Supplementary Appendix for: “The Super Rich and the Rest: Campaign Finance Pressures and the Wealth of Politicians”

Contents

A Campaign Financial Resources and Pareto Distributions

Figure A1 shows the distribution of gross household assets in the United States based on the 2019 Survey of Consumer Finances. Panel (a) shows the relative frequency of wealth and accords with Pareto's Law with a heavy right skew and a long right tail. Panel (b) shows the cumulative wealth by wealth percentile, resembling the shape of the red solid line in our conceptual framework in Figure 1. These observations are consistent with studies documenting the concordance of wealth distributions with Pareto's Law, both in the US and other countries (see Benhabib and Bisin, 2018).

Figure A1: US Wealth Distribution

(a) Relative Frequency of Wealth in the US **(b)** Asset Ownership by Wealth Percentiles

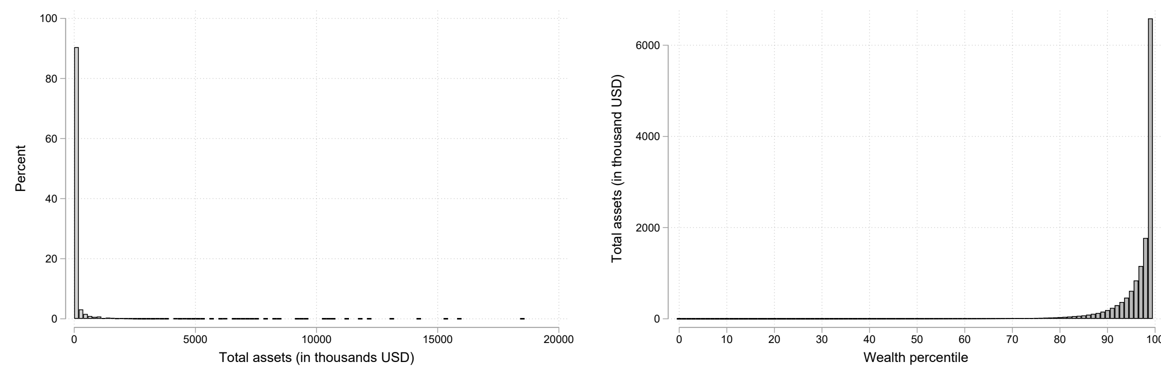
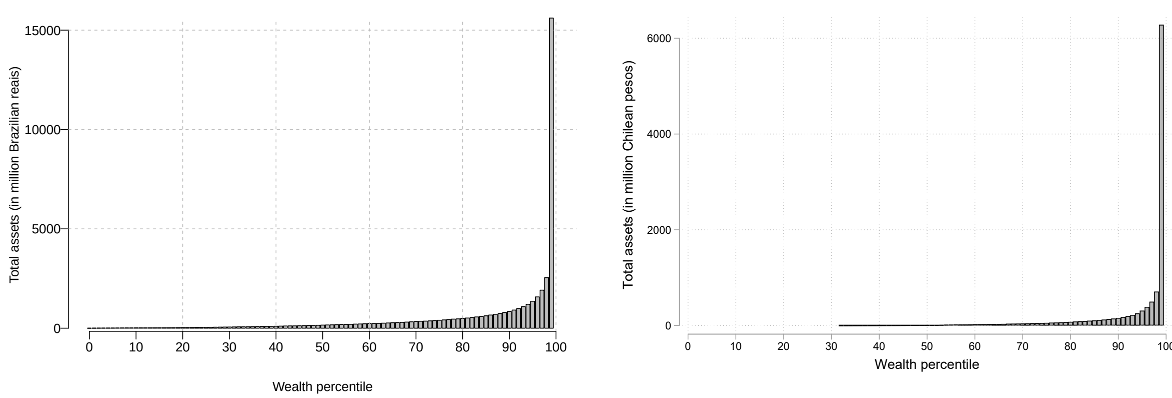


Figure A2 shows the distributions of cumulative wealth by wealth percentile among candidates in our two case studies: for local councils in Brazil (left panel) and for the Chamber of Deputies in Chile (right panel). As in the US, the distributions are highly right-skewed, resemble the upward-sloping line in our conceptual framework, and accord with Pareto's Law.

Figure A2: Candidate Wealth Distributions in Brazil and Chile

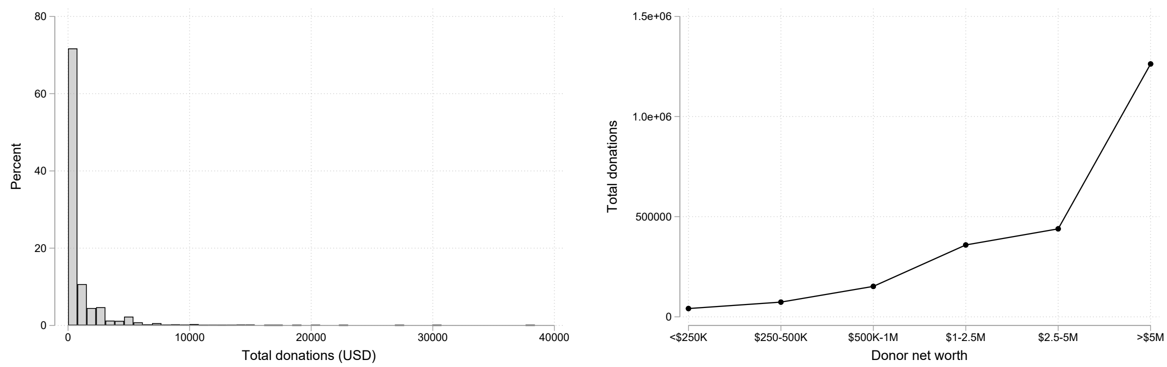
(a) Candidates for Local Councils in Brazil **(b)** Candidates for Chamber of Deputies in Chile



In the text, we argue that private donations are also heavily right-skewed. Based on the data from the survey of large individual donors to Senate candidates in 2012 in Barber, Canes-Wrone and Thrower (2017), panel (a) of Figure A3 shows a similarly thick-tailed distribution as that for wealth. Panel (b) of Figure A3 shows the total donations by the donors' self-reported net worth. Again, even in this sample of large donors the distribution of average donations is quite top-heavy.

Figure A3: Distribution of Donations in Donor Survey from Barber, Canes-Wrone and Thrower (2017)

(a) Relative Frequency of Donation Size **(b)** Total Donation Amount by Donor Wealth



B Formal Derivation of Hypotheses

In line with the discussion in the paper, suppose that the campaign resources available to candidates follow a C^2 Pareto Type I distribution with the shape parameter $\alpha > 0$ and the minimum possible value $x_{min} > 0$. The cumulative distribution function (CDF) is given by (see, for example Benhabib and Bisin 2018):

$$F(x; \alpha, x_{min}) = 1 - \left(\frac{x_{min}}{x}\right)^{\alpha}$$

To express the financial resources by percentile p, as in Figure 1 in the paper, use the inverse of the CDF:

$$Q(p; \alpha, x_{min}) = \frac{x_{min}}{(1-p)^{\frac{1}{\alpha}}}$$

H1. Consider the rate of change of area under Q (the financial resource curve) with respect to percentile p above a fixed line $y = c$ (the financing pressures line). The area is given by:

$$A(p; \alpha, x_{min}, c) = \int_{p_1}^{p_2} \left(Q(p; \alpha, x_{min}) - c\right) dp,$$

over the range $[p_1, p_2]$ such that $Q(p; \alpha, x_{min}) > c$. Differentiating w.r.t. c and applying the Leibniz rule:

$$\frac{dA}{dc} = -(p_2 - p_1)$$

Since it is natural to assume that campaigns are never costless, financing pressures are always positive ($c > 0$), and so $p_1 = 0$ and $\frac{dA}{dc} = -p < 0$, indicating that A is decreasing in c.

The second derivative of Q w.r.t. p is:

$$\begin{aligned}\frac{d^2}{dp^2} Q(p; \alpha, x_{min}) &= \frac{d}{dp}\left[\frac{x_{\text{min}} \cdot (1-p)^{-\frac{1}{\alpha}-1}}{\alpha}\right] \\ &= -\frac{\left(-\frac{1}{\alpha} - 1\right) x_{\text{min}} \cdot (1-p)^{-\frac{1}{\alpha}-2}}{\alpha}.\end{aligned}$$

For $0 < p < 1$ and $\alpha > 0$, the second derivative is strictly positive, indicating that Q is convex. Given the convexity of Q and a negative rate of change in A, it follows that the rate of decrease of A is increasing in c. $\square$

H2. As shown above:

$$\frac{d^2}{dp^2}Q(p;\alpha,x_{min}) = -\frac{\left(-\frac{1}{\alpha}-1\right)x_{\min}\cdot(1-p)^{-\frac{1}{\alpha}-2}}{\alpha} > 0$$

Consider how it changes with α:

$$\begin{aligned}\frac{d}{d\alpha}\left[\frac{d^2}{dp^2}Q(p;\alpha,x_{min})\right] &= \frac{d}{d\alpha}\left[-\frac{\left(-\frac{1}{\alpha}-1\right)x_{\min}\cdot(1-p)^{-\frac{1}{\alpha}-2}}{\alpha}\right] \\ &= -\frac{x_{\min}\cdot(\alpha\cdot(\alpha-\ln(1-p)+2)-\ln(1-p))}{(1-p)^{\frac{1}{\alpha}}(p-1)^2\alpha^4}.\end{aligned}$$

This expression increases as α decreases. By convexity of Q, a decrease in α implies that Q becomes more convex. Combined with the proof for H1, this means that A is more rapidly decreasing in c as α decreases. $\square$

H3. The derivations follow directly from the derivation of H1.

Another implication of our simple framework relates to the rate at which campaign financing pressures increase. A steeper increase (e.g. because pressures are much greater in one district or country than in another, or at one point in time within a geographic unit compared to another) along an upward-sloping resource curve will generate more disproportionate benefits for very well-resourced candidates than a more gradual increase. While we cannot directly evaluate this implication with our data, cross-sectional results presented in Figure 6 in the paper are consistent with this expectation.

C Complementary Effects of Different Aspects of Campaign Finance Regimes

In the text, we argue that our simple conceptual framework remains instructive even when we generalize beyond self-financing to include other sources of financing (private contributions and public funding). Still, there are other factors that may also affect the shape of the distribution of campaign resources or the level of financing pressures – or both. But we believe that they do not significantly alter our simple framework. For example, longer campaigns and larger constituencies plausibly increase financing pressures in a similar was as a spending limit does in our conceptual framework in Figure 1: they move the dashed line up or down.

Other aspects, however, may *simultaneously* affect the shape of the distribution of campaign resources *and* the level of financing pressure. Nonetheless, we believe these effects are very likely to be *complementary* and amplify the insights our conceptual framework provides, rather than undermining them. Namely, a feature that exacerbates resource inequities also likely increases financing pressures. Consider, for example, a ban on corporate contributions. By eliminating this likely source of fundraising advantage for the very wealthy, it should make the resource curve less upward-sloping. At the same time, it likely reduces financing pressures because everyone (and not just the very wealthy) would have fewer resources at their disposal. In the context of our framework in Figure 1, both of these effects would reduce the disproportionate advantage to the very wealthy. Similarly, greater reporting transparency likely increases the costs of obtaining illicit funds. This disincentive should both constrain everyone (thus lowering the financing pressures) and reduce the edge enjoyed by those who can more easily obtain such funds (limiting the resource inequities).

D Details for Cross-Country Data on Legislators' Wealth

Despite the near ubiquity of disclosure systems around the world (Rossi, Pop and Berger, 2016), our sample is more limited, comprising 41 countries, for several reasons. First, almost two-thirds of countries that require disclosures do not make them publicly available. In some of the remaining countries, particularly in the OECD, office holders only disclose publicly their financial interests and income thereof but not their assets. In other countries, such as Scandinavian countries, information on assets (as well as income) can be obtained directly from the tax authorities, however this service is only available to citizens or tax residents of those countries but not to foreign researchers. In yet other countries such as Mexico and Colombia before 2019, asset disclosures were mandatory but their public disclosure was voluntary. Perhaps not surprisingly, in those cases only a minority of existing disclosures are available. In several other countries, disclosures were only recently made public and we have not had a chance to collect them.

Among countries where data is publicly available, what and how politicians disclose varies in several ways. First, in ten of the 41 countries politicians report only their own wealth rather than household wealth. In such cases, the average household wealth in the population, which we use to benchmark the politicians' wealth for cross-country comparability, is not an ideal basis for comparison. However, we are not aware of any cross-national data that provide estimates of within-household breakdowns of wealth in the population. To account for this variation, we control in all our cross-national analyses for whether the unit of recording of politicians' wealth is the individual or the household.

The second challenge is that the components of disclosed wealth differ across countries. In many countries, disclosures cover immovable assets like real estate and land, movable assets such as vehicles and agricultural equipment, and financial assets such as deposits, stocks, bonds and other investments. In some countries, however, only a subset of assets is reported, such as financial assets. We are generally able to broadly account for these differences because Credit Suisse, our main source for the benchmarking population wealth data, provides separate estimates for financial and non-financial average household wealth, and we choose the population benchmark closest to what the politicians actually report. This is also the reason why we use average rather than median household wealth as the population benchmark: Credit Suisse does not report financial and non-financial median household wealth separately, only overall median wealth.

We note that we use the Credit Suisse data for average household wealth in the population as a benchmark for all countries in the sample except in the U.S, whose reporting system creates unique challenges. The U.S. is the only country in which office-holders only report financial assets or immovable assets that produce rental income, but not the value of residential properties and movable assets. Moreover, unlike any other country, assets and liabilities are overwhelmingly reported not in exact monetary values but in pre-listed value bands (for example, $50,001-100,000). This allows for calculating only an approximate value of an office-holder's (financial) wealth, not its exact value.[1] To accommodate such specificities, rather than using the Credit Suisse data we follow the approach

[1]For each reported asset and liability, we assign the midpoint of the value range as the asset value and subsequently sum such imputed assets and liabilities to get the value of total gross and net wealth.

by Eggers and Klašnja (2020) of using the tri-annual Surveys of Consumer Finances (SCF) to calculate as close as possible a population analog to the disclosed politician wealth.

The third challenge in assembling the cross-national data is that in some countries wealth is reported not in monetary terms but in kind. We collected additional information to estimate an approximate present value of each reported asset, so as to have a monetary value for wealth in each country. Such auxiliary data includes prices of real estate, land, vehicles, and other movable and financial assets. We relied on a variety of national and international public and private sources, such as Eurostat, the Bank of International Settlements, the OECD, national statistics offices, and local private asset sellers. To account for the variation in the type of reporting, we control where appropriate for whether wealth totals are directly reported or imputed in this way.

After collecting the data, we engaged in extensive data checks, particularly focusing on two aspects: observations with sizable wealth, and cases of large fluctuations in wealth for the same person over time (where we have over-time data). We exclude the observation from analysis where we could not find a plausible justification for unusual patterns. 206 observations are excluded this way (representing 0.3 percent of all observations).

We also note that in several countries, we obtained the data from a third party (academic, a civil society or a media organization) or directly from the government. While we subsequently further cleaned and/or imputed the values, we were not always able to inspect the original disclosures when they were not provided or accessible publicly. To account for this variation, we control for the mode by which we obtained the data.

Finally, an important question is whether the information in the disclosures is truthfully reported, as some politicians may hide their assets. While we cannot capture what may be unreported, we try to account for this measurement error by controlling for the scope that politicians have to misreport their assets, based on the information on the statutory and practical capacity of a country's disclosure system to verify the declarations (usually against other administrative data) and levy sanctions for false disclosures. We also show in Section E two sensitivity tests which indicate that our results are unlikely to be driven by this type of measurement error.

Table A1 summarizes the years covered, the number of observations, and the number of unique politicians in each country in our sample.

E Cross-Country Analysis

E.1 Background Details

Table A2 describes the components of the campaign financing pressure (CFP) index.

Figure A4 shows the average values of the CFP index across countries for which we have the legislators' wealth data.

The CFP index may fail to capture differences between the *de jure* rules and their *de facto* implementation. We control for measures of capacity and autonomy of a country's Electoral Management Body (EMB) from V-Dem (Coppedge, Michael et al., 2021); see Table A3. EMBs are usually in charge of implementing the campaign finance regulations. We further show sensitivity tests below which indicate that our results are unlikely to be driven by this type of measurement error.

Table A3 lists the covariates used in the cross-country analyses. Where applicable, the variable values and sources are also listed.

Table A4 shows the coefficient estimates underlying the results of cross-national analyses without country fixed effects in Figures 3-5. All coefficients are from ordered logit models with wealth sextiles as the dependent variable. Tables A5 and A6 show the coefficient estimates underlying the

Table A1: Composition of the Cross-National Sample

Country	Period	Unique Politicians	Obs.	Country	Period	Unique Politicians	Obs.
Argentina	2002-2018	582	1,500	Latvia	2001-2018	375	3,128
Armenia	2014-2019	131	303	Lithuania	1995-2012	424	977
Bangladesh	2008-2018	527	900	Moldova	2008-2017	101	683
Bolivia	2018-2018	149	149	Montenegro	2016-2020	91	585
Bosnia	2014-2019	765	933	North Macedonia	2018-2018	130	130
Brazil	2010-2018	1,146	1,785	Pakistan	2012-2015	496	1,734
Bulgaria	2008-2017	740	2,443	Paraguay	2013-2020	276	791
Chile	2017-2019	195	478	Peru	2008-2017	324	1,124
Colombia	2020-2020	203	204	Philippines	2012-2018	522	2,024
Croatia	2011-2020	205	848	Poland	2011-2017	560	2,795
Cyprus	2016-2017	71	76	Romania	2008-2019	1,003	2,615
Czech Republic	2002-2017	167	367	Russia	2009-2020	1,324	6,739
Dominican Republic	2016-2016	126	128	Serbia	2010-2019	382	1,565
Ecuador	2015-2018	188	372	South Africa	2010-2017	715	3,065
Georgia	2010-2016	229	919	South Korea	2011-2017	630	2,046
Greece	2016-2019	1,239	3,356	Spain	2008-2016	783	1,331
Guatemala	2019-2019	50	50	Taiwan	2008-2016	127	413
Hungary	1997-2016	1,045	8,882	Thailand	2007-2014	960	2,451
India	2004-2019	1,942	2,575	Ukraine	2011-2015	411	1,368
Indonesia	2001-2016	1,095	1,724	United States	1980-2018	1,814	9,101
Italy	2008-2017	808	3,597	**Total**		23,051	76,254

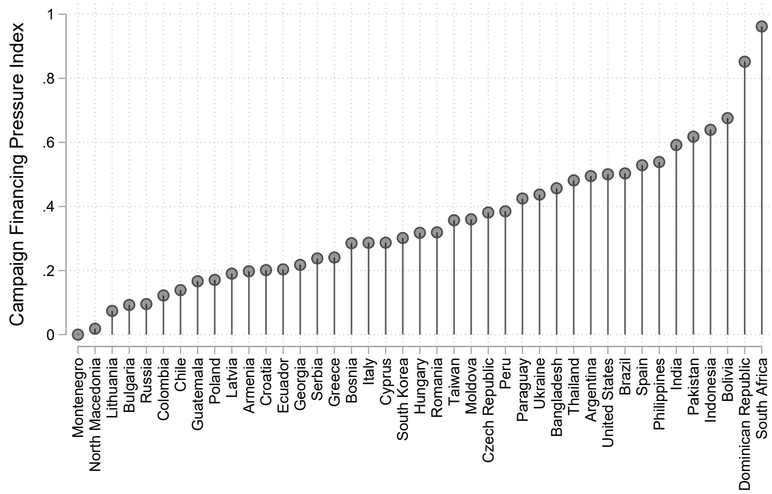
Table A2: Components of the CFP Index

Variable	Values
Ban on corporate contributions to parties/candidates?	No (1), yes (0)
Ban on union contributions to parties/candidates?	No (0), yes (1)
Ban on anonymous contributions to parties/candidates?	No (1), no, but specific limit (.5), yes (0)
Limit on non-election-specific contributions by legal and/or natural persons to paries?	No (1), for natural persons (.67), for legal persons (.33), for both (0)
Limit on election-specific contributions by legal and/or natural persons to parties?	No (1), for natural persons (.67), for legal persons (.33), for both (0)
Limit on contributions by legal and/or natural persons to candidates?	No (1), for natural persons (.67), for legal persons (.33), for both (0)
Limit on in kind contributions to parties/candidates	No (1), no, but specific limit (.5), yes (0)
Limit on self-financing?	No (1), yes (0)
Direct public funding?	No (1), for campaigns by seats/votes (.83), for campaigns equally (.67), regular by seats/votes (.5), regular equally (.33), for campaigns and regular by seats/votes (.17), for campaigns and regular equally (0)
Free or subsidized access to media for parties/candidates?	No (1), yes (0)
Spending limit on parties/candidates?	No (1), only when accepting public funds (.5), yes (0)
Spending limit on advertising?	No (1), for political parties (.8), for candidates (.6), for third parties (.4), for parties/candidates (.2), for parties/candidates/third parties (0)
Parties report finances regularly?	No (1), yes (0)
Parties/candidates report finances in campaigns?	No (1), yes (0)
Do lobbies report contributions to parties/candidates?	No (1), yes (0)
Are campaign receipts/expenses made public?	No (1), sometimes (.5), yes (0)
Is donor identity made public?	No (1), sometimes (.5), yes (0)
Length of official campaign calendar	Lowest quartile (0) to highest quartile (1)
Number of registered voters per legislative seat	Lowest quartile (0) to highest quartile (1)

Sources: IDEA (2018); Austin and Tjernström (2003); Fulguera, Jones and Ohman (2014); national regulations; national election laws; Inter-Parliamentary Union (2023).

results of cross-national analyses with country fixed effects presented in Figure 3 and 4, respectively. All coefficients in Table A5 are from OLS models run separately for each sextile as the dependent

Figure A4: Average CFP Index Values



variable. Coefficients for country dummies are omitted for space constraints. All coefficients in Table A6 are log-odds from logistic models run separately for each sextile as the dependent variable. We use logit models because OLS models in these specifications sometimes produce nonsensical predicted values, even for empirically-supported values of predictors.[2] The results with OLS models are substantively very similar and so our conclusions do not depend on modeling choices.

We also note that the coefficients on the CFP index main term in Table A6 do not have a natural interpretation, as they indicate the correlation between the wealth sextile and the CFP index when the share of wealth belonging to the top 1% minus the share of wealth of the bottom 50% is equal to zero. Empirically, no country-year observation in our sample comes close to this value, as the measure ranges from 11 to 65 percent. Therefore, the main effects represent purely hypothetical extrapolations that are mathematically inevitable in interaction term models, which is the motivation behind showing in the paper the predicted probabilities in graphical form using empirically plausible values of inequality.

Additional Information: Campaign Spending Limits Across Campaign Finance Regimes

For H3, we use the measure of campaign spending pressures. Because those pressures vary the most across countries, they allow us to better evaluate (descriptively) the variation from low to middle and middle to high pressures. Figure A5 shows the ratio of the statutory spending limit for the lower legislative chamber to a country's per capital income (both in current USD) across values of the CFP index for the 42 countries for which we could find information based on data from IDEA (2018) and national sources.[3] To make the figure more easily interpretable, we bin the CFP index into six equally-sized groups. Spending limits are on average similar for low CFP index values but rise steeply at higher values.

[2] For example, the predicted probability of belonging to the lowest sextile under high CFP and low inequality is below zero.

[3] Of the 42 countries, 19 are also in our cross-national sample. There is no limit in the US, for which we use the largest campaign amount spent for the 2018 House elections (slightly above $30 million, by Jon Ossoff in Georgia's 3rd district; see https://www.businessinsider.com/2018-congressional-candidates-raised-spent-most-money-data-2018-11#the-house-candidates-who-have-spent-the-most-money-2).

Table A3: Covariates Used in Cross-Country Analyses

Variable	Categories	Source	Notes
Disclosure system: Household		Djankov et al. (2010) and originally collected	Household or individual wealth disclosure
Disclosure system: Routine verification		Djankov et al. (2010) and originally collected	Routine (vs. ad-hoc) disclosure verification
Disclosure system: False disclosure penalty		Djankov et al. (2010) and originally collected	Existence of penalties for false disclosure
Disclosure system: Comprehensiveness		Djankov et al. (2010) and originally collected	Average coverage of 16 disclosure categories
Monetary values imputed (vs. reported)		Originally collected	Wealth imputed from assets listed in kind vs. reported in monetary terms
Data collection method	Pre-processed (baseline category), parsed, hand-coded, third-party obtained, combination	Originally collected	Dummies
Electoral management body capacity		V-Dem	The ordinal scale version of `v2elembcap`
Electoral management body autonomy		V-Dem	The ordinal scale version of `v2elembaut`
Politician gender	Female, male (baseline category)	GLP, WhoGov, and originally collected	Dummies
Politician age		GLP, WhoGov, and originally collected	In years
Politician profession	Top management, high-paying profession, higher education, entrepreneur, other (baseline category)	GLP, WhoGov, and originally collected	Dummies
Politician institution	Lower house (baseline category), upper house	Originally collected	Dummies
Party family	Left, right, center, independent (baseline category)	V-Party, DALP, and originally collected	Dummies
GDP per capita		WDI	Logged
Gini coefficient		WDI	
Electoral rules	Plurality, PR, mixed, appointed (baseline category)	DES	Dummies
Wealth inequality	Share of wealth to top 1 minus share to bottom 50 percent	WID	
Top 10 percent wealth share		WID	
Politician education	Less than college (baseline category), college degree, post-graduate	GLP, WhoGov, and originally collected	Dummies
Politician marital status	Not married (0) married or widowed (1)	GLP, WhoGov, and originally collected	Dummies
Region	North America (baseline category), South and Central America, Europe, Eurasia, South Asia, East Asia		Dummies
Country			Dummies

Sources: DPI: Cruz, Keefer and Scartascini (2021); WDI: World Bank (2023); WID: Chancel et al. (2021); GLP: Gerring et al. (2019); WhoGov: Nyrup and Bramwell (2020); QoG: Teorell et al. (2021); IDEA: IDEA (2018); Austin and Tjernström (2003); Fulguera, Jones and Ohman (2014); V-Dem: Coppedge, Michael et al. (2021); DES: Bormann and Golder (2013); V-Party: Lührmann et al. (2021); DALP: Kitschelt (2013).

E.2 Additional Results

Robustness Check: Results with Fixed Effects with Within-Country Sextiles

In our analyses, the wealth sextile variable is calculated across all country-year-legislator observations. Figure A6 shows the distribution of observations across sextiles in each country. For cross-sectional analyses across countries, we believe that this definition of sextiles is intuitive, since we are making comparisons across legislators in all countries. By norming the wealth ratios by country-year-specific benchmarks (of the wealth of an average household), the wealth of politicians is made comparable, thus allowing us to group politicians into sextiles across all countries consistently.

However, for cross-national analyses with fixed effects it is at least as intuitive to calculate sextiles *within* each country. Panels (a) and (b) of Figure A7 show the results with this variable for the tests of H1 and H2. The results are substantively similar.

Table A4: Coefficient Estimates for Cross-National Results without Country Fixed Effects

	Figure 4		Figure 5		Figure 6	
CFP index	1.94***	(0.43)	-0.10	(1.05)		
CFP index × Top 1% minus bottom 50% wealth share			0.06**	(0.03)		
CSP index					2.16***	(0.33)
Top 1% minus bottom 50% wealth share	0.08***	(0.03)	0.06*	(0.03)	0.04	(0.03)
Household (vs. personal) wealth disclosure	-0.74*	(0.38)	-0.86**	(0.39)	-0.06	(0.34)
Monetary values imputed (vs. reported)	0.27	(0.23)	0.27	(0.22)	-0.12	(0.22)
Data collection: Parsed	-0.51**	(0.25)	-0.69***	(0.25)	-1.24***	(0.28)
Data collection: Hand-coded	-1.10***	(0.36)	-1.14***	(0.34)	-1.42***	(0.37)
Data collection: Third-party	-0.64*	(0.38)	-0.61*	(0.37)	-1.34***	(0.40)
Data collection: Combination	0.89*	(0.53)	0.80	(0.52)	1.32***	(0.42)
Region: Latin America	0.75	(0.61)	0.85	(0.59)	2.83***	(0.62)
Region: Europe	0.44	(0.68)	0.28	(0.68)	3.39***	(0.82)
Region: Eurasia	1.85**	(0.82)	1.87**	(0.79)	4.93***	(0.92)
Region: South Asia/Africa	2.60***	(0.76)	2.47***	(0.76)	5.90***	(0.91)
Region: East Asia	1.74**	(0.68)	1.76***	(0.66)	4.16***	(0.72)
Disclosure comprehensiveness	-0.61	(0.42)	-0.61	(0.42)	-1.13***	(0.43)
Routine disclosure checks	0.04	(0.14)	0.04	(0.14)	-0.12	(0.13)
Penalties for false disclosure	-0.13	(0.16)	-0.03	(0.17)	-0.35**	(0.16)
Electoral management body capacity	0.08	(0.15)	0.09	(0.14)	0.24*	(0.14)
Electoral management body independence	-0.21	(0.14)	-0.21	(0.14)	-0.19	(0.15)
Female	-0.27***	(0.04)	-0.26***	(0.03)	-0.26***	(0.04)
Age	0.03***	(0.00)	0.03***	(0.00)	0.03***	(0.00)
Top management	0.60***	(0.06)	0.60***	(0.05)	0.59***	(0.05)
High-paying profession	0.24***	(0.04)	0.25***	(0.04)	0.26***	(0.04)
Academia	-0.01	(0.04)	-0.00	(0.04)	-0.03	(0.04)
Entrepreneur	0.34***	(0.04)	0.34***	(0.04)	0.35***	(0.04)
Upper house	0.23**	(0.09)	0.23**	(0.09)	0.21**	(0.08)
Left party	-0.22*	(0.12)	-0.17	(0.13)	-0.19*	(0.10)
Right party	0.04	(0.13)	0.08	(0.13)	0.02	(0.10)
Centrist party	0.06	(0.11)	0.12	(0.11)	0.13	(0.10)
GDP per capita	-0.62***	(0.16)	-0.65***	(0.16)	-0.21	(0.18)
Gini coefficient	0.00	(0.02)	-0.02	(0.02)	0.01	(0.01)
Plurality electoral system (lower chamber)	0.00	(0.26)	0.05	(0.27)	0.16	(0.27)
PR electoral system (lower chamber)	0.29	(0.28)	0.29	(0.27)	0.08	(0.26)
Mixed electoral system (lower chamber)	0.82***	(0.15)	0.73***	(0.13)	0.86***	(0.14)
Top 10% wealth share	-0.08*	(0.04)	-0.07*	(0.04)	-0.01	(0.04)
Observations	69572		69572		69572	

Note: *$p<0.1$; **$p<0.05$; ***$p<0.01$. Standard errors in parentheses are clustered by country-year.

Table A5: Coefficient Estimates for Cross-National Results with Country Fixed Effects in Figure 3

	Bottom sextile	2nd sextile	3rd sextile	4th sextile	5th sextile	Top sextile
CFP index	-0.09	0.03	-0.07	-0.06	0.07	0.12***
	(0.06)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)
Female	0.03***	0.03***	0.00	-0.02***	-0.03***	-0.01**
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)
Age	-0.00***	-0.00***	0.00***	0.00***	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Top management	-0.05***	-0.02***	-0.03***	-0.01	0.01	0.09***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
High-paying profession	-0.05***	-0.01	0.01*	0.03***	0.02***	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Academia	-0.03***	-0.00	0.02**	0.04***	0.01	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Entrepreneur	-0.04***	-0.03***	-0.00	0.02***	0.03***	0.02***
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Upper house	-0.01	-0.04***	-0.01	0.01	0.01	0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Left party	0.04***	0.03*	-0.02	-0.02**	0.02	-0.05***
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Right party	0.02	-0.00	-0.03*	0.00	0.06***	-0.05***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Centrist party	0.03**	0.02	-0.04***	-0.02**	0.03*	-0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
GDP per capita	-0.13	0.08***	0.04	-0.05	0.01	0.06**
	(0.10)	(0.02)	(0.05)	(0.04)	(0.03)	(0.03)
Gini coefficient	0.01***	-0.01***	-0.01***	-0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Plurality electoral system (lower chamber)	-0.07*	-0.05***	-0.08***	0.02	0.10***	0.08***
	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
PR electoral system (lower chamber)	-0.04	-0.03	-0.07***	-0.03	0.08***	0.09***
	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Mixed electoral system (lower chamber)	-0.06***	-0.06***	-0.04***	-0.01	0.08***	0.10***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Top 1% minus bottom 50% wealth share	0.03***	0.01***	-0.01***	-0.00	-0.01***	-0.01***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Top 10% wealth share	-0.03***	-0.01***	0.02***	0.01***	0.01***	0.01
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Electoral management body capacity	-0.03	0.00	0.01	-0.01	0.03**	0.00
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Electoral management body independence	-0.02	-0.01	0.03*	0.02*	-0.01	-0.01
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Constant	2.59**	0.33	-0.33	0.58	-0.92**	-1.24***
	(1.12)	(0.27)	(0.56)	(0.47)	(0.37)	(0.38)
Observations	60378	60378	60378	60378	60378	60378

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors in parentheses are clustered by country-year.

Table A6: Coefficient Estimates for Cross-National Results with Country Fixed Effects in Figure 4

	Bottom sextile	2nd sextile	3rd sextile	4th sextile	5th sextile	Top sextile
CFP index	-3.87^{***}	1.80^{***}	4.90^{***}	5.52^{***}	-1.33	-5.10^{***}
	(1.13)	(0.64)	(0.80)	(0.89)	(0.97)	(1.20)
CFP index × Top 1% minus bottom 50% wealth share	0.10^{**}	-0.06^{***}	-0.18^{***}	-0.17^{***}	0.04	0.19^{***}
	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Top 1% minus bottom 50% wealth share	0.21^{***}	0.05^{***}	-0.04	-0.03	-0.12^{***}	-0.07^{*}
	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Female	0.22^{***}	0.19^{***}	0.01	-0.17^{***}	-0.27^{***}	-0.09
	(0.04)	(0.03)	(0.04)	(0.04)	(0.05)	(0.06)
Age	-0.04^{***}	-0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}	0.03^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Top management	-0.46^{***}	-0.14^{***}	-0.23^{***}	-0.04	0.05	0.93^{***}
	(0.07)	(0.05)	(0.05)	(0.05)	(0.08)	(0.07)
High-paying profession	-0.36^{***}	-0.08	0.09^{*}	0.24^{***}	0.18^{***}	-0.13^{*}
	(0.04)	(0.05)	(0.05)	(0.04)	(0.06)	(0.07)
Academia	-0.23^{***}	-0.03	0.14^{***}	0.27^{***}	0.05	-0.50^{***}
	(0.07)	(0.06)	(0.05)	(0.05)	(0.06)	(0.10)
Entrepreneur	-0.33^{***}	-0.18^{***}	-0.03	0.18^{***}	0.25^{***}	0.28^{***}
	(0.04)	(0.04)	(0.03)	(0.05)	(0.05)	(0.10)
Upper house	0.02	-0.24^{***}	-0.03	0.05	0.21^{**}	0.56^{***}
	(0.08)	(0.06)	(0.07)	(0.10)	(0.10)	(0.20)
Left party	0.36^{***}	0.22	-0.14	-0.12	0.15	-0.69^{***}
	(0.14)	(0.15)	(0.10)	(0.07)	(0.16)	(0.14)
Right party	0.19	0.03	-0.19^{*}	0.06	0.49^{***}	-0.59^{***}
	(0.14)	(0.15)	(0.11)	(0.07)	(0.15)	(0.16)
Centrist party	0.27^{**}	0.13	-0.28^{***}	-0.12^{*}	0.23	-0.23^{*}
	(0.14)	(0.15)	(0.10)	(0.07)	(0.15)	(0.13)
GDP per capita	-1.54^{*}	0.57^{***}	0.70^{**}	0.30	-0.29	-0.36^{*}
	(0.84)	(0.19)	(0.35)	(0.31)	(0.27)	(0.21)
Gini coefficient	0.05	-0.04^{**}	-0.04^{***}	-0.04^{**}	0.03	0.05^{***}
	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Plurality electoral system (lower chamber)	-1.52^{***}	-0.59^{***}	-0.66^{***}	0.25	0.47^{*}	0.68^{*}
	(0.29)	(0.22)	(0.18)	(0.27)	(0.24)	(0.38)
PR electoral system (lower chamber)	-1.13^{***}	-0.56^{***}	-0.56^{***}	-0.17	0.15	0.29
	(0.30)	(0.21)	(0.19)	(0.19)	(0.18)	(0.32)
Mixed electoral system (lower chamber)	-1.42^{***}	-0.70^{***}	-0.35^{***}	-0.01	0.51^{***}	1.09^{***}
	(0.16)	(0.19)	(0.11)	(0.14)	(0.11)	(0.20)
Electoral management body capacity	-0.10	0.01	0.07	-0.08	0.19^{***}	0.06
	(0.12)	(0.06)	(0.11)	(0.07)	(0.06)	(0.14)
Electoral management body independence	-0.11	0.02	0.33^{***}	0.25^{**}	-0.03	0.02
	(0.11)	(0.10)	(0.09)	(0.10)	(0.09)	(0.12)
Top 10% wealth share	-0.26^{***}	-0.05^{**}	0.14^{***}	0.16^{***}	0.12^{***}	-0.01
	(0.07)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
Constant	25.14^{**}	-2.60	-13.66^{***}	-12.08^{***}	-6.17	-0.25
	(10.30)	(2.59)	(4.31)	(4.07)	(3.87)	(3.93)
Observations	51658	51658	51658	51658	51658	51658

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Standard errors in parentheses are clustered by country.

Figure A5: Steep Change in Campaign Spending Limits across Campaign Finance Regimes

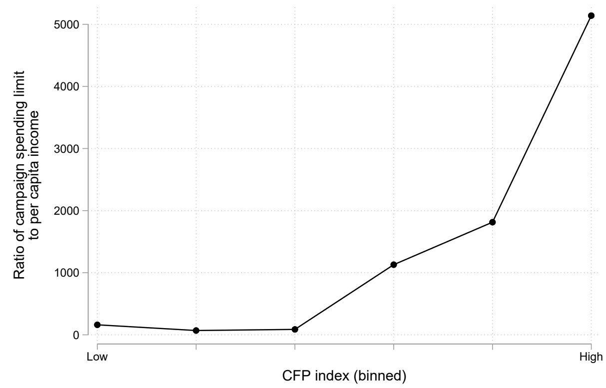
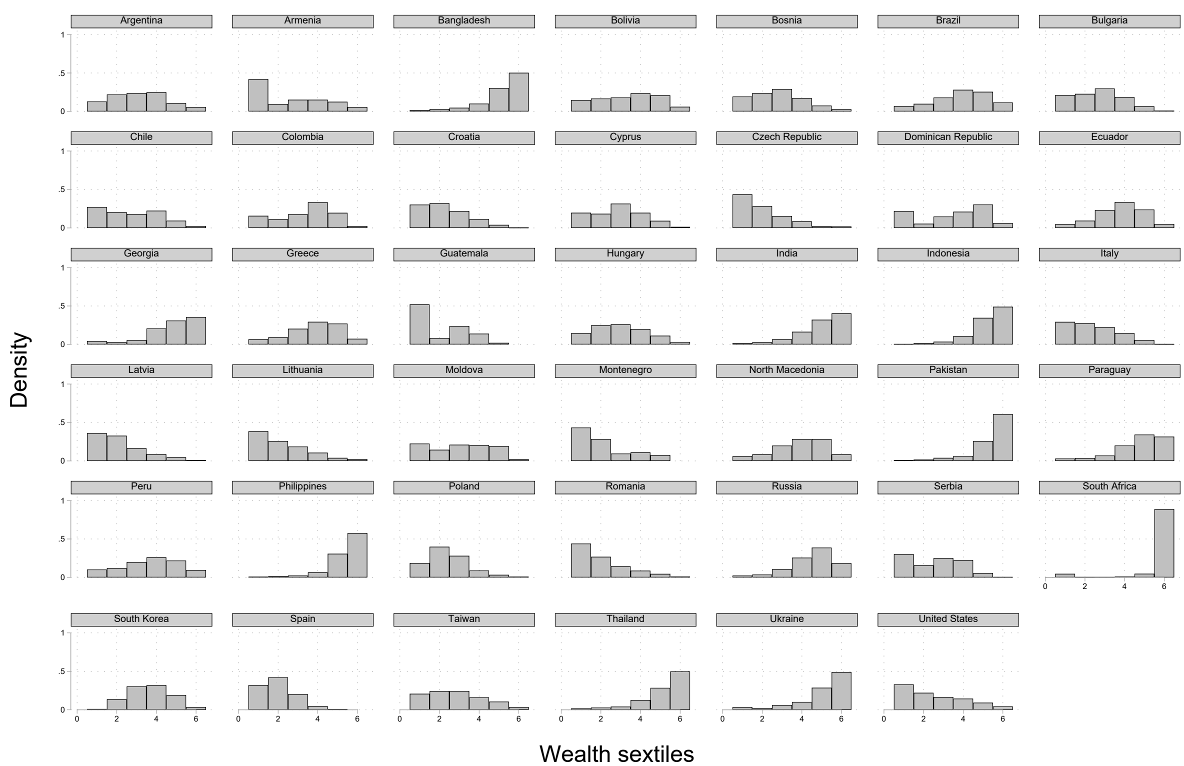


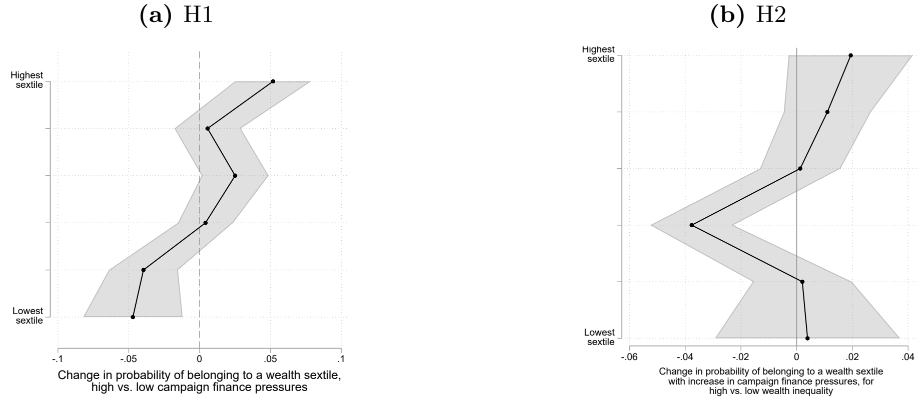
Figure A6: Wealth Sextiles by Country



Robustness Check: Alternative Campaign Financing Pressure Indices

The text presents results using the additive CFP index, which assumes that each campaign finance component receives equal weight and is treated independently. However, campaign finance rules are often bundled and complementary. Moreover, we use rules for both candidates and parties, but depending on the electoral system, regulations for one or the other may be less relevant. We address these issues by constructing two alternative indices: (a) a weighted average index, using the procedure in Anderson (2008), where the weights are derived from the inverse of the covariance

Figure A7: Results for H1 and H2 with Within-Country Sextiles



matrix of all the component variables; (b) a factor analysis score; the first factor captures around 40% of the variance, all but three variables have a positive factor loading, and the Cronbach's alpha is .82. Table A7 shows that the results are qualitatively unchanged. The measures in the table are standardized so that each coefficient represents an estimate of a one standard deviation change. In each column, we control for the same set of covariates included in our main analysis.

Table A7: Alternative Measures of Campaign Financing Pressures and Legislator Wealth

	(1)	(2)	(3)
Additive (default) CFP index	0.40***		
	(0.09)		
Weighted average CFP index		0.25***	
		(0.06)	
CFP factor score			0.36***
			(0.10)
Observations	69597	69597	69597

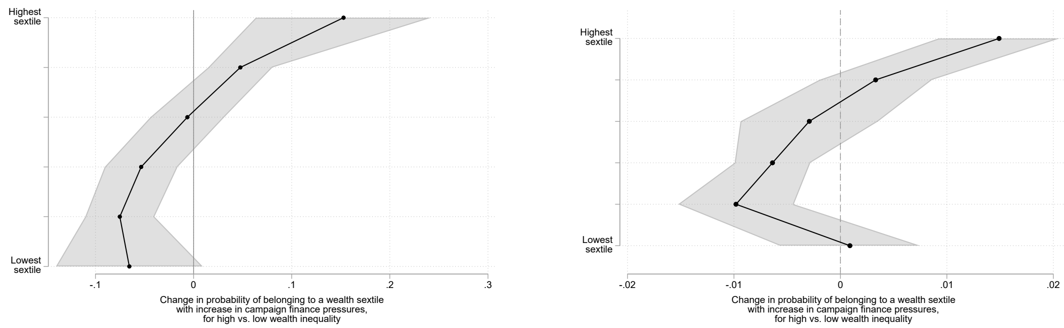
Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered by country.

Robustness Check: Alternative Interaction Effect Specifications

In the text, the results for H2 derive from models with a continuous-by-continuous interaction between the CFP index and our measure of wealth inequality (wealth share for the top 1% minus the wealth share for the bottom 50%). To ensure that the results are not driven by extrapolation (due to lack of overlap in the distribution of the moderator across values of the CFP index) and model dependence (due to an imposition of a linear interaction), we follow the approach by Hainmueller, Mummolo and Xu (2019) to find the optimal bins for the moderator. We use the default option of three bins, which produces the cutoffs at about the 30th and the 70th percentile of the distribution of wealth inequality. We divide our CFP index into three tercile bins, to make sure that there is abundant common support in all the 9 cells of this 3×3 interaction. Panel (a) of Figure A8 summarizes the results with this categorical-by-categorical interaction, showing the difference in the change in the probability of belonging to each wealth sextile at the highest vs. the lowest CFP bin, for the highest relative to the lowest wealth inequality bin.[4] The results using this approach are substantively identical to those in the main paper.

Figure A8: Evaluating H2 with Alternative Interaction Effect Specifications

(a) Binned Approach **(b)** Post-Double Selection Machine Learning



Furthermore, our model may be misspecified by not including the interactions of the wealth inequality measure with the other covariates in the model. To address this concern, following Blackwell and Olson (2022) we use a post-double selection machine learning method. Employing a penalized linear regression (LASSO), this method searches for an optimal set of interactions, balancing between overfitting arising from an inclusion of a large number of additional variables, and any bias from omitting any of the additional interactions. We apply this method separately for each wealth ratio sextile, and instruct LASSO to only perform the selection among the additional interactions terms, while always keeping all the control variable main terms (so that the model is as similar as possible to our main specification). The interaction term indicates the difference in the marginal impact of an increase in CFP on the probability of belonging to a sextile when wealth inequality increases from minimum to maximum. The main takeaway remains the same.

Robustness Check: Pooled and Weighted Data

The unit of analysis in our main model is the legislator-country-year. However, our CFP index only varies by country-year and so our results may be an artifact of an inflated sample size. Moreover, our sample is weighted toward countries with more observations. To address these issues, here we report the results for all three hypotheses at the country-year level, while down-weighting countries with more country-year observations and up-weighting countries with fewer country-year observations.[5] Using country averages does not allow us to examine the distributional consequences of campaign financing pressures across the sextiles of wealth. But we can run analyses with the average politician-to-population wealth ratios as the outcome (the ratio of legislator wealth to the wealth of the average household in a country).[6] In that case, H1 implies that the average wealth ratio should increase with the CFP; H2 implies that this increase should be larger when wealth

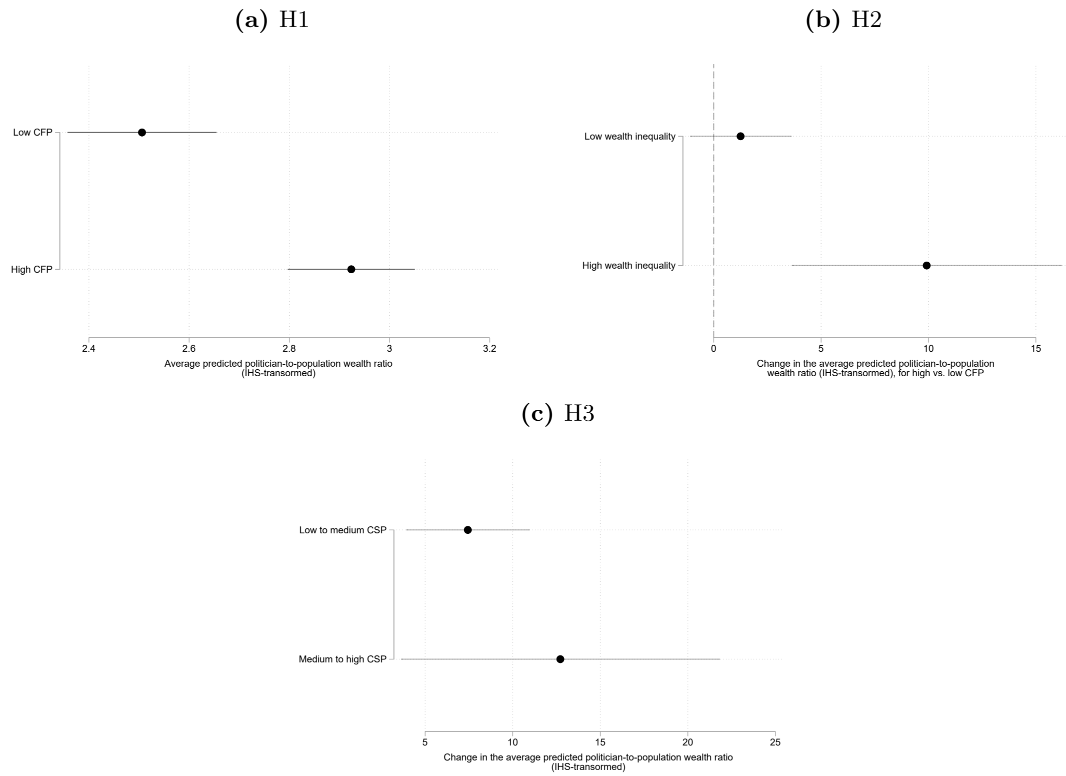
[4] We create the binned variables and run our original ordered logit specification rather than relying on the linear specification provided by Hainmueller, Mummolo and Xu (2019), given that our dependent variable is ordered-categorical.

[5] After rectangularizing our country-year dataset, we calculate the predicted probability of a 'response' (i.e. whether a country has a non-missing observation in a given country-year). The weights are the inverse of this probability (normalized so that they sum up to one).

[6] We note that the wealth ratios are highly right-skewed and also include zeros, and so, to avoid a large impact of outliers and to properly account for zero ratios we use the inverse hyperbolic sine transformation before taking country-year averages.

inequality is greater; H3 implies that the wealth ratio should increase more when the CFP increases from a higher base than from a lower base. The results are shown in Figure A9. The takeaways from these analyses are generally in line with our expectations, and with the results from the main paper; when transformed back (from inverse hyperbolic sine), the difference between the estimates in each panel is significant at conventional levels for H1 and H2, but not for H3.[7]

Figure A9: Evaluating our Hypotheses with Country-Year Re-Weighted Data



(a) H1 **(b)** H2 **(c)** H3

Robustness Check: Weighted Data

Our results in the main paper with the country-year-legislator unit of analysis remain substantively unchanged when we reweight the sample using the balancing weights mentioned in the previous section. Figure A10 shows those results.

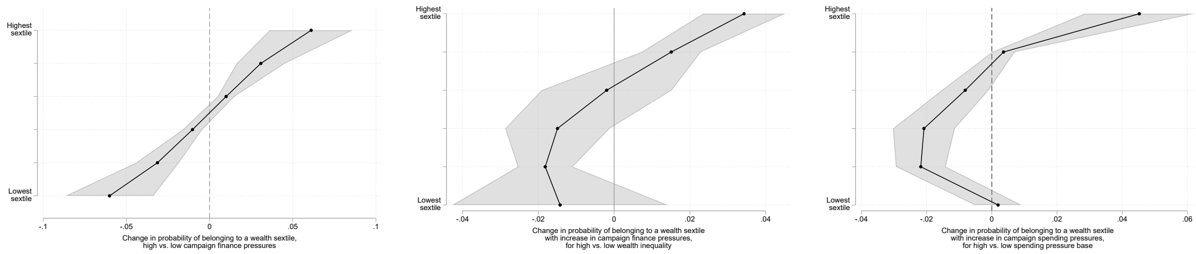
Robustness Check: Sensitivity Analysis

We evaluate the robustness of the results without the country fixed effects by performing sensitivity analyses. We use the recent approach by Cinelli and Hazlett (2020), which assumes that the observed confounding in a linear model can be seen as a substitute for unobserved confounding, and thus as the basis for characterizing the needed strength of omitted variable bias that would overturn the results. We do this sensitivity analysis for H1, and perform it separately for the four main groups of control variables in our main specification without fixed effects: (i) controls for disclosure formats, disclosure rules and campaign finance reporting system characteristics, (ii) individual legislators controls, (iii) party controls, and (iv) country characteristics.

[7]Differences in panels (a)-(c) are, respectively: 9.25 ($p < .01$), 9.03 ($p < .01$), and 5.28 ($p < .36$).

Figure A10: Evaluating our Hypotheses with Re-Weighted Data

(a) H1 **(b)** H2 **(c)** H3



The results of these sensitivity analyses are shown in Figure A11. In each plot, the x-axis shows the residual share of variation of the CFP index that is hypothetically explained by unobserved confounding. The y-axis shows the hypothetical partial R^2 of unobserved confounding with the politician-to-population wealth ratios.[8] The contours inside each plot show the point estimate for the CFP index that would have been obtained in the regression model that includes the unobserved confounders with such hypothetical relationships with both the CFP index and the wealth ratios. The points in the plot show several illustrative values of the strength of confounding. The positive effect of the CFP index on wealth ratios is robust to confounding of up to around 4 times as strong as that from the group of disclosure and campaign finance controls already included in the model (panel a), 8 times for individual-level controls (panel b), 18 times for party-level controls (panel c), and about 2 times for the country-level controls (panel d). While these sensitivity analyses rest on unverifiable assumptions, the results for all the control variable groups suggest that the amount of additional confounding needed to nullify our key result is rather large.

Robustness Check: Generalized Ordered Logistic Model

The classical ordered model imposes an assumption of proportional odds, meaning that the odds ratios for the effect of an explanatory variable are the same across all categories of an ordinal dependent variable. The generalized model relaxes this constraint, allowing for a more flexible relationship between an explanatory and a dependent variable. The plots in Figure A12 show the results from these models for hypotheses H1, H2, and H3, going from left to right, respectively.

The figure shows that the results from a more flexible approach differ somewhat from the classical approach, but only for H1. The relationship is similar for the top sextile (our main focus of interest), and the point estimate is almost twice as large. However, the association between the CFP index and the wealth sextile is more non-linear—and noisier—for lower sextiles under the generalized ordered model. These patterns are still consistent with an expectation that the super rich are the primary beneficiaries of greater campaign financing pressures. The differences between the two approaches for H2 and H3 are comparatively smaller, in terms of both the point estimates and confidence intervals.

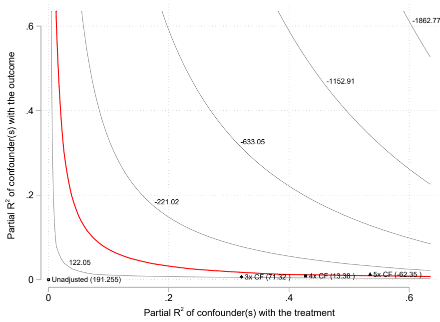
Robustness Check: Spending Limits instead of CFP

Figure A13 shows the results for H1 and H2 in panels (a) and (b), respectively, using the campaign spending limits (on own and party/candidate financing), rather than the composite CFP index, to make them more consistent with the results for H3 and closer to the campaign finance pressure

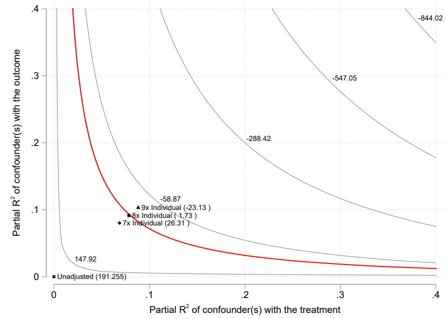
[8] Since this sensitivity test is designed for linear regression, we use the wealth ratios as the outcome variable.

Figure A11: Sensitivity Analyses based on Cinelli and Hazlett (2020)

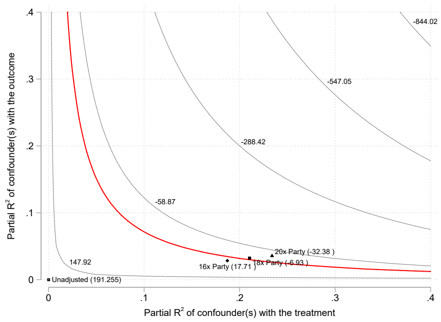
(a) Disclosure/Campaign Finance Controls



(b) Individual Controls



(c) Party Controls



(d) Country Controls

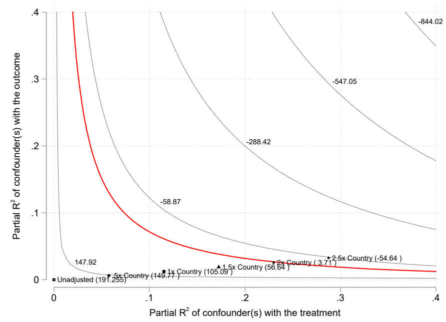
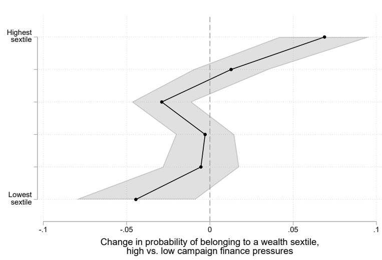
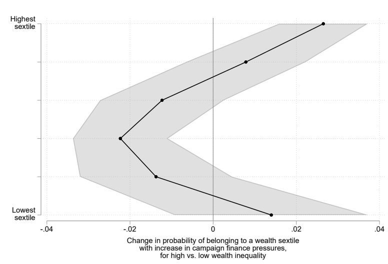


Figure A12: Results from Generalized Ordered Logit Models

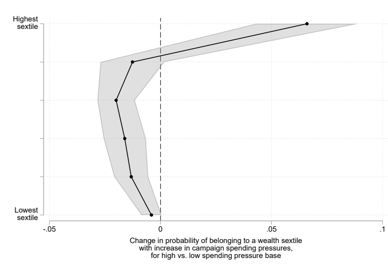
(a) H1



(b) H2



(c) H3



measures we use in our case studies. The results are qualitatively unchanged, being somewhat more precise for H1 and somewhat noisier for H2.

Robustness Check: Number of Wealth Categories

In the text, we show the results with wealth sextiles – legislators grouped into six wealth categories. Figure A14 shows similar results for H1 for wealth quintiles (five groups, panel a) and septiles (seven groups, panel b).

Figure A13: Evaluating H1 and H2 with the Spending Limits Measure

(a) H1 **(b)** H2

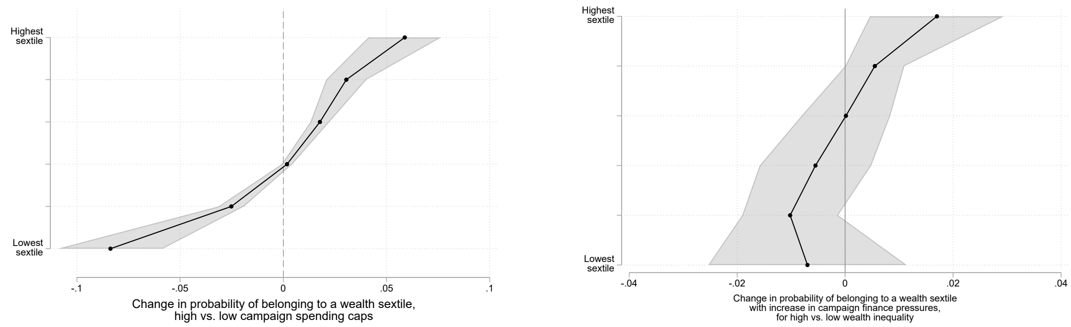
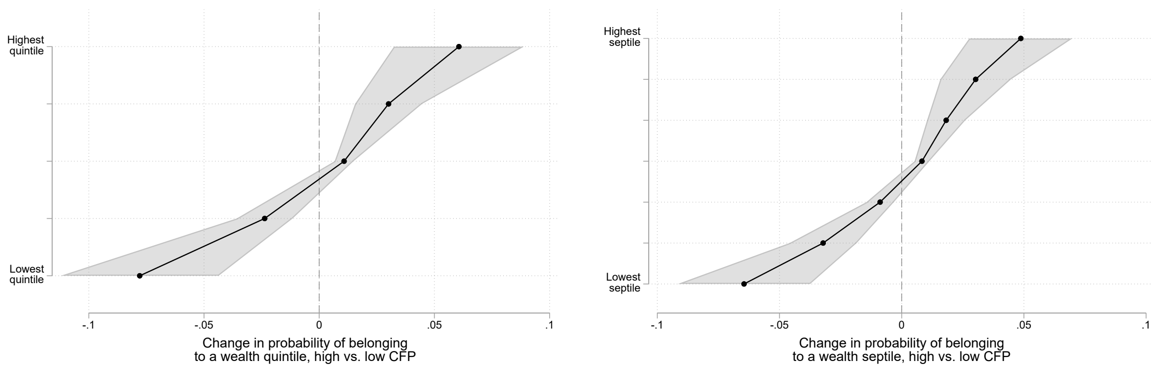


Figure A14: Results with Alternative Wealth Groups

(a) Wealth Quintiles **(b)** Wealth Septiles



Robustness Check: Non-Random Measurement Error

To address concerns about potential non-random measurement error affecting the politician wealth and campaign finance pressures variables, we follow Gallop and Weschle (2019) and examine how sensitive our results are to potential measurement error. Using politician wealth as an example, the idea behind the approach is as follows. Denote the true wealth of politician i by t_i. Instead of t_i, we observe y_i, which is the wealth stated by i in their disclosure. We suspect that the relationship between t_i and y_i is driven by a "corrupting" variable c_i. To examine the sensitivity of the results to this error, we specify a function $t_i = f(x_i|c_i)$ that describes the relationship between true and reported assets depending on the corrupting variable. Given this equation, we then simulate what the "true" wealth would look like as we sequentially increase the amount of non-random measurement error. Finally, for each set of the simulated "true" wealth data, we re-estimate our main specification (Model 1 from Table A4) and record the coefficient and standard errors of the CFP index. We examine four potential sources of non-random measurement error.

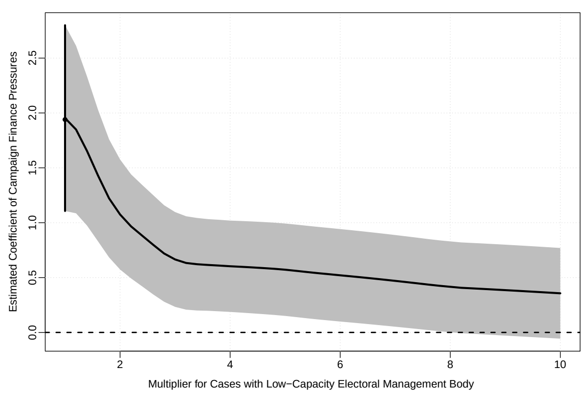
First, we may worry about our measurement of campaign finance pressures, since the regulation components included in the index capture *de jure* rather than *de facto* conditions. It might thus be the case that some countries are classified as having a very stringent regime, and thus low campaign finance pressures, when in practice they do not. To proxy for the likelihood of such mismeasurement, we use a binary variable that indicates whether a country's electoral body (which is usually the one enforcing the campaign finance rules) is considered to have a high capacity.

The idea is that high-capacity bodies enforce the rules better, so that there is less of a distinction between *de jure* and *de facto*, whereas in countries with low-capacity bodies *de facto* rules are less stringent than *de jure* regulation. While we control for this variable in Table A4, here we model "true" campaign finance pressure as $t_i = x_i$ when the country has a high-capacity electoral body and $t_i = \eta x_i$ if it has a low-capacity one, where t_i is the *de facto* and x_i the observed *de jure* campaign finance pressure, and η is a multiplier that quantifies how much measurement error there is for politicians subject to a lower-capacity electoral body. $\eta = 1$ means no measurement error. Since we are concerned that *de facto* rules are less stringent than the *de jure* rules, we examine the case where $\eta > 1$.[9]

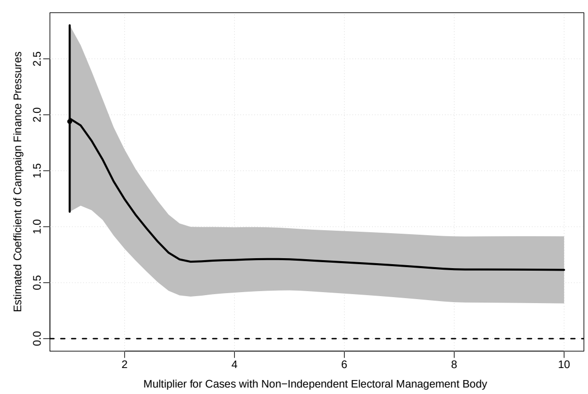
Figure A15(a) shows the CFP index coefficient depending on how much lower the "true" regulation scores of countries with low-capacity bodies is assumed to be.[10] As η increases, the coefficient remains positive and significant unless η is larger than 8. An η of 8 corresponds to a scenario where 20 out of the 24 countries with a low-capacity electoral body have a "true" campaign finance pressures score of 1, indicating that they have maximum spending pressure. Even in such an extreme scenario our results hold, and our coefficient of interest only becomes (just about) insignificant in even more extreme scenarios.

Figure A15: Robustness Check: Non-Random Measurement Error

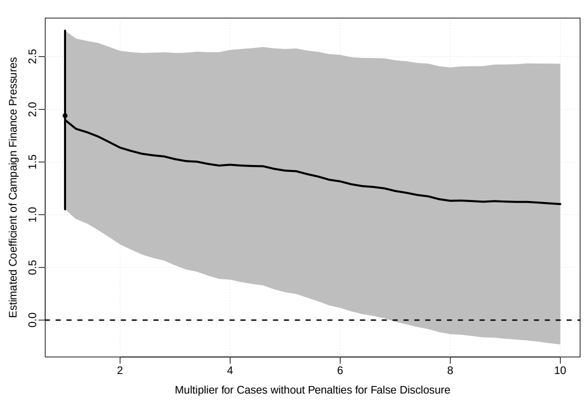
(a) Electoral Body Capacity



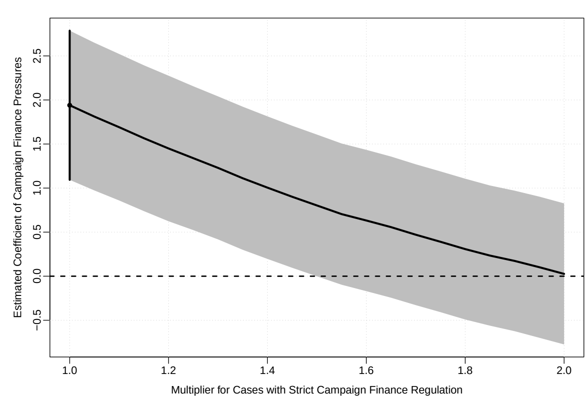
(b) Electoral Body Independence



(c) Disclosure System Stringency



(d) Campaign Finance Pressures



[9] Since the measure of campaign finance pressures ranges between 0 and 1, we impose the restriction that $t_i \leq 1$.

[10] Note that since we model "true" regulation scores as a function of the capacity of a country's electoral body, we drop the control variable for the latter from the regressions. We do the equivalent in the other tests below as well.

Second, rather than using the capacity of the electoral body as a proxy, we use its independence instead. We model "true" campaign finance pressures as $t_i = x_i$ when the country has an independent electoral body and $t_i = \eta x_i$ if it does not. Figure A15(b) shows the CFP index coefficient depending on how much higher the "true" financing pressures for countries with non-independent electoral bodies are. As η increases, the coefficient shrinks in size, but remains positive and significant for all levels of η. Indeed, the coefficient and confidence interval stabilize at higher levels of η, and the effect never becomes insignificant no matter how high the non-random measurement error gets.

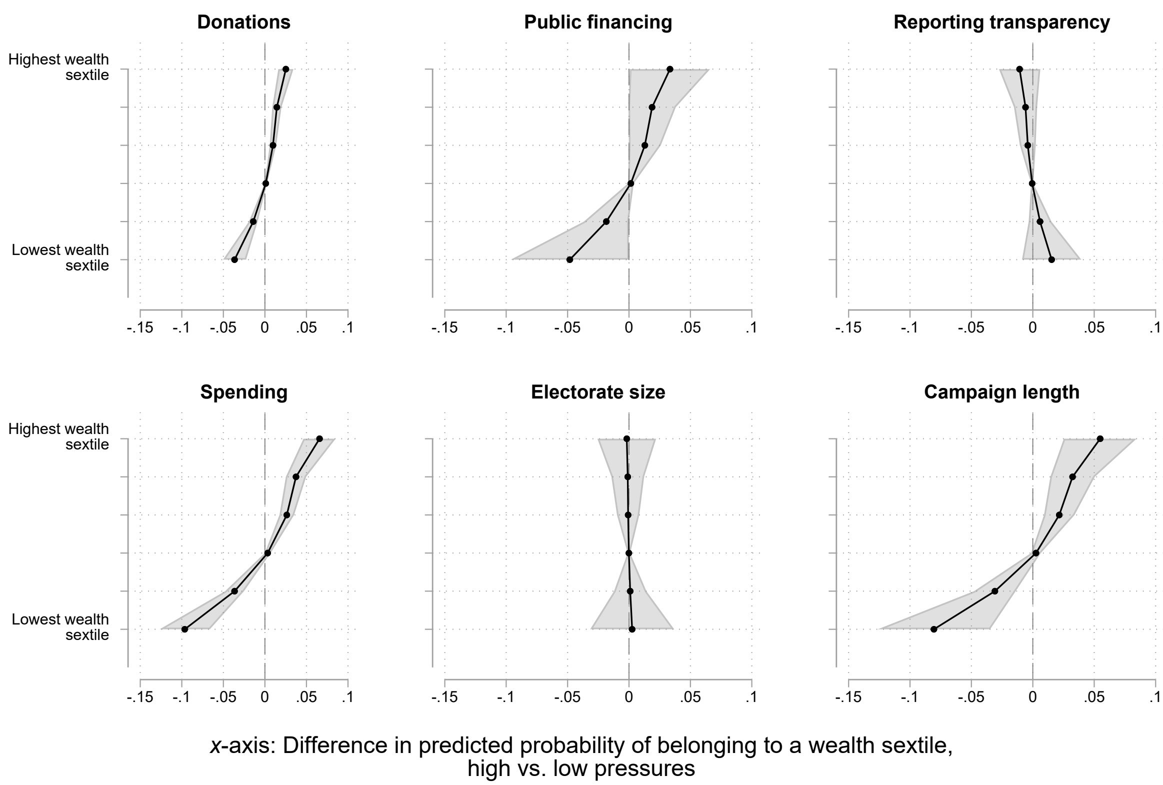
A third concern is that wealth misreporting is influenced by how stringent the national disclosure system is. If legislators in countries where the disclosure system is less stringent underreport their wealth, and the stringency of the disclosure system and of campaign finance pressures are correlated, this might account for our findings. As a proxy for disclosure system stringency, we use a binary variable that indicates whether there is a penalty for filing a false disclosure report or not. We model "true" politician wealth t_i as $t_i = y_i$ if there is a penalty for false disclosure reports, and $t_i = \eta y_i$ if there is not. Figure A15(c) shows the CFP index coefficient depending on the amount of underreporting by politicians who do not face any penalties for false disclosure. The association remains statistically significant for a considerable amount of misreporting—only if the true assets of politicians who do not face penalties for false disclosure are at least 7 times higher than the reported ones does the coefficient for CFP become insignificant.

A final concern is that the CFP index itself affects the misreporting of assets. For our results to potentially be driven by systematic measurement error, it needs to be the case that politicians hide more assets in countries where campaign finance pressures are *lower*. While this seems unlikely, we nevertheless simulate what the results would look like if the true wealth of politicians subject to lower campaign finance pressures is higher than what they report. We dichotomize our measure of campaign finance regulation into below or equal to the 25th percentile (lower pressure) and above it (higher pressure). We model "true" politician wealth t_i as $t_i = y_i$ if spending pressures are high and $t_i = \eta y_i$ if they are low. Figure A15(d) shows the coefficient of campaign finance pressures depending on the amount of underreporting by politicians in low-pressure regimes. The association again remains statistically significant for a considerable amount of misreporting, and becomes insignificnat only if the true assets of politicians who are subject to low campaign pressures are at least 1.5 times higher than the reported ones. But again, it is difficult to think of reasons why politicians who are subject to low campaign finance pressures would systematically underreport their assets, and it indeed seems more likely that politicians in high-pressure countries would do so (which would mean that Model 1 in Table A4 *underestimates* the effect of campaign finance pressures on politicians' wealth).

Additional Results: By CFP Component

Figure A16 shows the results for H1 across the six components of the CFP index: limits on private donations, public financing, reporting transparency, spending limits, average size of the electorate, and the length of the campaign calendar. All measures are scaled so that higher values indicate greater pressures and normalized so that a one unit increase represents an increase of a one standard deviation. The graphs show that various aspects of financing pressures are associated with greater advantages to the wealthy: in addition to the less restrictive spending limits, this includes the less restrictive donation limits, lower public funding, and longer electoral calendars.

Figure A16: H1 Results for the CFP Components



F Case Study 1: Brazil

F.1 Campaign Spending Near the Discontinuity

In this section, we provide additional details on candidates' campaign spending near the cutoff. As a first step, Table A8 provides results from a set of RD models at the municipality-level.[11] The first row shows that in municipalities subject to the higher spending limit, all candidates together spend about R$43,300 more. Given that the total spending in the average municipality near the cutoff on the left of the discontinuity is R$120,890, this represents a spending increase of about 36 percent. The second row shows that average spending by 2016 seat winners at the discontinuity increased by about R$630. In contrast, average spending by 2016 candidates who did not win increased only by a non-significant R$311 at the discontinuity.

Table A8: Effect of Higher Spending Limits on 2016 Campaign Spending at Municipality-Level

Dependent Variable	RD Estimate	SE
Total Campaign Money	43342.459	14098.279
Average Campaign Money Winners	632.382	320.155
Average Campaign Money Losers	310.754	241.761

Note: The bandwidth is equal for all models and obtained by averaging the MSE-optimal bandwidths for the three separate models (h=2,516, b=4,296).

To explore this further, Figure A17 shows the raw campaign spending data. The first row shows all candidates, the second shows winners only, the third shows "competitive" losers, and the final row shows "non-competitive" losers.[12] Within each row, we show candidates from the lowest to the highest wealth sextile. Candidates are colored according to how much they spend relative to the spending cap in their municipality.

First, we clearly see a high correlation between spending and winning. Seat winners spend more on average, and are more likely to spend near the cap. Candidates who end up losing, but achieve a competitive result spend less, and are less likely to spend near the cap. Finally, non-competitive losers spend the least, and they are unlikely to spend close to the cap.

Second, there are clear differences according to candidate wealth. Moving from the lowest to the highest wealth sextile in each row, we can see that more and more candidates spend close to the limit. This is especially apparent among those candidates who end up winning a seat, and to a somewhat lesser extent among competitive losers.

Finally, the graphs also show that a good number of candidates spend close to the limits, suggesting that the caps were binding and did prevent some candidates from spending more. This is especially apparent among wealthy candidates and those who ended up winning a seat.

Table A9 shows a series of RD models where the unit of observation is the candidate to provide further insight into campaign spending at the discontinuity. The first model shows that on average and for all candidates, campaign spending under higher spending limits 8.1 percent higher than under tighter caps. The next three models examine discontinuities in the source of the campaign money. Interestingly, the higher overall spending does not stem from candidates on the right of the discontinuity spending more of their own money. Instead, we observe that they raise 8.6 percent

[11] The bandwidth is equal for all models and obtained by averaging the MSE-optimal bandwidths for the three separate models (h=2,516, b=4,296).

[12] Losers are defined as competitive is they receive at least half as many votes as the least successful winner in their municipality.

more from individual donors. In addition, parties provide 4.1 percent more money to candidates on the right of the discontinuity. The final model uses campaign spending as a share of the spending cap. It demonstrates that, even though overall spending goes up under more permissive caps, candidates on average spend a smaller share of what is allowed on the right of the discontinuity.

Panel (a) of Figure A18 shows the effect of the higher campaign spending limit on candidates' campaign spending separately for each wealth sextile.[13] We see that the overall increase of 8.1 percent is driven by wealthy candidates, especially those in the highest sextile, who spend on average almost 18 percent more. In contrast, those in the lower sextiles spend no more (or only a little more) on the right of the discontinuity. Panel (b) shows the effect of the higher campaign spending limit on candidates' campaign spending relative to the limit, again separately for each sextile.[14] When they are subject to more permissive spending limits, candidates in the highest sextile spend roughly the same share relative to the cap as their wealthy counterparts subject to lower limits. In contrast, less wealthy candidates spend a lower share of the allowed amount on the right of the discontinuity.

Table A9: Effect of Higher Spending Limits on 2016 Campaign Spending.

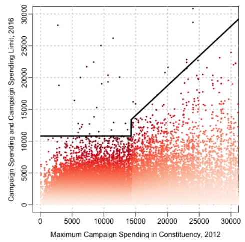
Dependent Variable	RD Estimate	SE
Total Campaign Money (log + 1000)	0.081	0.015
Source: Self (log + 1000)	-0.007	0.015
Source: Individual Donors (log + 1000)	0.086	0.014
Source: Party (log + 1000)	0.041	0.005
Spending as Share of Limit	-0.018	0.006

Note: The bandwidth is equal for all models and obtained by averaging the MSE-optimal bandwidths for the five separate models (h=2,215, b=4,777).

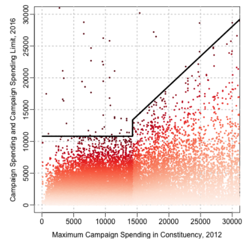
[13] The bandwidth is equal for all models and obtained by averaging the MSE-optimal bandwidths for the six separate models (h=4,176, b=6,719).

[14] The bandwidth is equal for all models and obtained by averaging the MSE-optimal bandwidths for the six separate models (h=4,676, b=7,569).

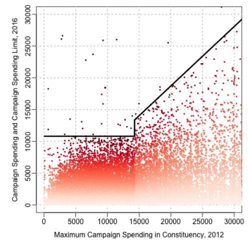
Figure A17: Campaign expenditures in Brazil by wealth sextile, for all candidates, winners, competitive losers, and non-competitive losers. Losers are defined as competitive is they receive at least half as many votes as the least successful winner in their municipality.



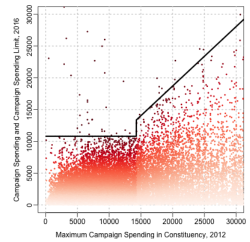
(a) All: Lowest sextile



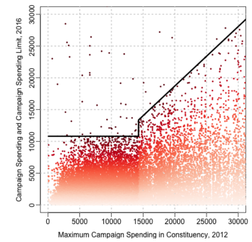
(b) All: 2nd sextile



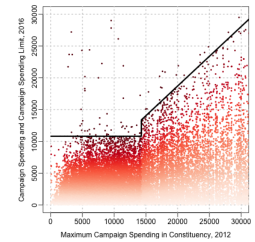
(c) All: 3rd sextile



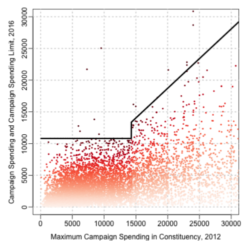
(d) All: 4th sextile



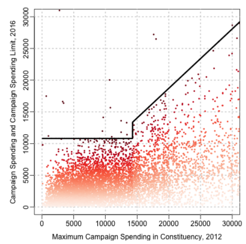
(e) All: 5th sextile



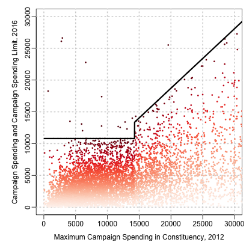
(f) All: Highest sextile



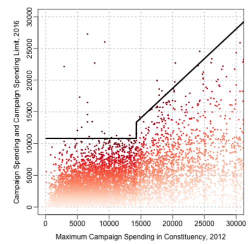
(g) Winners: Lowest sextile



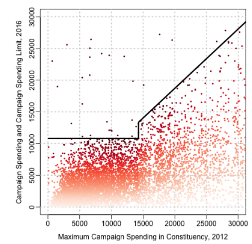
(h) Winners: 2nd sextile



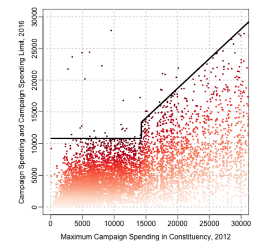
(i) Winners: 3rd sextile



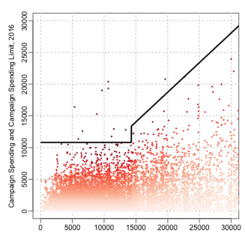
(j) Winners: 4th sextile



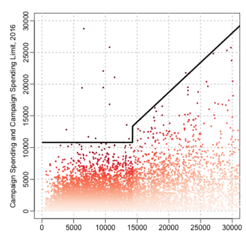
(k) Winners: 5th sextile



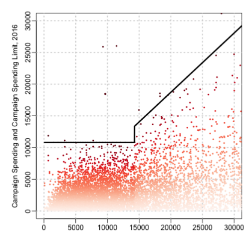
(l) Winners: Highest sextile



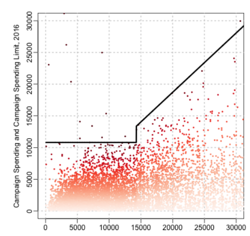
(m) Comp. Losers: Lowest sextile



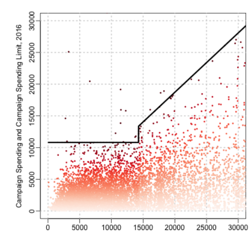
(n) Comp. Losers: 2nd sextile



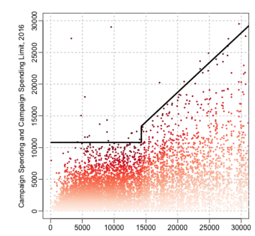
(o) Comp. Losers: 3rd sextile



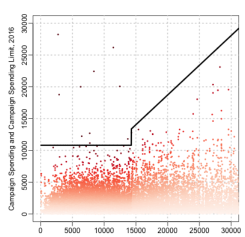
(p) Comp. Losers: 4th sextile



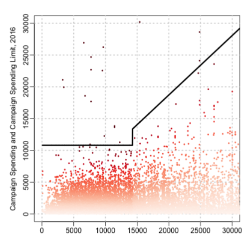
(q) Comp. Losers: 5th sextile



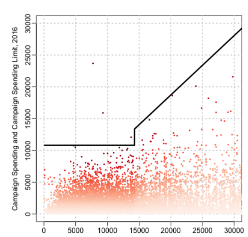
(r) Comp. Losers: Highest sextile



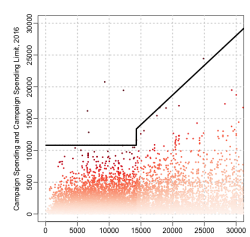
(s) Non-Comp. Losers: Lowest sextile



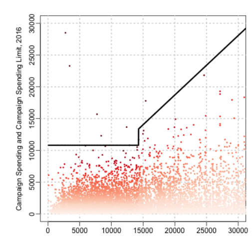
(t) Non-Comp. Losers: 2nd sextile



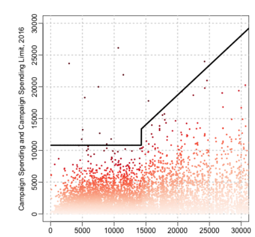
(u) Non-Comp. Losers: 3rd sextile



(v) Non-Comp. Losers: 4th sextile



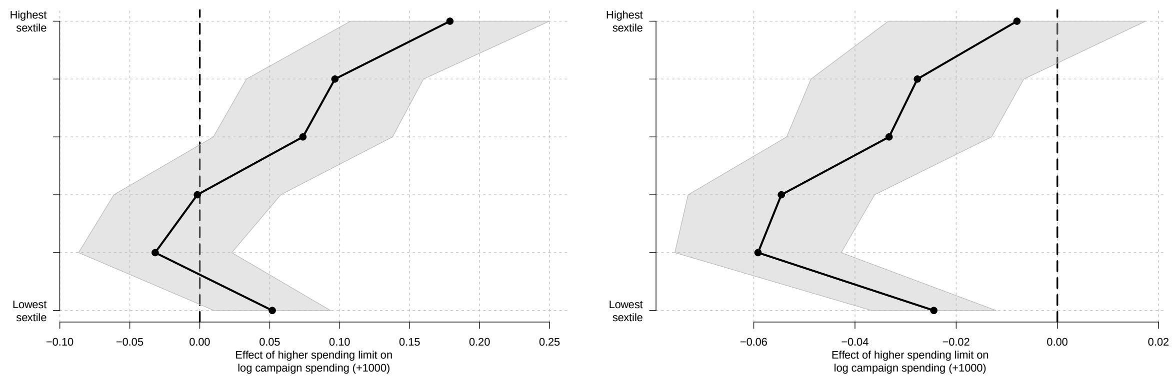
(w) Non-Comp. Losers: 5th sextile



(x) Non-Comp. Losers: Highest sextile

Figure A18: Effect of Higher Campaign Spending Limit on Campaign Spending

(a) Total Campaign Money (log + 1000) **(b)** Spending as Share of Limit



F.2 Diagnostic Tests

Table A10 shows that municipalities above and below the spending gap discontinuity are on average similar on a number of pre-determined characteristics. We observe a lack of smoothness in the municipal council size, which in turn depends on the municipal population. We control for council size in all our RD analyses using council size fixed effects.

Table A10: Covariate Smoothness Tests for the RD Analaysis

Variable (2012 Characteristics)	Estimate	Standard Error	N	BW
Candidate Age	0.465	0.492	5328	3719
Share Female Candidates	0.003	0.004	5328	5403
Candidate Education Share: Analphabete	0.000	0.000	5328	6409
Candidate Education Share: Read and Write	-0.005	0.008	5328	6088
Candidate Education Share: Primary Incomplete	-0.005	0.014	5328	4781
Candidate Education Share: Primary Complete	-0.003	0.008	5328	6259
Candidate Education Share: Secondary Incomplete	0.007	0.005	5328	5077
Candidate Education Share: Secondary Complete	0.002	0.014	5328	4702
Candidate Education Share: Higher Incomplete	-0.002	0.004	5328	6093
Candidate Education Share: Higher Complete	0.008	0.010	5328	5025
Turnout	-0.004	0.008	5328	4892
Eligible Voters (log)	0.232	0.087	5328	6225
Council Size	0.517	0.188	5328	5172

Figure A19 plots estimates of the density of the running variable on either side of the spending cap discontinuity, using the approach of Cattaneo, Jansson and Ma (2018).[15] It shows no evidence of strategic sorting of candidates into municipalities above or below the spending limit discontinuity (the p-value for the estimate of the jump in the density of the running variable is 0.78).

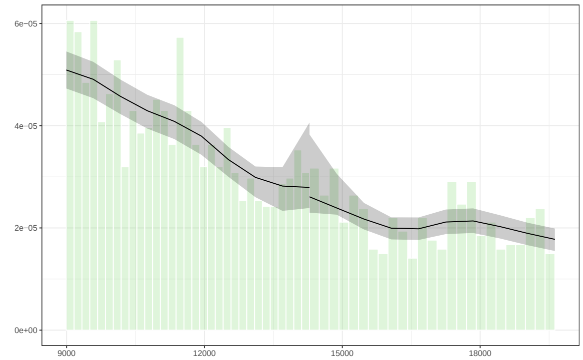
F.3 Robustness Checks

Robustness Check: Missing Wealth Data

Some legislators failed to submit a wealth declaration for the 2012 elections. In these cases, we used their declarations from other election years, if available, and benchmarked them to the median

[15]The unit of analysis is a municipality.

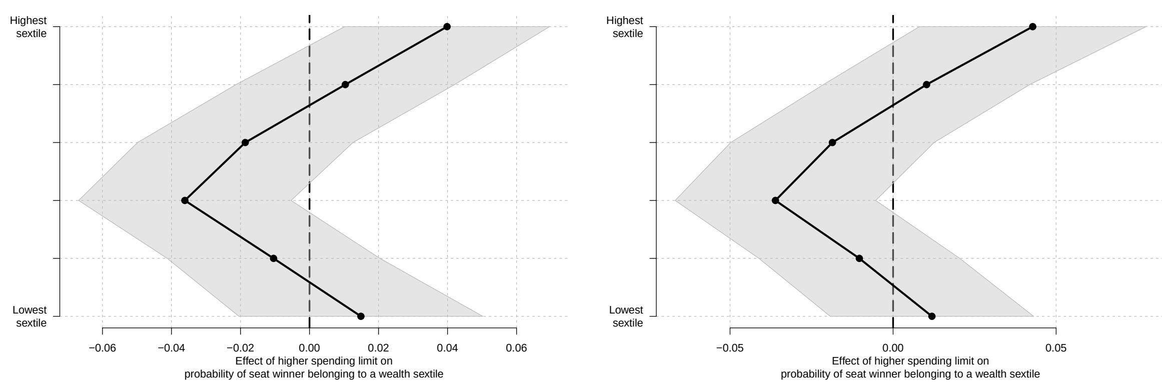
Figure A19: Test for Sorting around the Spending Cap Discontinuity



household wealth from those years. This allows us to reduce the share of legislators with missing wealth data from 18 to 6.5 percent. To ensure that our results are not sensitive to the fact that these observations are not included in our analyses, Figure A20 shows the results when assuming two extreme scenarios. In panel (a) we assume that all legislators with missing wealth information are members of the lowest sextile, and in panel (b) we assume they are all part of the highest sextile. In both cases, the results only change minimally compared to Figure 8.

Figure A20: Effect of Higher Campaign Spending Limit on Wealth of Council Members, Sensitivity to Missing Wealth Data

(a) All Missing Values Set to Lowest Wealth Sextile **(b)** All Missing Values Set to Highest Wealth Sextile



Robustness Check: Bandwidth

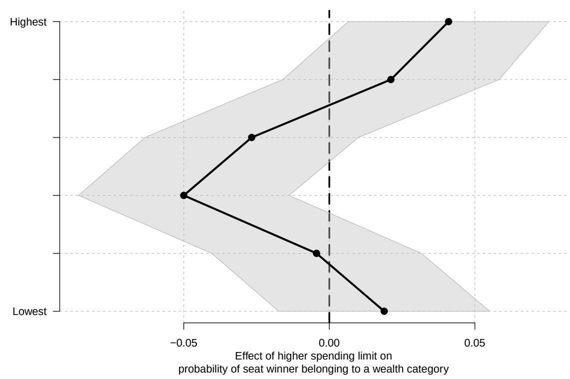
Figure 8 uses the bandwidth of 4,491, which we obtained by averaging the MSE-optimal bandwidths for six separate models (one for each wealth sextile as dependent variable). Figure A21 shows the results using bandwidths of 3,491, 3,991, 4,991, and 5,491, so 500 and 1,000 smaller and larger than the original bandwidth. In all cases, the results are similar to those in Figure 8.

Robustness Check: Number of Wealth Categories

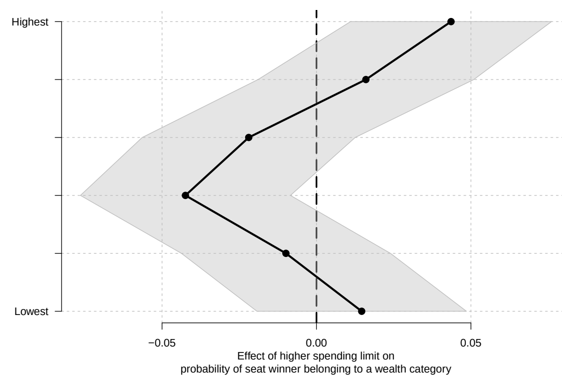
In the text, we show results for six wealth categories. Figure A22 shows that results are similar when using five or seven categories.

Figure A21: Effect of Higher Campaign Spending Limit on Wealth of Council Members, Sensitivity to Bandwidth Choice

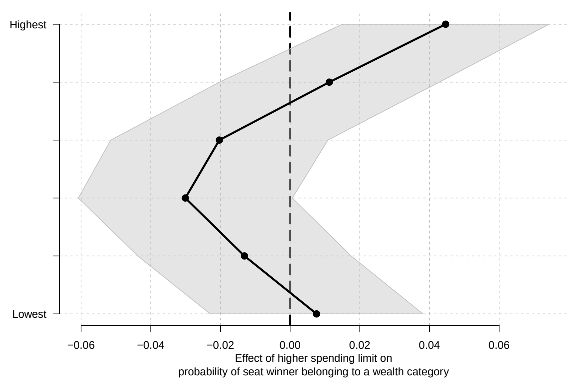
(a) Original Bandwidth - 1000



(b) Original Bandwidth - 500



(c) Original Bandwidth + 500



(d) Original Bandwidth + 1000

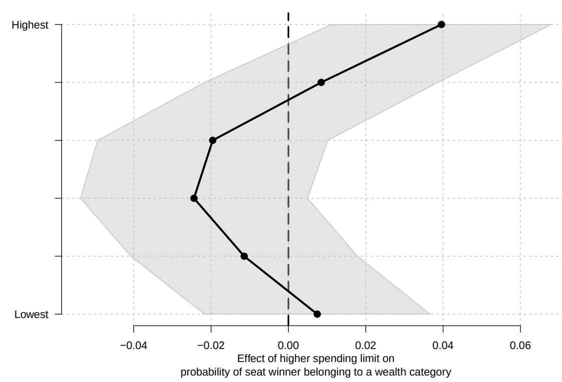
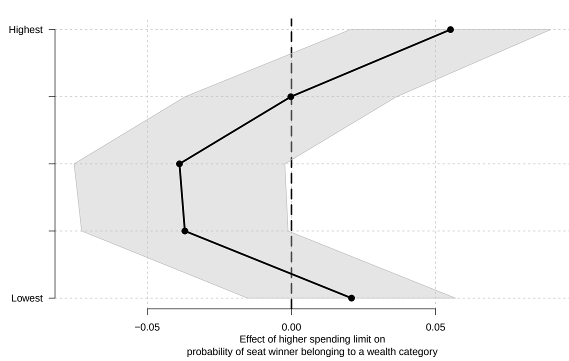
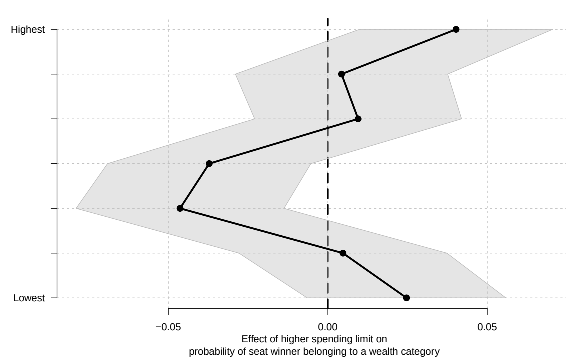


Figure A22: Effect of Higher Campaign Spending Limit on Wealth of Council Members, Sensitivity to Number of Wealth Categories

(a) 5 Categories



(b) 7 Categories



G Case Study 2: Chile

G.1 Background Details

As discussed in the paper, we make use of the variation in the change in financing pressures stemming from a combination of two reforms: an electoral reform passed in 2015, and a campaign finance reform passed in 2016.[16]

The campaign finance reforms followed a series of political corruption scandals involving bribery, tax evasion, and illegal campaign contributions.[17] In response, an independent, non-partisan council recommended a series of reforms that received near-universal support by all parties in the Chamber of Deputies and passed with 97 voting in favor, 4 against, and 2 abstentions.[18] Among other things, the reform changed the amount candidates could spend compared to the previous election in 2013. Before the reform, candidates could spend 1,500 UF plus 0.03 UF for each voter in their district (1 UF corresponding to 24,627 Chilean pesos, or $44 USD). After the reform, they could spend 700 UF plus 0.015 UF for each voter in the district (1 UF corresponding to 26,319.9 Chilean pesos, or $40 USD). This means that the 2017 spending limit per district was about half of what it was in 2013.

The electoral reform approved one year earlier made good on a long-standing campaign promise by President Michelle Bachelet and was intended to ease the candidate selection process within her coalition (see Gamboa and Morales, 2016), which had gained a large majority in the 2013 election. The reform was approved without the support of the main opposition parties.[19] The reform increased the number of deputies from 120 to 155 and decreased the number of districts from 60 to 28, changing the district magnitude from two seats for all districts to between three and eight seats in the new districts.[20]

The combination of these two reforms changed the spending limit unevenly across seats. As mentioned in the text, the 2017 campaign spending limit increases in the number of voters in a district, but does *not* take the change in district magnitude into account. This is mainly because the two reform packages were promulgated separately, by distinct groups of actors. The lack of coordination meant that candidates in new districts that saw a larger increase in magnitude had a higher spending limit than before, despite the campaign reform decreasing the limit across all districts compared to the 2013 election. By contrast, candidates in districts with a smaller increase in magnitude saw either a slight decrease, no change, or a small increase in the spending limit compared to 2013.

In addition to inducing variation in the spending limit across districts where there was no variation before, the combination of the two reforms also created variation in the spending limit *within* districts across seats. This is because the electoral reform decreased the number of districts, and so for some candidates their new, 2017 district could map to two or more 2013 districts. Therefore, different candidates fielded in the same new district could be subject to different changes in the spending limit. To calculate the change in the spending limit for each seat, we assume that

[16] See https://www.bcn.cl/leychile/navegar?idNorma=1107658&idVersion=2020-10-24 and https://www.bcn.cl/leychile/navegar?idNorma=1077039.

[17] See for example: Pascale Bonnefoy, "Executives are Jailed in Chile Finance Scandal." The New York Times, March 8, 2015, http://www.nytimes.com/2015/03/08/world/americas/executives-are-jailed-in-chile-finance-scandal.html.

[18] See: https://www.camara.cl/legislacion/ProyectosDeLey/votaciones.aspx?prmID=10201&prmBOLETIN=9790-07.

[19] See: https://www.gob.cl/noticias/camara-aprobo-en-tercer-tramite-el-fin-del-sistema-binominal.

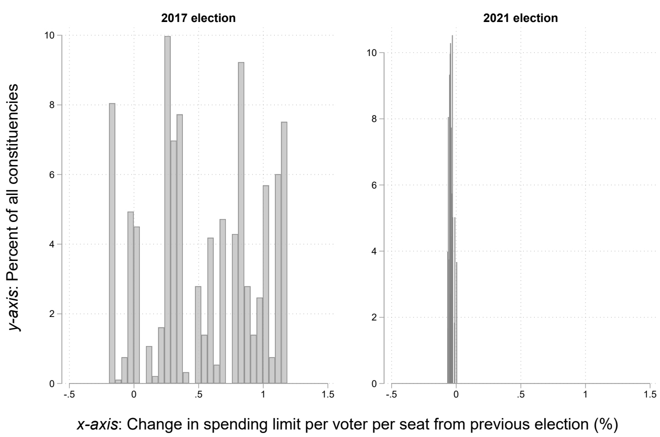
[20] The electoral reform also introduced gender quotas, but they apply generally and do not vary with district magnitude (Piscopo et al., 2022). The electoral formula – an open-list proportional representation system with seat allocation by the D'Hondt rule – was unchanged.

under the previous electoral system, a candidate would have run in the district where their residence is located. To verify this assumption, we looked at the candidates who ran in both the 2013 and 2017 elections. The correlation between their real district and our assumed district is 0.97. One of the reasons for such a high correlation is that candidates can be nominated in a district only if they maintain residency in the region where the district is located for at least two years before the election (Article 44 of the Chilean Constitution).[21] This limits the movement across districts over time.

Take, for example, Jaime Mulet Martinez and Sofia Cid Versalovic – both elected in new district 4. The comuna of residence of the legislators is *Vallenar* for Mulet Martinez (old district 6) and *Copiapo* for Cid Versalovic (old district 5). The spending limit was $CLP 1867.3 million in old district 5 and $CLP 2177.8 million in old district 6. The spending limit in new district 4 is $CLP 2382.9 million. This makes the percent change in the spending per voter per seat, 9.4 percent for Mulet Martinez and 27.6 percent for Cid Versalovic.

More generally, the left panel of Figure A23 shows the distribution of the changes in the spending limit between the 2013 election and the 2017 election. About a sixth of all constituencies saw a decrease or no change in the spending limit (with the largest decrease amounting to 20 percent), while about a fifth saw a 100 percent increase or more. The median change was a 50 percent increase.

Figure A23: Distribution of Changes in Spending Limit in Chile



By contrast, between the 2017 and 2021 parliamentary elections, the spending limit formula and the district boundaries and magnitudes remained the same, and so the changes in the spending limit were minimal, driven only by the changes in the number of registered voters in each district. These patterns are shown in the right panel of Figure A23, with changes in the limit ranging from a decrease of 6 percent to an increase of half a percent, with a median of -4 percent. The lack of large changes in the spending limits between 2017 and 2021 provide us with an opportunity to conduct a placebo test, which we discuss in Section G.4 below.

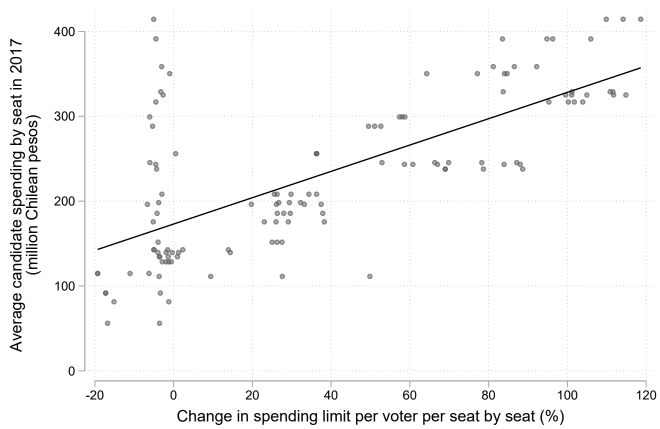
G.2 Research Design

Above, we described that some constituencies saw an increase in the spending limit in 2017, whereas others saw a decrease or no change. Figure A24 verifies that these changes in spending pressure correlate strongly with actual campaign spending in 2017. The gray dots represent the average

[21]See: https://www.camara.cl/camara/doc/leyes_normas/constitucion_politica.pdf.

expenditures by candidates in each constituency (expressed in millions of Chilean pesos); the black line is the linear best-fit line.

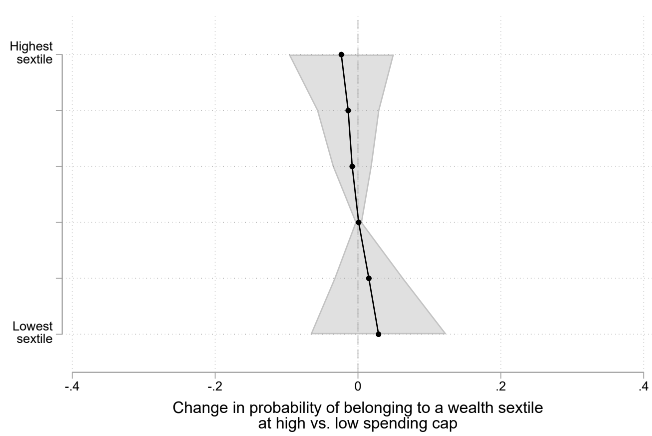
Figure A24: Correlation between Changes in Spending Limit and Actual Campaign Expenditures



The results shown in the paper are from an ordered logistic model of wealth sextiles of winners[22] in 2017 and 2021 on the change in the spending limit between the two elections, the 2017 district dummies (whose boundaries remained unchanged for the 2021 election), party dummies, as well as the following covariates: total number of candidates running in a district, legislator's gender, age (in years), martial status (married/divorced, or other) and whether the legislator ran in the previous election. The coefficient estimates are shown in Table A11.

Figure A25 shows the results for 2021 from this specification. As expected, given very little change in the spending limit between the 2017 and 2021 election, we observe null effects on the distribution of wealth of winners in the 2021 election across all sextiles. These results provide reassurance that our findings for 2017 are plausibly driven by the changes in the spending limit.[23]

Figure A25: Campaign Financing Pressures and Legislator Wealth in Chile, 2021



[22]To be consistent with the cross-national analyses and the Brazil case study, we focus only on the seat winners.

[23]Unlike for 2013-2017, we observe the wealth of winners for each seat in both 2017 and 2021, allowing us to examine the *changes* in the wealth of legislators as a function of changes in the spending limit between 2017 and 2021. The results are very similar to those shown in Figure A25.

Table A11: Coefficient Estimates for Chile

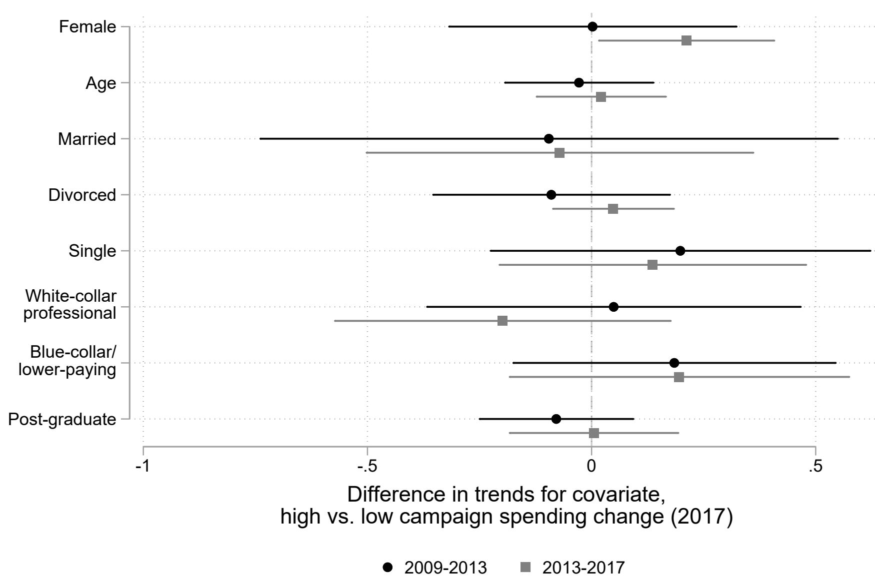
	(1)	
Above-median change in spending limit	1.66*	(0.87)
2021 election	0.76	(0.82)
Above-median change in spending limit × 2021 election	-1.94**	(0.92)
Female	-0.39	(0.64)
Age	0.88*	(0.51)
Marital status	0.90**	(0.39)
Ran in previous election	1.75***	(0.34)
New district 2	3.63***	(0.49)
New district 3	0.85	(0.85)
New district 4	2.67***	(0.72)
New district 5	0.89	(0.71)
New district 6	1.07	(0.79)
New district 7	2.38***	(0.76)
New district 8	2.96***	(0.78)
New district 9	2.73***	(0.83)
New district 10	1.44*	(0.84)
New district 11	2.27***	(0.83)
New district 12	3.08***	(0.88)
New district 13	1.29	(0.82)
New district 14	1.19	(0.74)
New district 15	0.65	(0.72)
New district 16	0.92	(0.81)
New district 17	0.99	(0.72)
New district 18	3.43***	(0.69)
New district 19	0.92	(0.69)
New district 20	1.13	(0.84)
New district 21	1.88**	(0.81)
New district 22	1.51**	(0.76)
New district 23	1.14	(0.81)
New district 24	1.58**	(0.74)
New district 25	1.30*	(0.67)
New district 26	1.72**	(0.70)
New district 27	2.44***	(0.43)
Party: CS	-1.24	(1.07)
Party: EVOPOLI	3.86**	(1.50)
Party: FRVS	3.12**	(1.26)
Party: PC	0.07	(0.65)
Party: PCC	-12.25***	(1.20)
Party: PDC	1.82*	(1.00)
Party: PDG	1.95	(1.83)
Party: PEV	0.48	(2.09)
Party: PH	1.28	(2.45)
Party: PL	1.39	(0.93)
Party: PODER	-17.15***	(1.80)
Party: PPD	2.17**	(0.88)
Party: PR	2.08	(1.28)
Party: PRO	-15.95***	(1.23)
Party: PRSD	2.91**	(1.48)
Party: PS	1.72*	(0.95)
Party: RD	-0.26	(0.89)
Party: REP	0.17	(1.31)
Party: RN	1.88**	(0.93)
Party: UDI	1.82	(1.14)
No. of party candidates in district	0.08	(0.28)
Observations	252	

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered by district.

G.3 Placebo Trends

While we cannot assess the plausibility of parallel trends because the wealth disclosures were only mandated in 2016, we can evaluate the trends in the demographic characteristics of politicians that typically correlate with wealth. We collected data on gender, age, occupation (white-collar professional vs. blue-collar/lower-paying occupations), marital status (married, divorced, or single), and education (university vs. post-graduate, given that there are exceedingly few legislators without at least a college degree) for two electoral cycles before the campaign reform: 2009-2013, and 2013-2017. We then matched the 2017 districts to the districts in 2009 and 2013, and examine the difference in trends in each characteristics in the two electoral cycles among the treated and untreated districts (i.e. those with the above-median vs. below-median change in the campaign spending limit per voter per seat in 2017). The results are shown in Figure A26, with the estimates for the 2009-2013 cycle in black and for 2013-2017 in gray. The patterns provide suggestive evidence that our results are unlikely to be driven by pre-treatment trends that are correlated with politicians' wealth.

Figure A26: Trends in Legislators' Demographic Characteristics Prior to Campaign Finance Reform in Chile



G.4 Additional Results

Robustness Check: Number of Wealth Categories

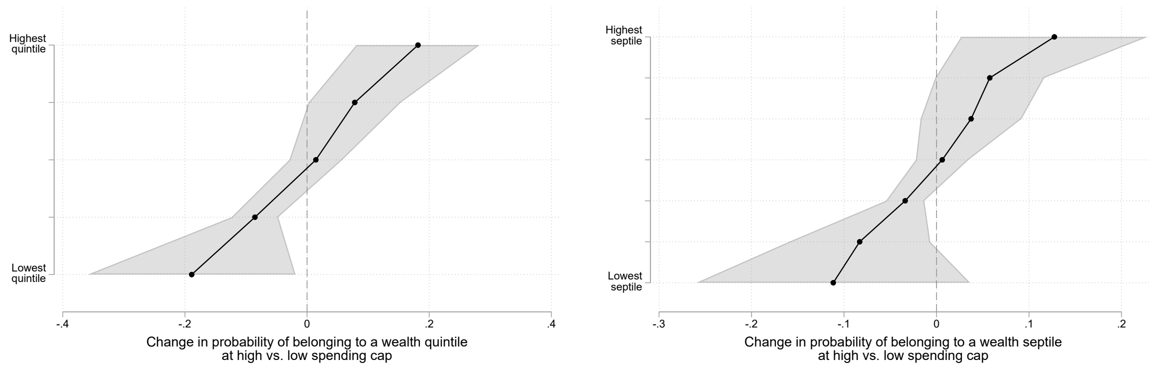
Figure A27 shows the results with five wealth categories (panel a) and seven wealth categories (panel b) instead of the six categories we use in the paper. The results are similar.

Additional Result: Party Transfers

As briefly mentioned in the text, the results in Chile are not fully consistent with H3, in that the advantages to the very wealthy legislators from greater spending pressures appear to accrue

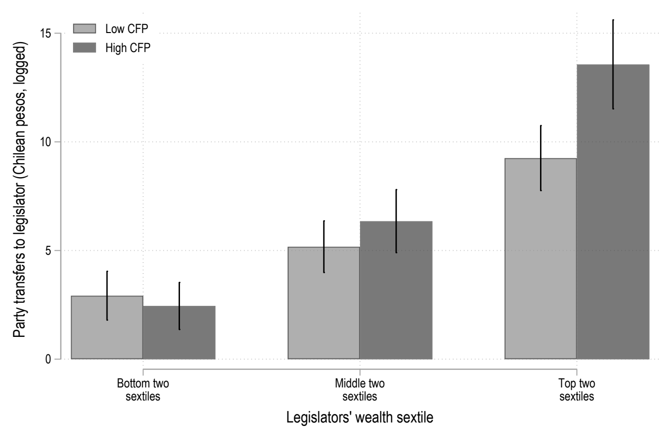
Figure A27: Results for Chile with Alternative Wealth Groups

(a) Wealth Quintiles **(b)** Wealth Septiles



at the expense of the relatively poor legislators, rather than the medium wealthy. This may be because, unlike in Brazil where parties play a very limited role in campaign financing, the parties in Chile are stronger and redistribute resources more actively. Figure A28 shows descriptively that in constituencies with greater financing pressures (those with a greater increase in the spending limit between 2013 and 2017), parties transferred resources *away* from the relatively poor and toward the medium and *especially* the very wealthy legislators, compared to the constituencies with lower financing pressures.

Figure A28: Party Campaign Transfers to Legislators by Spending Pressures and Wealth Sextiles



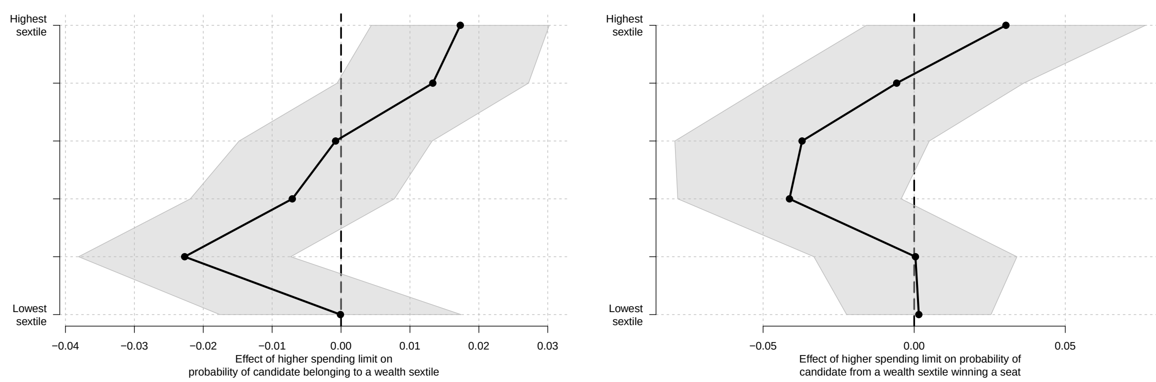
H Impact of the CFP on Running and Winning by Sextile in Brazil and Chile

Greater financing pressures may influence who wins office in two ways: (a) through selection into running, and (b) upon entering the race, through ability to deploy sufficient campaign resources. In the paper, we do not differentiate between these two channels, as we only examine the impact of greater CFP on winners. However, we can use the data on wealth of all candidates in Brazil and Chile to separately evaluate these two effects by wealth sextile.

Figure A29 examines the two mechanisms for Brazil. Panel (a) shows results from a set of regression discontinuity models where the unit of observation is the candidate.[24] The dependent variable takes the value of one if a candidate belongs to a certain wealth sextile.[25] On the right of the discontinuity, a given candidate is about 1.9 percentage points more likely to belong to the highest wealth sextile than on the left of the discontinuity. Given that the baseline probability of a candidate being part of the highest wealth sextile (in municipalities close to the cutoff subject to the stricter spending limit) is about 12 percent, this is a sizeable effect. Furthermore, a given candidate is about 1.3 percentage points more likely to be from the fifth wealth sextile under higher spending limits (relative to a baseline of 13.7 percent). In contrast, under higher spending caps, one is less likely to find candidates that are members of the second to fourth wealth sextile, although the effect is only statistically significant for the second group (1.8 percentage points less likely, from a baseline of 18.2 percent). There is no difference in the probability of a candidate being from the lowest wealth group. Overall, Panel (a) looks quite similar to our results in the paper, and thus indicates that selection into running is an important reason why permissive campaign spending limits lead to more wealthy politicians.

Figure A29: Effect of Higher Campaign Spending Limit on Candidate Pool and Election Success in Brazil

(a) Running **(b)** Winning | Running



Panel (b) examines the effect of higher spending limits on the probability of winning a seat *conditional* on having entered the race. The unit of observation is again the candidate, and the dependent variable takes on the value of one if they won a seat.[26] Candidates from the highest wealth sextile appear to be slightly more likely to win a seat, although the effect is not statistically significant. However, it is important to keep in mind that the effect on the candidate pool we see in panel (a) likely attenuates the wealth advantage in winning, by creating greater competition among wealthier candidates (because more of them enter the race). Figure A29 therefore suggests that more permissive campaign spending caps in Brazil advantage the very wealthy in both ways, by encouraging them to run, and making them more likely to win. The converse holds for those who are well-off, but not very wealthy.

Figure A30 examines the same two mechanisms in Chile. As in Brazil, the unit of analysis is the candidate. In panel (a), we compare the probability of observing a candidate in each sextile

[24] Importantly, the number of candidates is not significantly different across the discontinuity, so any effects can be interpreted as differences in the composition of the candidate pool.

[25] The bandwidth for all models is the same (h=4106.0, b=6554.5) and was obtained by averaging the MSE-optimal bandwidths for the 6 separate models.

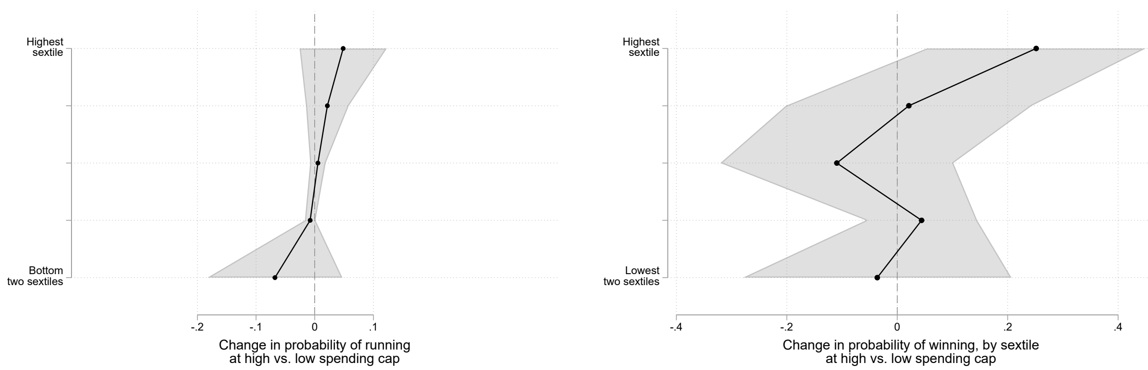
[26] Each model is estimated on a different subset. The bandwidth for all models is the same (h=4248.5, b=6793.2) and was obtained by averaging the MSE-optimal bandwidths for the 6 separate models.

in districts with a greater compared to a lower increase in the spending cap per voter per seat.[27] Unlike in Brazil, we do not find conclusive evidence that greater spending pressures deter the less wealthy candidates or advantage the wealthier ones.[28] The point estimate is positive for the higher sextiles, and negative for the lowest sextile, but not statistically distinguishable from zero.

On the other hand, in panel (b), which shows the impact of greater spending pressures on winning conditional on running, we do observe an advantage for the very wealthy: they are 25 percentage points, or three times more likely, to win in districts with greater spending pressures. We see effects of nowhere close this magnitude for other sextiles, suggesting that it is the very wealthy who reap unique benefits. Therefore, in Chile, it appears that the main channel is the increased electoral performance of wealthy candidates rather than candidate entry.

Figure A30: Effect of Higher Campaign Spending Limit on Candidate Pool and Election Success in Chile

(a) Running **(b)** Winning | Running



Why the difference between Brazil and Chile? While the definite answer is beyond the scope of our current study, we sketched a conjecture in the paper, which we discuss in more detail here. Our intuition is that an important difference between Brazil and Chile is that in the latter, parties play a much more important role, including in campaign funding. Figure A31 illustrates this, by showing the composition of campaign receipts in the two countries, broken into own resources, individual donations, and party transfers.[29] As seen in the top panel, a very small part of the receipts in Brazil come from the party, in terms of share (panel a) and amounts (panel b). By contrast, the bottom panel shows that party transfers are a much more significant source of funding in Chile in both relative (panel c) and absolute terms (panel d).

Greater party transfers to candidates may help ease the financing pressures for the less wealthy candidates, thus ameliorating the advantages enjoyed by the very wealthy. That said, we expect this counter-balancing effect to be only partially effective, because: (a) wealthier candidates should be able to recover some of their advantage under greater financing pressures by increasing the amount of self-financing; (b) parties always aim to maximize winning seats, and since greater financing pressures advantage the very wealthy, such pressures should make it hard for parties to credibly commit to *not* support the wealthier candidates to at least some degree. Combined,

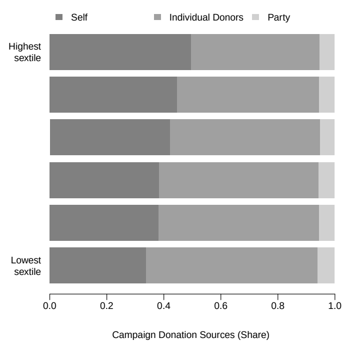
[27] We show the lowest two sextiles combined because many candidates in this group have zero assets declared, and thus do not uniquely belong to one sextile or the other.

[28] There are fewer candidates in districts with greater spending pressures, but those missing candidates seem to be similarly distributed across all wealth sextiles.

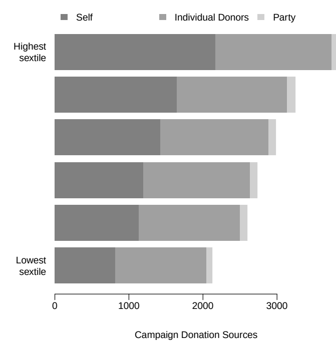
[29] These are the three key financing sources in Brazil. In Chile, another important component is the reimbursements from the state, which account for between 20-35 percent of candidates' receipts. We omit them here to make the descriptives comparable with Brazil, where such funds do not exist.

Figure A31: Campaign Receipt Sources by Wealth Sextile in Brazil and Chile

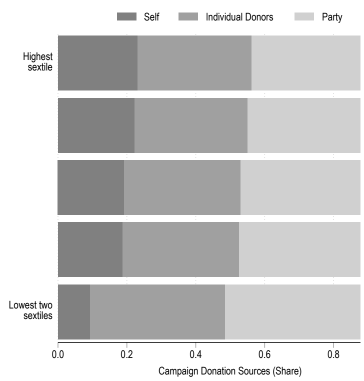
(a) Brazil: Share



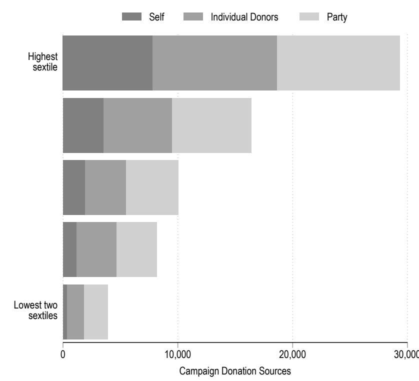
(b) Brazil: Amount



(c) Chile: Share



(d) Chile: Amount



these two factors would suggest that greater party transfers may lessen the potential deterring effect of greater financing pressures on the less wealthy, but not eliminate the resource advantages by the very wealthy. The differences in the effects of spending pressures on candidate entry and electoral performance by sextile in Brazil and Chile in Figures A29 and A30 are consistent with these conjectures.

References

Anderson, Michael L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association* 103(484):1481–1495.

Austin, Reginald and Maja Tjernström. 2003. "Funding of Political Parties and Election Campaigns." International Institute for Democracy and Electoral Assistance, Stockholm, Sweden.

Barber, Michael J., Brandice Canes-Wrone and Sharece Thrower. 2017. "Ideologically Sophisticated Donors: Which Candidates do Individual Contributors Finance?" *American Journal of Political Science* 61(2):271–288.

Benhabib, Jess and Alberto Bisin. 2018. "Skewed Wealth Distributions: Theory and Empirics." *Journal of Economic Literature* 56(4):1261–1291.

Blackwell, Matthew and Michael P. Olson. 2022. "Reducing model misspecification and bias in the estimation of interactions." *Political Analysis* 30(4):495–514.

Bormann, Nils-Christian and Matt Golder. 2013. "Democratic Electoral Systems Around the World, 1946–2011 (Version 3.0)." *Electoral Studies* 32:360–369.

Cattaneo, Matias D., Michael Jansson and Xinwei Ma. 2018. "Manipulation Testing Based on Density Discontinuity." *The Stata Journal* 18(1):234–261.

Chancel, Lucas, Thomas Piketty, Emmanuel Saez and Gabriel Zucman. 2021. "World Inequality Report 2022." World Inequality Lab, wir2022.wid.world.

Cinelli, Carlos and Chad Hazlett. 2020. "Making sense of sensitivity: Extending omitted variable bias." *Journal of the Royal Statistical Society Series B: Statistical Methodology* 82(1):39–67.

Coppedge, Michael et al. 2021. "V-Dem [Country-Year/Country-Date] Dataset v11.1." Varieties of Democracy Project, https://doi.org/10.23696/vdemds21.

Cruz, Cesi, Philip Keefer and Carlos Scartascini. 2021. "The Database of Political Institutions." Inter-American Development Bank, http://dx.doi.org/10.18235/0003049.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-de Silanes and Andrei Shleifer. 2010. "Disclosure by Politicians." *American Economic Journal: Applied Economics* 2(2):179–209.

Eggers, Andrew and Marko Klašnja. 2020. "Wealth, Fundraising, and Voting in the U.S. Congress." Manuscript, Georgetown University.

Fulguera, Elin, Samuel Jones and Magnus Ohman. 2014. "Funding of Political Parties and Election Campaigns: A Handbook on Political Finance." International Institute for Democracy and Electoral Assistance, Strömsborg, Sweden.

Gallop, Max and Simon Weschle. 2019. "Assessing the Impact of Non-random Measurement Error on Inference: A Sensitivity Analysis Approach." *Political Science Research and Methods* 7(2):367–384.

Gamboa, Ricardo and Mauricio Morales. 2016. "Chile's 2015 Electoral Reform: Changing the Rules of the Game." *Latin American Politics and Society* 58(4):126–144.

Gerring, John, Erzen Oncel, Kevin Morrison and Daniel Pemstein. 2019. "Who Rules the World? a Portrait of the Global Leadership Class." *Perspectives on Politics* 17(4):1079–1097.

Hainmueller, Jens, Jonathan Mummolo and Yiqing Xu. 2019. "How much should we trust estimates from multiplicative interaction models? Simple tools to improve empirical practice." *Political Analysis* 27(2):163–192.

IDEA. 2018. "Political Finance Database." International Institute for Democracy and Electoral Assistance, Stockholm, Sweden, https://www.idea.int/data-tools/data/political-finance-database.

Inter-Parliamentary Union. 2023. "Parline: Global Data on National Parliaments." https://data.ipu.org/.

Kitschelt, Herbert. 2013. *Democratic Accountability and Linkages Project.* Duke University. http://sites.duke.edu/democracylinkage/: .

Lührmann et al. 2021. "Varieties of Party Identity and Organization (V-Party) Dataset V1." Varieties of Democracy (V-Dem) Project, https://doi.org/10.23696/vpartydsv1.

Nyrup, Jacob and Stuart Bramwell. 2020. "Who Governs? a New Global Dataset on Members of Cabinets." *American Political Science Review* 114(4):1366–1374.

Piscopo, Jennifer M., Magda Hinojosa, Gwynn Thomas and Peter M. Siavelis. 2022. "Follow the Money: Gender, Incumbency, and Campaign Funding in Chile." *Comparative Political Studies* 55(2):222–253.

Rossi, Ivana M., Laura Pop and Tammar Berger. 2016. *Getting the Full Picture on Public Officials: A How-to Guide for Effective Financial Disclosure.* Washington DC: The World Bank.

Teorell, Jan, Aksel Sundström, Soren Holmberg, Bo Rothstein, Natalia Alvarado Pachon and Cem Mert Dalli. 2021. "The Quality of Government Standard Dataset." Version Jan21, University of Gothenburg: The Quality of Government Institute, http://www.qog.pol.gu.sedoi:10.18157/qogstdjan21.

World Bank. 2023. "World Development Indicators." The World Bank Group, http://wdi.worldbank.org.