely in access to education. Governments allocate more resources to education in the years of elections. Therefore, besides the immediate effect of elections on income inequality, there can be further longer term inequality-reducing gains as more resources are spent for education. Our findings point to the role of well-functioning democracies and increased political competition to tackle inequality within countries. This is even more crucial in the post-pandemic recovery, given the scarring of the COVID-19 shock in inequality.

It is worth to note that our findings do not imply that electoral cycles in economic policies are always good for the economy, or for the society, as a whole. We narrowly focus on the income inequality consequences of electoral cycles. Generally speaking, policies can be designed to address inequality without unintended consequences on the economy. However, whether electoral cycles in economic policies have repercussions on the economy is a big and still open question. Therefore, whether inequality gains from electoral cycles as we find overweigh, if any, other (negative) effects of electoral cycles on the economy, is hard to answer and beyond the scope of our paper. Lastly, we do not answer whether (or how much) inequality is good or bad for economies in this paper.

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Appendix

Variables

Table A1 provides a summary of the variables. For more details, see Section 2.

Table A1: *Description of variables*

Variable	Description	Source
Income inequality	Gini index	PovcalNet (and SWIID)
	Income shares(top/bottom 10 and 20 percentiles)	PovcalNet
Election	Election years for the chief executive	DPI
Political orientation	Left, center, right	DPI
Years in office	Years during which the executives in the office	DPI
Political system	Presidential, assembly-elected president, parliamentary	DPI
Vote shares	Parties' vote shares (used to define close elections)	DPI
Institutional quality	Political rights index	Freedom House
GDP per capita	Used in logarithm and constant in 2010 USD	WDI
GDP growth	Growth rate of GDP constant in 2010 USD	WDI
Trade	As share of GDP	WDI
Credit	As share of GDP	WDI
Population	Used in logarithm as weights in robustness	WDI
Government expenditure	As share of GDP	WDI
Capital account openness	De jure measure	Chinn-Ito
Banking crises	The dummy for years of crises	Laeven-Valencia
Human capital index	Years of schooling and rate of return to education	PWT
TFP	Aggregate total factor productivity	PWT
Education	Education expenditure as share of government expenditure	WDI

Summary statistics and correlations

Table A2 provides summary statistics for income inequality measures. As shown by Table A3 pairwise correlations are positive and statistically significant at the 1% level.

Table A2: *Summary statistics*

Variable	25th ptile	Mean	50th ptile	75th ptile	Std. dev.	Observations
Δ Gini (PovcalNet)	-0.90	-0.16	-0.20	0.50	1.47	815
Δ Top/Bottom 10 ptiles ratio (PovcalNet)	-0.76	-0.64	-0.09	0.46	19.48	815
Δ Top/Bottom 20 ptiles ratio (PovcalNet)	-0.34	-0.17	-0.05	0.19	1.71	815
Δ Gini (SWIID)	-0.10	0.03	0	0.20	0.30	3279

Table A3: *Correlations*

Variable	Gini (PovcalNet)	Top/Bottom 10 (PovcalNet)	Top/Bottom 20 (PovcalNet)	Gini (SWIID)
Gini (PovcalNet)	1			
Top/Bottom 10 (PovcalNet)	0.65***	1		
Top/Bottom 20 (PovcalNet)	0.91***	0.81***	1	
Gini (SWIID)	0.41***	0.28***	0.38***	1

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Countries

Argentina, Armenia, Austria, Belarus, Belgium, Bolivia, Brazil, Bulgaria, China, Colombia, Costa Rica, Croatia, Cyprus, Czechia, Denmark, Dominican Republic, Ecuador, El Salvador, Estonia, Finland, France, Georgia, Germany, Greece, Honduras, Hungary, Iceland, Indonesia, Ireland, Italy, Kazakhstan, Kyrgyz Republic, Lithuania, Luxembourg, Malta, Moldova, Netherlands, North Macedonia, Norway, Panama, Paraguay, Peru, Poland, Portugal, Romania, Russia, Slovak Republic, Spain, Sweden, Switzerland, Thailand, Turkey, Ukraine, United Kingdom, Uruguay.