

The impact of inequality on turnout - New evidence on a burgeoning debate

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Abstract:

A burgeoning literature has started to address the link between income inequality and electoral turnout. The majority of these studies find that higher income inequality decreases turnout. However, these studies only find this relationship for a small sample of Western countries. Somewhat perversely, this practice has fed on the idea that Western and non-Western democracies have different vote functions. For example, a recent paper in *AJPS* on participation explicitly justified a using sample restricted to Western democratic countries arguing that the clientelism pervasive in the non-Western countries made systematically altered the turnout function.

This paper tackle these two, seemingly disparate issues in an effort to expand our ability to predict democratic turnout around the world. First, taking a broad international sample of liberal democracies, we examine whether there is in fact any general connection between inequality and turnout. Second, in the process, we also explores whether turnout in Western and non-Western democracies is driven by fundamentally different models due to the persistence of clientelism in the latter.

Findings: We find almost no evidence that turnout is affected by inequality. We find strong evidence that more clientelistic countries have lower average turnout, but do not find that clientelism otherwise affects other variables in the turnout function. In fact, two policy variables, compulsory voting and scheduling elections appear to be decisive to raise turnout considerably around the world.

Introduction

Recent decades have seen a decline in the most conventional form of political participation – electoral turnout. Between the 1970s and the late 1990s turnout rates fell by 4.4 percent in established democracies (Franklin 2004, 10). These falling turnout rates are often seen as a mark of disengagement or disaffection by the citizenry (Dalton 2002; Norris 1999).

Simultaneous with this secular trend towards lower voter turnout, we have also witnessed increasing economic inequality among citizens in many of the democracies of the West. These two trends highlight a vital question in political economy: to what degree is economic inequality compatible with political equality (democracy)?

Several recent studies attempt to provide some insight to this question, focusing on the impact of inequality on political participation in elections. While electoral participation is only one part of what is implied by democratic equality, it is certainly an important one. Political analyses typically take as given that elections are a kind of poll of preferences in society. If economic inequality affects who votes, then elections may produce a biased sample of preferences. Even if, in the short run, inequality affects only participation (and not preference aggregation), participation in elections is foundational for democracy in the long run.¹

Despite the importance of this question in political economy, there are surprisingly few empirical studies of the relationship between economic inequality and participation. This is particularly true when it comes to the effects of social inequality on participation. As we

¹ As long as those who turnout reflect the relative distribution of preferences in the population, there is no “misrepresentation” in the legislature. Imagine 100 people can be grouped into 10 groups with identical preferences. As long as exactly 1 person in each group votes in the election, 10% turnout is the same as full turnout: interests are correctly represented.

elaborate below, existing studies of the relationship between participation and inequality are subject to several criticisms. One particularly important criticism is the disconnection between the restricted sample of countries used to evaluate the relationship between equality and participation and the more general inferences that are made about those findings.

The great expansion of democratic countries in recent decades has run ahead of scholarship on inequality and turnout. Existing studies are based on samples comprised almost exclusively of the wealthiest Western countries. (This is less true of studies of turnout more generally.) Indeed, many studies are limited to variations within the United States. Armed with data on a wider range of countries, we are in a position to explore how good our existing explanations are, and to uncover how different (or similar) new and old democracies are. To this end, we address two questions in this paper:

- a) Is there a general relationship between inequality and turnout among all democracies?
- b) Are the turnout functions describing turnout in Western and non-Western democracies basically the same, or very different?

The remainder of this paper proceeds as follows. In the next section, we very briefly review the three existing explanations relating inequality and voter turnout and we discuss limitations of the existing empirical research. In section three we present our international dataset and the standard model of turnout (which does not include inequality). We then present our estimates of the model and assess our two questions: First, is there evidence of an effect of equality on turnout? Second, is the turnout function different between Western and non-Western democracies? We then conclude and make some suggestions for future research.

Section 2: Previous Literature

There are three main theories about the relationship between inequality and electoral participation. Conceivably the most widespread view suggests that economic inequality *reduces* electoral participation (Dahl 2006; Goodin and Drysek 1980, Schattschneider 1960; Solt 2008). Perhaps the most explicit exposition of this view is in Goodin and Drysek (1980). The two authors argue that when economic power is skewed, the political “success” is also likely to be skewed, alienating the relatively poor from political participation. This leaves the political realm to be dominated by the competing interests of the better off. A second view of the electoral effects of inequality takes the exact opposite position. It suggests that higher social inequality *increases* participation, because inequality polarizes the policy preferences of the poor and rich, leading both groups to mobilize to higher degrees (Brady 2004; Oliver 2007). There is also a third explanation which suggests that there is no effect of inequality on average turnout. Any effects occur only within income groups in a country, as the relatively rich participate more and the poor less with higher inequality. This last explanation suggests that inequality may impart biases in representation, but should not impact overall turnout (Ansolabehere, de Figueiredo and Snyder 2003)

Previous empirical work on the question of inequality and turnout is very limited, and, as mentioned previously, tends to find that inequality suppresses turnout. Using data from the late 1950s, Goodin and Drysek (1980, 283) report that national-level inequality – measured as national gini coefficients – is inversely related to voter turnout in a sample of 37 countries. They list several studies with comparable findings. Merrifield (1993) using state-by-state turnout in

the 1982 US midterm Congressional election also finds a correlation between higher poverty rates and lower turnout.

More recently, Mahler (2002) compares regional inequality and voter turnout in sub-national regions within twelve wealthy democracies, and uncovers that more unequal regions have lower turnout. Boix (2003) and Solt (2004) also report evidence that inequality suppresses turnout in single-country studies of sub-national regions in the United States and Italy, respectively. In addition, Lister's (2007) pooled time series analysis of 15 wealthy countries finds evidence that inequality suppresses turnout. Finally, Solt (2008) as well as Anderson and Baramendi (2008), using individual level survey data for Western democracies, argue that individuals living in more unequal societies are less likely to vote. They also report that aggregate inequality suppresses turnout more among the poor than it did among the rich.

A distinguishing feature of all of these studies is that they rely on wealthy Western countries. Lister (2003), Mahler (2002) and Anderson and Baramendi (2008) rely exclusively on the richest developed countries. Solt (2008) adds several Eastern European countries that entered the European Union a decade after the collapse of the Soviet Bloc. Why are their samples restricted to these countries? The most obvious explanation is convenience.

Democracy is a historic rarity in non-Western countries. Moreover, until recently, information about details of elections in other countries was not widely available. With a notable exception (mentioned below), none of these studies discusses whether or not their results are really useful for non-Western democracies.

Reliance on these limited samples is increasingly out of date. The number of democratic countries has grown tremendously in the last two decades. It now extends well beyond the

West. There is increasingly robust information about theoretically important predictors of turnout (including data on inequality) outside of Western countries. Given the potential problems with “biased” case-selection (see Geddes 2003), analyzing data for a more representative sample of democracies is long overdue.

One argument against pooling including non-Western democracies is these countries are radically different societies vis-à-vis the West, requiring completely different explanatory models (for example, different sets of explanatory variables or functional forms). Solt (2008), for example, provides an explicit explanation for excluding non-Western countries in his study. In those countries, he suggests that voting is not an expression of political preferences, but an act of explicit clientelistic exchange. As he puts it:

Differences in the character of much political engagement in the democracies of the developing world similarly counsel against including these countries in this study. Many impoverished citizens in these poorer democracies trade their political support for particularistic benefits and lose access to these needed resources if they fail to vote as directed. Participating in clientelistic networks in this way is distinctly different from engaging in politics to express one’s political preferences; indeed clientelistic political engagement is often viewed as evidence of the absence of democratic rights... (50)

While the claim for a “domain restriction” like this may sometimes be justified, it should be used with caution. Arbitrary domain restrictions can undermine the idea of a progressive research program.² One can always “save” a theory by claiming that disconfirming evidence is not really a true test of the theory. But, at the very least, claims democracies in the West and

² One can compare the impact of this kind of approach to work by Blais (2000; 2006) on the impact of proportional representation on turnout. In these two papers, he finds a lack of empirical support for a positive effect of PR largely evaporates when one includes data from developed and developing countries. Rather than “rescuing” this hypothesis by asserting that it is a theory that only works in the West, he calls into question the conventional wisdom.

democracies not in the West are *not* properly covered by a theory should require some demonstration of plausibility. None of the literature on inequality and turnout that we are aware of cites research suggesting that inferences from these models be limited to the Western countries only. If we think about where the new democratic constitutions are being written, the need for a general model (or at least non-Western one) would be helpful.

Solt's (2008) claim that clientelistic networks in non-western condition the effect of other variables (i.e., inequality) in the turnout function seems too *ad hoc*. (It is at least made explicitly.) Clientelism is reasonably well-conceptualized (Kitschelt and Wilkinson 2007). And, there is little empirical evidence to support the idea that it systematically alters the turnout function in *democratic* countries. Simpser's dissertation (2005) does make a claim. Using his own data he argues that what he calls fraudulent elections result in lower turnout than do non-fraudulent elections. (He even argues that most models of turnout assume that the elections are not corrupt, an assumption that may be acceptable in developed countries, but not necessarily in developing ones.) However, this argument does not really apply in these cases. What Simpser (2005) defines as "fraudulent elections" (e.g., Mexico until perhaps the late 1990s) are predominantly those that Solt (2008) and the rest of the literature, explicitly eliminate as "non-democratic." For example, Blais and Dobrzynska (1998) and Bourbeau and Scruggs (2007) have broad international samples of elections that explicitly exclude countries that are not considered politically free by Freedom House or Polity IV. While we cannot verify it without access to the Simpser (2005) data set, we are likely excluding most of his fraudulent

elections from our sample.³ The upshot is then that Simpson's (2005) argument is not really germane to a discussion of turnout functions in liberal democracies.

Section 3 Theoretical and Empirical Model and Data

To evaluate the true impact of income inequality on turnout among all liberal democracies, we need to start with a basic model of turnout and a fully international sample of cases.

Simultaneously, we want to take seriously the possibility that, even within liberal democracies, more "clientelist" political systems fundamentally alter the turnout function. We thus start with a basic turnout model, drawing considerably on Blais' (2006) review. To that baseline model, we add two additional explanatory variables: inequality and clientelism.

Following previous literature on turnout, we exclude countries under authoritarian rule from this analysis, because political choices are severely limited in these cases. We only include elections that are considered "free and fair," which we operationalize as having an overall Polity IV score of 7 or higher. We depart from the standard literature by adding a more explicit "random sampling" procedure for our case selection. While, most studies rely on a contemporary cross-section or pool all relevant elections, we select one post-1945 democratic election per country among those countries for which inequality data is available.⁴ This gives us a sample of 62 elections, including 5 elections from the 1960s, 7 from the 1970s, 11 for the

³ Moreover, Simpson's (2005) results do not imply that having corrupt elections changes the effects of the remaining variables in the turnout function in true democracies. As we show below, corruption lowers turnout on average as an additive effect, but it does not interact with other variables, as we would expect if corruption affected the rest of the turnout function.

⁴ We also attach the condition that the country is currently democratic. The next draft of the paper will relax this limitation and sample from countries with inequality data that were "free" for at least some election in the post-war period.

1980s, 18 from the 1990s and 21 from the 2000s.⁵ Our main reason for not reporting results with all available data (the gini coefficient is the limiting variable) is that, despite being popular properties of “technical fixes” unbalanced panel analysis are still not well known, and in our experience, results are often very sensitive to “technical” specification details. Moreover, there is comparatively little work that explicitly models turnout dynamics, so trying to integrate both time series and cross-sectional elements would necessitate a more complex theoretical model than we currently possess.

A. baseline model:

While there is not a single, definitive model for a cross-national turnout function, Blais’s review of findings (2006) suggests the core of such a model, which can be derived from seminal works on cross-national turnout, such as Jackman (1987) and Powell (1982). On the right hand side, we include six controls which are commonly found to matter in previous studies of turnout. These are organized into three main categories: institutional, socioeconomic and contextual. After that, we address the specification of clientelism in the model.

Dependent Variable: Turnout. The standard measure of the dependent variable in the literature is the turnout rate of eligible voters: the percentage of eligible adult citizens that cast their ballot in the national legislative elections of their country. Turnout data were collected from the IDEA (2008) website or the website of the respective national parliaments.

⁵ This pattern partly reflects trends in democratization around the world—i.e. some countries were not democratic until the last two decades-- but the overly “recent vintage” is more of a reflection of limits on the quality of *inequality* data. We lack reliable measures of overall inequality for many countries prior to the 1990s.

Institutional Factors: The impact of institutional factors is measured in our model by a) the compulsory voting laws, b) the electoral system type and c) the decisiveness of the election. Compulsory voting laws are widely found to be associated with higher turnout. In this paper, we only code (via a dummy variable) a country as having compulsory voting rules when there is some official sanction for failing to vote. The second institutional factor is the electoral system type. Proportional representation in multi-member districts is commonly thought to produce higher turnout rates than systems with single member districts and plurality or majority voting. The rationale for this is that in a majoritarian system, voters have a particularly low incentive to cast ballots when a district is safe or undisputed (Ladner and Millner 1999). While in plurality parties devote their resources disproportionately to competitive districts, they have more incentives to mobilize people throughout the country under PR. Similarly, the electoral incentives for people to vote are higher under PR, because the number of votes approximately translates into the number of seats.⁶

Our last institutional variable is the decisiveness of the election. We hypothesize that the more important the election is in deciding the configuration of democratic political power in a country, the more people are likely to vote. A decisive election is one in which all of the “important” seats of government are standing simultaneously. An indecisive election is one in which significant numbers of the seats of power are contested at other times. As more powerful spaces are openly contested, the greater the interest in choosing and in mobilizing.

⁶ We code mixed-proportional systems, those that attempt to ensure proportionality between votes for party votes and its share of seats in the legislature, as proportional here. Some (like Lipjhart (2001)) also argue that presidentialism may reduce turnout for another reason: Presidential elections make legislative elections less decisive. Since we control for the decisiveness of the election (see below) we should be capturing this effect.

Following Blais and Dobrzyńska (1998), we define an election as maximally decisive (=1) if all major elected positions are determined in the election. For example, all elections in a unitary, unicameral parliamentary system are maximally decisive. The election is partially decisive (=0.5) if there is an alternative election cycle for one of the following: president, provincial government, or second chamber. If more than one of the three institutions of power are elected at a time that is not simultaneous with the lower house election in our sample, the observation is scored zero for decisiveness.⁷

Two examples of federal, bi-cameral, and presidential democracies – Brazil and the United States – illustrate the coding scheme. Congressional elections in the United States cycle have scored between 0 and 0.5 in their decisiveness score, depending on whether or not the presidency is being contested. (Almost all states' lower houses have 2 year terms which are held simultaneously with the federal election cycle.) Because only one-third of the Senate is elected in any two-year period, election years are never completely decisive. In Brazil, the lower house election in 2002 is considered very decisive because the Presidential (first round), Senate, and state-level elections all occurred simultaneously with the election of the Chamber of Deputies).

Socioeconomic factors: Blais (2006) reports that turnout is negatively affected by income, and positively affected by being a very small country. The precise mechanisms, in particular, for the latter finding are in dispute, but we include both in our model. We define

⁷ Data on elections (e.g. closeness, decisiveness, electoral system type, competitiveness) are from Bourbeau and Scruggs 2007, the Center on Democratic Performance 2009, the database Parties and Elections in Europe and the websites of the respective national parliaments.

national wealth as per capita income (at purchasing power parity), and "very small states" as having a population under two million.⁸

Contextual factors. Our final baseline control variable is the competitiveness of the race. Close races are widely found to trigger increased participation in elections. When the expected margin of success between two contenders is small, few votes might be decisive for an election outcome. In such a situation, individuals, who normally stay at home, but who slightly care for one of the two candidates might feel tempted to vote. Undecided races also trigger increased media coverage and campaign activities. Pressured to vote, individuals might actually turn out in higher numbers (Fauvelle-Aymar and Abel 2006). We code a race a competitive (=1) if the vote shares of the top two parties are within 5% of each other, and non-competitive otherwise. Data on party vote shares is from one of several sources: Bourbeau and Scruggs (2007), Election Results Archive (2008), European Parties and Elections in Europe (2008), and from the National Election Commission websites (for Panama and Mauritius).

B. Inequality

Our measure of inequality is the gini coefficient, which is relied on in all of the turnout studies we reviewed. Data for the gini coefficient are obtained from the UNDP Statistics Website (2007) and UNU-WIDER (2008). Though all cross-national studies of relative inequality are subject to a number of measurement problems, the WIDER (2008) dataset provides a number of gini statistics, of widely varying quality and international comparability. Whenever possible we rely

⁸ The source data for GDP per capita, measured at PPP come from the Penn World Table (Heston, Summers and 20xx), supplemented by UN data on real economic growth per capita after 2004. Population data are from the UN.

on gini coefficients based incomes that follow as closely to the standards used in the Luxembourg Income Study. That is, the gini should be computed from the disposable income (or consumption income) of households and weighted by the household size. Using the disposable or consumption income concept is generally considered superior to using market income, because it takes into account the redistributive effects of taxes and transfers. In our view, this is important, because such a distribution is more likely to account for “experienced inequality.” (For example, a gini computed from market income counts those on public pensions as having zero income, while a post-fiscal income concept would count their pension as disposable income.)

The gini coefficient and election years we use for each country are reported (see Appendix 1). Average turnout in our sample is approximately 74%, with a standard deviation of 14%. Turnout ranges from 38.3% in the first round of Mali’s 2001 *Assemblée Nationale* election to 96.3 % in Malta’s 2003 election to the *Kamra tad-Deputati* . The average gini coefficient is 36.5. The minimum value is 21.8 for Denmark in 1977, making it the most equal country in our sample. The maximum value is 73.9 for Namibia in 1994. Inequality is significantly lower in countries with above average income (.30 versus .42).⁹

C. Clientelism:

The final factor in our theoretical model is clientelism. As previously discussed, this indicator is included in this study to evaluate explicitly whether or not clientelistic ties affect voting behaviour, or at least the link between inequality and turnout. It is not obvious how we

⁹ The average country income in our sample is \$11847 per person at PPP.

should allow clientelism to enter the turnout model. Does clientelism simply have an additive effect on turnout, as is typically assumed of the other factors in the turnout function? If so, we can control for its effect on turnout simply by including it as a regressor in the model. An alternative view is that the terms of the standard turnout function vary depending on the extent of clientelism; this suggests that clientelism enters in interaction with the standard model.¹⁰ We test both here.

To our knowledge, there is no readily available indicator of clientelism. We use corruptions perceptions index from Transparency International (TI). Because the TI corruption scores are reversed, our variable takes a value of 1 if the country is *below* the median value in our sample and a zero if they are above. To summarize, the basic regression models for this study is computed as follows. The first has clientelism enter linearly; the second specification has it interact with the other elements of the model.

Specification 1

$$\text{Turnout} = \beta_0 + \beta_1 \text{GINI Coefficient} + \beta_2 D_ \text{Compulsory Voting} + \beta_3 D_ \text{Electoral System} + \beta_4 D_ \text{Decisiveness} + \beta_5 D_ \text{GDP per capita} + \beta_6 D_ \text{Small size} + \beta_7 D_ \text{Election Competitiveness} + \beta_8 \text{Clientelism} + \varepsilon.$$

¹⁰ Clientelistic countries may have a turnout function that is driven by a completely different set of regressor variables on the right hand side. As we will soon see, our results argue against this possibility. This model explains turnout as well or better in clientelistic democracies than it does in non-clientelistic.

Specification 2

Turnout

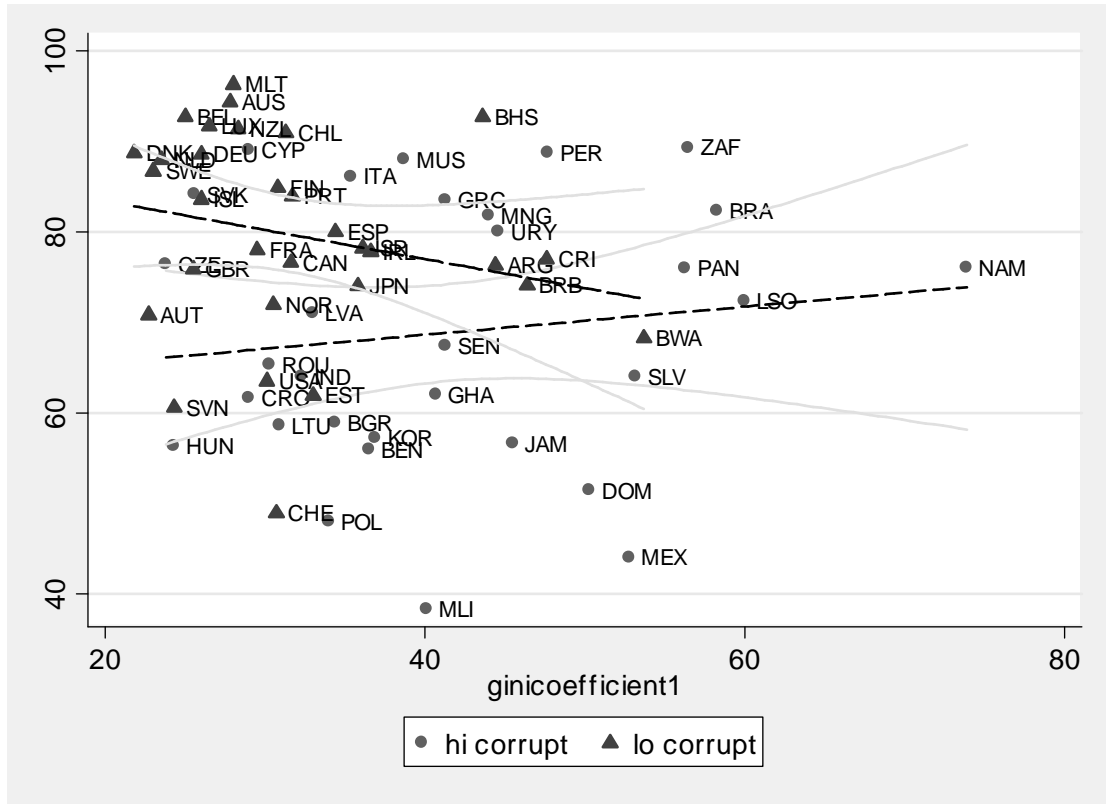
$$\begin{aligned} &= \beta_0 + \beta_1 \text{GINI Coefficient} + \beta_2 D_ \text{Compulsory Voting} + \beta_3 D_ \text{Electoral System} + \beta_4 \\ &\text{Decisiveness} + \beta_5 D \text{ Very Poor} + \beta_6 D \text{ Small size} + \beta_7 \text{Election Competitiveness} + \beta_8 \text{Clientelism} + \\ &\beta_9 \text{Clientelism} * \text{GINI Coefficient} + \beta_{10} \text{Clientelism} * D_ \text{Compulsory Voting} + \beta_{11} \text{Clientelism} \\ &* D_ \text{Electoral System} + \beta_{12} \text{Clientelism} * \text{Decisiveness} + \beta_{13} \text{Clientelism} * \text{GDP per capita} + \\ &\text{Clientelism} * \beta_{14} D \text{ Small size} + \beta_{15} \text{Clientelism} * \text{Election Competitiveness} + \varepsilon. \end{aligned}$$

To demonstrate parameter (in)stability and the leverage of our variables of interest, we report several versions of Specifications 1 and 2 that pare down the number of regressors.

Section 4 Empirical Results and Discussion

To get a very simple sense of how much the relationship between turnout and inequality varies depending on the chosen sample, Figure 1 illustrates the bivariate relationship between inequality and turnout. We use the corruption dummy described above to differentiate between high and low clientelist countries, and estimate a regression line for each subsample. High clientelist countries are represented by circles, while low clientelist countries are represented by squares. The confidence interval for each regression line is shown as well. The scatterplot provides little evidence to support the idea of distinct populations, because the confidence intervals of the two lines overlap along most of the range of the two respective regression lines

Table 1: Scatterplot of gini and turnout for high and low corruption subpopulations



We present the OLS estimates of regression models in Table 2. The reported standard errors are based on robust (i.e., Huber-White consistent) standard errors. The first specification makes no distinction between Western and non-Western democracies, and inserts no control for clientelism. This specification provides little evidence that inequality suppresses electoral participation. The estimated effect of inequality is negative (as expected), but it is substantively small and is far from statistical significance. (Based on the point estimates, if Namibians suddenly experienced Danish levels of income equality, expected turnout would increase by about 1%.)

Table 2:

Turnout	Model 1	Model 2
Gini Coefficient	-.021 (0.170)	.026 (.170)
PR	.467 (3.398)	1.13 (3.204)
Compulsory Voting	12.678*** (2.942)	12.353*** (2.873)
Decisiveness	20.222*** (4.447)	19.658*** (4.395)
Dummy Small State	-1.945 (3.904)	-1.832 (4.435)
GDP per capita	.0731** (.303)	.0414 (.328)
Closeness of the election	-2.392 (2.981)	-.879 (2.921)
Corruption		-7.563*** (3.292)
Constant	50.177*** (9.668)	55.429*** (9.910)
Rsquared	.42	.45
N	62	62

*p<.10, **p<.05, ***p<.001 (two tailed)

The two most important factors in the model are compulsory voting and the decisiveness of the election. Compulsory voting raises turnout by around 13%, which is in line with the range of estimates previously reported. The decisiveness of the election has a similarly large effect, increasing turnout over 19% when one moves to a system of simultaneous elections for powerful offices to elections that are staggered in time. Income per capita is

expected to raise turnout considerably. An additional \$10,000 in income per capita raises turnout by 7 %.

We find no support for several other conventional claims. Our estimates for population and the closeness of the election have the wrong sign, (although neither estimate is statistically significant). Consistent with Blais' recent observation, we find little support for the idea that, once one controls for the decisiveness of an election, the proportionality of the electoral system matters.

The second column of Table 2 adds only the dummy term for high corruption.¹¹ High corruption does appear to reduce turnout, by around 8%. Adding this variable has a large effect on the estimate for income per capita, reducing its estimated coefficient by around forty percent (from .73 to .41), while producing little impact on the standard error. The overall result makes the estimated effect of income per capita on turnout no longer statistically significant. (Income is negatively correlated with corruption.).

The other coefficients are not greatly affected by including our proxy for clientelism to the model this way. The estimated effect of inequality does switch signs, but the substantive impact is still very small and not statistically significant. The fact that corruption dummy matters, but has a small effect on the other individual estimates in the traditional turnout model is circumstantial evidence that turnout in clientelist democracies is explained by this turnout model as well as non-clientalist ones.

¹¹ Four of the countries in Solt's (2008) analysis are among the more corrupt. Czech Republic (5.37), Poland (5.1), Hungary (5) and Italy (4.1) are all below the median in our sample. This suggests that the domain limitation maintained at least explicitly in his paper (and implicitly in much of the literature) serves mostly to avoid empirical inconvenience than to uphold theoretical principle.

Interaction Model: The next set of estimates presents more definitive evidence that clientelist and non-clientelist turnout functions are the same, at least with respect to these variables. Columns 1 and 2 of table 3 contain estimates for the dummy variable interaction model that is our second specification. The coefficients in above the line in Columns 1-5 represent the estimates for non-clientelist countries (i.e., Corruption dummy=0). The coefficients below the line are the coefficients and inferences statistics for *differences* between the non-clientelist coefficients and clientelist country coefficients.

It is true that differences in coefficients across the two groups appear large in some cases, but none of the model variables, except for small countries and wealth, are close enough to conventional standards of statistical significance to justify the conclusion that there are different underlying relationships between the clientelist and non-clientelist democracies. Columns 4-7, pare down some of the regressors in an effort to see if we can find some differences based on a more unconstrained model, which does not change this result. Our initial sensitivity checks of the results—e.g., inspections of the partial plots and estimates with a robust estimator that reduces the impact of influential cases or those with high residuals—does not substantively alter the results, in particular for inequality.

Table 2:

Turnout	Model 1	Model 2	Model 3	Model 4	Model 5
Gini Coefficient	-.353 (.399)	-.352 (.394)	-.35 (.389)	-.317 (.391)	-.288 (.372)
PR	-2.484 (4.028)	-2.505 (3.913)	-2.329 (3.898)		
Compulsory Voting	11.635*** (3.508)	11.771*** (2.705)	11.58*** (2.577)	11.651*** (2.446)	11.8*** (2.392)
Decisiveness	17.751*** (5.429)	17.772*** (5.326)	17.72*** (5.195)	17.164*** (4.618)	18.2*** (3.968)
Dummy Small State	-3.839 (6.442)	-3.852 (6.323)	-3.885 (6.233)	-3.495 (6.134)	-4.181 (5.859)
GDP per capita	-.126 (.551)	-.126 (.544)	-.113 (.546)	-.152 (.531)	-.1 (.523)
Closeness of the election	.46 (3.251)	.468 (3.217)			
Corruption					
Constant	80.228 (21.012)	80.18 (20.766)	80.032 (20.549)	78.252 (20.647)	75.514 (19.55)
Corruption*Gini	.36 (.44)	.36 (.435)	.366 (.433)	.376 (.435)	.359 (.359)
Corruption*PR	5.163 (6.611)	5.228 (6.224)	5.238 (6.137)		
Corruption*Comp. Voting	.39 (5.583)				
Corruption*Decisiveness	3.393 (9.427)	3.358 (9.342)	2.772 (8.789)	3.633 (8.429)	
Corruption*Small State	11.016 (8.037)	11.054 (7.899)	11.36 (7.687)	10.416 (7.417)	11.203 (7.078)
Corruption*GDP per capita	.799 (.683)	.8 (.671)	.748 (.648)	.877 (.662)	.834 (.638)
Corruption*Closeness	-1.623 (5.706)	-1.561 (5.524)			
Corruption	-36.734 (23.995)	-36.762 (23.667)	-36.684 (23.436)	-35.572 (23.512)	-31.87 (21.05)
Rsquared	.57	.57	.57	.56	.56
N	62	62	62	62	62

*p<.10, **p<.05, ***p<.001 (two tailed)

Conclusion

Our results suggest some clear answers to our initial research questions. With respect to the first question, it appears that inequality does not have much impact (substantive or statistical) on turnout among modern democracies. Previous evidence from samples of industrial countries suggesting otherwise appears to be misleading. The second question, whether the idea that turnout functions differ among democracies of developed and developing countries seems to have little basis in empirical fact. The same model describes both in a reasonably similar way. Moreover, we find no significant differences in estimated effect of inequality on turnout across these two groups of countries. The upshot of this finding is that turnout research results have been, in fact, “affected by the cases selected.”

A positive finding of the paper is that corruption does reduce voter turnout. So we can say that reducing corruption is a way to generally *increase* turnout in democracies. (We cannot speak to what corruption does to turnout in non-democracies.) However doesn't the fact that “corruption matters” prove that we need a different model to explain turnout in corrupt countries? It does so only in the trivial sense that we need to add this variable to our basic model. It does not in the usual sense of phrase “new model” because the same model seems to work about the same across the clientelist/non-clientelist divide.

Our findings also highly confirm that it seems to be widely true that making elections more decisive and making voting a requirement considerably increases turnout (by 30 points or so if one moves from doing neither to doing both). Policy could also address these two parameters relatively easily, if higher voting is seen as desirable.

Perhaps it goes without saying, but future work, not only on impacts of comparative democratic institutions, needs to stop relying so overwhelmingly and confidently on empirical studies limited to Western democracies to make broadly applicable claims. In addition to the substantive results, we hope that this paper reinforces other work showing how failing to do so can lead down wrong research paths.

Appendix 1: Country Gini year and Election Year Index for democracies

Country	Year a country became a democracy	Available Gini data	randomly selected year
Argentina	1984	1984-2007	1993
Australia	1901	1945-2007 (with gaps)	1977
Austria	1946	1970-2007 (with gaps)	1983
Bahamas	1973	1973-2007 (with gaps)	1997
Barbados	1966	1966-1981 (with gaps)	1976
Belgium	1944	1973-2007 (with gaps)	1991
Benin	1991	2003	2003
Botswana	1973	1973-1994 (with gaps)	1994
Brazil	1985	1985-2007	2002
Bulgaria	1990	1990-2007	1997
Canada	1867	1951-2007 (with gaps)	1968
Chile	1989	1989-2007 (with gaps)	1993
Costa Rica	1874	1961-2007 (with gaps)	1982
Croatia	2000	2001-2007 (with gaps)	2003
Cyprus	1968	2003-2007 (with gaps)	2006
Czech Republic	1990	1990-2007	1998
Denmark	1915	1949-2007 (with gaps)	1968
Dominican Rep.	1996	1996-2007 (with gaps)	2002
El Salvador	1997	1998-2007	2000
Estonia	1990	1990-2007	2007
Finland	1917	1966-2007 (with gaps)	1966
France	1945	1956-2007 (with gaps)	1986
Germany	1949	1950-2007 (with gaps)	1983
Ghana	2001	2001-2007 (with gaps)	2004
Greece	1975	1975-2007 (with gaps)	1985
Hungary	1990	1990-2007	1998
Iceland	1944	2004-2007	2004
India	1952	1952-2007	1962
Ireland	1921	1973-2007 (with gaps)	1981
Israel	1948	1953-2007 (with gaps)	1969
Italy	1948	1948-2007 (with gaps)	1994

Jamaica	1959	1973-2007	2007
Japan	1952	1952-2007 (with gaps)	1967
Korea (South)	1988	1988-2007	2000
Latvia	1990	1991-2007	1998
Lesotho	2002	1999, 2007	2002
Lithuania	1990	1992-2007	2004
Luxembourg	1945	1985-2007 (with gaps)	1989
Mali	2000	2001-2007 (with gaps)	2007
Malta	1974	2000-2007 (with gaps)	2003
Mauritius	1968	1975-2001 (with gaps)	1997
Mexico	1992	1992-2007 (with gaps)	1996
Mongolia	1992	1995-2002	2000
Namibia	1990	1993	1994
Netherlands	1945	1946-2007 (with gaps)	1977
New Zealand	1874	1954-2007 (with gaps)	1960
Norway	1898	1963-2007 (with gaps)	1965
Panama	1989	1989-2007	1999
Peru	2001	2001-2007	2006
Poland	1990	1990-2007	1997
Portugal	1976	1980-2007 (with gaps)	1985
Romania	1996	1996-2007	2000
Senegal	2000	2001	2001
Slovak Rep.	1993	1993-2007	1998
Slovenia	1991	1991-2007	2004
South Africa	1994	1994-2007	2004
Spain	1978	1980-2007 (with gaps)	1982
Sweden	1917	1945-2007 (with gaps)	1991
Switzerland	1848	1987-2002	1995
United Kingdom	1837	1945-2007	1966
United States	1800	1945-2007	1978
Uruguay	1985	1985-2007 (with gaps)	1989

Sources:

Gini data: UNDP Statistics Website (2007) and UNU-Wider 2008

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