ESSAYS ON FINANCE AND POLITICAL INSTITUTIONS

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"2023"

This text is dedicated to my family, friends, and mentors. Despite being in the PhD program for the last five-and-a-half years, I have, in reality, been pursuing this degree for the last ten years. I could not have persevered through this process without you.

ABSTRACT

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Political institutions and policies significantly influence financial markets. This dissertation presents three essays exploring the relationship between finance, politics, and macroeconomics using the tools of academic finance.

The first essay finds that democratizations negatively impact asset valuations due to increased redistribution risk. Analyzing 90 countries over 200 years, risk premia during democratizations are elevated, comparable in magnitude to financial crises. Using a shift in Catholic Church doctrine supporting democracy, I establish causal evidence linking democratizations to elevated risk premia. Successful democratizations result in redistribution, public sector growth, reduced income inequality, and increased labor share of income. A model where wealthy asset market participants face redistribution risk during democratizations quantitatively explains these findings and the negligible response to autocratizations.

The second essay shows that foreign lobbying shapes US government spending and public policy. Introducing a dataset of 180,000 meetings between foreign agents and US legislators from 2000 to 2018, we find that meetings correlate with legislator lawmaking effectiveness and foreign affairs committee membership. Foreign agents maintain connections with legislators even after leaving important committees. Around these meetings, foreign countries benefit from increased financial aid and favorable tariffs, while foreign firms with more lobbying gain larger subsidies and US government contracts. We observe benefits for legislators, but no evidence of constituents punishing them for meeting foreign representatives.

The third essay reevaluates recent findings of increased US wealth inequality, which exclude the value of social insurance programs like Social Security. Including Social Security retirement benefits, we find no increase in top wealth shares over the last three decades, irrespective of potential tax and benefit changes. Discounting at the risk-free rate, real Social Security wealth rose from $4.9 trillion in 1989 to $52.6 trillion in 2019. Adjusting for long-run macroeconomic risk, the increase remains substantial, from over $4.0 trillion in 1989 to $41.2 trillion in 2019. By 2019, Social Security wealth represents 59% of the wealth of the bottom 90% of the wealth distribution.

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CHAPTER 1

Who values democracy?

1.1. Introduction

Over half of all countries have transitioned to democracy over the past 200 years. When successful, democratizations bring large changes to political institutions such as voting rights, fair elections, and rights to free expression. They also bring redistribution, more transparent institutions, and greater economic competition (Boix, 2003; Aghion et al., 2014; Acemoglu et al., 2015). These changes affect firms and investors alike. How do financial markets respond when democratization becomes more likely? Using a panel dataset that covers 90 countries over 200 years, I show that asset valuations fall when permanent transitions to democracy are more likely. This is because investors demand a higher risk premium due to the risk of large-scale redistribution of political and economic power. In the data, I document that this increase is similar in magnitude to what we observe in financial crises, suggesting that these periods are associated with increased systematic risk to investors.

What drives this large increase in risk premia? One possible explanation would be an increase in macroeconomic risk. However, this is not born out in the data. GDP or aggregate dividend growth do not fall in the 5 years after a democratization starts, nor do the distributions of GDP or aggregate consumption growth visibly change. A rise in generic political risk cannot fully explain the results either. Other periods of high political risk such as political crises and autocratizations—transitions from democracy to autocracy—exhibit substantially smaller increases in risk premia when compared to democratizations.

My paper proposes an alternative mechanism: redistribution risk. First, in the data, I compare successful and failed democratizations and find evidence that transitions to democracy come with substantial redistribution. After a successful transition, the size of the public sector grows, income inequality falls, and the labor share of income rises. Second, I propose a model of democratic transitions embedded within a standard asset pricing model to understand if this redistribution is quantitatively large enough to explain the asset pricing results. When calibrated to reasonable parameters from the asset pricing literature and to match the redistribution in the data, the model can explain approximately 60% of the rise in dividend yields observed during democratizations.

In the first part of the paper, I document that democratizations come with increased risk premia across several proxies. Of course, democratizations do not occur randomly and could be endogenous to economic conditions. Nonetheless, I show democratizations increase risk premia using two identification strategies. The first strategy uses exogenous variation in the probability of a successful democratization emanating from a shift in Catholic church doctrine in favor of democracy during the papacy of John XXIII from 1959 to 1963. This shift particularly impacted majority Catholic autocracies. Huntington (1991) labels the shift as one of the main reasons the third wave of democratization of the 1970s, 1980s, and early 1990s occurred and why it was concentrated in majority Catholic autocracies. Consistent with this narrative, I show that indices denoting the threat to the governing regime posed by civil society organizations and the size and frequency of democratic protests rose dramatically in majority Catholic autocracies compared to non-Catholic autocracies. This indicates that the doctrinal shift materially changed political realities on the ground in majority Catholic autocracies.

Using a difference-in-differences approach, this quasi-natural experiment is associated with a 4.9 to 11.1 percentage point increase in average excess returns for majority Catholic autocracies depending on the specification. The results display no pre-trends and are robust to various sample windows, the exclusion of outliers, and different estimation techniques. These results cement the link between an increase in risk premia and an increase in the probability of a successful democratic transition.

The second identification strategy follows Acemoglu et al. (2019) and uses regional waves of democratization as exogenous variation in the likelihood of a successful democratization. Democratizations that occur in the middle of regional waves are more likely to be driven by external pressure and are therefore exogenous to the local macroeconomic conditions of a particular country. These democratizations see a larger increase in dividend yields consistent with endogeneity biasing the results downward.

The second part of the paper investigates the mechanism driving the increase in risk premia: a rise in redistribution risk. Comparing successful and failed democratizations, I find that democratization redistributes resources in two ways. First, it increases explicit redistribution by raising the size of the public sector and lowering income inequality. In total, these results point to, on average, a 4.8 percentage point rise in the size of the public sector, a 2.3 percentage point decline in Gini coefficients, and a 6.7 percentage point rise in the labor share of GDP in the 20 years after a successful democratization.[1] Second, it also increases tacit redistribution. For example, autocracies allocate a greater share of government spending to elites (Tullock, 1986). They also provide more protection to incumbent firms from new entrants (Martinez-Bravo and Wantchekon, 2021). I find that, during successful democratizations, bribery and corruption indices fall while pro-competitive regulation and net entry of new firms rise. Since this also redistributes resources away from autocratic elites, it could also play an important role in the asset pricing results.

To understand whether the redistribution in the data is quantitatively large enough to explain the asset pricing results, I calibrate a model of democratic transitions in the style of Acemoglu and Robinson (2006) embedded within a standard asset pricing framework. The economy starts in autocracy in which the elites are the only financial market participants, have all the political power, and try to avoid redistributing their income to the more numerous poor citizens. The citizens influence the policies of the elites by threatening to revolt. Revolution is costly: all the elites are killed and a fraction of resources are destroyed, making it an undesirable state for both sides. When the fraction of resources destroyed is low enough, however, the citizens may prefer the revolution to their lives in autocracy. This cost the citizens bear from revolution, along with the revolutionary threat the elites face, varies over time. In equilibrium, the revolution never occurs because the elites institute temporary income redistribution through taxes and transfers.

However, when the revolutionary threat is great enough, temporary transfers are no longer enough. Faced with the threat of revolution, the elites would like to promise future redistribution, but such

[1] These results are also consistent with previous findings by Acemoglu et al. (2015), who find that tax revenues-to-GDP ratios rise by as much as 25 percent on average after democratizations, and Drautzburg et al. (2022), who find that the capital share falls by 2.3 percentage points in the three years after a democratic transition.

promises are not credible. The elites face a commitment problem: as long as they retain political power they cannot credibly commit to future transfers in the state of the world where there is little or no revolutionary threat. In this case, only conceding democracy can keep the revolution off the equilibrium path, as democracy acts as a mechanism for the elites to credibly commit to future redistribution. While democracy is a much better state for the elites than the revolution, the redistribution it brings is costly, making it, nonetheless, a deleterious state for the elites.

Democratization, in the model, is a state where a permanent transition to democracy becomes more likely. Since the elites price assets, uncertainty over whether a democratization will succeed—ushering democracy and redistribution—or fail—keeping society in autocracy—increases the risk to elite's future consumption, causing risk premia to rise. In this way, the consolidation of democracy and the redistribution of income and political power it brings, acts as a rare and disastrous outcome for the elites, explaining the increased risk premia observed during democratizations in the data (Rietz, 1988; Barro, 2006; Gabaix, 2012; Wachter, 2013). When calibrated to reasonable preference parameters and the explicit redistribution observed in the data, the model can explain 60% of the rise in dividend yields observed during democratizations. Several extensions to the model are then proposed to better understand the role that tacit redistribution plays in explaining the remaining portion.

Finally, the model can also explain the negligible asset pricing effect observed in autocratizations. In this extension, the elites have the ability to attempt to reverse democracy, but face a permanent loss of a fraction of their consumption if they fail. The key insight is that while democratization is a risk imposed on the elites, autocratization is a risk taken by the elites, which leads to an asymmetric effect on asset prices. At the point where the Elites are exactly indifferent between attempting an autocratization and accepting democracy, the consumption-wealth ratio is identical across states. The dividend yield is a levered claim to consumption, however, and increases slightly as the increased risk in the event of a failed autocratization matters more than the higher payoff upon success. This also leads autocratizations with a higher potential payoff to come with a larger rise in dividend yields, as the elites endogenously take more risk to achieve autocracy.

Taken together, these results provide a new mechanism for why risk premia vary over time: politically motivated redistribution shocks that primarily affect wealthy asset market participants. Spikes in risk premia coming through this channel give insight into the barriers that countries face during the democratization process, particularly true for developing and autocratic societies where economic and political inequalities are far greater. Better understanding these frictions is important for the many countries still living under autocratic political institutions.

These results are also relevant for the remaining 55% of democratic countries. Over the last four decades, the developed world has experienced a sharp rise in pre-tax income and financial wealth inequality (Auten and Splinter, 2019b; Smith et al., 2020b). The evidence from democratizations suggests that in curtailing wealth and income inequality through redistribution, countries may face elevated risk in the short run.

Finally, the results also speak to the financial market effects of declines in democratic institutions across the globe over the last decade. Insofar as the move away from democracy is accompanied by lower taxes, higher inequality, lower labor bargaining power, and decreased economic competition, the results in this paper provide a model through which future autocratic movements can be interpreted.

Related Literature This paper advances both the asset pricing literature focused on rare events and political and policy risk, and the political economy literature around the democratization process.

In disaster models, investors are exposed to large negative shocks that manifest with some small, usually time-varying, probability (Rietz, 1988; Barro, 2006; Gabaix, 2012; Wachter, 2013). Investors demand compensation for holding assets exposed to these disasters, allowing these models to match key asset pricing moments. My paper adds to this literature by noting that large political risks like democratizations can come with left-skewed distributional shocks—from the perspective of wealthy market participants—which also drive asset prices.

An alternative view is offered by models where aggregate shocks affect investors differently, often through their uninsurable labor income or human capital. (Mankiw, 1986; Constantinides and Duffie, 1996; Danthine and Donaldson, 2002; Constantinides and Ghosh, 2017; Schmidt, 2016; Paron, 2021). This leads these investors to demand compensation for holding stocks allowing these models to match the level, volatility, and cross-section of asset prices. However, to generate quantitatively important asset pricing effects, these shocks need to most strongly affect the wealthy capital holders (Catherine, 2022). This is the case during democratizations, as the shocks to inequality, tax policy, or political connections they bring mainly affect the wealthy.

This paper also builds on a literature examining the role of political and policy risk in asset pricing by noting that democratizations are accompanied by large increases in risk premia. Pástor and Veronesi (2012, 2013) propose a model in which government policy uncertainty drive variation in the risk premium. Pástor and Veronesi (2016) model the effect of redistributive taxation on inequality jointly with the effect on aggregate productivity and asset prices. Pástor and Veronesi (2021) examines how rising consumption inequality can influence to move toward populism even in a strong economy in a model in which agents are inequality averse. My paper builds on these papers by studying redistribution shocks explicitly in the context of democratizations and studying their quantitative impact on asset prices.

Empirical research on policy shocks and uncertainty has focused mostly on quantifying the affects of policy shocks in developed democracies. For example, Baker et al. (2016) develop an index of economic policy uncertainty and find that increases in this index are associated with greater stock price volatility and reduced investment and employment. Kelly et al. (2016) provide empirical support that political uncertainty is priced in the equity options market. Manela and Moreira (2017) show that variation in a text-based measure of macroeconomic and policy uncertainty co-moves with risk premia, lending credence to rare disasters theories. Their measure of policy uncertainty also predicts future tax changes in the United States. My paper differs from these by studying uncertainty over political institutions rather than over particular policy decisions. As such my work complements this body of research, showing that uncertainty over the institutions is also priced in financial markets.

My primary contributions to the political economy and democratization literature, are twofold. The first is theoretical: By adding asset prices to the seminal model in Acemoglu and Robinson (2006), this paper shows that increases in risk premia are consistent with increases in the redistribution risk faced by autocratic elites during periods of democratization. This provides a prediction through which redistributive models of democratizations can be tested (Boix, 2003; Acemoglu and Robinson, 2006). Moreover, by providing a theoretical link between asset prices and future redistribution in the event of a successful democratization, the model can also assess whether the redistribution observed in the data is quantitatively large enough to explain the rise in premia. In this way, the model can help clear a significant hurdle in this literature, namely, provide an understanding of whether the redistribution faced by the wealthy in autocracy is large enough to constitute a substantial friction to democratic transitions.

The second is empirical; the paper provides the first evidence of the effects of democratizations on equity markets. Prior research examining the asset pricing impact of democratizations has focused on the impact on sovereign debt yields in the pre-World War I sample. Consistent with my results, it has found that democratizations increase sovereign loan yields (Dasgupta and Ziblatt, 2021; Tunçer and Weller, 2022). Conversely, Delis et al. (2020) study corporate loan spreads from 1984–2014 and find that successful transitions to democracy are accompanied by reduced loan spreads for companies. These positive effects after transitions are not inconsistent with increased risk during the transition period, which this paper documents. My paper builds on this body of research by studying equity markets and providing the longest time series and widest panel to date to study the asset pricing impact of democratizations.

In addition to new empirical evidence on asset prices, the paper also provides a novel exercise to quantify the amount of redistribution after successful democratizations by comparing them to failed democratizations. As such, the paper compares two groups of countries that underwent a similar period of political change, but where one group experiences a sustained change and the other does not. These results, therefore, add to those reported in Acemoglu et al. (2015) and Drautzburg et al. (2022) who measure the impact of democracy on the size of the public sector and the labor share of income.

1.2. Data

The empirical analyses performed below rely on several databases. Here, I describe the data employed in this project and the construction of all the variables of interest.

Asset market data Equity data come from Global Financial Data (GFD), the Jorda-Schularik-Taylor Macrohistory Database (JST) used in Jorda et al. (2019), IBES Global, and Factset. GFD provides two main historical stock return indices for each country: the first is the aggregate return on stock exchanges within the country, and the second is the aggregate return of all companies headquartered in the country and listed on the London Stock Exchange. The JST data covers 17 developed countries from 1870 to 2016. To aggregate the data, I fill in the GFD home stock market series first with the JST data and then with IBES Global, Factset, and the GFD data coming from the London Stock Exchange. This is done for all equity rate variables, such as rates of return or dividend growth, or changes in level variables like changes in dividend yields. Combining these data sources yields an unbalanced panel data set of ex- and cum-dividend returns, dividend yields, and dividend growth that cover the longest time series possible for each country; for example, the data on changes in dividend yields span 201 years across 90 countries, with an average of 65 years of data per country. However, due to different coverage for each series, the observation counts differ throughout the paper. For more information on how the asset pricing series are constructed, see Appendix A.1, and for the series mnemonics and sources for all of the data used for each country, see the Supplemental Appendix.

Focusing primarily on stock market data is not an innocuous choice. Since many countries that democratize do not have stock markets, this choice introduces some selection over which events will be analyzed. The most prominent example of the kinds of democratizations that are excluded is the fall of the Soviet Union. Since the Soviet Union did not have stock markets, I cannot examine the behavior of stock prices or valuation ratios during this episode. This is true more generally of transitions from left-wing authoritarian regimes to democracy.

Political institutions data Data on political institutions come from the Varieties of Democracy (V-Dem) database.[2] V-Dem uses a team of over 3,500 country-specific experts to quantify levels of and trends in historical political institutions for every country over the last two centuries, providing the most detailed dataset possible to analyze changes in political institutions. In particular, V-Dem constructs indices ranking the level of electoral democratic institutions in every country, which is important for mapping data to theory, as changes in electoral institutions map most closely to the changes in institutions that occur in the model presented in Section 1.6. V-Dem also provides measures on various political outcomes not immediately related to democratic political institutions, but nonetheless useful for understanding the democratization process, such as the level and frequency of democratic protests, political violence, political polarization, and civil society activity. For more information on the construction of these measures see Coppedge et al. (2020).

Macroeconomic, government spending, and inequality data Real GDP are from Maddison Historical Statistics, who use and expand upon data from Barro and Ursua (2008) and provide the most comprehensive data available on these variables. Data on real consumption, the labor share of income, investment, the capital stock, and human capital are available from 1945 to the present from the Penn World Tables. Data on income inequality come from the Standardized World Income Inequality Database (SWIID) who provide data on the Gini coefficient for up to 155 countries from 1961–2018. Finally, data on government revenue-GDP ratios come from GFD and data on tax revenue-GDP ratios come from the Relative Political Capacity Dataset.

Events data A variety of events data are also collected and primarily used as controls in the regressions below to assure that any variation the proxies for risk premia observed in democratizations are not driven by adverse macroeconomic, political, or financial events. Data on financial crises come from JST and Reinhart and Rogoff (2009) and are combined into a single financial crisis variable.[3] Data on sovereign defaults from Reinhart and Rogoff (2009) and data on recessions come from the GFD Dates database, which compiles events throughout history on various topics. Data on the years in which wars occur and their location come from the Correlates of War (CoW) data.

[2] This paper uses version 10.0 of the data.

[3] These datasets use a narrative approach in constructing historical financial crises, often looking at large-scale bank runs or asset market failures, and are used extensively by other scholars.

From the CoW dataset, I also obtain country-level data on religious demographics from 1945–2010. Data on political crises come from the International Crisis Behavior (ICB) database as used in Berkman et al. (2011). Data on head of government and head of state deaths come from V-Dem and are supplemented with data from Wikipedia. Data on head of government and head of state attempted and successful assassination attempts come from Jones and Olken (2009).

1.2.1. Democratizations

Data on democratization periods primarily come from the Episodes of Regime Transformation (ERT)[4] data produced by V-Dem. This newly created dataset is the only dataset to my knowledge that provides full coverage of democratization (and autocratization) episodes. Since asset prices are forward looking, it is important to use data on democratizations that provide coverage over the full episode and not just the year transition. Importantly, the ERT data locates the start year of the democratization even if it is prior to the actual year of regime change. The ERT data locates democratic transitions by changes in V-Dem's electoral democracy index, which measures countries on a continuous scale from 0 to 1 according to how closely they resemble the principle of electoral democracy. This is measured by assessing to what extent the government respects principles of freedom of expression and association, the proportion of the population that can vote, and whether elections are competitive, clean, and fair.

Since the asset pricing data are available prior to 1900, I extend the ERT data to back to 1816, using the same procedure V-Dem uses to construct the post-1900 sample. This produces 10 additional democratization episodes. Finally, to obtain the latest possible end date for each democratization episode, I use data from Lindberg et al. (2018) to extend democratization episodes to their latest possible year.[5] In total, these data constitute 851 democratization years across 85 episodes from 1816–2018 for which I have dividend yield data.

Moreover, and in contrast to other sources, the ERT and Lindberg et al. data also provide detail on whether a democratization is sustained or quickly reverts back to autocracy. For simplicity, I

[4]This paper uses version 2.2 of the data.

[5]Lindberg et al. (2018) follows a similar procedure to the ERT data, but with less conservative conditions on what constitutes the end of a democratization episode.

refer to these two different potential outcomes as either "success" or "failure." These data allow me to assess the different outcomes for countries with successful versus failed democratizations.

More information on the process of how the ERT data identifies democratizations and determines if they are successful or failed is described in detail in Appendix A.1. Examples of successful and failed democratizations can be found in Appendix A.6, which provides two case studies of democratization episodes that go through the history, asset pricing response, and subsequent redistribution (or lack thereof) of the successful democratization in Sweden from 1917–1924 and the failed democratization in France from 1847–1848. Further, a list and event timeline of all democratizations used for the asset pricing results is provided in Appendix Table A.22.

1.3. Democratizations and risk premia

This section presents the stylized fact that risk premia rise during democratizations evidenced by a rise in dividend yields and other proxies for the change in the risk premium. I then present evidence that neither macroeconomic risk nor generic political risk are driving this result, and that potential selection effects seem to, if anything, bias the results downward.

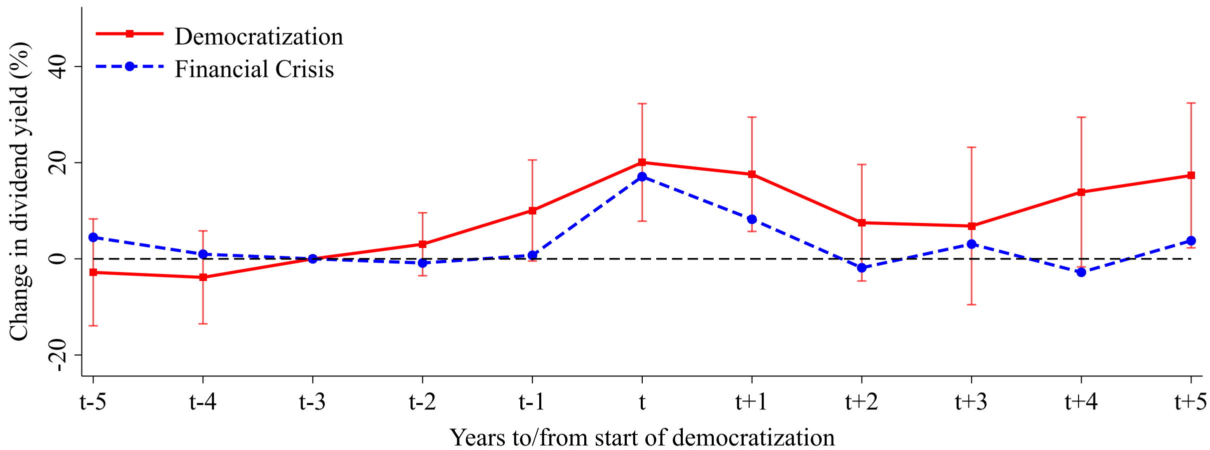
1.3.1. Risk premia during democratizations

Following Muir (2017), I use the change in the log dividend yield as a proxy for the change in the equity premium. The change in the dividend yield corresponds to the percent change in the discount rate (risk premium plus the riskfree rate), provided there are no changes to expected cash flows.

Fluctuations in expected cashflows could either increase or decrease estimates of the risk premium derived from changes in the dividend yield. If dividends fall but investors expect them to rebound quickly, the change in log dividend yield could mask an increase in risk premia. Permanent shocks to expected cash flow growth, conversely, would increase the dividend yield independent of a higher risk premium. To ensure the former is not a concern, I omit democratizations that start in sovereign defaults or in countries engaged in a war on their own continent. As discussed in Appendix A.2.1, democratizations that begin during nearby wars or sovereign defaults are connected with substantial dividend declines that rapidly recover. To assure the latter is not a concern, I provide evidence

Figure 1.1: Change in log dividend yields in democratizations

This figure presents an event study of log dividend yields around democratization and a financial crisis start years. Estimates are relative to the value three years prior to the event start to allow for the possibility that financial markets incorporate information about the events early. Endpoints (not shown) are binned. The red bars on the democratization line represents a 90% confidence interval of the point estimates with standard errors clustered by country and year.



below that the remaining democratizations are not associated with large effects on expected cash flow growth in the next subsection.

The magnitude of the shift in dividend yields is nicely summarized by Figure 1.1, which presents an event study of log dividend yields around democratization starts benchmarked to their value 3 years prior. Using the value at $t-3$ as the comparison value allows for the possibility that financial markets react to democratizations prior to when political scientists code the events as started. This seems to be the case as dividend yields begin to rise starting 2 years prior to the start of the democratization.

To show the economic magnitude of the increase in dividend yields, the point estimates for an event study around financial crises—which Muir (2017) notes are accompanied by large increases in risk premia—are also plotted. The headline result is clear: democratizations come with large and economically significant increases in dividend yields, of the same order of magnitude as what are observed in financial crises. Moreover, we can see that dividend yields remain elevated as far as

Table 1.1: Democratizations and changes in log dividend yields

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of a democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The same results for financial crises are included for purpose of comparison. In Columns (3) and (4) some observations are lost due to there only being one observation in a region-year or in a continent-regime-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields			
	(1)	(2)	(3)	(4)
Democratization start	17.95***	19.76***	23.12***	26.32***
	(5.74)	(5.78)	(6.89)	(8.10)
Financial crisis start	13.71***	16.27***	10.43*	11.46*
	(5.11)	(4.29)	(5.51)	(6.40)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	64	64	63	60
R^2	0.01	0.15	0.35	0.34
Observations	6,040	6,040	5,663	5,813

five-years after the democratization start.

Table 1.1 breaks the result down further, showing the 5-year change in log dividend yields at the start of democratizations. Column (1) presents the results: dividend yields rise substantially in the five-years leading up to a democratization. This means that, in the absence of changes to expected cashflows, the results can be interpreted as an average rise in discount rates of 17.9% over 5 years. To assure other events beside democratizations are not driving the results, all regressions include indicators for other events that may impact dividend yields such as financial crises, recessions, interstate wars, head of government deaths, sovereign defaults, coups d'etat, and attempted assassinations, and the level of military activation of a country. Further, to account for the fact that shocks to dividend yields within a year are correlated across countries, and that changes in dividend yields are correlated over time within a country, standard errors are clustered at the country and year level in all columns.

However, countries that experience democratizations may also see larger average changes in dividend yields in general, and democratizations may occur in years in which many countries experience a rise in dividend yields. For this reason, country and year fixed effects are included in Column (2). The results are essentially unchanged: the point estimates imply an average increase in risk premia of 19.8% increase in dividend yields over 5 years and remain significant at the 1% level.

Column (3) introduces more specificity by adding geopolitical region-by-year fixed effects where the region designation is defined as in Teorell et al. (2022). Adding these fixed effects compares the rise in dividend yields observed in the democratizing country to their regional neighbors, assuring that the results are not driven by common regional increases in dividend yields. Similarly, Column (4) adds lagged regime type-by-continent-by-year fixed effects, which compares the rise in dividend yields in the democratizing country to those in same continent and with the same regime type in the previous year (e.g. autocracy or democracy). The previous year regime type is used because the regime type sometimes changes at the start of the democratization. Both of these specifications yield similar results with dividend yields rising by 23.1% and 26.3%, respectively, with both significant at the 1% level.

Robustness Appendix A.2 provides various robustness checks on the rise in dividend yields at the beginning of democratizations. First, Appendix A.2.2 shows that the results hold across a broad range of other methods for determining a democratization start. The section first presents various methods of determining a jump in electoral democratic political institutions using the V-Dem data, and shows that 5 different methodologies yield very similar results. Of note is that observation that a broad class of democratic jumps show marked increases in dividend yields, though of smaller magnitudes than what are observed in the ERT data.

The section also presents the results for the democratic transitions used in Acemoglu et al. (2019), who combine Polity and Freedom House data with other sources to ascertain democratic transitions. These data cover the period 1960–2010. To extend the data to cover my entire asset pricing sample, I use consensus regime transitions between Polity and V-Dem from 1816–1959 following a similar procedure to Acemoglu et al. which is detailed in Appendix A.2.2. This procedure produces 32 democratization events for which asset pricing data are available and the results are quantitatively similar to those shown in Table 1.1.

Appendix A.2.3 presents alternative estimators for the change in log dividend yields and Appendix A.2.4 alternative standard errors. Table A.5 presents shorter differences in dividend yields such as the 1-year, 2-year, 3-year, and 4-year change in the log dividend yield and the results still point to a strong and statistically significant increase in dividend yields. Table A.6 also presents evidence that the level of dividend yields is significantly elevated not just relative to prior values of the dividend yield for that country, but relative to all country-year observations in the sample, as well. To account for the possibility that dividend yields reach their high point after the democratization start, Table A.7 presents results using to "peak-to-trough" style measures, one that subtracts the maximum dividend yield in 3 years prior to the democratization start or the 5 years after the democratization start from the minimum dividend yield from 4 to 8 years prior to the democratization start, and one that takes the maximum 5 year change in log dividend yields from two years prior to two years after the democratization start. Both measures provide a similar conclusion as those shown above. Finally, Figure A.1 shows that the increase in dividend yields in democratizations are almost entirely driven by price declines, in particular, a 23.0% decline over 5 years at the trough.

In the results presented above, all standard errors are clustered at the country and year level. However, clustering in this way may not fully account for the autocorrelation structure induced by overlapping observations by using the 5-year changes in log dividend yields. To assure the results are robust, Appendix A.2.4 also presents the results using Driscoll-Kraay standard errors, clustered at the year level. The results are still significant at the 1% level.

Additional evidence that other proxies for the change in the risk premium are elevated are presented in the remaining sections of Appendix A.2. First, vector-autoregression decomposed discount rate shocks, using methods from Campbell (1991), indicate that discount rate shocks are focused around democratization starts, with a cumulative shock of around 3.7–5.8 percentage points, as shown in Appendix A.2.5. Second, Section A.2.6 shows that an additional measure of market risk, equity volatility, is elevated at the start of democratizations, with an event study plot showing that it reaches it's peak 5-years after the democratizations start. Third, Appendix A.2.7 shows that corporate bonds yields are also significantly elevated—increasing by 10.9–20.0% over 5-years—during democratizations in the 11 democratization episodes for which I have data.

1.3.2. Ruling out macroeconomic risk and general political risk

The previous section presents robust evidence that dividend yields are elevated during periods of democratization which I attribute to an increase in risk premia due to democratizations. This interpretation has a couple of potential issues. First, increased dividend yields could be driven by lower expectations of cashflow growth. Second, other sources of risk that are potentially correlated with democratizations, such as macroeconomic risk or general political risk, could be driving the results. As such, it is important to rule out these potential channels as explanations for the results shown above, a task upon which I endeavor in this section.

Macroeconomic risk As mentioned above, the change in the log dividend yield is a valid proxy for the change in the risk premium if expected growth remains relatively constant. Table 1.2 presents evidence that this is the case. Columns (1)-(3) show that growth in log GDP per capita is flat in the 5 years after the start of a democratization.[6] Once again, to compare economic magnitudes, the effect in the 5 years after the start of a financial crises on log GDP per capita is also reported. In the specifications with either no fixed effects or country and year fixed effects growth is significantly negative during and after financial crises. When country fixed effects and region-year fixed effects are included, the effect size is substantially reduced and statistically indistinguishable from 0.

Realized dividend growth in democratizations does see a negative point estimate, but none are statistically different than 0, as shown in Columns (4)-(6). Realized dividend growth is, conversely, significantly negative during and after financial crises in all specifications. Overall, the results in Table 1.2 show that the change in the log dividend yield is a valid proxy for the change in the risk premium in the case of democratizations.

[6] Prior work has noted that democratizations tend to arrive around periods of low growth (Acemoglu et al., 2008; Bruckner and Ciccone, 2011; Acemoglu et al., 2019). This is almost entirely driven by democratizations that occur in countries engaged in a war on their own continent or that have defaulted on their external debt, which I have excluded here to keep these results comparable with the results on dividend yields.

Table 1.2: Democratizations, growth, and cash flows

This table presents regressions of log GDP per capita and dividend growth on indicator variables denoting if the year is in the first 5 years of a democratization. The regressions estimated take the form

$$\text{Cash Flow Growth}_{c,t} = \alpha + \beta \mathbb{1}_{c,t}\{\text{First 5 years of Democratization}\} + \epsilon_{c,t}$$

where α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The same results for financial crises are included for purpose of comparison. In Columns (3) and (6) some observations are lost due to there only being one observation in a region-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Log GDP per capita growth			Log dividend growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization, 5-years	0.12	0.04	0.10	-1.62	-1.35	0.58
	(0.31)	(0.30)	(0.31)	(1.70)	(1.68)	(1.47)
Financial Crisis, 5-years	-0.77***	-0.70***	-0.22	-6.20***	-7.34***	-6.29***
	(0.25)	(0.23)	(0.21)	(1.85)	(1.62)	(1.71)
Country FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No
Region × Year FE	No	No	Yes	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	1210	1210	1205	359	359	354
R^2	0.02	0.11	0.23	0.01	0.13	0.30
Observations	18,222	18,222	18,017	6,051	6,051	5,563

The increase in risk premia combined with no significant effects on average realized cash flows presents challenges for theories based on declines in aggregate consumption to explain these facts. Indeed, as shown in Table 1.2, the entire distribution of log GDP growth and log consumption growth is either shifted toward higher growth during democratizations or unaffected, as shown in Figure 1.2. It does not seem as though an increase in aggregate macroeconomic risk can explain the higher risk premia observed in democratizations.

Political risk While democratizations come with an increase in political risk, a natural question is whether the increased dividend yields observed in democratizations are *solely* a consequence of political risk. This is akin to asking if similar increases in dividend yields are observed in other events where political risk is high. To test this, I examine whether ICB political crises[7] and autocratizations in the ERT data display similarly large increases in dividend yields, as both these events display high levels of political risk, but without the same potential for a transition to democracy.

[7] In these regressions, I code ICB political crises that occur during democratization episodes as a 0 to more clearly identify the affect of general political risk on dividend yields.

Figure 1.2: Distribution of GDP and consumption in democratizations

Log GDP and consumption growth are winsorized at the 0.25% and 99.75% level. GDP data come from the Maddison Historical Statistics database. Consumption data come from the Penn World Tables and represent the period from 1945 to 2018.

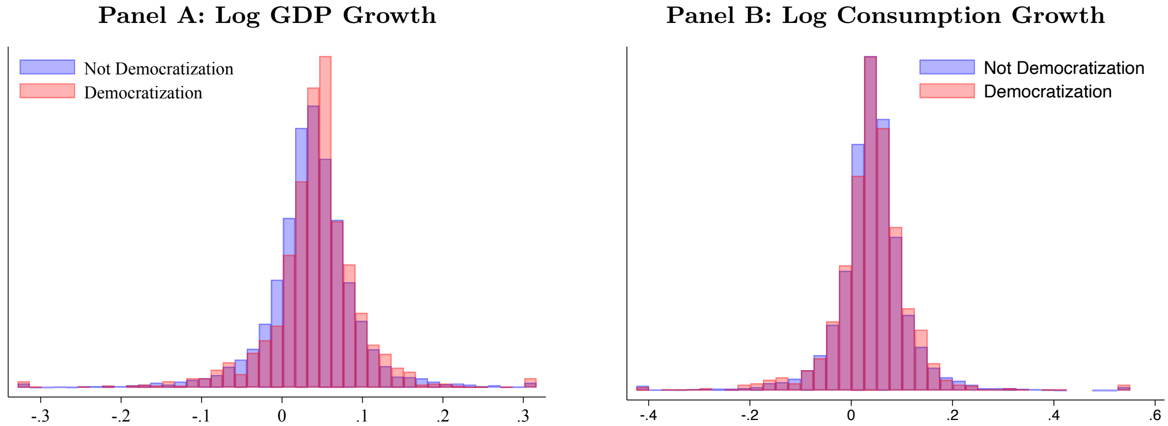


Table 1.3 presents the results for the 5-year change in the log-dividend yield at the beginning of these two episode alongside estimates for democratizations from the period 1918 the present, the period over which the ICB crisis data are available. In all specifications, both ICB political crises and autocratizations display dividend yield changes in the same direction as in democratizations, but democratizations display a larger rise in dividend yields. To assess whether these point estimates are statistically different, Table 1.3 also reports the p-values for two F-tests performed under the null hypothesis that the coefficients for democratizations and ICB crisis and democratizations and autocratizations are identical. These tests reveal that once regional-by-year variation in dividend yields are taken into account, democratizations display statistically larger increases in dividend yields than either event. This signifies that there is indeed something special about democratizations above and beyond a generic increase in political risk.

Robustness Appendix A.2.8 presents robustness checks on the results relating to macroeconomic risk. First, the results above on GDP per capita growth use all data available from the Maddison Historical Statistics. However, one may wonder if the results are similar in the subset of countries with data on dividend yields, which is more directly comparable to the results in Section 1.3.1. Table A.11 shows that the results are indeed similar in this subsample.

Table 1.3: General political risk and dividend yields

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of a democratization, International Crisis Behavior (ICB) political crisis, and autocratization. Data are reported from 1918 when the ICB crisis data begin. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Event Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. In Columns (3) and (4) some observations are lost due to there only being one observation in a region-year or in a continent-regime-year. The bottom of table presents the p-value of two F-tests testing the null hypothesis that the change in dividend yields in democratizations is the same as in ICB crises or autocratizations. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields			
	(1)	(2)	(3)	(4)
Democratization start	20.42***	26.62***	29.80***	32.52***
	(7.19)	(6.70)	(8.01)	(9.64)
ICB crisis start	14.39	11.58	11.52	5.35
	(9.47)	(9.18)	(7.47)	(6.95)
Autocratization start	7.93	7.82	2.77	2.60
	(7.88)	(7.38)	(7.63)	(8.00)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	48	48	47	46
Democratization vs ICB Crisis (p-value)	.583	.135	.068	.012
Democratization vs Autocratization (p-value)	.249	.055	.021	.02
R^2	0.02	0.17	0.37	0.35
Observations	4,112	4,112	3,959	4,047

Second, the section presents event study plots for both GDP per capita and dividend growth. GDP per capita declines slightly in the three years leading up to a democratization, but is not statistically different than a zero effect. Moreover, most of the decline in GDP per capita seems to be driven by a decline in investment. Indeed, investment-capital ratios decline significantly at the start of democratizations, consistent with an increase in risk premia, shown in Table A.14. Dividend growth, conversely, turns slightly negative 3-years after a democratization start, but none of the point estimates are statistically significant.

Third, Table A.12 reports the longer run effects on average GDP growth and dividend growth in the 10 years after a democratization start, which shows the results are quite similar to those reported in the 5-year window.

Fourth, Figure A.4 shows the distribution of GDP growth and consumption growth both inside and outside of financial crises as a comparison to the democratizations results. Financial crises, unlike democratizations, come with an increase in left tail outcomes in both GDP and consumption growth.

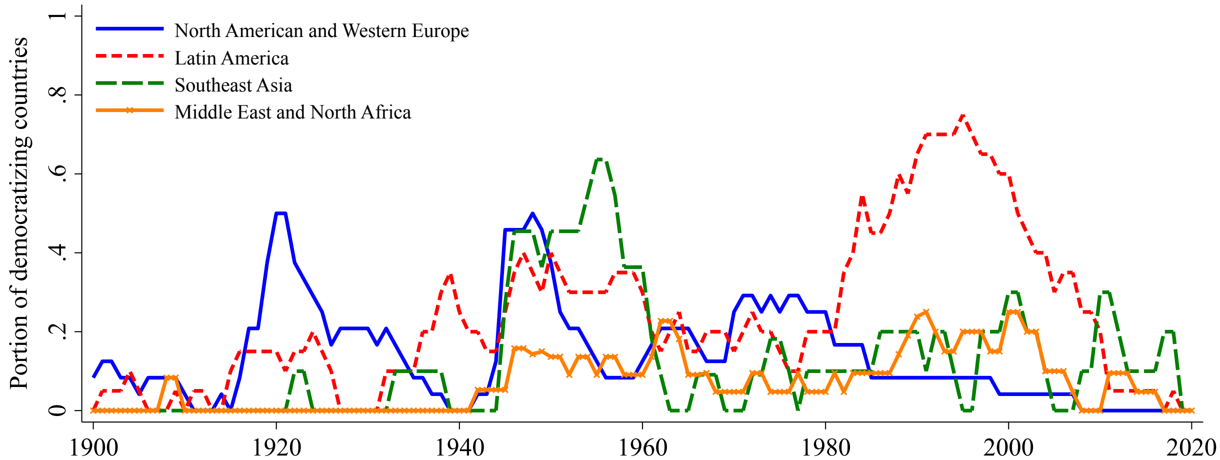
Finally, another potential concern is that, even though aggregate macroeconomic outcomes are not substantially different during democratizations, exiting foreign capital could be driving the equity market results. To this end, Figure A.5 presents an event study of net foreign direct investment (FDI) scaled by GDP coming from the World Bank from 1970–2018. The figure shows that FDI is not substantially changed in a 10 year window around a democratization start.

Appendix A.2.9 reports robustness on the political risk results. Table A.13 reports the change in dividend yields during democratization years that coincide with ICB political crises, finding that the ICB crises that occur during democratizations display 25.7% larger change in dividend yields over 5-years with the result significant at the 5% level. This indicates that the large effect of democratizations on risk premia is present within the sample of ICB crises.

One potential concern is that uncertainty, in general, is elevated during democratizations which increases risk premia. While I argue below that uncertainty over future redistribution is driving the results, one could think that other types of uncertainty are at play. That said similar effects are not seen in other times of high uncertainty—such as political crises and autocratizations—and realized macroeconomic outcomes using 200 years of data do not indicate a change in the distribution of consumption. One additional test that provides evidence in favor of the redistribution channel is the differences in behavior between physical capital and human capital. In a world where the wealthy own physical capital, whereas human capital is the predominant asset of the middle and lower classes, democratizations would act as “bad” news for physical capitalists and “good” news for human capitalists. This would cause a divergence in investment between the two series. Conversely, a rise in aggregate uncertainty should lead the two series to go in the same direction. Just such

Figure 1.3: Regional waves of democratization

This figure shows the proportion of countries undergoing a democratization in 4 regions according to the Episodes of



a divergence is seen in the data, and is shown in Appendix A.2.10. The divergence of investment between human and physical investment, therefore, provides solid evidence that there is substantial uncertainty for one group, but not another, consistent with the redistribution-based theories.

Additional evidence against other sources of uncertainty explaining the results is presented in Appendix A.2.11. Table A.15 reports the probability that adverse macroeconomic and financial events occur in democratizations. It finds that these events are no more likely to occur in democratizations than in other times.

1.3.3. Regional variation in democratizations

One potential concern is that democratizations are at least partially endogenous. Since only the democratizations that emerge in equilibrium are observed, this introduces some degree of selection bias. Moreover, it is not clear in which direction selection bias could go. If the autocratic elite, for whom democratizations are harmful, are more influential in choosing the timing, then democratic transitions will happen when the cost is lowest for them. If these wealthy Elites own a disproportionate share of the assets, then the observed equilibrium effect would understate the true effect. Conversely, if the general citizenry are more influential in choosing the timing, they too would choose a timing to maximize benefits they receive, the cost of which would be born disproportionately by

Table 1.4: Regional waves of democratizations and dividend yields

This table presents regressions of the 5-year change in log dividend yields on indicator variables represented the start of a regional or non-regional democratization. Regional democratizations are defined as democratizations that occur in countries with an above the median proportion of countries in their region undergoing a democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. In Columns (3) and (4) some observations are lost due to there only being one observation in a region-year or in a continent-regime-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields			
	(1)	(2)	(3)	(4)
Regional wave democratization start	20.60^{**}	29.70^{***}	29.56^{***}	36.07^{**}
	(10.09)	(9.13)	(10.40)	(14.13)
Non-regional wave democratization start	16.01^{*}	12.74	18.56^{**}	19.37^{**}
	(9.23)	(8.09)	(9.22)	(8.99)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	64	64	63	60
R^2	0.01	0.15	0.35	0.34
Observations	6,040	6,040	5,663	5,813

the autocratic elites. As such, if they get to choose the timing of the democratic transition the costs will be high. This could bias the estimates of the change in the risk premium in either direction.

As pointed out by Huntington (1991), democratizations generally occur in regional waves. Notably, this means that democratizations that occur in a country's nearby neighbors, could make them more likely to democratize. As Acemoglu et al. (2019) and Delis et al. (2020) point out, democratizations that occur in regional ways, therefore, are more likely to be unrelated to long-run country-specific macroeconomic, political, and cultural conditions.

In this spirit, to better understand the direction of potential selection effect, I examine the number of ongoing democratizations divided by the total number of countries in a given region, and separate the democratization start indicators equally using this quantity.[8] The time series of portion of countries

[8] The median democratization start occurs in a region where 20% of all countries are undergoing a democratization, compared to a median of 8.7% for countries not starting a democratization

undergoing a democratization is shown in Figure 1.3 for selected regions. The various waves of democratization are immediately apparent, with the first two occurring around the two world wars and the last starting in the late 1970s. Also apparent is that the waves of democratization occurred asynchronously: the regions shown here democratized at different times. Identification comes from the idea that these regional waves are likely orthogonal to country-specific macroeconomic and political conditions and less likely to be chosen by any interest group within a country. This allows for an estimate of the treatment effect of democratization on asset prices purged of these confounds and selection effects.

Table 1.4 present these results. Democratizations that occur in regional waves are accompanied by larger point estimates than those that occur outside regional waves. In Columns (2) and (4), the point estimates observed in regional waves democratizations is nearly double those that occur outside of regional waves.[9] This provides some evidence that the selection bias of democratizations in the data is more likely to bias estimates of the treatment effect of democratizations on risk premia downwards, as countries will generally choose to democratize when they incur the lowest cost. Regional democratizations, however, come as more of a surprise and are therefore accompanied by a larger increases in risk premia.

1.4. DID Estimates: John XXIII and the Second Vatican Council

The previous section presented evidence that risk premia are elevated during democratizations. Moreover, it also suggested that neither an increase in macroeconomic risk nor general political risk can explain the results. However, there are still some outstanding questions. First, democratization episodes and the probability of their success are likely endogenous. In particular, democratizations are both more likely to happen if their costs to the incumbent autocrats are low and if the benefits to the would be democrats are high. As such, some evidence coming from an exogenous shift in the probability of a democratization would go a long way to supporting up the results outlined above.

Moreover, the results above rely on changes in dividend yields rather than looking at average excess returns to ascertain changes in risk premia, the latter being more common in the asset

[9]These coefficient estimates are, however, not statistically different according to an F-test.

pricing literature and a more direct proxy for the risk premium. The reason for this is that the democratization events do not last long enough to get an accurate picture of risk premia after the initial shock using average excess returns. Long sample periods are needed to obtain risk premia from average excess returns (Merton, 1980), and the discount rate shocks at the start of democratizations would bias the results downward. An ideal experiment would increase the probability of a successful democratic transition outside of a democratization event, and leave this probability elevated long enough to accurately measure an effect.

This section addresses these challenges by using a quasi-natural experiment of a doctrinal shift in favor of democracy by the Catholic church in the early 1960s as an exogenous shock to the probability that democracy consolidates for majority Catholic autocracies. It then examines returns before and after the doctrinal shift in majority Catholic autocracies relative to other countries in a difference-in-differences setting.

Historical context For much of its history the Catholic church was widely considered as a barrier to the consolidation of democracy. For example, Hook (1940) writes of the Catholic church, "Catholicism is the oldest and greatest totalitarian movement in history." Similarly, Blanshard (1949) writes "You cannot find in the entire literature of Catholicism a single unequivocal endorsement by any Pope of democracy as a superior form of government." This arrangement changed in October 1958 with the election of Cardinal Angelo Giuseppe Roncalli to the papacy. Donning the name John XXIII—history recalls him as *il Papa buono*—not much was expected of the old Pope, who was nearing 77 years old when he began his pontificate. He shocked the world, however, when he called for a major review of Catholic church doctrine on January 25th, 1959, less than 90 days into his papacy (Alberigo, 2005). This review became the Second Vatican Council (Vatican-II), which began in 1962 and lasted into 1965, outlasting John XXIII, who died in June 1963 of stomach cancer.[10]

Before he passed, John XXIII made the doctrinal shift official with his publication of *Pacem in Ter-*

[10] Vatican-II was a fitting follow-up to the First Vatican Council in which the Catholic church condemned liberal democracy.

ris, the first text in the history of the Catholic church to explicitly endorse democracy.[11] Sigmund (1987) marks *Pacem in Terris* as the beginning of the decisive shift in Catholic church policy in support of liberal democracy, and, according to Huntington (1991), the publication of *Pacem in Terris*, and Vatican-II which succeeded it, is one of the main reasons the third wave of democracy—which took place from the mid-1970s to the 1990s—occurred. Huntington also surmises this is the reason the Democracy's Third Wave began with majority Catholic autocracies. After 1963, the Catholic church played an active role in opposing authoritarian regimes in places like Argentina, Brazil, Chile, the Philippines, Poland, Spain, and many Central American countries, actively working as an advocate of democracy (Huntington, 1991; Fukuyama, 1992).

The announcement of Vatican-II and the doctrinal shift it brought about was likely not foreseen by investors and constitutes a plausibly exogenous shock to the probability a successful democratization occurs in majority Catholic autocracies. What evidence is there that majority Catholic and autocratic countries were primarily affected by this shock? First, most of the countries that underwent successful democratizations (according to the Lindberg et al. data) from 1964 to 1983 were majority Catholic: In 1963, 25% of autocracies were majority Catholic, yet these countries made up 55% of all successful democratizations over the next 20 years.[12]

Second, majority Catholic autocracies began to face more pressure from their citizens during the papacy of John XXIII. Civil society organizations (CSOs) became a much larger threat to autocratic regimes in majority Catholic countries, compared to non-Catholic autocracies, during the period of 1959–1963, as shown in the event study plot shown in Panel A of Figure 1.4. Moreover, we see a large increase in anti-regime CSO activity during the years of treatment from 1959–1963, whereas, prior to 1959 the trend was identical in the two groups of countries. Such increases in anti-regime

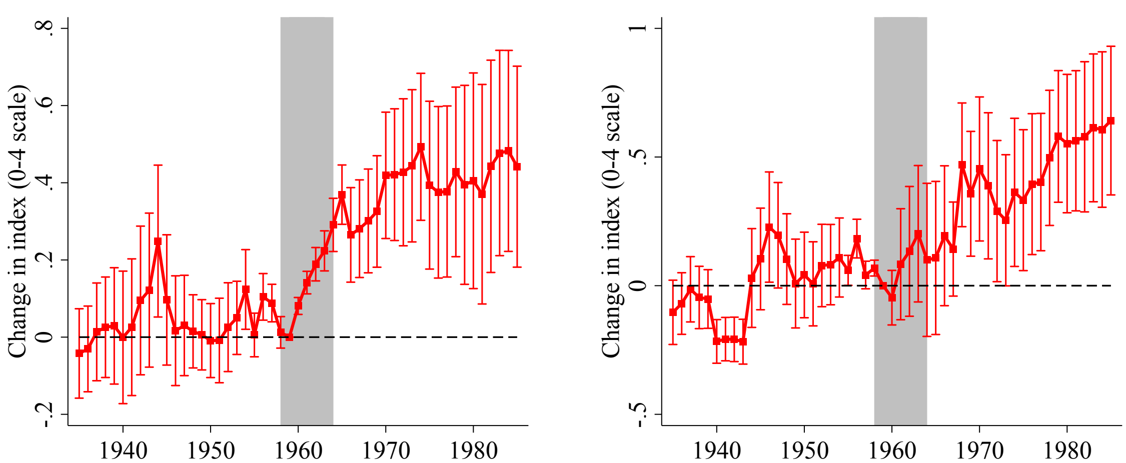
[11] In particular, *Pacem in Terris* says "[...] the dignity of the human person involves the right to take an active part in public affairs and to contribute one's part to the common good of the citizens. [...] The human person is also entitled to the juridical protection of his rights." This support is followed up with support for democracy explicitly in Point 52: "The fact that authority comes from God does not mean that men have no power to choose those who are to rule the State, or to decide upon the type of government they want, and determine the procedure and limitations of rulers in the exercise of their authority. Hence the above teaching is consonant with any genuinely democratic form of government."

[12] This is based on successful democratization *starts* meaning that the democratizations that were initiated during this time were were more likely to be successful.

Figure 1.4: Anti-regime civil society organization activity and democratic mobilizations

This figure presents an event study comparing majority Catholic autocracies to non-Catholic autocracies in their anti-regime civil society organization (CSO) activity and frequency of democratic mobilizations and protests as determined by indices from the V-Dem database. The reference year is set to 1959, the first year of the doctrinal shift. Endpoints are binned and are not shown. The anti-regime CSO activity index ranks the threat posed by anti-regime civil society organizations on a scale of 0 to 4, where 0 is no anti-regime civil society organization activity, and 4 is a major present threat to the governing regime from anti-regime civil society organizations. The democratic mobilization index assesses the number of small- and large-scale demonstrations in favor of democracy in a given year with a maximum value of 4. The autocracy designation is also constructed from V-Dem data, and includes all closed or electoral autocracies from their "regimes of the world" variable. Data on the percentage of the population that is Catholic comes from the Correlates of War database. These data are extended backward using the first year of data. The vertical grey bars show the treatment window from 1959–1963.

Panel A: Anti-regime CSO activity **Panel B: Democratic mobilizations**



CSO activity are important as this activity is a strong predictor of future democratizations, as shown in Figure A.7.

Third, democratic mobilizations, small and large scale protests in favor of democracy, became significantly more prevalent in majority Catholic autocracies compared to non-Catholic autocracies, as shown in Panel B of Figure 1.4. From 1935–1958, democratic demonstrations were similar across autocracies. After 1959 and *Pacem in Terris*, the tight grip of autocracy began to loosen in the majority Catholic countries and by 1985, large-scale democratic protests were far more commonplace.

Identifying the treatment window First, it is important to specify the treatment window, which I take to be the period from 1959 to 1963, from the unexpected announcement of Vatican-II to the official announcement of the doctrinal shift with the publication of *Pacem in Terris*, as the years of treatment. The first reason for choosing a range of years, instead of a single event year, is that in this window there are many events which signaled the doctrinal shift. Since financial markets are forward looking, information about the doctrinal shift was likely incorporated into asset prices prior to the official announcement in 1963.

The earliest candidate for a treatment year is 1959 when Pope John XXIII unexpectedly called Vatican-II to update Church doctrine on a variety of topics. It is hard to understate how surprising this decision was, even to high ranking Church officials. For example, Alberigo (2005) quotes interviews from Cardinals at the time who were purportedly unaware Vatican-II would be called. The surprise of the Cardinals also indicates that the decision to elect John XXIII to the papacy was unlikely driven by a desire for liberalization within the College of Cardinals.

Moreover, Alberigo (2005) makes quite clear that John XXIII was aware of the geopolitical environment in which he was operating, speculating that the rising tensions of the Cold War were front and center in the Vatican in 1959. Amid "great darkness," John XXIII sought to bring about "times of renewal" in the Church (Alberigo, 2005).[13]

Vatican-II convened from late 1962 to 1965, but the direct announcements regarding democracy were focused in 1963 and toward the end of Vatican-II in 1965. However, the interim period after the announcement of Vatican-II but before it convened was filled with announcements that signaled change. For example, John XXIII's 1961 writing, *Mater et Magistra*, singles out economic and political inequalities on a number of occasions.[14] Further, a structural break test[15] on the

[13]A *Macleans* article from June 20th, 1959 points that the turn toward democracy was suspected once Vatican-II was called. The article notes John XXIII's support for party competition in Itay, implying a more tolerant attitude toward left-wing parties and notes an almost immediate change in culture toward one of more free and fair expression (Neville, 1959).

[14]In particular "Among citizens of the same political community there is often a marked degree of economic and social inequality. [...] Where this situation obtains, justice and equity demand that public authority try to eliminate or reduce such imbalances. It should ensure that the less developed areas receive such essential public services as their circumstances require, in order to bring the standard of living in these areas into line with the national average. Furthermore, a suitable economic and social policy must be devised which will take into account the supply of labor, the drift of population, wages, taxes, credit, and the investing of money, especially in expanding industries. In short, it should be a policy designed to promote useful employment, enterprising initiative, and the exploitation of local resources."

[15]To obtain a single time series to test, the majority Catholic autocracy average is subtracted from the non-Catholic autocracy average for each variable to obtain a difference. Then, two structural break tests are run on each series on the period 1940–1989, a supremum Wald test and a supremum likelihood-ratio test. Each test indicates the

democratic mobilizations and anti-regime civil society organizations series, presented in Figure 1.4, indicates a change in trend around 1959 or 1962 in the majority Catholic autocracies, indicating that the political reality on the ground began to change before 1963. In short, while 1963 is the earliest year where I can be sure investors were aware of the doctrinal shift, the process was underway long before then.

The second reason for choosing a range of years as the treatment window comes from the nature of shocks to expected returns—positive discount rate shocks correspond with *negative* contemporaneous returns. As such, if the treatment window starts too late, the estimated treatment effect on risk premia will be biased upward, as the contemporaneous negative returns associated with discount rate shocks will be located in the pre-period, inflating the estimated treatment effect. Similarly, if the treatment window starts too early, the treatment effect will be biased downward.

Identifying assumptions Political institutions and religion are not randomly assigned; they are the result of myriad historical, economic, social, and cultural processes that mold society over centuries. The identifying assumption underlying this exercise, therefore, does *not* rely on random assignment of religious demographics or political institutions. Instead, it relies on the assumption that absent the doctrinal shift, the treated (majority Catholic and autocratic countries) and control (all other countries) groups would have experienced similar returns, conditional on the relevant controls and fixed effects. In essence, the parallel trends assumption must hold. Evidence in favor of parallel trends is provided below in the discussion of the results.

Additionally, to asses the dimensions along which the groups of countries differ, Table 1.5 shows the balance of characteristics in the pre-period (1946–1958). Majority Catholic autocracies tend to be poorer, have higher inflation, higher resource inequality, and lower debt-to-GDP ratios than the average country in the sample. However, they are much more closely matched to non-Catholic autocracies, where the only major differences lie in GDP per capita and GDP growth in the pre-period. To address this, I directly control for GDP per capita in all regressions and add country fixed effects, which capture differences in persistent variables like inequality, debt-GDP ratio, and

same break date on each series: 1959 for democratic mobilizations and 1962 for anti-system CSO activity. The test statistics represent a high degree of statistical significance ($p < 0.001$).

Table 1.5: Balance of characteristics, 1946–1958

This table shows various characteristics of each of the different types countries used in the difference-in-differences framework. In the first 3 columns, the group means are reported. Columns (4) and (6) reports the point estimates on the regression

$$\text{Outcome}_{c,t} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Majority Catholic Autocracy}\} + \epsilon_{c,t}$$

on either all countries or the subsample of autocracies. Columns (5) and (7) report the standard errors on those estimates, which are clustered by country. The coefficients on rate variables have been multiplied by 100. The risk adjustment procedure for returns uses a two-factor model as described below in Equation (1.4.2). ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Maj. Cath. Autocracy	**Non-Catholic Autocracy**	**Democracy**	**All Country Diff**	**S.E.**	**Autocracy Diff.**	**S.E.**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Excess Returns (%)	10.3	5.7	10.1	2.1	2.5	4.5	3.3
Risk-adjusted Returns (%)	-0.2	1.0	-1.7	0.3	2.1	-1.2	3.4
GDP Per Capita ($)	4,347	2,347	9,406	-2,131***	392	2,000**	767
Inflation (%)	18.5	7.1	5.3	12.5***	2.1	11.4*	6.1
Annual GDP per capita Growth (%)	3.4	1.3	3.4	11.4*	6.1	2.2**	0.9
Debt/GDP (%)	29.3	35.5	60.0	-22.5***	4.0	-6.2	11.8
Gini Coefficient	47.4	47.6	34.2	10.7***	2.2	-0.2	6.8
Resource Inequality Index	0.75	0.78	0.24	0.28***	0.03	-0.03	0.07
Dividend Growth (%)	10.7	6.4	8.4	2.8	5.2	4.3	7.5

resource inequality. Finally, I also estimate the difference-in-differences specification both using all other countries as the control group and only using non-Catholic autocracies as the control group.

Further, I treat the doctrinal shift in favor of democracy as exogenous, implicitly assuming away any reverse causality—in essence, identification by God. This essentially assumes that John XXIII did not take asset prices or the asset portfolio of the Catholic church into account when making decisions on religious doctrine. In reality, however, things are more complex. While the sources in the Catholic theology literature do not point to economic reasons as the basis for the doctrinal shift in favor of democracy, the change may, in part, be due to the relatively high growth rates of protestant democracies compared to majority Catholic autocracies. The timing of the decision to start changing church doctrine, however, does seem random, as does the date which the information was made public, meaning that a partially economic basis for the doctrinal shift would not affect the validity of the identification strategy.

Finally, I also assume that investors have access to the same riskfree investment, in particular, the return on the U.S. treasury bill. The main reason for this is that the return on government bonds for the countries in my sample are not risk free, and may be exposed to time varying risks that equity assets are not exposed to (Miller et al., 2020). Constructing excess returns using government bonds may, therefore, erase part of the risk premium or induce measurement error in dependent variable, reducing the statistical power of the results.

Specification To assess the treatment effect of the doctrinal shift on majority Catholic autocracies, I employ a difference-in-differences framework of the form

$$\text{Excess Returns}_{c,t} = \alpha_c + \alpha_t + \beta \mathbb{1}_{c,t}\{\text{Post} \times \text{Catholic} \times \text{Autocracy}\} + \omega \text{Controls}_{c,t} + \epsilon_{c,t} \quad (1.4.1)$$

where c represents each country and t each year, and I exclude the years of treatment, 1959 to 1963, from the regression. This compares the pre- and post-period change in average excess returns for treated majority Catholic autocracies to what was experienced by the other countries in sample, where β provides the treatment effect of interest. This specification is estimated on two different samples: either all countries or using autocracies only. Both estimations are informative of the effects of the doctrinal shift. The all countries subsample is informative of the average treatment effect on majority Catholic autocracies when compared to all other countries. Given the relatively large sample size in this estimation, it gives the chance for greater precision in estimation.

However, it also comes with drawbacks. As we saw in Table 1.5, majority Catholic autocracies are not well-matched on observable characteristics with democratic countries. As such, variation in global conditions that load more heavily on these differences could introduce noise into the estimation. As such, I also estimate the specification using non-Catholic autocracies as the control group, who are much better matched on observable characteristics than democratic countries, as shown in Table 1.5. This also allows me to assess whether majority Catholic autocracies were indeed the treated group, or if Vatican-II was just correlated with some other unobserved event that drove up average excess returns for all autocracies.

Controls and orthogonal risk-adjustment Each regression also includes a matrix of binary controls for macroeconomic and political events and continuous controls to better identify variation in risk premia due to democratization risk from realized returns. In particular, binary event controls for head of government deaths, financial crises, ICB political crises, wars, sovereign defaults, and recessions are included. Controls for the macroeconomic environment are also added, and include log-GDP growth and the level of log-GDP per capita. Each of these controls allow for a less attenuated estimate of the change in risk premia due to the doctrinal shift, as they absorb potential shocks unrelated to democratization risk that affect realized returns.

In addition to the above controls, I also adjust the average excess returns series for systematic risk that is not associated with risk coming from an increased probability of democratization. In particular, I implement a risk-adjustment which removes global and continental time-varying risk. This is done by estimating, on each country, the regression

$$R^e_{c,j,t} = \alpha_{c,t} + \beta_{1,t} R_t^{e,global} + \beta_{2,t,j} R_{j,t}^{e,continent} + \varepsilon_{c,j,t} \tag{1.4.2}$$

where $R_t^{e,global}$ denotes the total return in excess of the return on U.S. treasury bills on a GDP-weighted global market portfolio, $R_t^{e,continent}$ denotes the total return in excess of the return on U.S. treasury bills on a GDP-weighted region-specific market portfolio, and c denotes the country, j denotes the region,[16] and t denotes the year. The β's are estimated on a rolling basis over 10-years, and require a minimum of 5-years to be estimated.[17] The two-factor risk model has good explanatory power for returns in the cross-section of countries, with an average (median) coefficient of determination, or R^2, of 0.47 (0.47), and the unexpected returns for all groups of countries (i.e. non-Catholic autocracy, Catholic democracy, etc.) from 1946–1958 are insignificantly different than zero when standard errors are clustered at the country and year level.

Measuring risk premia Estimation is performed over two symmetric sample windows: the first from 1946–1976 and the second from 1939–1983. The first estimation window begins in 1946 to

[16]The regions used include: 1) South and Central America, 2) North America plus Europe, 3) Asia and Oceania (less the Middle East),4) Africa and the Middle East.

[17]The 5-year minimum requirement causes 12 observations to be lost.

make it such that the pre-period does not contain the Second World War in the sample. This means that the pre-period estimate for expected returns is based on a 13-year sample. However, as pointed out by Merton (1980), a long time horizon is needed to estimate expected returns on the stock market from a single time series. Estimating over a longer sample window and showing the results hold, mitigates this issue. This issue is also mitigated by two other methodological choices.

First, there are 43 countries in the sample, 9 of which are majority Catholic autocracies. This means that expected returns are averaged across a large group of countries, instead of just looking at individual time series. This is also the rationale for estimating the results on two potential control groups, all non-majority Catholic autocracies and all non-Catholic autocracies. While the later is likely a more viable control group, the former provides a more reliable estimate of risk premia. For this reason, both estimates are informative.

Second, the two-factor model described above removes two risk factors—global and continental risk—that would make the estimation of a country-specific increase in expected returns more difficult, as the effectively represent a form of measurement error in the dependent variable. Removing these risk factors allows for greater statistical power by removing orthogonal sources of variation. For these reasons, the methods I am employing are likely to pick up differences in risk premia due to democratization risk despite the somewhat short time series of the sample.

Results The results for the difference-in-differences estimation are shown in Table 1.6. Given the global and regional factor model performs quite well in the pre-period—in that no countries display significant abnormal returns—Panel A presents the results without fixed effects. Columns (1) and (2) show the results using all countries that are not majority Catholic autocracies as the control group. Results are shown over two symmetric samples around the treatment period: one from 1946–1976, which avoids WWII in the estimation, and one from 1939–1983 which allows for a longer estimation period, but includes WWII.[18] Both specifications exclude the treatment years, 1959 to 1963. These columns indicate a 4.9 to 5.7 percentage point rise in global and regional risk-adjusted average excess returns after the doctrinal shift in favor of democracy. These estimates

[18]Avoiding WWII and using 1946–1983 for the second sample period yields very similar results.

are in line with the estimates implied by Section 1.3 and are statistically significant at the 5% level. Columns (3) and (4) provide the estimates using all autocracies as the control group and find very similar results—approximately a 8.5 to 8.8 percentage point treatment effect. This is reassuring as it indicates that majority Catholic autocracies are indeed differentially treated by the doctrinal shift compared to other autocracies in the same time period. Moreover, it also indicates the lack of balanced characteristics between majority Catholic autocracies and democracies do not seem to play a large role in altering the points estimates.

Panel B presents the results for the more standard static difference-in-differences specification by adding two-way fixed effects.[19] Once again, Columns (1) and (2) show the results using all countries that are not majority Catholic autocracies as the control group and indicate slightly larger treatment effects, 6.2 to 10.5 percentage points, than in the previous panel. This is primarily due to the lower returns earned by majority Catholic autocracies in the post-war years relative to other countries. All estimates in these columns are statistically significant at the 1% level. Finally, Columns (3) and (4) provide the estimates using all autocracies as the control group, finding a 9.9 to 11.1 percentage point treatment effect.

To understand how these effects evolved over the sample, Figure 1.5 presents an event study plot on the autocratic subsample of a five-year moving average of global and continental risk-adjusted returns.[20] In the years prior to the doctrinal shift, the returns of majority Catholic autocracies closely correspond with the returns of other autocracies in the sample, with 0 falling within a 90% confidence interval in all estimates. From 1959 to 1963, however, majority Catholic autocracies earned much lower returns than their autocratic counterparts around the globe. These lower returns in this period are consistent with a discount rate shock occurring during the treatment period. This is reversed in the remainder of the sample, with majority Catholic autocracies earning higher global

[19] As pointed out in Goldsmith-Pinkham et al. (2022), the recent critiques around negative weights in linear regression do not apply in a static difference-in-differences setting, where by static I mean cases where treatment is not staggered. Since treatment occurs for all countries in 1959–1963, this is not a problem in my analysis and the β in Equation (1.4.1) can be thought of as a convex combination of potentially heterogeneous treatment effects on majority Catholic autocracies.

[20] As these are returns data, the unaveraged series presents substantial variation that make a pattern difficult to discern. A moving average, instead, makes these patterns clear.

Table 1.6: Difference-in-differences, 1959–1963 treatment window

This table shows the regression coefficients for the difference-in-differences specification in Equation (1.4.1) on two sample windows, one from 1946–1976 and the other from 1939–1983, and for two different samples, one for all countries and the other on autocracies only. In each regression, 1959 to 1963 are the years of treatment and are excluded. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100, and standard errors are in parentheses. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: No Fixed Effects

	All Countries		Autocracies Only	
	(1)	(2)	(3)	(4)
Majority Catholic Autocracy × Post	5.71**	4.94**	8.48**	8.80***
	(2.69)	(1.99)	(3.13)	(1.59)
Country FE	No	No	No	No
Year FE	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Sample	1946–1976	1939–1983	1946–1976	1939–1983
R^2	0.04	0.02	0.04	0.04
Observations	1,069	1,592	512	736

Panel B: Two-Way Fixed Effects

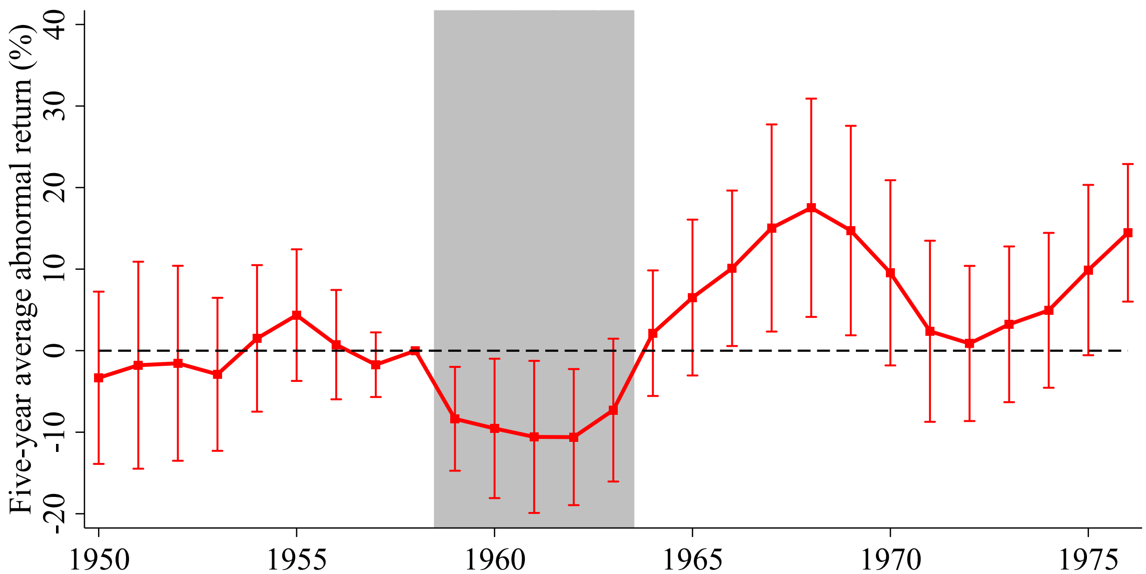
	All Countries		Autocracies Only	
	(1)	(2)	(3)	(4)
Majority Catholic Autocracy × Post	10.45***	6.20***	11.06***	9.89***
	(2.67)	(2.04)	(3.02)	(2.49)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	1946–1976	1939–1983	1946–1976	1939–1983
R^2	0.14	0.08	0.16	0.13
Observations	1,069	1,592	512	736

and regional risk-adjusted returns than other autocracies. Moreover, since the negative returns are focused in the treatment years, which are excluded, results do not seem to be biased by low realized returns in either the pre- or post-periods.

Robustness Appendix A.3 provides various additional robustness checks. Appendix A.3.2 provides a first falsification test for the results, estimating a difference-in-differences specification using the First Vatican Council (Vatican-I) from 1864–1870 as treatment. Vatican-I provides an interesting test, as this doctrinal meeting centered around a rejection of liberalism and democratic principals, and likely strengthened the hold on power of for autocrats in majority Catholic countries. Given the equity data from GFD are available well into the 1800s, I can estimate a similar exercise to the difference-in-difference specification above to understand the effect of Vatican-I. Consistent with the

Figure 1.5: Event study plot of global and continental risk-adjusted returns

This figure presents an event study plot of a five-year moving average of global and continental risk-adjusted returns estimated from the factor model given by Equation (1.4.2). The shaded bars represent the treatment period, 1959–1963. The red bars represent a 90% confidence interval with standard errors clustered by country and year.



results above, I find 4.9 to 5.5 percentage point lower average excess returns for majority Catholic autocracies in the 15 and 20 years after 1870. The results, however, are statistically insignificant likely due to there being fewer observations in the estimating samples. Appendix A.3.3 provides a second falsification test by shifting the window of treatment forward and backward from 1959–1963. The results would have only been significant in a narrow window around the years of the doctrinal shift.

Appendix A.3.4 reports the results using different end dates for the estimation windows, which also does not materially effect the results. The results are large and significant regardless of the particular end date chosen from 1970–1983, but the point estimates get smaller as the end date moves further in the future in the specification with all countries. In the autocracies subsample the point estimates are identical across end years, but only become statistically significant once enough observations are included to precisely pin down the treatment effect.

To assess whether any particular pair of countries are driving the results, Appendix A.3.5 reports the point estimates and t-statistics for the difference-in-differences specification excluding every pair of countries on both sample periods and in both sample groups. The results remain statistically significant across all specifications when any group of countries is excluded.

The results above point to a larger treatment effect than the estimates presented in Section 1.3. One potential explanation for this result is that the treatment effect of democratizations on risk premia is better identified using the doctrinal shift in the Catholic church, and it may very well be that the true effect from Section 1.3 is indeed larger. However, it is also possible that the estimates from the difference-in-differences exercise are less representative than the estimates from Section 1.3, as they cover a smaller time series and cross-section and may be driven by anomalously large observations. In this case, the true increase in risk premia may be smaller than the point estimates above suggest. To test to what extent this is the case, Appendix A.3.6 presents three different strategies for dealing with anomalously large observations: (1) winsorizing at the 5% and 10% levels, excluding the high return years from 1967–1969, and using outlier robust regression weights via Li (2006). In each specification the results are statistically significant, but speak to a smaller treatment effect of approximately 4–7 percentage points, in line with the results from Section 1.3.[21]

Finally, the factor model used to adjust average excess returns for time-varying global and continental risk could be absorbing some of the variation, especially for the majority Catholic autocracies in Latin America. To assure this is not driving the results, Appendix A.3.7 presents the results adjusting average excess returns for global risk only. The results without adjusting for continental risk, are similar to those without the risk adjustment except with larger point estimates and larger standard errors in some specifications.

1.5. Democracy and redistribution

The results outlined above indicate that democratizations are viewed as risky events by investors. However, we have yet to understand why this is the case. In this section, I propose a plausible

[21]The 4–7 percentage point treatment effect is also consistent with the 30-35% decline in prices seen in the treatment period.

mechanism: fear over future redistribution. Indeed, a popular and varied group of political science and political economy theories highlight the key role that inequality, class struggles, and redistribution play in democratizations. Moreover, this class of theories enjoy support in the data when comparing democracies and autocracies along several proxies for redistribution. Democracies, on average, have tax revenue-GDP ratios 7.8 percentage points higher, Gini coefficients that are 6.8 points lower, and labor shares of income that are 10.4 percentage points higher. These estimates are in line with the existing literature[22] who find that democracies tend to have a larger public sector, be more equal, have lower barriers to entry, and lower corruption and ability to divert rents from the government.

It is important to note, however, that the political economy literature is mixed on exactly whether and how democratizations cause redistribution. A comprehensive review of this literature comes from Acemoglu et al. (2015), where the authors show that, while tax revenues-to-GDP rise between 10 to 25 percent after democratizations, while there is no statistically robust effect on inequality. Additional evidence of redistribution can by found in Drautzburg et al. (2022) who note that the bargaining power of labor rises and the capital share of income declines after democratic transitions. They similarly find that autocratic coups reverse this, lowering the bargaining power of labor, raising the capital share of income. My paper pushes the literature forward to adding new evidence supporting the presence of redistribution after successful democratizations.

1.5.1. Evidence for redistribution after successful democratizations

This section seeks to establish a more robust link between democracy and redistribution by using a unique feature of the ERT data, namely that it distinguishes between successful and failed democratizations. The identification strategy relies on the idea that failed democratizations provide an appropriate counterfactual for successful democratizations after adding the relevant controls and fixed effects. More explicitly, the estimates can be interpreted as the treatment effect of democracy under the assumption that the success or failure of a democratization is as-if random.

To assess the effect of successful democratizations on the various outcomes of interest, I estimate

[22] A synthesis of this literature can be found in Boix (2003) and Acemoglu et al. (2015).

the following specification:

$$
\begin{aligned}
y_{c,t} = \alpha_t + \alpha_c + \beta_1 \text{Democratization}_{c,t} + \\
\beta_2 \text{Successful Democratization}_{c,t} + \beta_3 \text{Post-Democratization}_{c,t} + \\
\beta_4 \text{Post-Successful Democratization}_{c,t} + \omega' \text{Controls}_{c,t} + \varepsilon_{c,t} \quad (1.5.1)
\end{aligned}
$$

where $y_{c,t}$ is the outcome of interest. The post-democratizations variables in Equation (1.5.1) represent an indicator variable equal to 1 if the year-country observation is within twenty years of the end of a democratization or successful democratization, respectively. The remainder of the section presents the estimated effect of a successful democratizations on both *explicit redistribution*—increase in the size of the public sector, reductions in inequality, and/or increases in the labor share—and *tacit redistribution*—changes in the distribution of public goods and increased entrepreneurship and competitiveness. Explicit redistribution will feature in the baseline version of the model presented in Section 1.6, where the estimates from Equation (1.5.1) will be used in the model calibration. The tacit redistribution will feature in some of the extensions to the baseline model. One important caveat is that the variables used below do not necessarily cover the same sample of democratizations used to generate the asset pricing results. This is because, when possible, I use all available data to estimate the effect sizes, and the data on redistribution cover a much shorter time series but broader cross-section than those for asset prices.

Explicit redistribution As Acemoglu and Robinson (2006) and Boix (2003) suggest, democracies should come along with an expanded role of the government. Therefore, a natural first place to turn is to examine what happens to the size of the public sector after a successful democratization. Along these lines, Table 1.7 reports the results of the average year-over-year change in government revenue-GDP ratios and tax revenue-GDP ratios after successful democratizations relative to failed democratizations. Government revenue-GDP ratios and tax revenue-GDP ratios rise substantially in the 20 years after a successful democratization, increasing by 0.24 and 0.20 percentage points year-over-year, respectively. Accumulating the effects points to a 4.8 percentage rise in government revenue-GDP ratios and a 4 percentage point rise in tax revenue-GDP ratios. These estimates are right in line with those reported by Acemoglu et al. (2015), who find that government revenue-GDP ratios rise by 1.9 to 4.8 percentage points and tax revenue-GDP ratios by 2.4 to 4.1 percentage points after countries transition to democracy.[23]

In addition to the the size of the public sector, democracy may also have an effect on inequality through its impact on the bargaining power of labor and its creation new opportunities for the previously disenfranchised. Indeed, Drautzburg et al. (2022) finds that the capital share of income declines significantly after democratic transitions and rises after autocratic reversals. To assess whether this is the case after successful democratizations in my sample, Table 1.7 reports the results of the average year-over-year change in the Gini coefficient and the labor share of income for employees. The Gini coefficient declines significantly, by 0.11 percentage points year-over-year in the after 20 years after the end of a successful democratization, pointing to a cumulative decline of 2.3 percentage points. Similarly, the labor share of income for employees also increases significantly, by 0.34 percentage points year-over-year in the after 20-years after the end of a successful democratization, pointing to a cumulative increase of 6.7 percentage points. This effect is mostly driven by the large decline in the labor share observed after failed democratizations. These estimates are also in-line with those in prior literature: Acemoglu et al. (2015) finds similar declines in inequality in long-run, albeit without statistical significance. One reason for this difference is that my sample includes a substantially longer time series and more than twice as many events, allowing for a more precise estimation of the effect. The effect on the labor share of income for employees is in line with Drautzburg et al. (2022) who find a 2.3 percentage point increase in the labor share in the 3-years after a democratic transition.

Tacit redistribution Successful democratizations do not just come with higher taxes and transfers and lower inequality but also tacit redistribution—the loss of privileges for the elites currently benefiting from the autocratic system (Tullock, 1986). For example, these autocratic elites may lose their ability to influence the government via corruption and bribery. Moreover, autocratic forms of government allow for the more easy formation monopolistic and oligopolistic industrial organi-

[23]These number are achieved by taking the minimum and maximum long-run effect estimates and multiplying them by the sample average for autocracies.

Table 1.7: Successful democratizations and explicit redistribution

This table presents regressions of the year-over-year change in the government revenue-GDP ratio, tax revenue-GDP ratio, Gini coefficient, labor share of income from employee compensation on indicators denoting if a year is between 1 and 20 years after a democratization or successful democratization end. Regressions are specified as in Equation (1.5.1). The regressions also control for log GDP per capita and the lag of log GDP per capita. Country and year fixed effects are included in all regressions. Standard errors are clustered by country and reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Public Sector Size		Inequality and Labor Power	
	Δ Govt Rev/GDP	Δ Tax Rev/GDP	Δ Gini Coef	Δ Labor Share Emp
	(1)	(2)	(3)	(4)
Post-Successful Democratization (20-years)	0.24**	0.20**	-0.11***	0.34**
	(0.10)	(0.09)	(0.03)	(0.17)
Post-Democratization (20-years)	-0.11	-0.15	0.02	-0.31*
	(0.09)	(0.10)	(0.02)	(0.16)
Successful Democratization	0.08	-0.04	-0.02	0.41
	(0.16)	(0.12)	(0.04)	(0.29)
Democratization	0.13	0.07	0.02	-0.17
	(0.15)	(0.10)	(0.02)	(0.27)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Episode obs.	106	238	141	101
R^2	0.10	0.03	0.29	0.08
Observations	5,521	8,296	4,987	3,899

zations (Li and Resnick, 2003; Karolyi and Liao, 2017). As such, democratic transitions may bring increased competitive pressure to industrial incumbents from skillful entrepreneurs in addition to the loss of political connections (Fisman, 2001a).

To test the empirical relevance of tacit redistribution, I examine the effect that successful democratizations have on the public sector corruption and bribery indices provided by V-Dem. These indices examine to what extend corruption and bribery are tolerated within the federal government. As Table 1.8 shows, there is a substantial reduction in both corruption and bribery during successful democratizations relative to failed ones. As I have transformed these to both be 0-100 scales and given that a democratization lasts on average 8.5 years, this represents a 5.8 percentage point reduction in corruption and a 5.4 percentage point reduction in bribery. This provides evidence that the ability to seek rents seems to be reduced after successful democratic transitions.

Moreover, competitive pressure also increases during successful democratizations. Regulation favoring competition, as measured by the Economic Freedom Index from the Fraser Institute, rises

Table 1.8: Successful democratizations and tacit redistribution

This table presents regressions of the year-over-year change in the V-Dem corruption index, V-Dem bribery index, the Fraser Institute's Pro-Competitive Regulation Score, and the net entry of public firms on indicators denoting if a year is in a democratization or successful democratization. Regressions are specified as in Equation (1.5.1). Country and year fixed effects are included in all regressions. Standard errors are clustered by country and reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Rent Extraction		Competition and New Entry	
	Δ Corruption	Δ Bribery	Δ Pro-Comp. Regulation	Δ log(Firms)
	(1)	(2)	(3)	(4)
Successful Democratization	-0.68***	-0.63***	1.17**	3.98*
	(0.24)	(0.20)	(0.54)	(2.11)
Democratization	-0.07	-0.04	-0.65	-1.76
	(0.19)	(0.17)	(0.54)	(1.43)
Post-Successful Democratization (10-years)	0.14	0.19	-0.06	0.76
	(0.14)	(0.13)	(0.37)	(1.87)
Post-Democratization (10-years)	0.04	0.08	-0.15	-1.13
	(0.11)	(0.10)	(0.35)	(1.66)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Episode obs.	383	384	91	92
R^2	0.02	0.02	0.26	0.11
Observations	24,150	24,142	2,948	3,679

by approximately 10 percentage points and the net entry of new public firms increases by approximately 32% given an average democratization link of 8.5 years. This indicates that incumbent firms may face additional pressure from new entrants in successful democratizations.

Cost of democratization over time Additional evidence in favor of the redistribution based theories can be found in Figure 1.6 which performs a regression of the 5-year change in log dividend yields on an indicator for the start year of a democratization (as in Table 1.1) on rolling 60-year estimation windows.[24] The coefficient estimates rise until the end of the estimation window moves into the 1950s and then plateau at approximately 20% until the present. Interestingly, the coefficients begin to rise after the first wave of democratizations beginning at the end of the First World War, with the estimated coefficient jumping dramatically in 1919. This is in line the with the narrative of Luebbert (1991) that prior to 1914, Elites could more effectively bargain with the middle class, shutting out the then nascent workers movements. This changed after the war as democratizations became more labor driven, and as a result focused on increasing labor bargaining

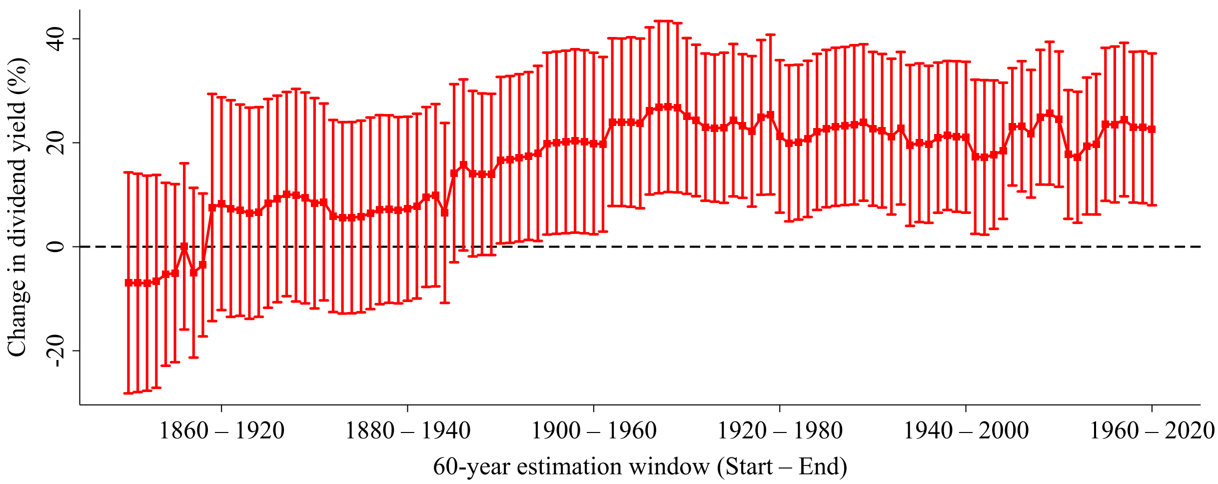
[24] A 60-year window is used to assure that enough democratizations are in the sample to allow for accurate estimation.

Figure 1.6: Rise in dividend yields around democratization starts, Rolling estimation

This figure presents coefficient estimates on 5-year change in log-dividend yields estimated on rolling 60-year windows. Horizontal axis represents the estimation window. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha_c + \alpha_t + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield. Standard errors are clustered by country and year.



power and reducing inequalities. This figure provides evidence that these increased costs in the Elites in the event of a successful democratizations brought with them higher risk premia in the transition period.

Moreover, the relative stability of these estimates challenges one of the main predictions of modernization theory, namely that global economic development has reduced the "cost" of democracy over time. This set of theories, starting with Lipset (1959) but based in the sociological tradition described in Weber (1946), highlight the idea that economic modernization may cause societies to become more democratic. The general thrust of these theories, come down to the idea that economic modernization makes democracy less expensive for those in power. This theory is then used to understand why democratizations have become more prevalent over time as the world has become wealthier, and why democratizations have occurred in higher income countries. Since the increase in risk premia observed in the data can be thought of as reflecting the cost of democratizations born by investors. Under these modernization theories, this means that the cost of democracy should be falling over time. The results from Figure 1.6 show there is little evidence of a decline in the effect of democratization on risk premia over a 150-year sample window.

Robustness and additional results Appendix A.4 presents robustness checks on these results. Appendix A.4.1 presents event study plots that show the timing of the government revenue-GDP ratio increases and the Gini coefficient declines, indicating that they begin right after the end of a successful democratization.

Appendix A.4.2 presents additional evidence that investors view successful democratizations negatively. In particular, Figure A.13 shows that prices decline upon the realization of a successful democratization, and rise if the democratization is reversed or co-opted. This supports the idea that a permanent, successful transition to democracy presents a risk to investors.

Finally, Appendix A.4.3 presents additional evidence that links the rise in risk premia to redistribution risk. First, Table A.20 shows that the increase in dividend yields from Table 1.1 are primarily driven by countries where the business elites, the political elites, or the aristocracy are the most powerful political group prior to the democratization.[25] This is directly in line with the model presented below. Table A.21 provides evidence that democratizations that see larger price declines also see significantly larger declines in inequality 5 or 10 years later.

1.6. Model

The previous section presented evidence that successful democratizations come with substantial redistribution. However, it is not yet clear whether this amount of redistribution observed is quantitatively large enough to explain the asset pricing results from Sections 1.3 and 1.4. Because of the potentially nonlinear relationship between prices and future redistribution, this task requires a model.

This section presents a consumption-based asset pricing model with democratic transitions. Democratizations are modeled similarly to Acemoglu and Robinson (2006); a consortium of political elites in an autocracy attempt to maintain control of the state from a larger group of citizens. If democracy is implemented, the citizens redistribute income toward themselves by way of taxes

[25] This is determined using V-Dem's regime information data.

and transfers. For the elites, the consolidation of democracy comes with a large reduction in their consumption and, therefore, acts similar to a "rare disaster." An increase in the probability that this happens raises risk premia (Gabaix, 2012; Wachter, 2013). As such, the model will generate a rise in risk premia during democratizations where the probability of a permanent transition is more likely. This increase in risk premia during democratizations is then shown to be proportional to the loss of Elite consumption if the democratization succeeds.

Macroeconomic environment A closed economy is populated by a mass $\delta < \frac{1}{2}$ of identical Elites and a mass $1 - \delta$ of identical Citizens that share coconuts (Y) from a Lucas tree. Time is discrete and infinite. The log growth rate of coconuts from the Lucas tree is exogenous and follows

$$\log \frac{Y_t}{Y_{t-1}} = \bar{y} + \sigma_y \varepsilon_t. \tag{1.6.1}$$

where $\bar{y}$ is the average growth rate, σ_y is the standard deviation, and $\varepsilon \sim \mathcal{N}(0,1)$ is an independent and identically distributed, lognormal shock. The Elites receive a proportion $\theta^{\mathcal{I}} > \delta$ of the coconuts, meaning the per capita coconut income for each type of agent is

$$\bar{Y}_t^r(\theta^{\mathcal{I}}) = \left(\frac{\theta^{\mathcal{I}}}{\delta}\right) Y_t \tag{1.6.2}$$

$$\bar{Y}_t^p(\theta^{\mathcal{I}}) = \left(\frac{1-\theta^{\mathcal{I}}}{1-\delta}\right) Y_t, \tag{1.6.3}$$

where the superscript r denotes the (rich) Elites and the superscript p denotes the (poor) Citizens. The parameter θ dictates the level of pretax income inequality in the economy: The higher is θ, the more unequal is the economy. The superscript $\mathcal{I} \in \{A, D\}$ dictates the political regime that the economy operates in, either autocracy or democracy. This implies that inequality is permitted to depend on the political institutions in place, allowing for the possibility that democracy reduces inequality outright.[26]

[26] In a production model with Cobb-Douglass production, this would be mapped into a reduction in the capital share through an increase in, for example, the bargaining power of labor, as the results above and in Drautzburg et al. (2022) suggest. Labor bargaining shocks of this type have been studied in Danthine and Donaldson (2002) for asset prices and Drautzburg et al. (2021) for real fluctuations in the United States.

In the economy there is one fiscal instrument: a linear tax on individual income that is paid back as an identical transfer to all agents. The tax rate is set by whoever has political power—the Elites in autocracy and the Citizens in democracy. The average coconuts available to members of each group, after taxes, is given by the expression

$$\hat{Y}_t^i(\tau_t, \theta^{\mathcal{I}}) = (1 - \tau_t)\bar{Y}_t^i(\theta^{\mathcal{I}}) + (\tau_t - \frac{1}{2}\omega\tau_t^2)Y_t \tag{1.6.4}$$

where $i \in \{r, p\}$ and $\frac{1}{2}\omega\tau_t^2$ is the cost of taxation, which introduces a reduced form Laffer curve. This is mainly for convenience; in the absence of a convex cost to taxes and transfers, the Citizens would choose a tax rate of 100%. An alternative model where the tax rate has some upper bound would provide a similar result. Moreover, this also makes optimization over taxes a static, rather than a dynamic, problem, simplifying the analysis.

Both groups want to maximize their pre-tax income. This means that the optimal tax rate for the Elites is $\tau^{r*} = 0$ since the transfer is less than their pre-tax income, $\bar{Y}^r > Y$. The optimal tax rate for the Citizens, on the other hand, is positive and given by the revenue maximizing tax rate

$$\tau_{\mathcal{I}}^{p*} = \frac{\theta^{\mathcal{I}} - \delta}{\omega(1 - \delta)}, \tag{1.6.5}$$

which is between 0 and 1 since $1 > \theta > \delta$. When the Citizens preferred tax rate is implemented their average post-tax income is (assuming $\omega = 1$)

$$\hat{Y}_t^p(\tau^{p*}) = \underbrace{\bar{Y}_t^p}_{\text{Endowed income}} + \underbrace{\frac{1}{2}\left(\frac{\theta^{\mathcal{I}} - \delta}{1 - \delta}\right)^2 Y_t}_{\text{Maximum transfers}}. \tag{1.6.6}$$

Both agents have have Epstein and Zin (1989) preferences over coconuts. The Elites are assumed to be the only agent that can access to financial markets meaning they are the marginal investors in this economy.

Autocracy and revolution In autocracy, in which the model starts, only the Elites have the right to vote, meaning whatever policy they choose will be enacted. Absent any counteracting force, the Citizens would never receiving voting rights and the Elites would set taxes to zero in each period. However, the numerically superior Citizens can indirectly control the the choices of the Elites through the threat of revolution. If the Citizens revolt, they are assumed to be successful, all the Elites are killed, and they take control of the economy for the remainder of history. But, this victory comes at a cost; a fraction μ of the Lucas tree is permanently destroyed. The expected present value of their utility after the revolution (scaled by the average income at time t) is, therefore,

$$v^p(R, \mu_t) = \left(\frac{1-\beta}{1-\beta^\star}\right)^{\frac{1}{1-1/\psi}} \left(\frac{1-\mu_t}{1-\delta}\right) \tag{1.6.7}$$

where $\beta^\star \equiv \beta e^{(1-1/\psi)\bar{y}+\frac{1}{2}(1-\gamma)(1-1/\psi)\sigma_y^2}$, β the rate of time discounting common across agents, γ the coefficient of relative risk aversion, and ψ the elasticity of intertemporal substitution (EIS). This expression for the value function is derived in Appendix A.5.1.

Variation in μ ultimately drives the dynamics in the model.[27] When μ is high, the Citizens cannot credibly threaten revolution, as the destruction wrought makes them better off under autocracy. When μ is low, conversely, the Citizens can credibly threaten revolution, which constrains the Elites from setting their optimal tax policy, tilting the equilibrium policy toward the optimal policy of the Citizens.

Conceding democracy If the threat of revolution is great enough, then even temporarily granting the optimal policy of the Citizens is not enough to prevent a revolution. In such cases, the Elites would like to promise transfers in future periods, but such transfers are not credible; if μ returns to a high value, then the Elites would no longer find it optimal to follow through on the promised transfers in any Markovian equilibrium.[28] In this case, it becomes optimal for the Elites to extend voting rights to the Citizens, ushering in democracy. Democracy makes the promise to redistribute

[27] Variation in the cost of revolution μ is a reduced form way of modeling a complex collective action problem that the Citizens must solve to successfully mount a revolution. A revolution cannot be successful if just one Citizen wakes up one morning and decides to revolt; she must be accompanied by others to pose a true threat. Variation in μ, therefore, represents that solving this problem is "hit-or-miss." Explicitly modeling the collective action problem that the Citizens face is beyond the scope of this paper.

[28] Other path dependent equilibria do exist and could make future promises of redistribution credible, though I do not examine them in this paper. Acemoglu and Robinson (2006) provide an extension of this setup to include path dependency and find that it does not change the overall conclusions of the model.

in future periods credible, as the more numerous Citizens become the median voter. This effectively grants them power over all future tax policy decisions, since once the economy becomes a democracy, it remains a democracy forever. As such, the present value of the Citizens' utility (once again, scaled by the average income at time t) is expressed by

$$v^p(D) = \left(\frac{1-\beta}{1-\beta^\star}\right)^{\frac{1}{1-1/\psi}} \hat{y}^p(\tau^{p*}, \theta^D) \tag{1.6.8}$$

which is the expected present value of receiving the maximum transfer income from Equation (1.6.6) in each period under Epstein and Zin utility, where $\hat{y}^p(\tau^{p*}, \theta^D) \equiv \hat{Y}_t^p(\tau^{p*}, \theta^D)/Y_t$.

Political environment as a game The political environment can be modeled formally as a game. The order of the decisions is as follows (with mathematical notation in parentheses):

1. Nature reveals the cost of revolution (μ_t) to both the Elites and the Citizens.

2. The Elites choose to either concede democracy ($\phi_t = 1$) or keep autocracy ($\phi_t = 0$).

3. Both the Elites and Citizens choose the tax rate (τ_t^i) they want to implement. If the society is an autocratic, then the tax rate chosen by the Elites is implemented; if the society is democratic then the tax rate chosen by the Citizens is implemented.

4. The Citizens, after observing the tax rate, choose to revolt ($\rho_t = 1$) or not revolt ($\rho_t = 0$).

The choice set of the Elites in time t is given by $\varsigma_t^r = \{\tau_t^r(\mu_t), \phi_t(\mu_t)\}$ where their chosen tax rate and the choice of whether to concede democracy are functions of the cost of a revolution. Further, if $\phi_t = 1$ then $\phi_{t+s} = 1$ for $s > 0$, meaning that once democracy is conceded, it is conceded forever.

The choice set of the Citizens in time t is given by $\varsigma_t^p = \{\tau_t(\phi_t), \rho_t(\mu_t, \phi_t)\}$ where their chosen tax rate and the choice to revolt are functions of the political institutions in place and the cost of a revolution. Further, if $\rho_t = 1$ then $\rho_{t+s} = 1$ for $s > 0$, meaning if the revolution occurs, its effects are permanent.

The Citizens' problem There are, in essence, three relevant political institutions for the Citizens: autocracy, democracy, and revolution. The value functions for the Citizens in the revolution and democracy are given by Equations (1.6.7) and (1.6.8) above, and the payoff in autocracy is given by

$$v^p(A,\mu_t)^{1-1/\psi} = (1-\beta)\hat{y}_t^p(\tau_t)^{1-1/\psi} + \beta^\star \left(\mathbb{E}_t\left[v^p(\mu_{t+1})^{1-\gamma}\right]\right)^{\frac{1-1/\psi}{1-\gamma}} \tag{1.6.9}$$

where

$$v^p(\mu_{t+1})^{1-\gamma} = \begin{cases} v^p(D)^{1-\gamma} & \text{if } \phi_{t+1} = 1 \\ v^p(A,\mu_{t+1})^{1-\gamma} & \text{if } \phi_{t+1} = 0 \\ v^p(R,\mu_{t+1})^{1-\gamma} & \text{if } \rho_{t+1} = 1 \end{cases}$$

is the continuation value of the Citizens utility in the various possible future states.

The revolution constraint Since the Elites die if a revolution occurs they are always willing to transfer coconuts to the Citizens to avoid a revolution. This imposes a *revolution constraint* on the tax rate the Elites can choose in autocracy in that the Citizens must be at least indifferent between revolting and the autocratic status quo, meaning

$$v^p(A,\mu_t) \geq v^p(R,\mu_t). \tag{1.6.10}$$

The revolution constraint introduces two critical values of μ: the first is $\underline{\mu}$ which is where Equation (1.6.10) holds with equality when there are no taxes,

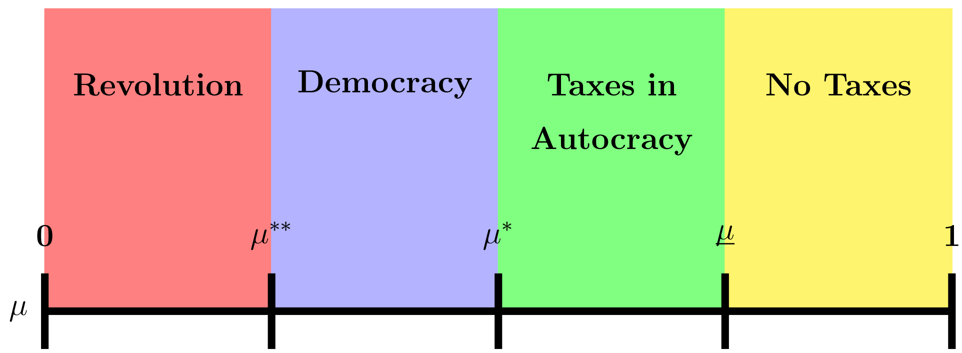
$$v^p(A,\underline{\mu};\tau_t = 0) = v^p(R,\underline{\mu}), \tag{1.6.11}$$

and the second is μ^* where Equation (1.6.10) holds with equality when the preferred tax rate of the Citizens is in place,

$$v^p(A,\mu^*;\tau^{p*}) = v^p(R,\mu^*). \tag{1.6.12}$$

When $\mu \in [\underline{\mu}, 1]$ then the revolution constraint does not bind; no transfers are needed to avert a revolution, as the Citizens are, at least, weakly better off under autocracy. As the cost of revolu-

Figure 1.7: Equilibrium Outcome for Regions of μ



tion falls such that $\mu \in [\mu^*, \underline{\mu})$, the Elites must raise taxes and transfers to prevent the Citizens from revolting. I define the tax rate that makes the Citizens indifferent between a revolution and autocracy as $\hat{\tau}(\mu_t)$, which satisfies

$$v^p(A, \mu_t; \hat{\tau}(\mu_t)) = v^p(R, \mu_t). \tag{1.6.13}$$

What happens if μ falls below μ^*? It is no longer sufficient for the Elites to raise taxes for just one period, and they are unable to credibly promise future redistribution in the states of the world where μ is high. In this case, the Elites must concede democracy to prevent a revolution. Conceding democracy prevents a revolution provided that $\mu \in [\mu^{**}, \mu^*)$ where μ^{**} equates Equation (1.6.7) and Equation (1.6.8) and is given by (assuming $\omega = 1$)

$$\mu^{**} = \theta^D - \frac{1}{2}\frac{(\theta^D - \delta)^2}{1-\delta}. \tag{1.6.14}$$

This expression takes into account both the higher taxes and transfers and lower inequality in democracy.

Finally, if $\mu \in [0, \mu^{**})$, the Elites can do nothing to prevent a revolution, as the Citizens are better off in the revolutionary state than in democracy. In the special case of this economy I solve, however, I only examine cases where $\mu \in [\mu^{**}, 1]$. The action regions and their associated thresholds are shown in Figure 1.7.

Stochastic process for $\boldsymbol{\mu}$ The cost of revolution μ evolves according to a three-state, Markov process with the transition matrix

$$\mathbf{P} = \begin{pmatrix} p_{11} & p_{12} & p_{13} \\ p_{21} & p_{22} & p_{23} \\ p_{31} & p_{32} & p_{33} \end{pmatrix} = \begin{pmatrix} 0.99 & 0.01 & 0.00 \\ 0.10 & 0.80 & 0.10 \\ 0.00 & 0.50 & 0.50 \end{pmatrix}, \tag{1.6.15}$$

where $\mu^1 = \mu^2 = \mu^A$ and $\mu^3 = \mu^{**}$, and the calibrated probabilities[29] of transition are shown after the second equality. In the first state, the *autocracy state*, the Elites do not face a revolutionary threat in this period.

In the second state, the *democratization state*, the Elites now face a potential major revolutionary threat in the next period, with μ having a 10% chance of being equal to μ^{**} in $t+1$. If this comes to pass, then all the Elites can do to prevent a revolution is to concede democracy. However, also note that there is also an equal probability that the democratization will fail, returning the economy to autocracy. This is the key intuition for why democratization affects the risk premium: it is uncertain whether it will succeed or fail.

Such uncertainty over the success or failure of a democratization is present in the data. Just over 52% of democratizations fail, meaning that the society reverts to autocracy only a short time after the initial jump in measures of democratic institutions. In sum, the primary mechanism for the rise in risk premia in the model enjoys support in the data.

In the third state, the revolutionary threat is realized and the Elites need to concede democracy. Note, that while there is a 50% chance of returning back to the democratization state in the transition matrix, this probability is immaterial; when democracy is conceded, it is an absorbing state. This means that once democracy is conceded the state variable μ becomes irrelevant. Here is the model is parameterized to produce a democratic transition in the final state, but more general calibrations which lead to either an autocratic equilibrium or revolution are possible. These

[29] These probabilities are calibrated to match (1) the probability of starting a democratization in any given year of 1%, (2) a 50% success rate of democratizations, and (3) a median democratization length of 5 years.

generalizations are discussed in Appendix A.5.2.

Equilibrium I consider Markov perfect equilibria, meaning that all strategies must be a best response and can only depend on the current state, not the history of past states. A Markov perfect equilibrium consists of a choice set for the Elites and the Citizens for each combination of state variables (namely, the current value of μ and political institutions from the previous period). But, all of the consequential choices take place in autocracy. If the revolution occurs, taxes are always zero, as the Elites have been killed, and the Citizens have equal income, and, therefore, no desire for taxes and transfers. In democracy, the Citizens preferred tax rate is always chosen. Based on the assumed process for μ, the only equilibrium to the political game is for the economy to be an autocracy in autocracy and democratization states and transition to democracy the democracy state. In the case, taxes will be equal to $\hat{\tau}(\mu)$ in the first two states and equal to τ^{p*} in the last state. A revolution never occurs in equilibrium under this calibration.

1.6.1. Asset pricing implications

The Elites are the only financial markets participants, so they price all assets. Since the Elites are negatively affected by a democratic transition, an increase in the probability that this happens will correspond with a higher risk premium. This increase in the risk premium will be a combination of the probability of transition along with the reduction in Elite consumption associated with the democratization. Using this relationship, the model will allow for the decomposition of the relative importance of the different forms of redistribution outlined in Section 1.5.

The Elites' problem The Elites have Epstein and Zin utility and trade in the consumption claim and a zero-net supply riskfree bond. The recursive formulation of their utility in autocracy can be written similar to the Citizens' utility and is given by

$$v^p(A, \mu_t)^{1-1/\psi} = (1-\beta)(\hat{y}_t^r(\tau_t))^{1-1/\psi} + \beta^\star \left(\mathbb{E}_t\left[v^r(\mu_{t+1})^{1-\gamma}\right]\right)^{\frac{1-1/\psi}{1-\gamma}} \tag{1.6.16}$$

where

$$v^r(\mu_{t+1})^{1-\gamma} = \begin{cases} v^r(D)^{1-\gamma} & \text{if } \phi_{t+1} = 1 \\ v^r(A, \mu_{t+1})^{1-\gamma} & \text{if } \phi_{t+1} = 0 \\ -v^r(R) & \text{if } \rho_{t+1} = 1 \end{cases}$$

with $-v^r(R)$ representing the utility of the Elites in the revolution. The budget constraint is the standard relation

$$W_{t+1} = (W_t - C_t^r)R_{W,t+1} \tag{1.6.17}$$

and market clearing requires that Elite income equals Elite consumption in the aggregate and that the aggregate Elite portfolio place a weight of 1 on the consumption claim (following from the riskfree asset being in zero-net supply). This is because there is no trading between the Elites and the Citizens in autocracy. The pricing kernel revolves around the growth rate of the consumption of the Elites. This can be decomposed as

$$\frac{C_{t+1}^r}{C_t^r} \equiv \left(\frac{Y_{t+1}}{Y_t}\right)\left(\frac{c_{t+1}^r}{c_t^r}\right) \tag{1.6.18}$$

where c^r is Elite consumption scaled by aggregate income. The growth rate of scaled consumption is given by

$$\frac{c_{t+1}^r}{c_t^r} \equiv \begin{cases} Z & \text{if } \phi_t = 1; \phi_{t-1} = 0 \\ 1 & \text{otherwise} \end{cases} \tag{1.6.19}$$

where $Z < 1$ represents the penalty the Elites face to their consumption upon a successful transition to democracy, given by

$$Z = \frac{\hat{y}_t^r(\tau^{p*}(\theta^D))}{\hat{y}_t^r(\tau_t)}. \tag{1.6.20}$$

Consistent with Wachter (2013), I model the equity claim as a levered claim to consumption, with $D_t = C_t^\lambda$. This allows the equity claim to have sufficient risk to have a chance at matching the targeted change in dividend yields observed in democratizations. The details of the derivation are in Appendix A.5.3.

Since the the increase in redistribution is in part driven by an increase in taxes, one may question whether modeling the dividend claim as a levered claim to consumption is appropriate. What this implies literally is that the dividend claim will see larger increases in taxes than does consumption. While this may or may not be the case, it is more accurate to think about leverage as a reduced form way of modeling the impact of democratization on incumbent firms. As Section 1.5.1 shows, pro-competitive regulation and net entry increases in successful democratizations. This disproportionately effects the owners of incumbent firms. These firms are also the ones for whom we see an increase in dividend yields at the start of the democratization. Since entry an exit is not present in the model, the redistribution of dividends from incumbent firms to new entrants is also missing. The leverage parameter can be thought of as a reduced form way of modeling this more complex phenomenon. This link will be made explicit in some of the extensions below.

The movement in μ mimics variation in the disaster probability as in Gabaix (2012) and Wachter (2013): As a permanent transition to democracy becomes more likely, risk premia and dividend yields rise. Qualitatively, this will allow the model to match the increase in dividend yields in democratizations. However, it is not obvious that the quantities of redistribution, especially explicit redistribution, are great enough to match the large increase in dividend yields observed in the data. I turn to this exercise in the following subsections.

1.6.2. Extensions

The baseline version of the model allows for two types of redistribution: higher taxes and lower inequality. However, the rise in taxes and decline in inequality is treated as homogeneous across all democratizations. Moreover, as shown in Section 1.5 there are other types of tacit redistribution that may occur in democracy such as the loss of political rents and increased economic competition. This section presents extensions to the baseline model that allow for these features.

Uncertain redistribution The baseline version of the model treats the redistribution in democracy as though it is known with certainty. In practice, however, the amount of redistribution that could occur is uncertain; it depends on the new political equilibrium that emerges in democracy. In this extension, I allow for the Elites to be uncertain about the redistribution that democracy brings.

In particular, the Elites will recapture some fraction χ of their old political power. This mean that the resulting tax policy is given by $\tau = (1-\chi)\tau^{p*} + \chi\tau^{r*}$.

In the version calibrated below, I take $\chi \in \{\chi^L, \chi^H\}$ to have two values which realize with probability $1-q$ and q, respectively. This allows for cases where democracy redistributes little versus cases where it redistributes much. Unsurprisingly this leads to larger increases in dividend yields: As the left tail becomes longer, so grows the risk investors face.

Loss of political rents In the baseline model, there are no taxes in autocracy unless the Elites are forced to concede them. Therefore, the only way to match the average government revenue-GDP ratio in autocracy is to calibrate μ in autocracy to match it. This blunts the redistributive effects of democracy, since taxes are already elevated in autocracy. However, it could be the case that the Elites are able to extract a disproportionate share of rents from the government. If so, the Elites may want to raise tax revenue they can then divert toward their own consumption. Quantitatively, this further reduces Elite consumption after a successful democratic transition.

This is model by introducing two new features. First, I allow for government revenue to be spent on "particularistic public goods" of which the Elites can consume a fraction ν. Second, whoever has political power can choose which fraction of government spending goes to these particularistic public goods, denoted by $\aleph$. This implies that Elite post-tax income is now

$$\hat{Y}_t^r(\tau_t) = (1-\tau_t)\bar{Y}_t^r + \left(\left(1 + \aleph(\frac{\nu}{\delta} - 1)\right)\tau_t - \frac{1}{2}\omega\tau_t^2\right)Y_t. \tag{1.6.21}$$

In autocracy, when the Elites face no revolutionary threat, they optimally choose the corner case $\aleph = 1$ and an optimal tax rate $\tau^{r*} = \frac{\nu - \theta^A}{\omega\delta}$ provided $\nu > \theta^A$. Conversely, in the democratic equilibrium, the Citizens do not benefit from these particularistic public goods, and set $\aleph = 0$.

In this version of the model, the Elites face a steeper reduction in consumption when transitioning to democracy than in the baseline version, leading to a larger asset pricing effect. This can be disciplined by calibrating the model to the V-Dem resource inequality index, which ranks countries on a 0 to 100 scale of how unequally public goods are shared. This index is 68.4 in autocracies as compared to 27.9 in democracies. Alternatively, one could calibrate to the educational spending Gini coefficient in autocracies, 57.5 (as compared to 24.6 in democracies). The calibration below requires 60.7 to match the tax rate observed in autocracy in the data.

Increased competition for incumbent firms Section 1.5 provides evidence that economic competition increases during successful democratizations. Pro-competitive regulation rises as does net entry of publicly traded firms. This is also in line with the literature around structural transformation and democratization, detailed in Aghion et al. (2014) and Martinez-Bravo and Wantchekon (2021). In this literature, the improvement in democratic institutions leads to an increase in creative destruction, which presents a risk to incumbent firms.[30] Since the increase in dividend yields at the start of democratizations is measured on the set of incumbent firms, this could present an important source of risk, similar to the displacement risk described in Gârleanu et al. (2012).

Therefore, in this extension instead of modeling the dividend claim as a levered claim to Elite consumption, I model it as the set of incumbent firms in autocracy. By definition, these firms receive all dividends as they have the entire market share within the autocratic economy. When democracy comes, barriers to entry fall and these firms lose a fraction ξ of their market share and profits to the new entrants, where this is modeled as a one time permanent shock. The growth rate of dividends is, therefore, given by,

$$\frac{D_{t+1}}{D_t} \equiv \left(\frac{Y_{t+1}}{Y_t}\right)^{\Upsilon} \left(\frac{d_{t+1}}{d_t}\right) \tag{1.6.22}$$

where d_{t+1} is dividends scaled by aggregate income which has a growth rate given by

$$\frac{d_{t+1}}{d_t} \equiv \begin{cases} \left(\frac{1-\tau^D_{Div}}{1-\tau^A_{Div}}\right)\xi & \text{if } \phi_t = 1; \phi_{t-1} = 0 \\ 1 & \text{otherwise} \end{cases} \tag{1.6.23}$$

where $\tau^{\mathcal{I}}_{Div}$ is the dividend tax rate in either autocracy or democracy.

[30] One mechanism that could bring this about is a loss of political connections for incumbent firms, similar to the model presented in Grotteria (2019).

Table 1.9: Model calibration

This table shows the calibration of the parameters in the model. A description of the moment matched and the source of the data or parameter value are provided alongside the calibrated value.

Parameter	Value	Description	Source
Lucas Tree:			
$\bar{y}$	0.014	Income growth in autocracy	Maddison Historical Statistics
σ_y	0.073	Income standard deviation	Maddison Historical Statistics
Inequality parameters:			
θ^A	0.570	Inequality in autocracy	WIID
θ^D	0.525	Inequality in democracy	Author estimation
δ	0.10	Fraction of elites	Assumption
τ_A^{p*}	0.190	Tax rate in autocracy	Autocracy Gov. Rev.-GDP ratio
ω	1.96	Cost of taxation	Democracy Gov. Rev.-GDP ratio
Dividend claim:			
λ	2.6	Leverage of dividend claim	Wachter (2013)
Preference parameters:			
β	0.9572	Subjective discount rate	Match PD ratio in autocracy
γ	5	Relative risk aversion	Bansal, Kiku, Yaron (2010)
ψ	1.5	IES	Bansal, Kiku, Yaron (2010)

The advantage of modeling the dividend claim in this way, as opposed to purely a levered claim to Elite consumption, is that in the data I do not observe large decline in aggregate dividends after successful democratizations, but rather declines in prices as described in Appendix A.4.2. In this extension, the model comes with a redistribution of dividends to new entrants in the next period, meaning the aggregate level of dividends need not decline. However, prices would decline for incumbents upon the realization of the shock.

Finally, it is also possible that the increase in competition presents a shock to Elite consumption in addition to a shock to the cashflow of the dividend claim for incumbent firms. For example, if Elites are overexposed to certain industries and this risk is not diversifiable, this would add to the quantitative impact of an increase in competition. I do not add this to the model here, so the calibrated ξ below should be thought of as aggregating both the direct effect on incumbent cashflows and the shock to Elite consumption it potentially brings about. Allowing for this would lower the ξ parameter needed to match the rise in dividend yields.

1.6.3. Calibration

The model is calibrated using a combination of data moments from various sources, the reduced form estimates from above, and the asset pricing literature as outlined in Table 1.9.

The average growth rate and standard deviation in autocracy are set to their data counterparts in the Maddison Historical Statistics data. To calibrate the inequality parameters, I assume that the elites constitute 10% of the population. This assumption is without loss of generality, as what really matters for inequality is the difference between θ and δ. The inequality parameter θ is calibrated to match the average Gini coefficient in autocracy equal to approximately 47. Since this is a two agent model where agents have two different incomes, the Gini coefficient is equal to the income share less the number of agents in that group, or $\theta - \delta$. Conversely, the reduction in θ could also be thought of as a reduction in capital share, as would be the case in an economy with Cobb-Douglass production. Therefore, θ in democracy is calibrated to match the average of the reduced form evidence of a 2.3 percentage point decline in the Gini coefficient and a 6.7 percentage point rise in the labor share, equal to a decline of 4.5 percentage points.

The tax rate in autocracy is equivalent to the government revenue-GDP ratio, equal to 19%. I calibrate μ such that the government revenue-GDP ratio matches its average in autocracy. The cost of raising tax revenue ω is calibrated to match the observed 4.8 percentage point increase in government revenue-GDP ratios found in Section 1.5.

The values for relative risk aversion and the EIS are standard and taken from Bansal et al. (2010) and the amount of leverage present in the dividend claim is taken from Wachter (2013). Finally, the subjective rate of discount β is calibrated to match the dividend yield in autocracy (0.05).

1.6.4. Model results

Table 1.10 presents the rise in dividend yields when transitioning from autocracy to democratization in the model. The model generates a 11.9% increase in dividend yields, approximately 60% of the rise in dividend yields observed in the data when country and year fixed effects are added. Risk over future redistribution can explain a substantial portion of the rise in dividend yields seen in

Table 1.10: Model results

This table shows the results for the change in the log dividend yield in the model and data for the baseline version and various extensions.

Panel A: Elites cost of democracy		
Inequality reduction $\theta^A - \theta^D$		0.045
Tax increase $\tau(D) - \tau(A)$		0.048
Reduction in Elite consumption (%)		13.1
Panel B: Baseline Model	Model	Data
Dividend yield autocracy	0.050	0.050
Dividend yield democratization	0.056	0.060
Change in dividend yield (%)	11.9	19.8
Panel C: Extensions change in dividend yield (%)	Model	Data
Uncertain redistribution	17.5	19.8
Loss of political rents ($\nu = 0.6072$)	30.0	19.8
Increased competition for incumbent firms ($\xi = 0.619$)	19.8	19.8

democratizations.

Also, presented are the results for the several model extensions. The extension that allows for a mean-preserving increase in uncertainty over future redistribution generates a rise in dividend yields of 17.5%, explaining 88% of the asset pricing results. The extension that allows for the Elites to divert public goods for their own means generates a very large increase in dividend yields, approximately 30.0%, due to the steeper decline in consumption this version of the model generates. Each of these model results are well within the standard error bars in the data.

Finally, the increased competition specification is calibrated to match the rise in dividend yields, hence the numbers match exactly. In particular it does this by assuming an increase in taxes equal to the difference in corporate taxes between autocracies and large democracies reported in Genschel et al. (2016) which is approximately 10%. The ξ parameter needed to match the data is then 0.619 which implies that new entrants take approximately 38% of incumbent market share right after transition. Translated into year-over-year changes, this would represent a 2 to 2.5 percent change in incumbent market share relative to the autocratic equilibrium in the 20 years after transition. This would require a decline in incumbent firm valuations, consistent with evidence from

Fisman (2001a), who finds a 24% reduction in connected firm value after the fall of the Suharto regime in Indonesia, and consistent with the results I present in Appendix A.4.2.

1.6.5. There and back again: Autocratic reversals

Section 1.3 presents the potentially puzzling result that dividend yields are, on average, flat in autocratizations. One may question, given this theoretical framework, why the estimated coefficients are not negative. This subsection endeavors to answer this question.

Imagine now that democracy is reversible. The Elites can choose to attempt to overthrow the government and reinstate autocracy. The proposition of overthrowing the government is, however, risky: if the Elites fail, they are ostracized and lose permanently a fraction Z of their consumption. We can see already how this differs from the model of democratization. In the baseline model above, democratization is a risk *imposed* on the Elites by a potentially revolutionary citizenry; autocratization, conversely, is a risk *taken* by the Elites to gain greater future consumption.

If the Elites initiate an autocratization, they are successful with probability q, fail with probability q, and the autocratization continues with probability $1-2q$. If successful, Elites overthrow the government and implement autocracy once more. If it is unsuccessful, the society remains democratic and the Elites cannot attempt another democratization. The cost Z is known to them at the time of initiating the autocratization. Moreover, for simplicity I assume, as I did in the democratization model, that if the Elites are successful, the society will permanently transition to autocracy and they will receive the value function $V^r(A)$ which is equivalent to the present value of their income in permanent autocracy.

Randomly, the Elites get a take-it-or-leave-it offer to overthrow the government at potential cost Z. They then choose to either initiate an autocratization or stay in democracy. If they stay in democracy, society remains democratic forever. In this setup, therefore, an attempted autocratization is a positive net present value, high-risk bet. If successful, the Elites receive a large payoff through lower redistribution. If failed, they face a large, permanent shock to their consumption.

The value function of the Elites from undertaking an autocratization is given by

$$v^r(T,Z_t)^{1-\frac{1}{\psi}} = (1-\beta)(\hat{y}_t(D))^{1-\frac{1}{\psi}} + \beta^\star \mathbb{E}_t\Big[qv^r(D,F)^{1-\gamma} + (1-2q)v^r(T,Z_t)^{1-\gamma} + qv^r(A)^{1-\gamma}\Big]^{\frac{1-\frac{1}{\psi}}{1-\gamma}}, \quad (1.6.24)$$

where the value function from declining the autocratization is given by Equation (1.6.8), the value function from a failed autocratization is given by

$$v^r(D,F) = \left(\frac{1-\beta}{1-\beta^\star}\right)^{\frac{1}{1-1/\psi}} \hat{y}_t(D)(1-Z)$$

where $\hat{y}_t(D)$ is the Elite's post-tax income in democracy. The indifference point for the Elites, Z^*, is the point at which Equation (1.6.8) is equal to Equation (1.6.24). For autocratizations that occur at this indifference point, there is no effect on the consumption wealth ratio, as the potential growth effects offset the increase in risk.

However, for the levered claim to consumption, the risk effects dominate, leading to a small increase in dividend yields. Indeed, when calibrating to the parameters in Table 1.9, the dividend yield displays a 2.3 percent rise in an autocratization, almost identical to the results in Columns (3) and (4) of Table 1.3.

The model also provides another counter-intuitive result: the larger the benefits to instituting autocracy, the larger the increase in dividend yields for autocratizations attempted near the threshold. This is because of the endogenous respond of the Elites to take greater risk in autocratizations. In particular, the threshold value Z^* rises as autocracy becomes more attractive.

1.7. Conclusion

Any financial history of the last 200 years that excludes democratizations is incomplete. Risk premia are significantly elevated during periods of democratization, a fact present across multiple proxies in 90 countries over 200 years. At the same time, democratizations are not accompanied by an increased risk to aggregate consumption nor GDP and other periods of high political risk do not see the same effect. Finally, exogenous variation in the probability of a successful democratization stemming from a change in Catholic church doctrine causes a marked increases in average excess returns for treated countries using a difference-in-differences framework. A plausible mechanism, redistribution risk, is proposed to explain the results. This mechanism enjoys support in the data, as the size of the public sector rises and income inequality falls after successful democratizations relative to failed ones. Combining a model of democratic transitions in the style of Acemoglu and Robinson (2006) with a standard asset pricing model with limited asset market participation, this redistribution can explain 60% of the asset pricing results. The same model, can also explain the lack of an asset pricing effect observed in autocratizations.

While the results in the paper highlight several potential channels of redistribution that can generate the asset pricing results, reconciling the evidence on redistribution with the other macroeconomic effects of democracy—for example, higher growth (Acemoglu et al., 2019)—would be a natural path for future research. A better understanding of the interplay of these phenomena could help the field to better understand not just periods of democratization, but how policy and political risk affect individual, firm, and government decision making more broadly. Further, understanding how the increased globalization of asset markets influence the results would be a fruitful road to explore as well.

Finally, this paper provides new avenues of study in consumption-based asset pricing by focusing on political institutions and how they interact with the distribution (and redistribution) of resources. In particular, I show that neither an increase in the probability of a large drop in aggregate consumption nor an increase in the volatility of aggregate consumption is necessary for an increase in risk premia, but rather that the consumption risk faced by relatively wealthy equity market participants need only be affected. This paves additional roads through which heterogeneity and the risk of redistribution can lead to variation in risk premia.

CHAPTER 2

Foreign Influence in US Politics

Joint with Marco Grotteria and S. Lakshmi Naaraayanan

Contribution to the joint work All authors contributed equally to this project and each was involved in the production of each result in the paper.

> *Washington lobbyists may not be a group with high ethical standards, and foreign lobbyists, with their track record of working for Nazis, drug-running despots, and death squad dictators, are widely thought to be the lowest type of Beltway pond scum.*
>
> Ken Silverstein, *Turkmeniscam*

2.1. Introduction

Foreign governments have spent billions of dollars in the recent years to hire Washington lobbyists and influence United States (US) legislators (Opensecrets). The purpose of this lobbying is as varied as the countries that undertake it. For example, in 2007, Ethiopia's representatives successfully lobbied US legislators who were trying to restrict the foreign aid package to Ethiopia (Silverstein, 2007). For the last several years, lobbyists for Saudi Arabia have been winning congressional support for arms sales to Riyadh (Opensecrets). In 2021, Russia spent millions of dollars to lobby the US amid an escalating Ukrainian conflict (Opensecrets). Despite the intense public debate on the subject and the potential negative effects on public policies highlighted in anecdotes, large sample evidence on foreign lobbying and foreign influence is lacking.

This paper constructs a novel dataset by extracting information on meetings between foreign agents (i.e. agents working in the US on behalf of foreign principals) and US legislators from the Department of Justice (DOJ). These filings only cover meetings if the public or political interests of a foreign government or a political party are involved. Moreover, among foreign principals, nearly

90% are foreign governments and political parties, leading us to group together all foreign principals from the same country under the label of *foreign country*.

Using this dataset, we first characterize the nature and scope of connections between foreign agents and legislators from 2000 to 2018. We then study which country and legislator characteristics relate to meetings. On the extensive margin, countries that lobby are important trade partners to the US, receive more diplomatic visits by the US presidents and at the same time experience more political violence and have more corruption relative to non-lobbying countries. Among legislator characteristics, lawmaking effectiveness and membership in the foreign affairs committee are important correlates of these connections.

Foreign countries benefit from these meetings through (a) an increase in foreign aid and financial assistance, (b) lower product tariffs, and (c) more corporate subsidies and government contracts to firms headquartered in the countries whose agents meet more often with legislators. We also document benefits to legislators: more meetings between representatives of a given foreign country and US legislators are related to (d) an increase in the share of registered voters with ethnic affiliations to the foreign country and (e) an increase in the number of privately-sponsored trips to these countries. Finally, we document (f), on average, economically small costs to legislators, as proxied by changes in political contributions or in the probability of being re-elected. In sum, we show that foreign lobbying is effective in shaping public policy and resource allocation in the US, and find no evidence that legislators are punished by their constituents because they meet with foreign country representatives.

Our primary data come from filings under the Foreign Agents Registration Act (FARA) enforced by the DOJ. The FARA, originally enacted in 1938 to fight Nazi propaganda, imposes reporting requirements on all foreign principals lobbying in the US. We link each meeting with a US legislator to a single foreign client represented by a foreign agent (lobbyist) using detailed supplemental filings. In total, our dataset covers over 180,000 in-person meetings by 146 foreign countries lobbying approximately 1,200 US legislators across 10 Congresses.

The analysis proceeds in four parts. The first part of the analysis identifies foreign country and US legislator characteristics important for lobbying connections. Several characteristics influence a foreign country's lobbying efforts in the US, including, trade partnerships with the US, the frequency of visits by US presidents, and whether the foreign country has been sanctioned by the US or has a history of political corruption. Regarding legislator characteristics, we find that foreign agents meet disproportionately with legislators who are most effective at advancing bills through the legislative process or members of power committees, the Foreign Affairs Committee, and the Security and Intelligence Committee. On the intensive margin, only changes to lawmaking effectiveness and membership in the Foreign Affairs Committee positively correlate with changes in meeting intensity. Moreover, foreign agents meet with *all* legislators, including low-ranked legislators, even after they leave committees important to foreign countries.

The second part of the analysis studies benefits to foreign countries around meetings. To better identify the causal link between meetings and benefits, we exploit deaths as an exogenous shock to foreign country connections in tests where we can cleanly link outcomes benefiting foreign countries to legislators. This allows us to establish that meetings with legislators identify political connections that benefit foreign countries. We find that when foreign agents meet more often with legislators, these countries are more likely to receive assistance from the US. More meetings are associated with larger amounts of aid and assistance. Using deaths as shocks to political connections, our estimates imply a substantial per-meeting loss of $12.8 million in the form of aid to foreign countries. Further, meetings positively relate to a legislator's propensity to sponsor a tariff bill that is favourable to the foreign country. We also show that meetings between foreign countries and legislators sitting in a given committee are related to a larger probability that a favourable bill passes that committee or an unfavourable bill does not advance that specific committee. Moreover, we document that firms headquartered in foreign countries whose representatives meet more frequently with a legislator obtain more and larger subsidies and government contracts in the state or congressional district represented by the legislator.[31] Exploiting legislator deaths as an exogenous shock to foreign

[31] We explore the robustness of these findings and show that the pattern holds up under a variety of specifications. We employ panel estimations at year-month levels to rule out differences in timing between meetings and benefits to foreign countries and firms. Moreover, we conduct congressional-district level regressions and use legislator fixed

country connections, we find a substantial per-meeting loss of about $350,000 in local subsidies and government contracts.

The third part of the analysis examines the benefits to legislators. To determine benefits to legislators, we rely on a novel dataset on voter registration that allows us to link the ethnicity of the electoral base to that of the foreign country meeting with legislators. We document that more frequent meetings are associated with an increase in the share of registered voters with ethnic affiliations to the foreign country. Additionally, more meetings with legislators are associated with more foreign trips sponsored by private entities linked to the foreign country.

The last part of the analysis shows that, on average, costs to legislators are economically small. We examine aggregate political contributions made by individuals to legislators around meetings with foreign representatives. Additionally, we study whether these meetings influence the likelihood that an incumbent legislator wins an election in a specific election year. In both cases, we consistently find that the costs are negligible. One possibility could be misreporting important meetings that are detrimental to a legislator's reputation. However, this seems an unlikely explanation given that, as we discuss below, the costs for non-compliance with FARA are large, and we also have widely documented benefits for both foreign countries and legislators. Instead, there are two likely alternative explanations.

First, meetings are set strategically. Therefore, legislators may forgo meetings expected to be too costly to their reputation. As our sample focuses only on realized meetings, one would expect that legislators receive benefits that exceed their costs (Grossman and Helpman, 1996). Second, even though resources may be diverted to foreign governments or firms, the resulting policies can still lead to economic growth that benefits a legislator's constituents.[32] While it is likely that voters base their assessment of an incumbent's performance on recent economic conditions, it is difficult that they can accurately evaluate the efficiency of resource allocation or policies and use it as a factor in deciding whether to punish legislators in elections.

effects to verify that legislators indeed allocate more resources when they meet more often with foreign countries' representatives.

[32]For instance, see KIA plant in Georgia.

Taken together, our study identifies the determinants of connections between foreign countries and legislators and examines the role that these connections play in shaping public policy and the allocation of government resources. By analyzing meetings, we provide insights that we hope can inform the design of more effective political institutions. Additionally, our dataset offers new empirical facts that can be used to test and refine theories of lobbying in political economics and public finance, and we believe it will be of interest to a wide community of scholars in these fields. Overall, our results shed light on how access to legislators is obtained and distributed, a question of significant practical and theoretical importance.

Related literature. This paper contributes to the literature in three ways. First, we provide novel large-sample evidence of foreign influence in US politics by showing that meetings between foreign country representatives and US legislators favorably tilt resource allocation and public policy toward foreign governments and firms. These findings contribute to the extant literature in political economics where theoretical models of lobbying stress the importance of special interest groups in determining trade policy, budget priorities, and public good expenditures (Grossman and Helpman, 1994, 2001; Persson and Tabellini, 2002). On the empirical side, papers linked the intensity of lobbying by domestic firms to changes in trade tariffs (Goldberg and Maggi, 1999; Nunn and Trefler, 2010; Bombardini and Trebbi, 2012; Kim, 2017), or to government contracts, support in times of distress, more favorable regulation, or protection against political risk (Fisman, 2001b; Faccio, Masulis, and McConnell, 2006; Duchin and Sosyura, 2012; Goldman, Rocholl, and So, 2013; Tahoun, 2014; Adelino and Dinc, 2014; Schoenherr, 2019; Acemoglu, Johnson, Kermani, Kwak, and Mitton, 2016; Hassan, Hollander, van Lent, and Tahoun, 2019; Ağca and Igan, 2020; Brogaard, Denes, and Duchin, 2021; Grotteria, 2022).[33]

Second, we contribute to the literature that relies on campaign contributions, donations, and past employment networks to proxy for connections. In contrast, our dataset sheds light on the com-

[33]This also relates our work to the literature that seeks to estimate the value of political connections to firms. Changes in political connections relate to abnormal returns (Jayachandran, 2006; Claessens et al., 2008; Ferguson and Voth, 2008; Goldman et al., 2009; Cooper et al., 2010; Akey, 2015; Brown and Huang, 2020; Child et al., 2021), firm value (Faccio, 2006; Borisov et al., 2016), profitability and revenues (Amore and Bennedsen, 2013; Cingano and Pinotti, 2013), and investment (Cohen et al., 2011; Akey and Lewellen, 2017) A recent literature also stresses the role of partisan affiliation on firms' choices and investment decisions: see for instance Colonnelli et al. (2020) and Kempf et al. (2021).

plementary role of meetings with individual US legislators. This links our paper to work relying on the Lobbying Disclosure Act (LDA), which regulates lobbying activities of domestic interest groups. The LDA requires lobbyists to disclose the identity of the chamber of Congress or the federal agency contacted, but *does not* require disclosure of the identity of contacted persons, which we instead observe. While useful, the LDA data have left many questions unanswered regarding the identities of legislators and how intensely they were contacted (Bombardini and Trebbi, 2020; De Figueiredo and Richter, 2014). We add to the literature on domestic lobbying by providing a new and complementary direct measure of connections, allowing us to examine the scope, intensity of meetings, and their concomitant real effects on public policy for foreign governments and firms.

In addition, our work highlights a novel indirect benefit of political connections for firms headquartered in countries whose representatives meet with US legislators. Prior work has focused on the role of affinity between regions such as ancestry, social ties, and trust, that correlate with economic outcomes (Burchardi et al., 2018). We add to this literature by showing that political connections are an important determinant of local subsidies and new government contracts to foreign firms. These novel results contribute to the literature on local subsidies to firms, as well (Slattery, 2018; Slattery and Zidar, 2020). Relative to prior work, our paper provides the first evidence that political connections sway subsidies toward foreign firms whose governments meet more often with US legislators.

Third, observing meetings with individual legislators allows us to quantify benefits and costs to them. Theoretically, models of interest group lobbying link legislative benefits to the quantity of political donations (Grossman and Helpman, 1994). They also note that legislators face costs from adopting policy positions that may be unpopular with their constituents (Grossman and Helpman, 1996). Research in political science has highlighted that legislators benefit from connections to lobbyists through more frequent fundraising events (McKay, 2018) and access to political information and legislative labor (Hall and Deardorff, 2006). Our paper complements this literature by showing that meetings with foreign countries are associated with changes in the share of registered voters with ethnic affiliations to the foreign country and more frequent privately sponsored trips to that country. We also provide novel evidence that foreign lobbying imposes on average negligible costs to legislators, a result absent from the extant literature that tends to focus on political scandals (Hamel and Miller, 2019; Pereira and Waterbury, 2019).

2.2. Data

In this section, we offer a description of the datasets used in the paper.

2.2.1. Foreign Agents Registration Act

To study foreign lobbying and influence in the US, we construct a novel dataset listing all in-person meetings with exact dates between lobbyists working on behalf of foreign clients and members of the Congress.[34][35] We hand-collect data from supplemental statements filed under FARA, providing detailed information on meetings between representatives of individual foreign countries and US legislators. This comprehensive dataset allows us to examine the characteristics of US legislator or foreign countries that relate to meetings, and simultaneously provide us with the unique opportunity to investigate the scope and nature of foreign influence in US politics.

We obtain our data from over 12,000 semi-annual lobbying disclosures made under the FARA from the US DOJ. The FARA requires agents operating in the US (*foreign agents*) to register with the DOJ and file disclosures if they work on *covered activities* — political/lobbying activity or public relations — on behalf of a *foreign principal.* Of all foreign principals 87.8% are foreign governments and political parties, 7.3% are non-profit associations, whereas 6.4% are foreign corporations.[36] Importantly, FARA imposes disclosure requirements on the foreign agents who work on activities involving the public or political interests of a foreign government or political party. At the same time, these requirements exempt activities that are purely commercial or trade interests (i.e., purchase and sale of property, services, or commodities).

[34] For brevity, throughout the text, we may refer to the "meetings between the foreign agents working on behalf of foreign countries and US legislators" as "meetings between foreign countries and US legislators."

[35] Prior work on foreign lobbying focuses only on either a small subsample of meetings for specific countries over a few years or on semi-annual summary reports (Gawande, Krishna, and Robbins, 2006; Montes-Rojas, 2013; You, 2020; Lee, 2020). We also use the semi-annual reports to provide an overview of topics foreign countries lobby for in the US in the online appendix Section B.2. These reports lack information both on the the identities of individual US legislators with whom lobbyists meet and date of the individual meetings between lobbyists and legislators.

[36] Foreign principals can fit in multiple categories, e.g., state-owned enterprises. Hence, we assign foreign individuals to parties, corporations, or associations depending on their role at the time of the lobbying.

We digitize and manually transcribe the supplemental filings, creating a dataset that covers more than 180,000 in-person meetings to approximately 1,200 members of Congress by 500 unique lobbying firms.[37][38] We determine the country of origin for each foreign principal, allowing us to link each meeting with a unique foreign country.[39] This gives us 146 unique foreign countries as clients during the period 2000–2018.

We focus on question 12 and the corresponding attachments from all Supplemental Statements filed under FARA. Question 12, reproduced in Panel A of Figure 2.1, asks about political activities undertaken on behalf of foreign principals during the previous six-month period. The activities include public relations, policies sought to be influenced, any sponsored or delivered speeches, and lectures or TV broadcasts, among others. Importantly, in the corresponding attachments the lobbyist must report the date and subject of the meeting and which US legislator they met with, as seen in Panel B of Figure 2.1.

The penalties for non-compliance with FARA are quite severe. Violations are punishable by a fine of $10,000 and up to five years in prison. Since 2000, the DOJ brought 13 criminal FARA cases against 14 organizations and individuals that have reached resolutions to date.[40] This has resulted in 13 parties being convicted and 1 party having the charges dropped. Most famously, Donald Trump's former campaign manager Paul Manafort was sentenced to five years in prison for not registering his 2017 lobbying activities.

In comparison, the LDA requires lobbyists working on behalf of domestic agents to register with both the clerk of the U.S. House of Representatives and secretary of the U.S. Senate on behalf of a client. LDA also suggests that violations are punishable by a fine of up to $200,000 per violation or up to five years in prison. However, from 1995 to 2017, there have been nine LDA enforcements

[37]Examples of popular legislators are included in Table B.3 in the online appendix.

[38]The results discussed in this manuscript remain unchanged once we include e-mails and phone calls reported through FARA. These estimations were reported in the previous version of the manuscript and for brevity, in the current version, we only consider in-person meetings.

[39]For each foreign principal, we determine the country of origin using data from the World Bank and assign the associated geographical location using the International Organization for Standardization (ISO) three-letter country codes defined in ISO 3166-1. Throughout our analyses, we drop autonomous regions as they lack data on regional characteristics.

[40]Examples of cases prosecuted under FARA can be found here.

Figure 2.1: Supplemental Statement question 12

Panel A reproduces the text of question 12 as it is in the official FARA supplemental statement. Panel B shows part of the attached document D, which details meetings with US legislators. These screenshots were taken from the following supplemental statement.

(A.) Example of question 12

12. During this 6 month reporting period, have you on behalf of any foreign principal engaged in political activity[5] as defined below?
Yes ☑ No ☐

If yes, identify each such foreign principal and describe in full detail all such political activity, indicating, among other things, the relations, interests and policies sought to be influenced and the means employed to achieve this purpose. If the registrant arranged, sponsored or delivered speeches, lectures or radio and TV broadcasts, give details as to dates and places of delivery, names of speakers and subject matter.

See Attachment D

(B.) Corresponding attachment

Attachment D - Section III, # 12 **Reporting period – July 1 – December 31, 2007**

The Embassy of the People's Republic of China

Date	Office of	Met with	Issues Discussed
07-27-2007	The Speaker of the House	Jon Stivers	Chairman Wu visit
08-06-2007	The Speaker of the House	Jon Stivers	Chairman Wu visit
08-30-2007	The Speaker of the House	Jon Stivers	Chairman Wu visit
09-27-2007	The Speaker of the House	Nancy Pelosi	Chairman Wu visit
10-31-2007	House Ways & Means Committee	Jason Kearns	China-related legislation
11-29-2007	The Speaker of the House	Jon Stivers	China Bilateral relationship
12-07-2007	Senate Majority Leader	Michael Castellano	China-related legislation

settled via civil penalties of $200,000 or less, which could be related to the fact that the clerks of the House and Senate do not have the enforcement capacity of the DOJ, which oversees FARA (Thurber et al., 2019).[41]

2.2.2. Country and legislator characteristics, outcomes of interest, and other data

We combine the resulting dataset of meetings extracted from FARA supplemental filings to data from a variety of sources on foreign countries' and US legislators' characteristics. In the online appendix Section B.1, we provide details about the data and the construction of variables.

[41] Only one criminal case, outside our sample, occurred in 2020 where lobbyist Jack Abramoff pled guilty to violating registration requirements under the LDA.

Country characteristics. We collect several characteristics of foreign countries such as GDP, population, and civil violence from sources mentioned in the online appendix Section B.1. Additionally, country-level trade flows using bilateral goods trade data come from CEPII (Gaulier and Zignago, 2010; Bailey, Gupta, Hillenbrand, Kuchler, Richmond, and Stroebel, 2021). We include data on political institutions and the electoral democracy index from the Varieties of Democracy Database. We rely on a time-varying measure of each country's political preferences based on votings on resolutions in the UNGA as estimated by Bailey et al. (2017). This measure is a common proxy of bilateral distance between foreign countries' political attitude and the US. Lastly, we rely on information on the total length of international sanctions imposed by the US (Felbermayr, Kirilakha, Syropoulos, Yalcin, and Yotov, 2020; Kirikakha, Felbermayr, Syropoulos, Yalcin, and Yotov, 2021) and the total number of US presidential diplomatic visits to foreign countries (Malis and Smith, 2021).

Legislator characteristics. We then merge data on US legislators' characteristics from a variety of sources. Data on election results and party affiliations come from the MIT Elections Lab, and data on House and Senate committee and sub-committee assignments from Stewart (2017). To measure lawmaking effectiveness, we use the Legislative Effectiveness Score (LES) developed by Volden and Wiseman (2014, 2018). This measure captures the ability of legislators to advance the bills they sponsor through the legislative process. Political ideology for each legislator is measured using the dynamic weighted NOMINATE (DW-NOMINATE) score, as developed by Poole and Rosenthal (1985, 2011). Under this measure, a score closer to 1 reflects a more conservative ideology whereas a score closer to -1 reflects a more liberal ideology.

Main outcomes of interest. In Section 2.4, we examine whether meetings between foreign agents and legislators are positively associated with benefits to foreign countries or US legislators. We extract data on trade policies relating to product tariffs from GovTrack. These data contain information on the identities of the sponsoring legislators and all actions on bills including the dates and decisions taken by committees and sub-committees. Further, we also rely on data on foreign aid and financial assistance to foreign countries granted by US agencies. To quantify benefits to foreign firms, we rely on the disbursement of state and local subsidies to foreign-owned firms, and the allocation of federal government contracts to such firms.

Additionally, we employ data on the near-universe of registered voters from L2, a leading non-partisan data vendor used by political parties and the academic literature (e.g., Allcott et al., 2020, Brown and Enos, 2021, Bernstein et al., 2022, Spenkuch et al., 2021). L2 has complete coverage of the US voter population starting from 2014. The database also contains an estimate of the ethnic description of registered voters. The sample of registered-voter population for which this description is not available is about 10% of the total register voter sample. Using this dataset, we compute, for each congressional district/state each year, the share of registered voters by party affiliation and ethnic affiliation to the foreign country.

Lastly, we obtain data on all official foreign travels by members and staff of the US House of Representatives and US Senate as well as, for house members, any gift of foreign travels and travel-related expenses paid for by private individuals or entities for travel outside the House duties.[42][43]

2.3. Describing the variation in meetings

We begin by presenting an overview of the patterns in the data, which help us to motivate some features of our empirical specifications in the next section where we test whether meetings are related to changes in public policy and political outcomes.

2.3.1. Descriptive analysis

Figure 2.2 presents a yearly summary of the number of foreign countries met by each congressperson (Panel A) as well as of the number of congresspeople met by each foreign country (Panel B). From 2000 to 2018, the annual average and the median number of meetings grow for both series. Moreover, both distributions are highly skewed—e.g., the median foreign country meets on average with 11 legislators while the 90^{th} percentile foreign country meets with almost 90 legislators.

[42] Common sponsors of these gift trips include the American Israel Education Foundation, a charitable organization affiliated with AIPAC, i.e., the America's pro-Israel lobby, or The German Marshall Fund (GMF), a public policy think tank that seeks to promote cooperation and understanding between North America and the European Union.

[43] Gift of foreign travels data that include information on the destination are available for Senators only starting from 2018.

Country characteristics

Table 2.1 presents the summary statistics of the countries in our sample that have non-missing observations for all the variables we use in our analysis. The median country in our sample has a larger GDP than the median country in the World Bank database, median Log(GDP) in $ millions of 12.11 vs 9.61, and a smaller population, Log(population) in thousands of 3.44 vs 4.15. Moreover, the sample countries have a greater trade exposure to the US, e.g., Log(Exports) in $ millions are 8.04 vs 5.32 and Log(Imports) in $ millions are 8.08 vs 5.09.

In terms of institutional characteristics, the median foreign country in our sample is more politically polarized than the median country in the V-Dem database for the same period (0.075 vs -0.07) and has a slightly smaller political corruption index (0.53 vs 0.55). In terms of identities of foreign countries, online appendix figure **??** presents the heatmap of meeting frequency for specific years in the sample with varying color intensity representing the number of meetings with US legislators in a given year. Even though the data are representative of nearly every region across the globe, the countries in our sample are slightly tilted towards large economies.

Legislator characteristics

Personal. Table 2.2 reports the summary statistics for individual legislators where we collapse individual meetings at the legislator-country-year level. A foreign country holds on average 3.4 meetings every year with a given legislator. Moreover, the standard deviation in the meetings variable suggests large variation in the intensity with which they meet. When examining individual characteristics, we find that a foreign country meets with a legislator who is on average 59-year old and holds 16% of their meetings with women legislators and 8% of their meetings with underrepresented minority legislators (latin american or african american).

Political. House members represent 72% of all meetings and the average contacted legislator won their election with a vote share of 66%. Democrats account for 50.2% of the meetings with foreign countries. Table 2.2 highlights foreign countries meet more often with legislators who, on average, have served for six terms in the Congress.

Table 2.1: Summary statistics: Country characteristics

The table presents the descriptive statistics for countries in the sample. We include the following country-level economic characteristics: total GDP in US$ millions (Log), total population (Log), exports from and imports from US in US$ millions (Log). Further, we include the following country-level characteristics relating to conflicts: total unrest from the Cline Center Historical Phoenix Event Data where we separate the number of times the country was a source or a target, the use of physical force to achieve political objectives by non-state actors (political violence) and the number of coups d'état during a given year. Lastly, we include the following country-level institutional characteristics: transition to democracy comes from the Episodes of Regime Transformation (ERT) data which uses changes in electoral democracy index from the Varieties of Democracy (V-Dem) project to determine the start and end years of democratizations, electoral democracy index capturing the extent to which electoral democracy is achieved within the country, political corruption index which combines six distinct types of corruption and measures the level of corruption in a given year, political polarization capturing the extent to which political differences affect social relationships beyond political discussions, total length of international sanctions imposed by the US (Felbermayr, Kirilakha, Syropoulos, Yalcin, and Yotov, 2020; Kirikakha, Felbermayr, Syropoulos, Yalcin, and Yotov, 2021), similarity in foreign policy preferences to the US based on voting on resolutions in the United Nations General Assembly (UNGA) measured using absolute distances between the ideal points of countries (Bailey, Strezhnev, and Voeten, 2017), and the total number of US presidential diplomatic visits to the country(Malis and Smith, 2021).

	N	Mean	Median	Std. dev
	(1)	(2)	(3)	(4)
GDP (Log)	29,521	12.112	12.025	1.741
Population (Log)	29,521	3.446	3.538	1.517
US Exports (Log)	29,521	8.037	8.038	2.116
US Imports (Log)	29,521	8.081	8.127	2.448
Total unrest – source (Log)	29,521	4.277	4.511	1.414
Total unrest – target (Log)	29,521	4.223	4.431	1.363
Political violence	29,521	-0.502	-0.589	1.307
Coup	29,521	0.018	0.000	0.131
Transition to democracy	29,521	0.106	0.000	0.307
Electoral democracy index	29,521	0.471	0.461	0.259
Political corruption index	29,501	0.527	0.577	0.271
Political polarization	29,521	0.075	0.089	1.179
US-imposed sanction length (years)	29,521	9.656	0.000	15.985
Country political preference, UNGA Voting (Log)	29,521	0.983	1.110	0.324
US Presidential diplomatic visits (Log)	29,521	0.435	0.000	0.490

Ideological. Foreign countries meet equally with legislators across the ideological spectrum—both conservatives and liberals—a finding that holds when examining different definitions of political ideology. Motivated by prior work, we focus on the first dimension of the DW-NOMINATE score, DW-NOMINATE 1, which captures the economic and governmental aspects of the ideological left-right spectrum. A second dimension of the score, DW-NOMINATE 2, captures differences within the major political parties on currency, nativism, civil rights, and lifestyle issues. Both measures

Table 2.2: Summary statistics: legislator characteristics

The table presents the descriptive statistics for the sample of individual meetings at the legislator-country-year level. *Meetings* is the number of times a foreign agent and a legislator met in a given year. *Age* is the age of the legislator, *Woman* is whether the legislator is a woman, *Underrepresented minority* is whether the legislator is from an underrepresented minority group. *House member* is whether the legislator is a member of the House of Representatives, *Vote share* is the vote share in the elections, *Democrat* is an indicator capturing party affiliation, and *Seniority* is the number of terms a legislator has served in the Congress. We also include the following ideological characteristics: *DW-NOMINATE 1* and *DW-NOMINATE 2*. Lastly, we also consider characteristics that are important for influence and resource allocation. *Majority* captures whether the legislator is a member of the party in control of the Senate, *Legislative Effectiveness Score* is the lawmaking effectiveness of the legislator, *Committee chair* and *Sub-committee chair* capture whether the legislator is the chair of either a senate or house committee or a sub-committee. We also capture whether the legislator is a member of, either a senate or house committee, the following committees: (i) the rules, ways and means, and appropriations, (ii) foreign affairs, (iii) Security & Intelligence, (iv) Armed Services, and (v) Energy & Commerce.

	N	Mean	Median	Std. dev
	(1)	(2)	(3)	(4)
Meetings	36,555	3.454	2.000	5.051
		Personal		
Woman	36,555	0.157	0.000	0.364
Underrepresented minority	36,555	0.082	0.000	0.274
Age	36,555	58.99	59.00	10.54
		Political		
House member	36,555	0.720	1.000	0.449
Vote share	36,555	66.00	63.00	12.74
Democrat	36,555	0.496	0.000	0.500
Seniority	36,555	6.225	5.000	4.647
		Ideological		
DW-NOMINATE 1	36,555	0.045	0.091	0.430
DW-NOMINATE 2	36,555	-0.042	-0.053	0.292
		Importance for influence		
Majority	36,555	0.539	1.000	0.499
Legislative Effectiveness Score	36,555	1.064	0.646	1.351
Committee chair	36,555	0.092	0.000	0.289
Sub-committee chair	36,555	0.283	0.000	0.451
Power committee membership	36,555	0.400	0.000	0.490
Foreign affairs membership	36,555	0.255	0.000	0.436
Security & Intelligence membership	36,555	0.175	0.000	0.380
Armed services membership	36,555	0.170	0.000	0.375
Energy & Commerce membership	36,555	0.131	0.000	0.337

have average values close to zero, implying foreign countries meet equally with legislators also across the ideological spectrum.

As an example, Figure 2.3 presents the contact pattern of foreign agents on behalf of the government of Turkey over time, where a contact is defined as a year-month with at least one meeting between a foreign agent and a legislator. The horizontal axis indicates the date of contact and the vertical axis indicates the DW-NOMINATE 1 score of the legislator they meet with. Each dot in the figure represents a contact. From the figure, we note that Turkey meets with legislators from both parties and across the ideological spectrum. This pattern is not unique to Turkey, but it is the norm. In our sample, foreign countries meet with legislators across different political ideologies and party affiliations consistently over time.

Legislator's importance for influence. Table 2.2 shows that foreign countries meet more often with legislators that belong to the party in control of the Senate. We also note that a foreign country meets with a legislator with an average legislative effectiveness score of 1.06. This score is approximately the cutoff for effectiveness in the top tercile among all legislators, suggesting that countries meet with legislators who effectively sponsor and advance bills through the legislative process.

Figure 2.4 plots the evolution of meetings with effective lawmakers. The horizontal axis indicates the meeting year, as the lawmaker effectiveness scores are only available at the annual frequency. The vertical axis plots the fraction of meetings with the most effective lawmakers relative to all the legislators a foreign country meets in a year. We consider three definitions of "most effective lawmakers"—top 5%, top 10%, and top 20% of legislators by LES score. Though not entirely unexpected, foreign countries meet relatively more often with the most effective legislators. For example, the fraction of meetings attributable to the most effective 20% of legislators is almost always larger than 20%, with a minimum value of 19.42% in 2011. This result, that on average foreign countries meet more frequently with the most effective legislators, holds across definitions of effectiveness.

At the same time, Table 2.2 shows a foreign country has on average nearly 30% of their meetings with sub-committee chairs and 40% of their meetings with members of "power" committees, which groups together the rules, ways and means, and appropriations committees (Volden and Wiseman, 2014) . More importantly, 25% of the meetings are with members of the foreign affairs committee alone. Members of the armed forces (security and intelligence) committee account for 17% (17.5%)

of meetings with foreign agents. Meetings with members of energy and commerce committee are fewer at 13%.

The average values reported in Table 2.2 hide substantial time variation in the meetings with a given committee in a year. Panel A of Figure 2.5 plots the percentage of meetings with members of foreign affairs, armed forces, security and intelligence, and energy and commerce committees over time. Foreign countries meet more often with members of the foreign affairs committee, which increased by 10 percentage points over the sample period, and passed from 30% to 40% of all meetings in a given year. During the same period, meetings with Armed services and Security & Intelligence committee members more than doubled accounting for 10% of all meetings in 2000 and 20% of all meetings in 2018. Meetings with members of Energy & Commerce saw only a modest increase.

To understand the importance of committee chairs to foreign countries, for each committee-year we compute the number of meetings with chairs and the average number of meetings with members in the same committee. We then scale meetings with chair by the average number of meetings with members in the same committee in that year. This accounts for differences in committee sizes and highlights the importance of meetings with the committee chair relative to meetings with an average committee member. In Panel B of Figure 2.5 we plot this series over time for foreign affairs, armed forces, security and intelligence, and energy and commerce committees and we note considerable fluctuations for all committees. The figure suggests that foreign countries increase their meeting intensity with chairs in specific years, e.g., in 2015 they meet with the Security & Intelligence committee chairs 300 times more often than the average member of the same committee. We observe large fluctuations in the series of other committees as well.

Overall, these descriptive statistics are informative of the variation in the country and legislator characteristics that we explore in detail below. Our findings provide novel insights into the nature and scope of legislator links with foreign countries. We next proceed to relate meetings with country and legislator characteristics to understand which characteristics matter for connections with foreign countries.

2.3.2. Which countries use FARA to lobby US legislators?

Table 2.3 relates country characteristics to an indicator variable capturing whether a country lobbies US legislators in a given year. This analysis can inform about upfront costs of engaging in lobbying, which have been carefully studied in other contexts, such as domestic firm lobbying (Kerr et al., 2014). In particular, we estimate:

$$\mathbb{1}(Meetings > 0)_{ft} = \gamma_f + \delta_t + \beta \text{Country characteristics}_{ft-1} + \epsilon_{ft}, \tag{2.3.1}$$

where f represents the country for which the foreign agent is lobbying, and t represents the meeting year. The unit of observation is a foreign country-year dyad. The empirical specification includes country fixed effects to control for unobserved time-invariant regional characteristics in addition to year fixed effects to allow for macroeconomic fluctuations. We cluster standard errors at the country-level (Bertrand, Duflo, and Mullainathan, 2004).

In column 1, we find that, on average, more populated countries and those with larger trade exposures to the US are more likely to lobby, consistent with the idea that they have larger incentives. When we relate meetings to conflict (column 2), we find that countries that have been target of conflicts are more likely to lobby US legislators. Similarly, regarding institutional characteristics (column 3), countries with more diplomatic visits from US presidents and countries that have been target of more severe sanctions imposed by the US are more likely to lobby US legislators. In column 4, we combine all the characteristics together to account for cross-correlations and find similar results.

In column 5, exploiting within-country changes in characteristics, we find that strengthening diplomatic and trade relationships with the US is associated with the decision to lobby US legislators. Lastly, in the online appendix Table B.12, we show that for house members there is a higher probability to meet with the representatives of a foreign country if a more significant fraction of her electoral base was born in that foreign country. These results provide novel insights into the role of social ties and ancestry in explaining political connections between foreign countries and

US legislators (Burchardi, Chaney, and Hassan, 2018; Bursztyn, Chaney, Hassan, and Rao, 2021; Burchardi and Hassan, 2013).

2.3.3. Which legislators meet foreign agents more often?

Having established which countries lobby US legislators, we now shed light on the relative importance of legislator characteristics in explaining meeting frequency with those countries. We include country-by-year fixed effects and use variation in the characteristics of legislators. In particular, we estimate:

$$\log(\text{meetings})_{lft} = \gamma_{ft} + \delta_l + \beta \text{Legislator characteristics}_{lt} + \epsilon_{lft}, \tag{2.3.2}$$

where l represents the legislator being lobbied for, f represents the country for which the foreign agent is lobbying, and t represents the meeting year. The unit of observation is a legislator-foreign country-year triad and, as before, we cluster standard errors at the country-level.

Table 2.4 presents the estimates from the regression. Column 1 relates meeting intensity to the political characteristics of legislators without controlling for time-invariant legislator characteristics. We find that, on average, a foreign country meets more often with more senior legislators who win by larger margins. Interestingly, foreign countries meet less often with legislators if they represent the party that controls the Senate. Finally, consistent with the descriptive analyses, foreign countries meet with legislators irrespective of party affiliation.

Turning to ideological characteristics, in column 2 we find that meeting intensity is negatively correlated with the legislator's political ideology. In column 3, we focus on legislator characteristics that may influence resource allocation and public policy for foreign entities. We do not find a statistically significant relationship between meeting intensity and the legislator's status as committee chair. However, we observe a positive relationship with the following committee memberships: Power committees, Foreign affairs, and Security and Intelligence.

Column 4 presents the empirical specification including all characteristics at once. We omit the "Democrat" indicator variable, as it is highly negatively correlated, -94%, with the DW-NOMINATE score. When considering the characteristics jointly, we find that the relative importance of a legislator, captured by the LES, and whether the legislator is a chairperson of a subcommittee is positively related to more meetings with foreign agents. Membership of power committees remains a significant correlate of meeting intensity together with membership of foreign affairs and security and intelligence committee. Finally, more liberal legislators (DW-NOMINATE 1) meet more often with foreign agents. In sum, these associations highlight the relevance of political ideology and legislative effectiveness, as well as committee membership for meetings with foreign agents.

Moreover, we are interested in understanding whether conditional on deciding whom to meet with, changes in legislator characteristics relate to meetings intensity. Such an analysis is informative on which characteristics matter for a connection to persist, a question previously unexplored in the literature. To do so, column 5 adds legislator fixed effects to our previous empirical specification and relates *within* legislator changes in characteristics to changes in meeting intensity. The results suggest an increase in a legislator's effectiveness, an increase in their vote margins, and becoming a member of the foreign affairs committee are all positively related to an increase in meetings. Moreover, we find that foreign agents meet more often with legislators when they become senators, potentially due to an increase in the length of their term. Interestingly, changes in legislator ideology (DW-NOMINATE 2), capturing individuals who become more conservative on social issues such as immigration, are positively correlated to changes in meeting intensity. Further, in the online appendix Table B.4, we show that these results are robust to Poisson estimation.

Overall, our findings provide new observations that meetings are associated with the effectiveness of the legislators, their status as a member of specific important committees, in particular the foreign affairs committee, and their ideology.

2.3.4. The role of committee assignments

Next, we explore whether committee assignments can explain meetings between foreign countries and US legislators. Building on the observations described above, we focus on the foreign affairs, and the security and intelligence house and senate committees as well as important committees that in prior work have been shown to influence resource allocation in the US (Cohen, Coval, and Malloy, 2011; Brogaard, Denes, and Duchin, 2021).[44]

We test whether foreign agents meet *less often* with legislators after they depart from important committees, holding constant the relative importance of the committee to foreign countries. In Table 2.5, we examine changes in meeting intensity around the time of departure of legislators from important committees. The committee assignments for legislators are available at the monthly level.

Given the granularity of our data we can account for several confounding factors. We control for local economic confounds through the inclusion of state-by-year-month fixed effects and for differences across Congresses that may influence meetings with legislators through the inclusion of Congress fixed effects. Our empirical specifications hold constant the relative importance of departing committees for foreign countries by including country-by-committee fixed effects. Additionally, we include legislator-by-committee fixed effects to control for influential legislators departing from the same committee at different points in their tenure. Finally, we also consider changes in meeting intensity can be driven by the relevant importance of the issue to a lobbyist rather than a foreign country (Bertrand, Bombardini, and Trebbi, 2014).[45] To do so, in some specifications we include lobbying firm fixed effects and account for lobbying firm switching issues in a predictable way when a legislator departs from a committee.

We organize our analyses at the committee-lobbying firm-month level. Results in Table 2.5 suggest that foreign countries continue to meet with *all* legislators even after they leave important committees. These results are inconsistent with the prediction of the short-term "quid-pro-quo channel" according to which foreign countries only meet politicians sitting in important committees in that specific moment. Our results are instead consistent with two alternative hypotheses: (1) an "information channel" that predicts that lobbyists and legislators exchange useful information, hence

[44]The committees influencing resource allocation include budgetary and oversight committees in the House of Representatives and the Senate: House Committee on Appropriations; House Committee on Oversight and Reform; House Committee on Armed Services; House Committee on the Budget; House Committee on Transportation and Infrastructure; House Committee on Energy and Commerce; Senate Committee on Appropriations; Senate Committee on Homeland Security and Governmental Affairs; Senate Committee on the Budget; Senate Committee on Commerce, Science, and Transportation; and Senate Committee on Energy and Natural Resources.

[45]The granularity of our data allows us to account for this explanation. Specifically, our sample consists of 500 unique lobbying firms with the median firm working on two topics on behalf of three foreign countries.

they continue to meet as frequently even if legislators leave these committees; (2) a quid-pro-quo channel that operates with long-term relations, e.g., legislators even after leaving a committee may be able to influence their colleagues, or may ascend in the future to even higher positions.

Overall, our results speak to the importance that maintaining connections with *all* legislators is essential to foreign countries beyond the committee to which legislators are assigned at a given moment.

2.4. Benefits to foreign countries and legislators around meetings

Empirical specification. In this section, we examine benefits that accrue to foreign countries whose agents meet more often with legislators as well as benefits to US legislators. We separate benefits into 3 categories: direct benefits to foreign countries including foreign aid and financial assistance and beneficial changes in US product tariffs; indirect benefits that accrue to firms headquartered in countries whose agents meet with legislators; direct benefits to legislators measured by the share of registered voters with ethnic affiliations to a foreign country the legislator meets often or privately sponsored trips outside the office duties.

For each of the above-mentioned benefits, using panel regressions, we relate meetings between legislators and foreign countries to the benefit. We study two margins of adjustment, and estimate the following panel regressions:

$$\mathbb{1}\{\text{Benefit} > 0\}_{lsft} = \gamma_f + \delta_{st} + \beta \text{meetings}_{lsft} + \eta \text{Controls}_{ft} + \epsilon_{lsft} \tag{2.4.1}$$

$$\log(\text{Benefit}_{lsft}) = \gamma_f + \delta_{st} + \beta \text{meetings}_{lsft} + \eta \text{Controls}_{ft} + \epsilon_{lsft}, \tag{2.4.2}$$

where l represents the legislator met with, s represents the state associated with the legislator, f represents the foreign country whose agents meet with the legislator, and t represents the meeting year.

As we are interested in studying both the intensive and the extensive margins of adjustments, we work with a balanced panel. Equation (2.4.1) quantifies the extensive margin, i.e., increase in the probability of the benefit of interest, and Equation (2.4.2) quantifies the intensive margin, i.e., increase in the value of the benefit. The coefficient of interest is β, identified by variation in meetings between multiple foreign countries and legislators. Standard errors are corrected for heteroscedasticity and autocorrelation and clustered at the country-level.

The empirical specification allows us to rule out concerns about location-specific and country-specific effects that may affect outcome variables for two reasons. First, state-by-year fixed effects are included to control for local economic confounds (e.g., state or regional macroeconomic trends) and general policies that potentially affect meetings or benefits. Second, country fixed effects are added to control for time-invariant country characteristics that may simultaneously drive meetings or the benefits to foreign countries or US legislators.

We also explore the robustness of our findings and examine whether the pattern holds up under a variety of specifications. When possible, we employ panel estimations at year-month level to rule out differences in timing between meetings and benefits. We also employ congressional-district level regressions and use legislator fixed effects to confirm whether meetings with foreign countries representatives indeed positively relate to benefits.[46]

Finally, while the panel regressions are informative of the association between meetings and benefits, it is unclear whether omitted factors and reverse causality drive the relationship. To circumvent such issues, we examine changes in benefits to foreign countries that unexpectedly lose a connection due to the death of a representative or a senator. Under the null hypothesis that connections to the legislators do not matter, then the loss through deaths should be unrelated to benefits. Note that we can perform such an analysis only in instances where we can cleanly link individual legislator decisions to benefits. We reject this null hypothesis and provide evidence that larger benefits accrue to foreign countries whose representatives meet often with legislators, conditional on being *politically active*.

[46] In online appendix Section B.3, we further show robustness documenting that meetings between representatives of a foreign country and a legislator are associated with more official trips by the same legislator to that foreign country and more Twitter mentions by the legislator about the foreign country.

It is unclear whether the null hypothesis should be one of zero benefits to each party. Our sample by construction relies on realized meetings, where both parties have decided and agreed to meet, suggesting that in expectation, there are likely positive benefits to each party. Yet, our results provide novel evidence on the types of benefits to foreign countries and US legislators and, importantly, offer a first-order quantification informing economists, political scientists, and US voters.

2.4.1. Direct benefits to foreign countries

Foreign aid. We begin by focusing our analyses on resource allocation to foreign countries themselves. Here, we rely on foreign aid given its importance in winning support in major international affairs, maintaining political regimes, or strengthening international alliances (Alesina and Dollar, 2000). Further, prior work highlights the importance of political relations in determining foreign aid and assistance (Kuziemko and Werker, 2006; Sims, 1980). Hence, foreign aid offers a setting to examine the importance of meetings for resource allocation to foreign countries and an easy one where to quantify benefits.[47] Results are reported in Table 2.6.

Column 1 reports the results with only state-year fixed effects, finding that an increase in meetings with a legislator is positively associated with receiving aid and assistance from the US. On the intensive margin, meetings are associated with larger aid and assistance. These results are robust to adding controls, country fixed effects, and legislator fixed effects. We examine the relationship between meetings and foreign aid at the year-month level, as reported in online appendix Table B.5, and show that differences in timing between meetings and aid do not explain our results. Our results provide novel evidence that more and larger foreign aid are assigned to countries whose representatives meet more often with US legislators.

Trade policy. A large literature in economics proposes an important role for interest groups in the determination of trade policy (Grossman and Helpman, 1994). Much of this work has focused on domestic lobbying groups, but more recent work points to a disproportionate influence of foreign lobbying for trade policies (Hillman and Ursprung, 1988; Gawande, Krishna, and Robbins, 2006; Antràs and i Miquel, 2011). Given this, we focus on tariff bills that were ever sponsored in the

[47] Prior work has highlighted the role of legislators in influencing federal agencies in the allocation of public resources (Brogaard, Denes, and Duchin, 2021).

Congress during our sample period. We manually classify bills as "favourable" ("unfavourable") to a particular foreign country depending on whether they propose lower (higher) product tariffs within specific trade agreements. We study actions on bills starting from the date at which the bill is sponsored by a legislator and follow the bills through their evolution within committees and Congress.

In our empirical specification, we examine two outcomes of interest. First, we study whether meetings relate to a legislator's propensity to sponsor a bill that is favourable to a foreign country. Second, we examine whether meetings between foreign countries and legislators sitting in a given committee are related to a larger probability that a favourable bill passes that committee or an unfavourable bill does not advance that specific committee.

We organize our analyses at the legislator-committee-foreign country-year-month level, allowing us to account for several confounding factors. The empirical specifications hold constant the relative importance of committees over time by including committee-by-year fixed effects. Additionally, we include legislator fixed effects to control for time-invariant legislator characteristics. Finally, we add country-by-year fixed effects to control for time-varying determinants of trade relationships.

Table 2.7 reports the results. In column 1, we find that more meetings in a given year-month are associated with a larger probability of a favourable bill being sponsored. This increase translates to a change from a baseline probability of 0.039% to 0.040% for every new meeting between foreign agents and legislators. In column 2, we focus on the sub-sample of effective legislators, defined as those that are above-median LES, and find that the estimates are similar for these legislators. In column 3, we find that more meetings with a legislator in a given committee are associated with a higher probability of the favourable bill passing that committee or an unfavourable bill not advancing that committee. Lastly, in column 4, as before, we find that the sensitivity of actions to meetings is similar across more and less effective lawmakers. Overall, these estimates suggest that the sensitivity of actions to meetings is perhaps large, which can be potentially explained by a very small baseline probability of action.

2.4.2. Indirect benefits to foreign firms

Next, we examine the indirect benefits to firms from countries that meet more often with legislators. The ability to capture indirect benefits is a unique feature of our comprehensive dataset. Specifically, meetings by country representatives with US legislators could influence resource allocation to foreign firms by reducing information asymmetries. We examine two settings: (1) state and local subsidies and (2) government contracts, which allow us to cleanly link meetings with legislators to increased resource allocation.

State and local subsidies. As a starting point, we focus on subsidies for two reasons. First, state and local governments spend billions of dollars in subsidies each year to attract and retain firms, and local legislators use considerable discretion to allocate subsidies (Slattery, 2018; Slattery and Zidar, 2020).[48] Second, the localized nature of subsidies helps us to cleanly link meetings of foreign countries with individual US legislators to the subsidy received by foreign firms in that location. Together, these facts make subsidy allocation a nearly ideal setting to examine whether meetings and connections to legislators are associated to more frequent and larger subsidies.

Table 2.8 reports estimates from panel regressions examining both the extensive and the intensive margins. Column 1 reports the results with only state-year fixed effects, finding that an increase in meetings with a legislator is positively associated with receiving a subsidy from the state of the legislator. On the intensive margin, meetings are associated with larger subsidies. These results are robust to adding controls, country fixed effects, and legislator fixed effects.

In the online appendix Table B.6, we show that our results are robust to examining this relationship at the congressional district level instead of the state. Across an identical set of empirical specifications as in Table 2.8, we find that more meetings with a representative in a congressional district are positively associated with greater subsidies. While encouraging, the results at the congressional district level come with two caveats. First, we extract the cities awarding the subsidy and link them

[48] Slattery and Zidar (2020) note that in 2014, states spent \$5-\$216 per capita on incentives for firms. The total state and local incentive spending amounted to at least \$30 billion, with the average discretionary subsidy to the tune of \$178 million. Moreover, discretionary subsidies are roughly 1/4 of total incentive spending within a state, thus highlighting the importance of legislator discretion in the subsidy allocation process.

to the congressional districts of the representatives using zip codes.[49] In doing so, when zip codes are associated with more than one congressional district, we apportion the subsidy across them equally. Second, these analyses can only be conducted for house of representatives, as senators run for the office in statewide elections. Even with these sources of noise, we consistently find a positive relationship between meetings and subsidy allocation.

Government contracts. Next, we focus on federal government contracts as prior work highlights the role of political influence on initial contract allocation and renegotiation. Given the importance of political influence in this setting, we investigate whether foreign influence is a determinant of government contract allocation.

We begin by estimating panel regressions and report the results in Table 2.9. The specification in Column 1 includes only state-year fixed effects, finding that an increase in meetings with a legislator is positively associated with receiving new government contracts administered in the state of the legislator. On the intensive margin, meetings are associated with larger contract values. These results are robust to adding controls, country fixed effects, and legislator fixed effects. In the online appendix Table B.7, we show that our results are robust to examining this relationship at the year-month level.

Interestingly, the sensitivity of meetings to contract allocation is lower than it is on subsidies. One potential explanation for the lower sensitivity is that it reflects differences in the flexibility of the local legislator to influence state subsidies relative to federal government contracts. In particular, state subsidies are determined at the state level, hence meetings with a legislator can have large sway over their outcomes. In contrast, federal government contracts are allocated at the congress-level, meaning that meetings with any single legislator may matter less. Nevertheless, meeting with key legislators in important budgetary and oversight committees might still be important and ultimately influence contract allocation.

[49] We use the crosswalk provided by the US Census Bureau for the 108^{th} Congress to 111^{th} Congress. For Congresses starting after 2010, we use the crosswalks provided by the US Department of Housing & Urban Development.

2.4.3. Deaths of legislators as shocks to connections

While the panel regressions are informative of the association between meetings and resource allocation, it is unclear whether omitted factors and reverse causality drive the relationship. For instance, in the case of aid, there is a possibility that a country with more aid at stake tends to meet more often with legislators. To mitigate general concerns that unobserved factors that affect both meeting intensity and resource allocation, we study countries that unexpectedly lose a connection due to the death of a representative or senator.[50] The null hypothesis is that if meetings or connections to the legislators do not matter, then the loss of a connection through deaths should be unrelated to resource allocation. Thus, the identification strategy estimates the effect of losing a connection by utilizing the clean link between local political activity and resource allocation, holding constant country-level and local economic conditions.

In particular, our empirical analyses compare the outcomes for a country exogenously losing a political connection (treated) relative to another country losing a political connection later (control). This analysis exploits the differences in the timing of losing political connection due to deaths. As before with panel regressions, we hold constant time-invariant unobservable country and time-varying location-specific characteristics by including country fixed effects and state-by-year fixed effects, respectively. Moreover, we restrict our analysis to outcomes within one year around the death of the legislator to mitigate issues related to overlapping election cycles.

We estimate the following difference-in-differences specification:

$$\mathbb{1}\{\text{Outcome} > 0\}_{sft} = \gamma_f + \delta_{st} + \beta \text{Lost connection}_{sf} \times \text{After}_t + \epsilon_{sft} \tag{2.4.3}$$

$$\log(\text{Outcome amount}_{sft}) = \gamma_f + \delta_{st} + \beta \text{Lost connection}_{sf} \times \text{After}_t + \epsilon_{sft}, \tag{2.4.4}$$

where Equation (2.4.3) quantifies the extensive margin and Equation (2.4.4) the intensive margin.

[50]Deaths have been used to identify importance and ascribe value in several contexts including political ties (Faccio and Parsley, 2009; Brogaard, Denes, and Duchin, 2021), independent directors (Nguyen and Nielsen, 2010), executives and CEOs (Johnson, Magee, Nagarajan, and Newman, 1985; Bennedsen, Pérez-González, and Wolfenzon, 2020; Fee, Hadlock, and Pierce, 2013). We focus on deaths of legislators who are most effective in lawmaking, defined as those with above-median LES in the death sample, as they drive all the variation in outcomes of interest.

Here, the coefficient of interest is β which can be interpreted as the effect of losing a political connection (treatment effect) conditional on being politically active and the set of fixed effects. Standard errors are double clustered at the country-event level and state or congressional-district level, depending on the unit of the analysis.

We examine foreign aid and assistance granted by federal agencies in the US around deaths of legislators. We report results from this exercise in online appendix Table B.8. The estimates imply that countries that lose a connection with a legislator through death are 10 percentage points less likely to receive an aid and the aid amount they receive are 90 percent lower. Relative to the average aid value of $795 million, this loss represents a total drop in foreign aid of $715 million, translating to a per-meeting loss of $12.8 million. Thus, the loss of a connection substantially reduces the foreign aid that the connected foreign countries receive.

In online appendix Table B.9 we report our analyses regarding the allocation of local and state subsidies using legislator deaths as a shock to connections. The estimates imply that firms whose countries lose a connection with a legislator through death are 6 percentage points less likely to receive a subsidy and the subsidies they receive are 67 percent lower. Relative to the average subsidy value of $26 million, this loss represents a total drop in subsidy value of $17.4 million, translating to a per-meeting loss of $311,535.

We confirm that these results hold when we repeat the analyses at the congressional-district level.[51] The results are reported in online appendix Table B.10, which shows that the magnitude is very similar to our baseline estimate, suggesting the importance of political connections in influencing resource allocation. Thus, the loss of the connection of country representatives substantially reduces indirect benefits in the form of local subsidies that the foreign firms receive. Overall, our findings provide novel evidence of indirect benefits to firms when their country representatives meet more often with US legislators.

Finally, Table B.11 shows the results regarding the allocation of government contracts using leg-

[51] In these tests, we exploit variation among foreign countries connected to two legislators but unexpectedly lose connection to one. We do this by the inclusion of district-by-year fixed effects.

islator deaths as a shock to connections. The estimates imply that firms whose countries lose a connection with a legislator through death are 6.7 percentage points less likely to receive a new government contract and the contract value they receive is 48 percent lower. Relative to the average value of $3.4 million, this loss represents a total drop in value of about $2 million, translating to a per-meeting loss of $32,000. Collectively, our findings establish that meetings affect resource allocation indirectly for firms in the form of government contracts.

2.4.4. Benefits to US legislators

Having documented benefits to countries around the time in which their representatives meet with US legislators, we now move on to analyse whether legislators also benefit around meetings with foreign country representatives. If legislators need to exert some efforts (i.e., meeting is *costly*), or there are potential risks involved in meeting with foreign country representatives, then in equilibrium we should also observe net benefits to legislators conditional on both parties agreeing to meet.

Voter registration by ethnicity. We analyse changes in voter registration at the state and congressional-district level, using a novel dataset that records voter registration for different ethnic groups, for the near-universe of the US voting population. Unconditionally, 1.80% of voters switch from republican to democrat or vice versa (Engelberg et al., 2022).[52] In particular, for each state and congressional district we count the numbers of voters who are registered with the democratic party and divide this value by the total number of voters of a given ethnicity in the same area. We show our results in a panel regression separating between senators and members of the House of representatives. Specifically, we relate the number of meetings between an individual legislator and a foreign country to changes in the share of voters registered for the legislator's political party and who share ethnic affiliation to that foreign country.[53]

Table 2.10 presents the results. Column 1 focuses on Senators and finds a higher sensitivity of meetings with representative of foreign countries to the share of registered voters in their state. We find a similar higher sensitivity when we focus on House members in column. The empirical

[52]It should be noted that the significance of this percentage may vary depending on the competitiveness of the election. In particularly close elections, this number could potentially be a deciding factor for one of the candidates.

[53]The data provides a very granular disaggregation of political affiliation of individuals. Hence, when computing the share of registered voters, we consider all affiliations including *independent*, *non-partisan*, and *unknown*.

specification controls for time-varying national-level confounds that can explain these patterns. Moreover, country fixed effects exploit within country changes in meeting propensity and hence rules out concerns regarding time-invariant country characteristics that simultaneously drive meetings and the share of registered democrats of a given ethnicity.

Privately-sponsored trips to foreign countries. Next, we examine legislators' trips sponsored by private organizations and interest groups to foreign countries around meetings with foreign country representatives. These trips have been shown to correlate positively with legislative effectiveness, providing legislators with more policy-relevant information, and help in building legislative coalitions in domestic politics (McGee and Moniz, 2021).

Table 2.11 presents the results. We study both the extensive and the intensive margins of privately-sponsored trips. In all specifications we include country fixed effects to control for time-invariant country characteristics and state-by-year-month fixed effects for time-varying local economic confounds. We add legislator fixed effects to control for time-invariant legislator characteristics. We find that more meetings are significantly related to (a) a larger probability of trips to that foreign country whose representatives the legislator meet more often, and (b) to a longer stay in the country.

2.5. Are there costs to US legislators?

We find that, on average, costs to US legislators are economically small. We focus on incumbent legislators who were in Congress at least once between 2000 and 2018. We study aggregate political contributions made by individuals to these legislators in the election years during their political campaigns. Moreover, we test whether meetings between legislators and foreign country representatives influence the likelihood that an incumbent legislator wins an election in a specific election year. In both cases, our findings are consistent with costs being on average negligible for legislators.

Political contributions. If politicians do not enact policies aligned with the interests of their constituents, then they might receive lower political contributions from them. To test whether it is indeed the case that politicians who meet more frequently with the representatives of foreign countries end up receiving fewer contributions from individual constituents, we relate both variables in a panel regression that includes legislator and state-by-year fixed effects to control for time-invariant legislator characteristics and time-varying location-specific characteristics. We also control for the total amount of money the candidate receives (i.e., contributions from individuals, party committees, and other political committees).

Table 2.12 shows the results. In column 1, we find that the elasticity of political contributions to meetings is zero. We also exploit differences in foreign country characteristics to further probe the nature of the relationship between meetings and political contributions. In column 2, we find that the elasticity of political contributions by individuals to legislators' meetings with more corrupt foreign countries is lower than the elasticity of political contributions by individuals to legislators' meetings with less corrupt countries. Similarly, in column 3, we find that meetings between legislators and representatives of more democratic countries relate to a higher elasticity of political contributions from individual constituents to meetings. However, we note that the effect is economically small and significant only at the 10% level.

Re-election. Another potential cost to politicians is that a failure to enact policies aligned with constituency interests may result in them being voted out of office during the subsequent election. We evaluate whether meetings between legislators and foreign country representatives influence the likelihood that an incumbent legislator stays longer in office. We focus on the sample of legislators who re-run for office. Accordingly, we employ a hazard model and relate meetings and legislator characteristics to the time they exit their office (Cox, 1972). Table 2.13 finds that more meetings are actually positively associated with the likelihood that an incumbent legislator stays longer in the office.[54]

These findings suggest that meetings between foreign country representatives and legislators have, if anything, an economically small cost on average. One reason underlying these results on economically small costs could be that meetings are set strategically, with both parties deciding and agreeing to meet. Therefore, when a meeting is expected to be too costly for the legislator's reputation, they may decide not to meet with the foreign representatives. One implication is that our sample focuses on meetings that have happened, with the implicit assumption that the legislators

[54]The coefficients in the Cox (1972) regression relate to hazard, i.e., a negative coefficient implies more meetings are associated with a longer stay in office.

expect larger benefits than costs from these meetings.

2.6. Concluding remarks

We introduce a new comprehensive dataset allowing us to study foreign lobbying and foreign influence in the US. Using 180,000 date-stamped in-person meetings between foreign country representatives and US legislators, we show that countries that lobby are important trade partners to the US, receive more diplomatic visits by the US presidents and at the same time experience more political violence and have more corruption relative to non-lobbying countries. Among legislator characteristics, lawmaking effectiveness, and membership of foreign affairs committee are important correlates of these connections.

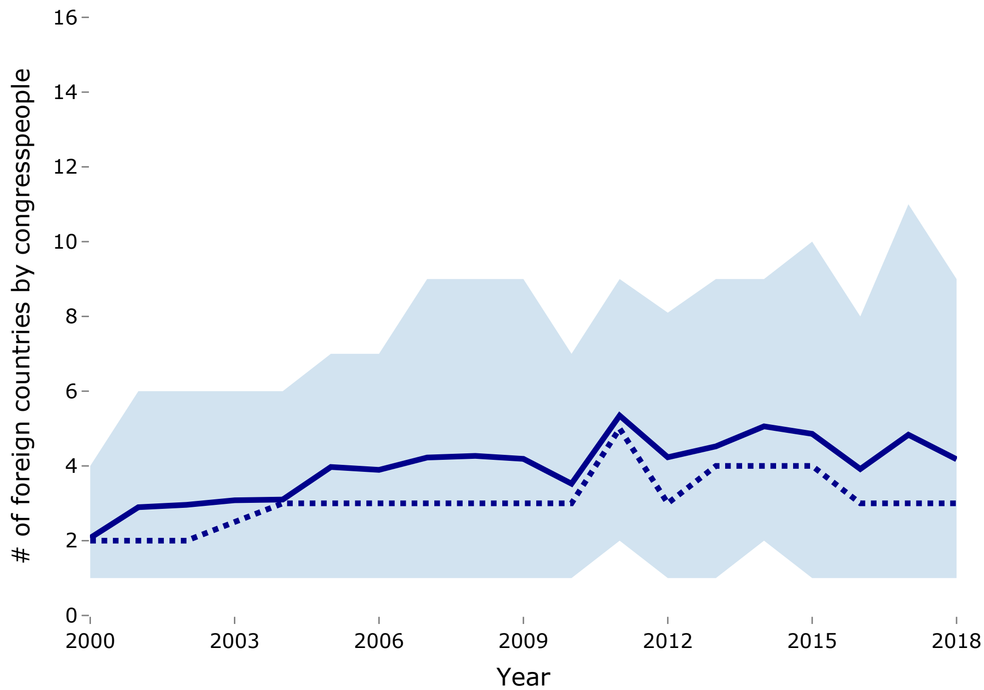
Using this new dataset, we quantify benefits and costs to foreign countries and legislators. Specifically, we document (a) an increase in foreign aid and financial assistance, (b) lower product tariffs, and (c) more corporate subsidies and government contracts to firms headquartered in the countries whose agents meet more often with legislators. We also document more meetings between representatives of a given foreign country and US legislators are related to (d) an increase in the share of registered voters with ethnic affiliations to the foreign country and (e) an increase in foreign trips to these countries sponsored by private organizations. Finally, we document that (f) costs to US legislators are economically small. Overall, our study provides novel insights on the nature and scope of foreign lobbying in US politics.

Understanding how access to legislators is gained and distributed in the economy is an important question of practical and theoretical relevance. From a positive perspective, our study highlights the determinants of connections between foreign countries and legislators and examines the sources of influence for public policy. From a normative perspective, our paper's findings can guide efforts to design more effective political institutions. Lastly, our dataset provides new observations that can be used to inform the selection of alternative theoretical models of lobbying and we expect it to be useful to a large community of scholars in political economics and public finance.

Figure 2.2: The time series of meetings

The figure presents the yearly summary of the number of foreign principals whose representatives met with each congressperson (Panel A) as well as of the number of congresspeople who meet with the representative of each foreign principal (Panel B). The solid line represents the time series of the yearly average, the dashed line is the median, and the extremes of the shaded area are the 10^{th} and 90^{th} percentile.

(A.) Number of foreign countries meeting with each congressperson



(B.) Number of congresspeople meeting with each foreign country

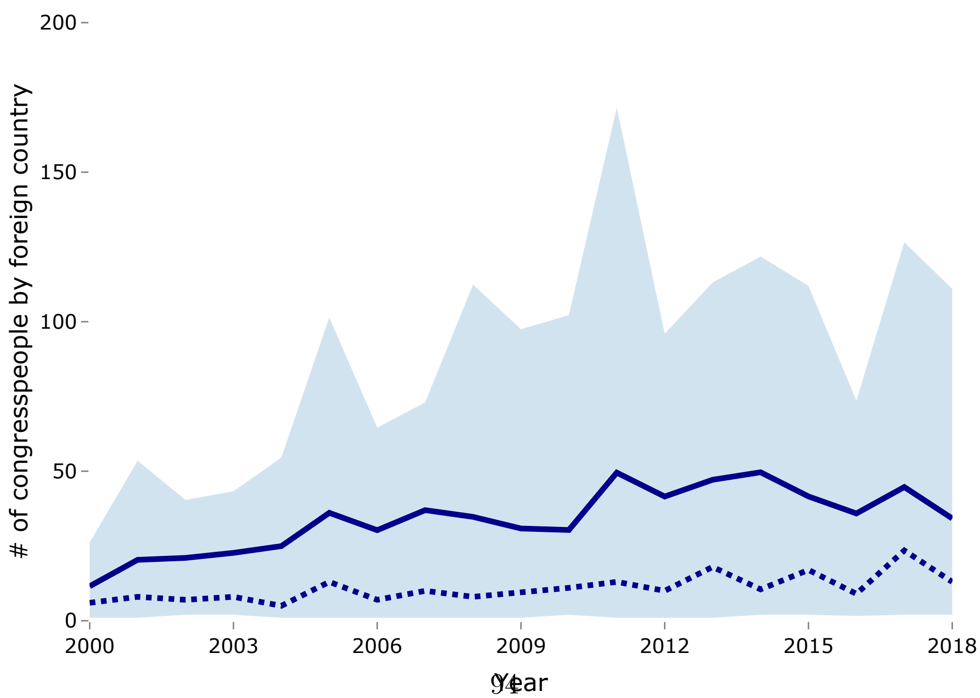


Figure 2.3: Meetings with congresspeople by party affiliation, Turkey

The figure shows the contact pattern over time for the government of Turkey. A contact is defined as a year-month with at least one meeting between a representative of a foreign country and a legislator. Each dot in the figure represents a contact. meetings with republican legislators are shown as red squares, with democrats as blue circles, and with independents as violet triangles. The shaded area in the background is blue if democrats had the majority in the Senate. The vertical axis indicates the DW-NOMINATE 1 score from Poole and Rosenthal (2011).

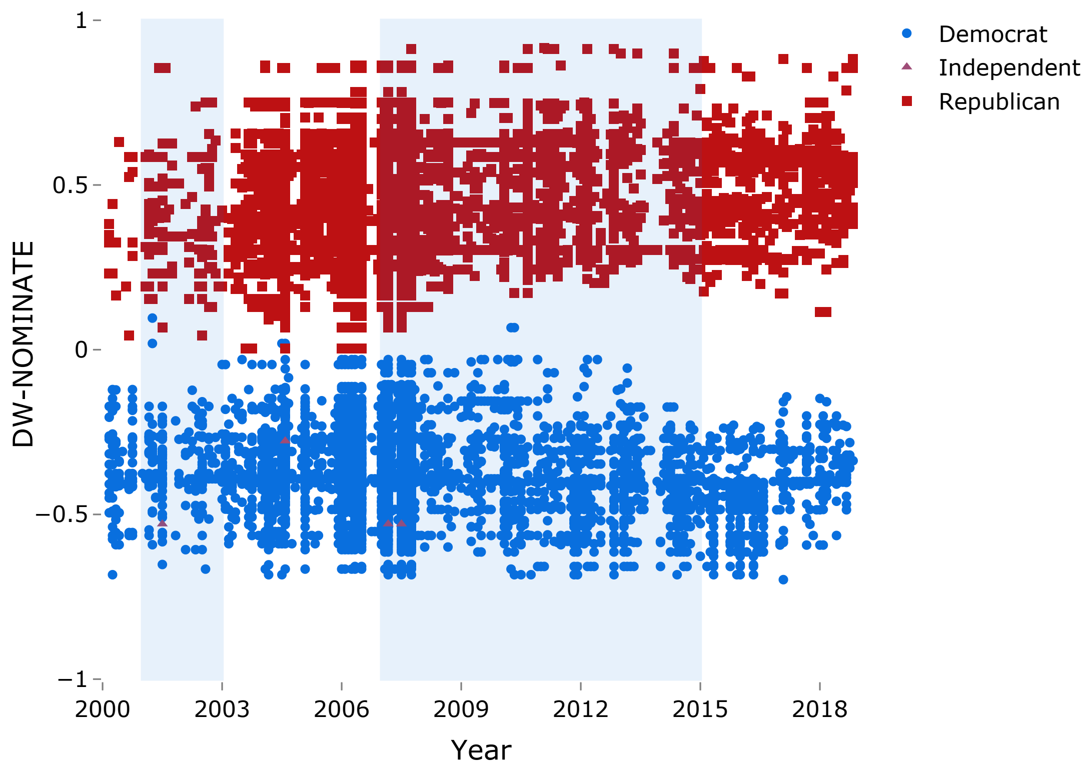


Figure 2.4: Meetings with effective lawmakers

The figure shows the meetings with effective lawmakers over time for all foreign countries. We rank each legislator by their lawmaker effectiveness score (LES) from the Centre of Effective Lawmaking. We then compute the fraction of meetings with top 5% of legislators (blue circles), top 10% of legislators (orange squares), and top 20% of legislators (green crosses) relative to all the legislators a foreign country meets in a year.

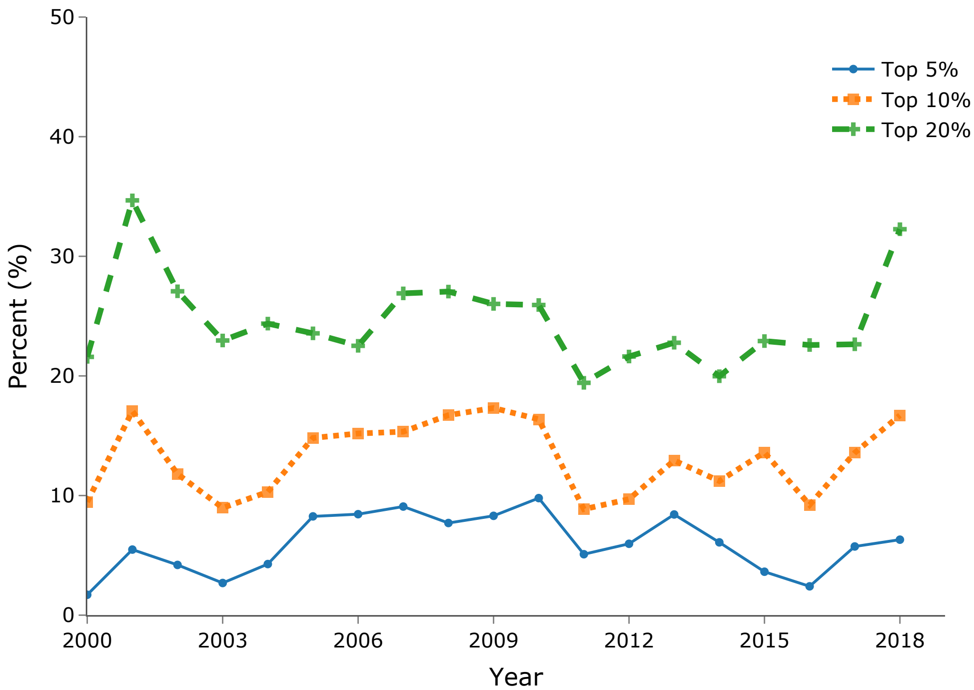
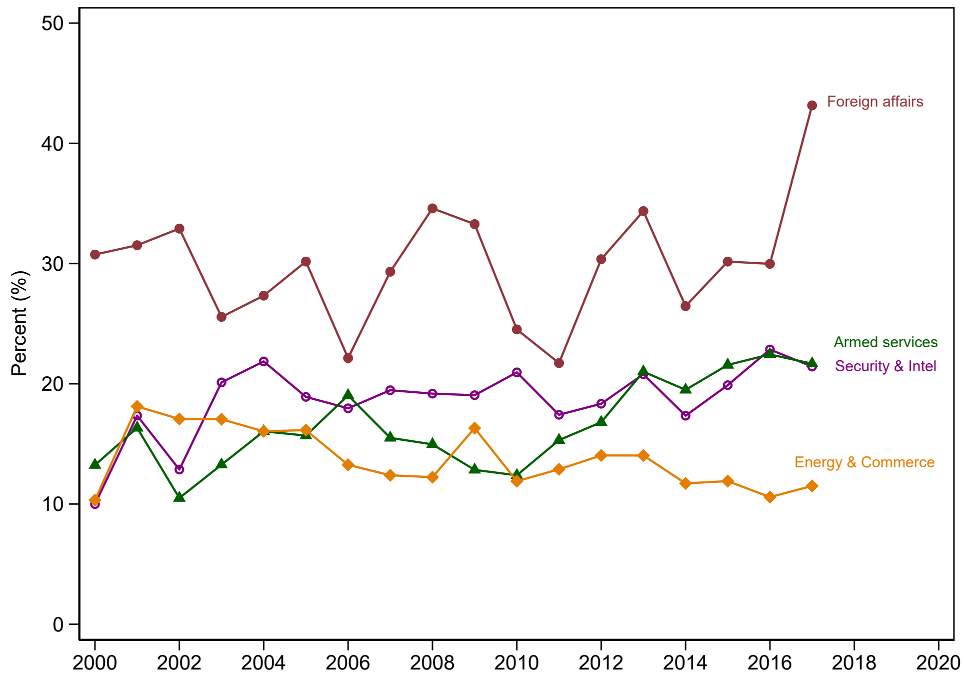


Figure 2.5: Which committees get the most meetings?

Panel A shows the percentage of meetings with a given committee relative to the total meetings each year. Panel B shows the meetings with chairs scaled by the average number of meetings with members in a given committee. The horizontal red dashed line is set at 1. This value represents the case in which the number of meetings of the committee chair is the same as the average number of meetings with members in the corresponding committee.

(A.) Percentage of meetings with members of a given committee



(B.) Meetings with chairs scaled by the average number of meetings with members in a given committee

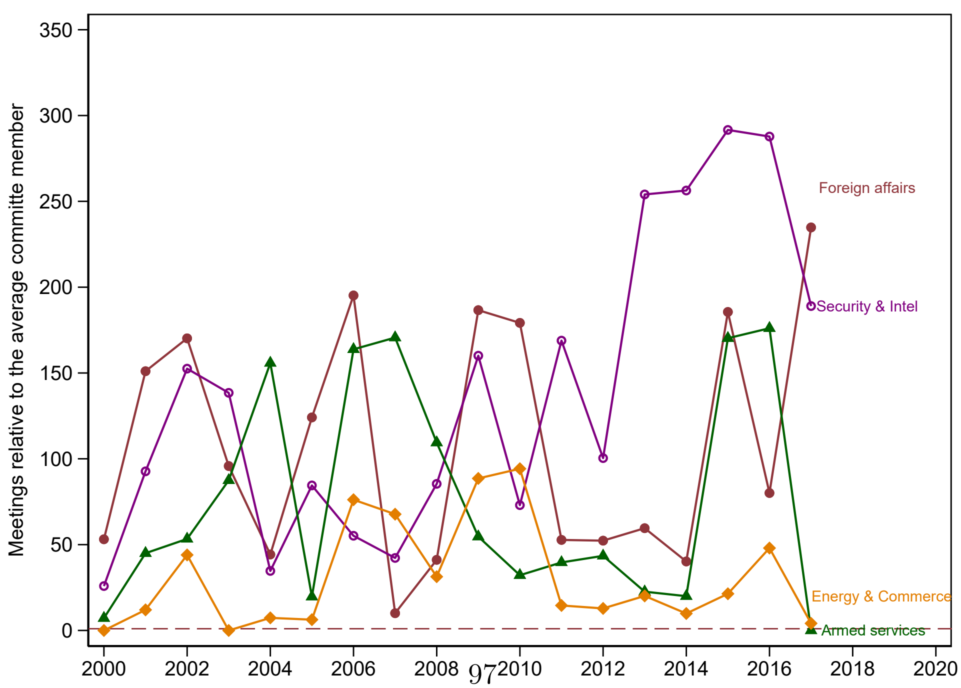


Table 2.3: Which countries use FARA to lobby US legislators?

The unit of analysis is a country-year dyad. The dependent variable is an indicator for whether representatives of foreign government held at least one in-person meeting in a year with US legislators, $\mathbb{1}_{Meetings>0}$. In column 1, we include the following country-level economic characteristics lagged by one year: total GDP in US$, total population, exports from and imports from US in US$. In column 2, we include the following characteristics relating to conflicts: total unrest from the Cline Center Historical Phoenix Event Data where we separate the number of times the country was a source or a target, the use of physical force to achieve political objectives by non-state actors (*political violence*) and the number of coups d'état during a given year. In column 3, we include the following country-level institutional characteristics: transition to democracy, electoral democracy index capturing the extent to which electoral democracy is achieved within the country, political corruption index which combines six distinct types of corruption and measures the level of corruption in a given year, political polarization capturing the extent to which political differences affect social relationships beyond political discussions, the total length of international sanctions imposed by the US (Felbermayr, Kirilakha, Syropoulos, Yalcin, and Yotov, 2020; Kirikakha, Felbermayr, Syropoulos, Yalcin, and Yotov, 2021), similarity in foreign policy preferences to the US based on voting on resolutions in the United Nations General Assembly (UNGA) measured using absolute distances between the ideal points of countries (Bailey, Strezhnev, and Voeten, 2017), and the total number of US presidential diplomatic visits (Malis and Smith, 2021). All specifications include *Year* fixed effects and specification 5 includes *Country* fixed effects to control for time-invariant country characteristics. We use ordinary least squares (OLS) regressions to estimate the coefficients. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	$\mathbb{1}_{Meetings>0}$				
Characteristics	Economic	Conflict	Institutions	All	All
	(1)	(2)	(3)	(4)	(5)
GDP (Log)	-0.026			-0.004	-0.027
	(0.026)			(0.031)	(0.046)
Population (Log)	0.046**			-0.038	0.091
	(0.021)			(0.023)	(0.165)
US Exports (Log)	0.043**			0.053***	0.013
	(0.019)			(0.018)	(0.022)
US Imports (Log)	0.019*			0.027**	0.024***
	(0.010)			(0.011)	(0.008)
Total unrest – source (Log)		0.030		0.010	-0.013
		(0.021)		(0.019)	(0.017)
Total unrest – target (Log)		0.053**		0.040**	0.024
		(0.021)		(0.019)	(0.016)
Political violence		0.010		0.007	0.018
		(0.024)		(0.023)	(0.039)
Coup		-0.014		0.017	0.028
		(0.129)		(0.115)	(0.062)
Transition to democracy			-0.026	0.006	-0.039
			(0.041)	(0.038)	(0.034)
Electoral democracy index			-0.159	-0.170	0.056
			(0.149)	(0.135)	(0.189)
Political corruption index			0.026	0.074**	0.021
			(0.035)	(0.033)	(0.054)
Political polarization			0.025	0.018	0.020
			(0.026)	(0.025)	(0.035)
US-imposed sanction length			0.055*	0.047*	-0.000
			(0.030)	(0.026)	(0.026)
Country political preference, UNGA Voting (Log)			-0.100	-0.027	0.156
			(0.081)	(0.093)	(0.219)
US Presidential diplomatic visits (Log)			0.268***	0.090**	0.074***
			(0.047)	(0.038)	(0.023)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	No	Yes
R^2	0.12	0.09	0.09	0.21	0.49
Observations	2,660	2,660	2,660	2,660	2,660

Table 2.4: Which legislators meet foreign agents more often?

This table relates meetings between foreign country representatives and US legislators to individual characteristics. The unit of analysis is a country-legislator-year triad. The dependent variable is the natural logarithm of the number of meetings in a year with U.S legislators, *Log (Number of meetings)*. In column 1, we include the following legislator characteristics: whether the legislator is a member of the House of representatives (*House member*), vote share in the elections (*Vote share*), an indicator capturing party affiliation (*Democrat*), member of the party that is in control of the senate (*Majority*) and rank within the party (*Seniority*). In column 2, we include the following ideological characteristics: measures of legislator ideology, *DW-NOMINATE 1* and *DW-NOMINATE 2*. In column 3, we include the characteristics that might affect influence: lawmaking effectiveness of the legislator (*Legislative Effectiveness Score*), whether she is a senate or house committee and sub-committee chair (*Committee (Sub-committee) chair*), a member of rules, ways and means, and appropriations committee (*Power committee membership*). We also capture whether the legislator is a member (ranking member or chair) of, either a senate or house committee, the following committees: (i) the rules, ways and means, and appropriations, (ii) foreign affairs, (iii) Security & Intelligence, (iv) Armed Services, and (v) Energy & Commerce. All specifications include *Country*$\times$*Year* fixed effects and specification 5 includes legislator fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) regressions to estimate the coefficients. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Log (meetings)				
Characteristics	Legislator	Ideology	Importance	All	All
	(1)	(2)	(3)	(4)	(5)
House member	-0.022			0.023	-0.165***
	(0.028)			(0.032)	(0.059)
Vote share (Log)	0.172***			0.149***	0.148***
	(0.025)			(0.027)	(0.035)
Democrat	0.016				
	(0.021)				
Majority	-0.038***			-0.063***	-0.023
	(0.012)			(0.017)	(0.035)
Seniority	0.005***			0.006***	0.012*
	(0.001)			(0.002)	(0.007)
Distance from median (ideology)		0.034		-0.048	-0.007
		(0.031)		(0.034)	(0.090)
DW-NOMINATE 1		-0.030***		-0.020*	0.854
		(0.010)		(0.011)	(0.527)
DW-NOMINATE 2		-0.009		0.002	0.450**
		(0.008)		(0.008)	(0.196)
Legislative Effectiveness Score			0.010**	0.010**	0.014***
			(0.004)	(0.004)	(0.004)
Committee chair			-0.021	-0.031*	-0.023
			(0.017)	(0.017)	(0.021)
Sub-committee chair			-0.018**	0.013	-0.002
			(0.009)	(0.010)	(0.012)
Power committee membership			0.055***	0.054***	0.010
			(0.019)	(0.016)	(0.015)
Foreign affairs			0.154***	0.173***	0.144***
			(0.028)	(0.032)	(0.032)
Foreign affairs (chair)			0.036	0.030	0.044
			(0.053)	(0.052)	(0.070)
Foreign affairs (Ranking member)			0.095**	0.008	-0.020
			(0.044)	(0.044)	(0.080)
Security & Intelligence			0.046***	0.054***	-0.007
			(0.013)	(0.013)	(0.018)
Security & Intelligence (chair)			0.046	0.063	0.022
			(0.045)	(0.045)	(0.058)
Security & Intelligence (Ranking member)			0.003	-0.022	-0.041
			(0.042)	(0.041)	(0.041)

Dependent variable:	Log (meetings)				
Characteristics	Legislator	Ideology	Importance	All	All
	(1)	(2)	(3)	(4)	(5)
Armed services			-0.005	0.019	0.017
			(0.017)	(0.018)	(0.028)
Armed services (chair)			-0.127***	-0.168***	-0.006
			(0.045)	(0.046)	(0.076)
Armed services (Ranking member)			0.142***	0.079	0.083
			(0.048)	(0.050)	(0.070)
Energy & Commerce			-0.036**	-0.020	-0.015
			(0.017)	(0.018)	(0.031)
Energy & Commerce (chair)			-0.126	-0.168**	-0.226**
			(0.081)	(0.083)	(0.095)
Energy & Commerce (Ranking member)			-0.152**	-0.219***	-0.245**
			(0.064)	(0.069)	(0.104)
Legislator fixed effects	No	No	No	No	Yes
Country × year fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.31	0.31	0.32	0.32	0.40
Observations	36,555	36,555	36,555	36,555	36,525

Table 2.5: Meetings around legislators switching important committees

This table presents regressions estimating the relationship between meetings with legislators around the time they switch out of important committees for resource allocation. Columns 1 and 2 focus on all legislators departing from important committees while columns 3 and 4 focus on top five legislators based on ranking within committees. The unit of analysis is legislator-state-foreign country-lobbyist-year month. The dependent variable is, *Log (1+meetings$_t$)*, the natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators sitting on important committees. The independent variable of interest is *After x Switcher* which is an indicator variable taking the value of one if the US representative or senator switches out of an important committee. The important committees include: the House Committee on Appropriations, House Committee on Oversight and Reform, House Committee on Armed Services, House Committee on the Budget, House Committee on Transportation and Infrastructure, House Committee on Energy and Commerce, House Committee on Homeland security, House Committee on Foreign Affairs, House committee on Intelligence, Senate Committee on Appropriations, Senate Committee on Homeland Security and Governmental Affairs, Senate Committee on the Budget, Senate Committee on Commerce, Science, and Transportation, Senate Committee on Foreign Relations, Senate Committee on Intelligence, and Senate Committee on Energy and Natural Resources. All regressions include: *Lobbying firm* fixed effects to control for time-invariant differences in lobbying firm characteristics, *Legislator × committee* fixed effects to control for influential legislators departing from the same committee at different points in their tenure, *Country × committee* fixed effects to control for relative importance of departing committee for foreign countries, and *State × year-month* fixed effects to control for local economic confounds. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Log (1+meetings)			
	All legislators		*High-ranked legislators*	
	(1)	(2)	(3)	(4)
After x Switcher	-0.001	-0.001	-0.003	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)
Congress	No	Yes	No	Yes
Lobbying firm	No	Yes	No	Yes
Legislator × committee	Yes	Yes	Yes	Yes
Country × committee	Yes	Yes	Yes	Yes
State × year-month	Yes	Yes	Yes	Yes
R^2	0.06	0.07	0.06	0.07
Observations	584,860	584,860	381,912	381,912

Table 2.6: Meetings with legislators and aid received by foreign countries from the US

This table presents panel regressions estimating the relationship between meetings and aid received by foreign countries from the US. The unit of analysis is legislator-state-foreign country-year. The dependent variable in panel A is, $\mathbb{1}_{Aid>0}$, an indicator for receiving foreign aid from the US while in panel B the dependent variable is, *Log (1+Aid amount)*, natural logarithm of one plus the aid amount received by a foreign country from the US. The independent variable of interest is *Log (1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators from the respective state. In columns 2, 4, and 6, we include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), share of labour compensation in GDP (*Labour share*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *State* $\times$ *year* fixed effects to control for local economic confounds and general state policies. Specifications 3 and 4 additionally include *Country* fixed effects to control for time-invariant country characteristics while specifications 5 and 6 further include *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country-level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Extensive margin					
Dependent variable	$\mathbb{1}_{Aid>0}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.614***	0.614***	0.614***	0.614***	0.614***	0.614***
	(0.020)	(0.020)	(0.021)	(0.020)	(0.021)	(0.021)
Controls	No	Yes	No	Yes	No	Yes
State $\times$ year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Politician fixed effects	No	No	No	No	Yes	Yes
R^2	0.78	0.78	0.78	0.78	0.78	0.78
Observations	276,853	276,853	276,853	276,853	276,853	276,853

	Panel B: Intensive margin					
Dependent variable	Log (1+Aid amount)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	11.123***	11.122***	11.109***	11.108***	11.109***	11.108***
	(0.396)	(0.396)	(0.411)	(0.409)	(0.416)	(0.414)
Controls	No	Yes	No	Yes	No	Yes
State $\times$ year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Politician fixed effects	No	No	No	No	Yes	Yes
R^2	0.76	0.76	0.77	0.77	0.77	0.77
Observations	276,853	276,853	276,853	276,853	276,853	276,853

Table 2.7: Meetings around tariff bills

This table presents panel regressions examining product tariff bills advantageous to foreign countries around meetings with US legislators. The unit of analysis is politician-foreign country-year-month. In columns 1 and 2, the dependent variable is, $\mathbb{1}_{Favourablebill}$, defined as an indicator for whether the legislator was sponsoring or co-sponsoring a product tariff bill favourable to the foreign country with whose representatives he/she met. The dependent variable in columns 3 and 4 is, $\mathbb{1}_{Favourableaction}$, defined as an indicator for whether a product tariff bills favourable to the foreign country passed a committee in which the legislator sat. The independent variable of interest is, *Log(1+Meetings$_t$)*, defined as the natural logarithm of one plus the number of meetings between representatives of a foreign country and a legislator. All regressions include: *Legislator* fixed effects to control for time-invariant differences in legislator characteristics, *Committee* $\times$ *year* fixed effects to control for importance of committees over time, and *Country* $\times$ *year* fixed effects to control for time-varying determinants of trade relationships. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	$\mathbb{1}_{Favourablebill>0}$		$\mathbb{1}_{Favourableaction>0}$	
	(1)	(2)	(3)	(4)
Log (1+Meetings$_t$)	0.001***	0.001**	0.001**	0.001*
	(0.000)	(0.001)	(0.001)	(0.000)
Legislator fixed effects	Yes	Yes	Yes	Yes
Country $\times$ year fixed effects	Yes	Yes	Yes	Yes
Committee $\times$ year fixed effects	No	No	Yes	Yes
R^2	0.01	0.02	0.05	0.06
Observations	4,045,632	1,258,152	1,665,903	828,428

Table 2.8: Meetings with legislators and local government subsidies to foreign firms

This table presents panel regressions estimating the relationship between meetings and state-level subsidies granted to foreign firms. The unit of analysis is legislator-state-foreign country-year. The dependent variable in panel A is, $\mathbb{1}_{Subsidy>0}$, an indicator equal to one if a foreign firm whose representatives meet with a US legislator, receives a subsidy from the legislator's state while in panel B the dependent variable is, *Log (1+Subsidy amount)*, natural logarithm of one plus the amount of subsidy received by a foreign firm from that state. The independent variable of interest is *Log (1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators from the respective state. In columns 2, 4, and 6, we include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), share of labour compensation in GDP (*Labour share*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *State* × *year* fixed effects to control for local economic confounds and general state policies. Specifications 3 and 4 additionally include *Country* fixed effects to control for time-invariant country characteristics while specifications 5 and 6 further include *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country-level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Extensive margin					
Dependent variable	$\mathbb{1}_{Subsidy>0}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.038**	0.038**	0.039**	0.039**	0.039**	0.039**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Controls	No	Yes	No	Yes	No	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.06	0.08	0.10	0.10	0.10	0.10
Observations	277,099	277,099	277,099	277,099	277,099	277,099

	Panel B: Intensive margin					
Dependent variable	Log (1+Subsidy amount)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.531**	0.524**	0.541**	0.543**	0.539**	0.541**
	(0.259)	(0.252)	(0.253)	(0.252)	(0.251)	(0.250)
Controls	No	Yes	No	Yes	No	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.06	0.07	0.09	0.10	0.10	0.10
Observations	277,099	277,099	277,099	277,099	277,099	277,099

Table 2.9: Meetings with legislators and new government contracts to foreign firms

This table presents panel regressions estimating the relationship between meetings with US legislators and new government contract allocated to foreign firms in the state. The unit of analysis is legislator-state-foreign country-year. The dependent variable in panel A is, $\mathbb{1}_{Contract>0}$, an indicator equal to one if a foreign firm, whose representatives meet with a US legislator, received a new government contract to be performed in the legislator's state while in panel B the dependent variable is, *Log (1+Contract amount)*, natural logarithm of one plus the contract amount received by the foreign firm to perform the contract in that given state. The independent variable of interest is *Log (1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators from the respective state. In columns 2, 4, and 6, we include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), share of labour compensation in GDP (*Labour share*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *State* $\times$ *year* fixed effects to control for local economic confounds and general state policies. Specifications 3 and 4 additionally include *Country* fixed effects to control for time-invariant country characteristics while specifications 5 and 6 further include *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Extensive margin					
Dependent variable	$\mathbb{1}_{Contract>0}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.026***	0.026***	0.027***	0.027***	0.027***	0.027***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Controls	No	Yes	No	Yes	No	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Politician fixed effects	No	No	No	No	Yes	Yes
R^2	0.04	0.05	0.07	0.07	0.07	0.08
Observations	276,982	276,982	276,982	276,982	276,982	276,982

	Panel B: Intensive margin					
Dependent variable	Log (1+Contract amount)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.281**	0.280**	0.293**	0.294**	0.293**	0.294**
	(0.112)	(0.110)	(0.114)	(0.113)	(0.114)	(0.113)
Controls	No	Yes	No	Yes	No	Yes
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Politician fixed effects	No	No	No	No	Yes	Yes
R^2	0.04	0.05	0.07	0.07	0.07	0.08
Observations	276,982	276,982	276,982	276,982	276,982	276,982

Table 2.10: Share of registered voters with ethnic affiliations to the foreign country

This table presents panel regressions estimating the relationship between meetings and the share of registered voters with ethnic affiliation to the foreign country whose representatives the legislator is meeting. The unit of analysis is politician-region-foreign country-year. The dependent variable in is the share of registered democrat voters belonging to a given ethnic group within a state (column 1) or a congressional district (column 2). The independent variable of interest is Log(1+Meetings$_t$), natural logarithm of one plus the number of meetings between representatives of a foreign country and US senators from the respective state (column 1) or a House member from the respective congressional district (column 2). Democrat is a dummy variable equal to 1 if the legislator is from the Democratic party and 0 otherwise. All regressions include *Year* fixed effects to control for national-level policies and *Country* fixed effects to control for time-invariant country characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country-level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	Share of registered democrats$_{t+1}$	
	Senate	House member
	(1)	(2)
Log (Meetings$_t$)	-0.003	-0.000
	(0.006)	(0.005)
Democrat	0.122***	0.127***
	(0.010)	(0.007)
Log (Meetings$_t$) × Democrat	0.020*	0.012*
	(0.011)	(0.007)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
R^2	0.38	0.28
Observations	970	2,506

Table 2.11: Privately-sponsored trips to foreign countries

This table presents panel regressions examining privately-sponsored trips of legislators around meetings. The unit of analysis is politician-foreign country-year-month. In columns 1 and 2, the dependent variable is $\mathbb{1}_{Trip>0}$, defined as an indicator for whether the legislator undertook a privately-sponsored trip to the foreign country with whose representatives they met with. The dependent variable in columns 3 and 4 is *Log (1+# days)*, defined as the natural logarithm of one plus of total number of days of the privately-sponsored trip to the foreign country with whose representatives they met with. The independent variable of interest is *Log(1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and legislators. We include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), share of labour compensation in GDP (*Labour share*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *State × year-month* fixed effects to control for local economic confounds and general state policies, *Country* fixed effects to control for time-invariant country characteristics and *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	$\mathbb{1}_{Trip>0}$		Log (1+# days)	
	(1)	(2)	(3)	(4)
Log (1+Meetings$_t$)	0.002***	0.002***	0.004***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes	Yes
State × year-month fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Legislator fixed effects	No	Yes	No	Yes
R^2	0.01	0.01	0.01	0.01
Observations	2,124,840	2,124,840	2,124,840	2,124,840

Table 2.12: Political contributions by individuals around meetings with foreign representatives

This table presents panel regressions estimating the relationship between meetings and political contributions by individuals to legislators. The unit of analysis is legislator-foreign country-year. The dependent variable is *Log(Political contribution by individuals)* defined as the natural logarithm of the total political contribution made by individuals to legislators in the election year during their political campaigns. The independent variable of interest is *Log(Meetings$_t$)*, natural logarithm of the number of meetings between representatives of a foreign country and legislators. We also consider the following institutional characteristics of the foreign country: political corruption index which combines six distinct types of corruption and measures the level of corruption in a given year (column 2) and electoral democracy index capturing the extent to which electoral democracy is achieved within the country (column 3). Additionally, the regression controls for the total political contributions received by legislator in the election year. All regressions include *Legislator* fixed effects to control for time-invariant legislator characteristics and *State* × *year* fixed effects to account for time-varying regional economic confounds. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	Log(Political contributions by individuals)		
	(1)	(2)	(3)
Log (Meetings$_t$)	0.033	0.031	-0.036
	(0.026)	(0.026)	(0.039)
Political corruption index		0.037	
		(0.031)	
Log (Meetings$_t$) × Political corruption index		-0.039	
		(0.025)	
Electoral democracy index			-0.189*
			(0.110)
Log (Meetings$_t$) × Electoral democracy index			0.151*
			(0.086)
Legislator fixed effects	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes
R^2	0.87	0.87	0.87
Observations	17,792	17,792	17,792

Table 2.13: Timing of re-election and meetings with foreign country representatives

A Cox proportional hazards model is fitted to understand determinants of time taken to vacate an office after an election. The unit of analysis is legislator-year. The main independent variable of interest is the natural logarithm of the number of meetings in a year with US legislators, *Log (meetings$_t$)*. In column 2, we include the following legislator characteristics: whether the legislator is a member of the House of representatives (*House member*), natural logarithm of vote share in the elections (*Vote share*), an indicator capturing party affiliation (*Democrat*), member of the party that is in control of the senate (*Majority*), and the rank within the party (*Seniority*). In column 3, we include the following ideological characteristics: measures of legislator's political ideology, *DW-NOMINATE 1* and *DW-NOMINATE 2*. In column 4, we consider characteristics that are important for influence and resource allocation: lawmaking effectiveness of the legislator (*Legislative Effectiveness Score*), whether she is a senate or house committee and sub-committee chair (*Committee (Sub-committee) chair*), a member of rules, ways and means, and appropriations committee (*Power committee membership*). We also capture whether the legislator is a member, ranking member, or chair of, either a senate or house committee, the following committees: (i) the rules, ways and means, and appropriations , (ii) foreign affairs, (iii) Security & Intelligence, (iv) Armed Services, and (v) Energy & Commerce. All specifications include *State* fixed effects to control for time-invariant state characteristics. Standard errors are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Characteristics	None	Legislator	Ideology	Importance	All
	(1)	(2)	(3)	(4)	(5)
Log(Meetings$_t$)	-0.059	-0.109**	-0.060	-0.128***	-0.146***
	(0.041)	(0.042)	(0.041)	(0.048)	(0.048)
Controls	No	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2,658	2,627	2,658	2,658	2,627

CHAPTER 3

Social security and trends in wealth inequality

Joint with Sylvain Catherine and Natasha Sarin

Contribution to the joint work All authors contributed equally to this project and each was involved in the production of each result in the paper.

3.1. Introduction

As work dating back to Feldstein (1976) shows, total wealth—inclusive of Social Security—is much more evenly distributed than private wealth alone. This paper highlights that including Social Security does not just change the level of wealth inequality, but its evolution as well. Specifically, although it is widely believed that wealth inequality in the United States is on the rise, we find that top wealth shares have not increased in the last three decades when Social Security is properly taken into account.

Accounting for Social Security matters for two reasons. First, asset price increases drive recent trends in U.S. wealth inequality (Moll, 2020; Hubmer, Krusell, and Smith, 2020). Indeed, shifts in interest rates have a disproportionate effect on the market value of long duration assets, which are primarily owned by wealthy households, leading to greater wealth inequality (Greenwald, Leombroni, Lustig, and Nieuwerburgh, 2021). However, the focus on market wealth overlooks an important long duration asset which represents a disproportionate part of the balance sheet of low and middle-class households and has increased in value: their Social Security benefits.

Second, as the simple conceptual framework we begin with shows, because mandatory Social Security contributions represent a larger fraction of the balance sheets of those outside of the top of the distribution, modest variations in savings rates have larger consequences on their private wealth accumulation. Because of this mechanical leverage effect, an overall decline in saving rates translates into greater market wealth inequality, but does not necessarily affect the distribution of total wealth, inclusive of Social Security. This becomes especially important when one considers that contribution rates into Social Security have increased from less than 2% in the 1950s to roughly 11% since 1988

(Miron and Weil, 1998).

Excluding Social Security wealth from inequality measures has broad policy implications. Perversely, existing wealth concentration measures that ignore the substitution between private and public wealth could mistakenly conclude that progressive social programs increase inequality, rather than redress it. A more inclusive wealth concept, in contrast, enables proper evaluation of the role redistributive public programs play in curbing inequality.

To incorporate Social Security into top wealth estimates, we must know both the aggregate size of the Social Security program and how Social Security wealth is distributed across the marketable wealth distribution. This paper derives estimates of the stock and distribution of Social Security wealth by simulating households' future benefits and payroll taxes, relying on data from the Survey of Consumer Finances (SCF). Our estimates are conservative since we focus on Social Security's old-age retirement program, excluding disability insurance which would lead to an even larger reduction in top wealth shares if included.

For retirees, we can calculate Social Security wealth from the SCF directly using reported benefits. For workers who are still in the labor force, we simulate earnings trajectories by relying on previous empirical work that provides a labor income process that matches many moments of the SSA administrative panel data (Guvenen, Karahan, Ozkan, and Song, 2021). We then apply the Social Security benefit and tax formulas to construct estimates of future retirement benefits that these households will accrue, net of the taxes that they will pay. We validate these estimates by comparing them to aggregate wealth estimates reported by the SSA and to benefits reported for retirees in the SCF. Finally, we determine the share of Social Security wealth going to the top of the wealth distribution based on the relationship between Social Security and marketable wealth for retired workers, readily observable in the SCF.

Computing the present value of Social Security wealth also requires choosing an appropriate discount rate. For example, Novy-Marx and Rauh (2011) discount public employee pension promises using state yield curves. Similarly, we first offer a risk-free valuation of Social Security wealth using the

Treasury market yield curve. We find that the share of "marketable wealth" owned by the top 10% and top 1% grew by approximately 10 percentage points (pp) between 1989 and 2019, in line with existing estimates (Smith, Zidar, and Zwick, 2020a). Once Social Security wealth is included, these trends disappear: the share of the top 10% and top 1% actually dropped by 5.4 and 1.3 pp, respectively.

However, discounting should reflect the risks associated with the Social Security program. As such, our second set of results accounts for the labor market risk inherent in pay-as-you-go systems. Social Security is wage-indexed, so future benefits are directly tied to economic growth. Given the cointegration between the labor and stock markets (Benzoni, Collin-Dufresne, and Goldstein, 2007), it is important to adjust for the market beta of future Social Security payouts (Geanakoplos and Zeldes, 2010). Our risk-adjustment decreases the stock of Social Security wealth by nearly 20%, with a disproportionate effect on the bottom 90%. Even after this correction, inequality trends are substantially attenuated relative to past estimates that exclude Social Security. From 1989 to 2019, the top 10% wealth share decreases by 2.4 pp. The top 1% share remained flat, increasing by 0.2 pp.

Our findings are also robust to alternative assumptions that incorporate the risk of future benefit cuts (or tax hikes), differences in life expectancy among the rich and the poor, adding a substantial liquidity premium to discount rates, and the possibility of persistently low economic growth.

Overall, Social Security dramatically impacts inequality trends because the growth in Social Security wealth has outpaced the growth in marketable wealth over the last three decades. This increase can be attributed to three factors. First, Social Security expanded in scope over our sample period, as the share of earnings subject to Social Security payroll taxes increased from a maximum of 1.25 times average annual earnings to 2.5 times. Second, there have been demographic shifts: the U.S. population is aging and living longer. The share of workers near retirement age and for whom Social Security wealth is at its peak (because they have paid in fully to the fund, but have yet to receive any benefits) grew by nearly 50%. Moreover, life expectancy increased by nearly 4 years.

Finally, and most importantly, real interest rates have fallen. This means that, in order to fund the same level of consumption during retirement, an investor in 2019 has to save considerably more (since less interest will accrue) or buy a higher-priced annuity than an investor in 1989. As such, Social Security's value rises, since the future purchasing power of contributions corresponds to more marketable wealth when workers face low rates of returns on their private savings.

Considering the role rates play in the evolution of Social Security wealth is imperative, since, in economic terms, the cashflows generated by Social Security and marketable assets must enter the intertemporal budget constraint of households in a consistent way. The prevailing low rate environment has driven up the valuation of marketable assets. As a simple life-cycle framework makes clear, when optimal savings rates decline for whatever reason, private savings fall disproportionately for low-earners. This is because mandatory Social Security contributions—which represent a disproportionate share of low-earning households' total savings—are not a lever for adjustment.

Falling interest rates affect wealth inequality by redistributing wealth away from holders of short-duration assets, favoring those with long-term investments (Auclert, 2019). Because long duration assets represent a greater share of the private wealth of those at the top of the distribution, marketable wealth inequality rises when interest rates fall (Greenwald et al., 2021). However, the relationship between households' wealth and the average duration of their assets is much weaker when we include Social Security, a long-term investment representing a disproportionate share of the total wealth of the bottom 90%. As Catherine, Miller, Paron, and Sarin (2022) show, the large implicit Social Security wealth of poorer households reduces the optimal share of long-term assets in their market wealth, resulting in lower capital gains than wealthy households when interest rates fall. Overall, by focusing on marketable wealth alone, previous studies have taken into account the increased value of privately long-duration assets disproportionately owned by the rich but not the increased value of those owned by the rest of the population, mainly the increased market value of their Social Security benefits.

For essentially any question related to inequality trends that researchers seek to answer, a broader wealth concept that includes Social Security is valuable. For example, one reason to care about wealth inequality is that it is a measure of consumption or welfare inequality, since accumulated wealth funds retirement consumption. In this case, Social Security's inclusion is important because retirement benefits serve the same purpose. Additionally, to understand the evolution of inequality across countries or regimes, it is imperative to consider differences in pension systems that can encourage or discourage private savings. Another reason to study wealth inequality is that wealth brings political power. There too including Social Security is important. The more households can rely on public programs during retirement, the more they can afford to spend private wealth on political causes. Further, Social Security is revelatory on the counterfactual: private wealth would have evolved differently if individuals had to fund their later-life consumption through private savings.

Our findings contribute to the inequality literature in several ways. First, we provide new estimates of wealth concentration in the U.S., documenting the significant effect of the inclusion of Social Security. Specifically, we quantify how the value of Social Security has increased over the last thirty years, finding that it attenuates the upward trend in marketable wealth inequality. Our results illustrate how Social Security has decreased households' exposure to the low rate environment: In the absence of Social Security, with rates of return near zero, households would have to save more to finance a given level of consumption in retirement. But, in reality, the rate of return on Social Security contributions has not decreased as much as the return on private wealth.

Second, we shed new light on the recent divergence in savings behavior across the wealth distribution. Saez and Zucman (2016) credit a decrease in private savings for the bottom 90%, which has been 0% since 2000, as precipitating the rise in wealth inequality. This is true mechanically: If savings rates are 0% for the bottom 90%, then all the increase in wealth is captured by the top of the distribution. But private savings rates would not be zero if workers were not saving through the Social Security program. Strangely, existing estimates of savings rates and thus wealth concentration would arrive at different conclusions in the counterfactual world where Social Security contributions were invested in private accounts with the same rate of return and progressivity. Ignoring the known substitution between Social Security contribution and private savings (Attanasio and Brugiavini, 2003; Attanasio and Rohwedder, 2003; Scholz, Seshadri, and Khitatrakun, 2006) is misleading.

Third, we build a measure of wealth inequality that is better suited for policy evaluation and cross-country comparisons than prior work. A focus on narrow marketable wealth inequality means that expansions of the social safety net may well register as increasing inequality, because public funds for retirement, or to cover healthcare costs or unemployment shocks, displace the need for private wealth accumulation. This is why a broader wealth concept is important. Our approach also enables comparison with other countries, as different retirement structures or the generosity of social insurance arrangements should not mechanically drive conclusions about how unequally distributed resources are.

To be sure, this is an incomplete undertaking: we too exclude important components of wealth from our estimates, for example, the provision of public healthcare benefits. It is our hope that this paper represents a first step toward a broader wealth concept that will enable accurate measurement and analysis of inequality trends.

Related Literature Narrowly defined marketable wealth understates the wealth of workers and consequently overstates inequality substantially. It also ignores a long literature that documents the importance of Social Security for the distribution of income and wealth. For instance, Wolff (1992, 1996) shows that the inclusion of pension and Social Security wealth impacts both the level of and changes in measured wage inequality. Gustman, Mitchell, Samwick, and Steinmeier (1999) investigate the importance of pension and Social Security wealth for those nearing retirement, showing that it accounts for half—or more—of the total wealth of all those below the 95th percentile of the wealth distribution. Poterba (2014) also sheds light on the importance of Social Security to the elderly, documenting that for people over age 65, this stream of cash flows accounts for more than half of total income for the bottom three quartiles of the income distribution. Outside of the US, evidence confirms that ignoring the effects of redistributive pension programs inflates measured wage inequality (Domeij and Klein, 2002).

We build on the insights of past literature to augment our definition of wealth by including the Social Security benefits that workers accrue. Feldstein (1974) does such an exercise to show that in 1962, the ownership of total wealth, inclusive of Social Security, was much less concentrated than the ownership of market wealth. We focus on trends in wealth inequality, showing that this pattern remains true, and the differences between the "market wealth" and "total wealth" series are of growing importance over time. We thus contribute to the literature by documenting the sizable impact of Social Security on trends in wealth inequality. Our exercise confirms Weil (2015) who suggests that the concept of market wealth is incomplete and overstates inequality by ignoring transfer wealth, which is both large and, unlike market wealth, not skewed to the top of the distribution. A related point has been made by Auten and Splinter (2019a) in the context of income inequality, who highlight that including government transfer programs decreases top income shares, and by Auerbach, Kotlikoff, and Koehler (2019) who point out that their measure of remaining lifetime spending is much more equally distributed than net wealth or current income.

3.2. Conceptual Framework

Should Social Security wealth be accounted for in estimates of wealth inequality? To answer this question, we calibrate an illustrative model in which households prepare for retirement by accumulating private savings and contributing to Social Security. Because of the design of the program, Social Security savings represent a larger share of the total savings of low-earning households. Further, these contributions are mandatory, which means that when optimal savings rates decline, they are not a lever of adjustment for households. As a result, low-earners adjust private savings proportionately more to arrive at a equivalent reduction in overall retirement resources. As our simple model illustrates, this means that marketable wealth inequality can rise—while consumption inequality is unchanged.

3.2.1. Model

In the model, agents choose their consumption C to maximize lifetime expected utility:

$$V_{it} = \mathbb{E} \sum_{s=t}^{T} \beta^{s-t} (1 - m_s) \frac{C_{it}^{1-\gamma}}{1-\gamma}, \tag{3.2.1}$$

where β is the discount factor, γ the coefficient of relative risk aversion, $1 - m_{is}$ the age-dependent probability of being alive in year s, and T the maximum lifespan.

Agents receive labor earnings L before retirement and Social Security benefits B thereafter. They invest their private wealth W in a risk-free asset with return r. Hence, the dynamic budget constraint is:

$$W_{it+1} = \left(W_{it} + L_{it} + B_{it} - \mathrm{T}_{it} - C_{it}\right)(1+r), \tag{3.2.2}$$

where T denotes Social Security payroll taxes.

Social Security Payroll taxes represent 10.6% of earnings, up to a earnings cap, which we call the Social Security wage base SSWB_t:

$$\mathrm{T}_{it} = .106 \cdot \min\left\{L_{it}, \mathrm{SSWB}_t\right\}. \tag{3.2.3}$$

For simplicity, we assume that workers retire at full-retirement age R. Benefits depend on each individuals' average indexed yearly earnings (AIYE). A worker's indexed taxable earnings in year t are:

$$\mathrm{L}_{it}^{\text{indexed}} = \min\left\{L_{it}, \mathrm{SSWB}_t\right\} \frac{L_{1,60}}{L_{1,t}}, \tag{3.2.4}$$

where $L_{1,60}$ and $L_{1,t}$ denotes the value of the national wage index the year of his 60th birthday. In practice, the AIYE is the average of the best 35 years of indexed earnings up to retirement. To keep the model tractable, we assume, for the moment, that the AIYE is the average of all working years. Social Security benefits are progressive because the replacement rate is a concave function of the AIYE. The marginal replacement rate drops at two "bend points" b_1 and b_2, which, since the 1980, have represented 21% and 125% the wage index. Hence, yearly benefits in the first retirement year are:

$$B_{it} = \begin{cases} .9 \cdot \mathrm{AIYE}_i & \text{if } \mathrm{AIYE}_i < b_1 \\ .9 \cdot b_1 + .32(\mathrm{AIYE}_i - b_1) & \text{if } b_1 \leq \mathrm{AIYE}_i < b_2 \\ .9 \cdot b_1 + .32(b_2 - b_1) + .15(\mathrm{AIYE}_i - b_2) & \text{if } b_2 \leq \mathrm{AIYE}_i. \end{cases} \tag{3.2.5}$$

After retirement, benefits grow at the rate of inflation.

Labor Income Earnings are the product of the wage index and an idiosyncratic component $L_{2,i}$:

$$L_{it} = L_{1,t} \cdot L_{2,it}. \tag{3.2.6}$$

Throughout the paper, we model idiosyncratic risk using the rich income process estimated in Guvenen, Karahan, Ozkan, and Song (2021). Specifically, we assume that the idiosyncratic component of a worker's earnings $L_{2,i}$ evolves as follows:

$$
\begin{aligned}
&\text{Idiosyncratic earnings:} && L_{2,it} = (1-\nu_t^i)e^{\left(g(t)+\alpha^i+\beta^i t+z_t^i+\varepsilon_t^i\right)} && (10.1)\\
&\text{Persistent component:} && z_t^i = \rho z_{t-1}^i + \eta_t^i && (10.2)\\
&\text{Innovations to AR(1):} && \eta_t^i \sim \begin{cases} \mathcal{N}(\mu_{\eta,1}, \sigma_{\eta,1}^2) & \text{with prob. } p_z \\ \mathcal{N}(\mu_{\eta,2}, \sigma_{\eta,2}^2) & \text{with prob. } 1-p_z \end{cases} && (10.3)\\
&\text{Initial condition of } z_t^i\text{:} && z_0^i \sim \mathcal{N}(0, \sigma_{z,0}^2) && (10.4)\\
&\text{Transitory shock:} && \varepsilon_t^i \sim \begin{cases} \mathcal{N}(\mu_{\varepsilon,1}, \sigma_{\varepsilon,1}^2) & \text{with prob. } p_\varepsilon \\ \mathcal{N}(\mu_{\varepsilon,2}, \sigma_{\varepsilon,2}^2) & \text{with prob. } 1-p_\varepsilon \end{cases} && (10.5)\\
&\text{Nonemployment duration:} && \nu_t^i \sim \begin{cases} 0 & \text{with prob. } 1-p_\nu(t, z_t^i) \\ \min\{1, \mathrm{Exp}\{\lambda\}\} & \text{with prob. } p_\nu(t, z_t^i) \end{cases} && (10.6)\\
&\text{Prob. of Nonemp. shock:} && p_\nu^i(t, z_t) = \frac{e^{a+bt+cz_t^i+dz_t^i t}}{1+e^{a+bt+cz_t^i+dz_t^i t}} && (10.7)
\end{aligned}
$$

The persistent component of earnings z_i follows an AR(1) process with innovations drawn from a mixture of normal distributions. Transitory shocks ε_i are also drawn from a normal mixture. These normal mixtures capture high-order moments of the distribution of income shocks. Workers can experience a non-employment shock with some probability p_ν that depends on age, income, and gender, and exponentially distributed duration. In Equation (10.1), $g(t)$ captures the life-cycle profile of earnings common to all workers. The vector (α_i, β_i) determines heterogeneity in the level and growth rate of earnings and is drawn from a multivariate normal distribution with zero mean and correlation coefficient $\mathrm{corr}_{\alpha\beta}$. Heterogeneity in initial conditions of the persistent process is captured by z_0.

Calibration We assume a risk aversion $\gamma = 1$ and a discount factor $\beta = 0.97$. The wage process parameters are calibrated as shown in Table C.1. Survival probabilities are from the Human

Mortality Database for the year 2018.

3.2.2. The role of Social Security in household portfolios

Lifetime consumption is bounded by accrued financial resources and future labor income. The intertemporal budget constraint is:

$$\sum_{s=t}^{T} \frac{C_{is}}{(1+r)^{s-t}} \leq W_{it} + \sum_{s=t}^{T} \frac{B_{is} - \mathrm{T}_{is}}{(1+r)^{s-t}} + \sum_{s=t}^{T} \frac{L_{is}}{(1+r)^{s-t}}. \tag{3.2.11}$$

Thus, in addition to future earnings, agents have access to two resources: marketable wealth, and the net present value of Social Security cash flows. We define total wealth as the sum of these two resources:

$$\overline{W}_{it} = W_{it} + S_{it}, \tag{3.2.12}$$

where S_{it} is the net present value of Social Security cash flows:

$$S_{it} = \sum_{s=t+1}^{T} \frac{\mathbb{E}\left[\mathrm{B}_{is} - \mathrm{T}_{is}\right]}{\left(1 + r_{ts}\right)^{s-t}} \tag{3.2.13}$$

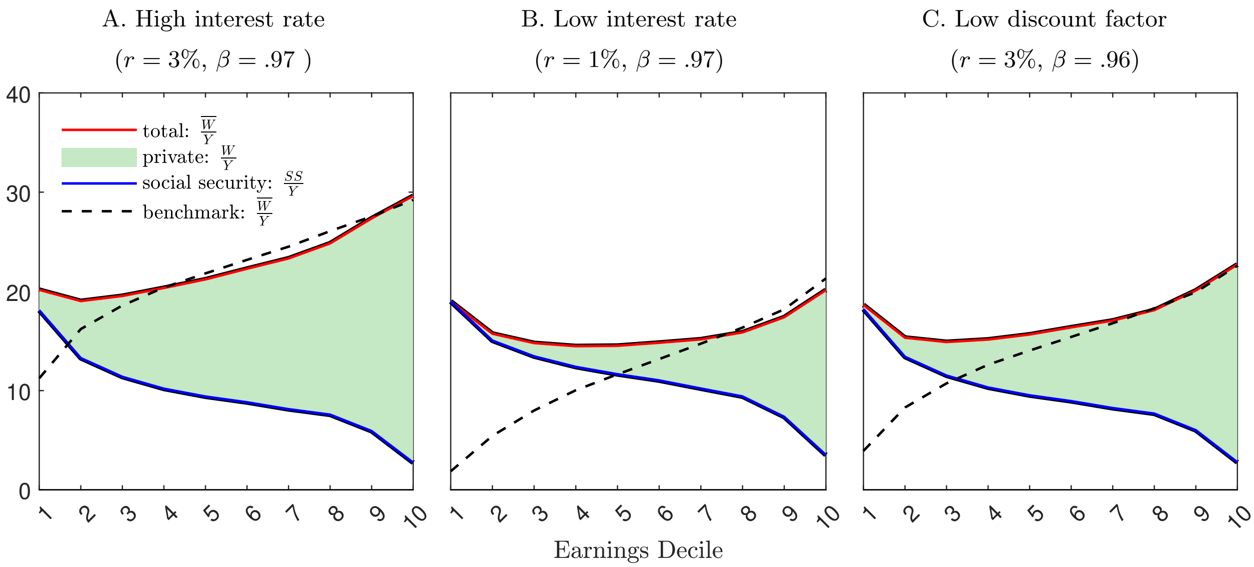
Conceptually, we show that the net present value of Social Security cash flows should be treated as wealth for two reasons.

The first reason is that Social Security and private wealth are substitutes: the more workers accumulate Social Security wealth, the less they need to save privately. By contrast, wealth and earnings tend to be complements: high earners accumulate more wealth. To illustrate the substitution between private and Social Security wealth, let's decompose the total saving rate into a private and a Social Security component:

$$\frac{\overline{W}_{it+1} - \overline{W}_{it}}{Y_{it}} = \frac{W_{it+1} - W_{it}}{Y_{it}} + \frac{S_{it+1} - S_{it}}{Y_{it}} \tag{3.2.14}$$

where $Y_{it} = L_{it} + \overline{W}r$ is yearly revenues. When interest rates are low, economic theory suggests that households should favor consumption, rather than saving, today. Reflecting this insight, Figure 3.1

Figure 3.1: Saving rates by earnings decile



decomposes the saving rates of 40-year old workers by earnings decile for different levels of interest rates r and discount factors β. As a benchmark, we also report savings rates in the absence of mandatory public savings through Social Security.

Figure 3.1 illustrates an attractive property of a holistic wealth concept: for unconstrained households, total saving rates are nearly identical with and without Social Security. Each dollar's worth of Social Security savings reduces private savings by the same amount as long as workers have enough liquid precautionary savings.[55]

Figure 3.1 shows that high earners have slightly higher total saving rates but lower Social Security rates. In our model, the relationship between total saving rates and earnings is a consequence of income uncertainty. Workers in the top deciles earn more than they expected, and thus their optimal consumption path for retirement has risen faster than their current level of savings—so their savings rate must rise. The opposite is true for workers in the bottom deciles.

On the other hand, Social Security savings rates are decreasing with earnings. Because payroll taxes

[55] Social Security wealth would not necessarily be more valuable if it were liquid as the program could no longer offer insurance against longevity and adverse income shocks. Importantly, many forms of marketable wealth such as retirement accounts, real-estate and private businesses are not perfectly liquid either.

are capped, low earners contribute a larger share of their earnings to Social Security. Moreover, because the benefit formula is progressive, low earners buy a larger annuity for each dollar of contribution. Overall, Social Security reduces the private savings rate of low earners much more than that of high earners and thus contributes to market wealth inequality.

The second reason Social Security should be treated as wealth is that, like private wealth, and unlike future labor earnings, it allows households to consume without working. Ideally, we would like to disentangle the share of Social Security wealth that households have already accrued through past work from benefits that they will earn from years in the labor force going forward. However, the nature of the Social Security formula means that the value of benefits that workers have already accrued is derivative of their future earnings trajectory: For example, note that workers early in their careers will be mislabeled as low earners (with high Social Security replacement rates) if we estimate their Social Security benefits only based on years already worked.

But in practice, the difficulty of disentangling accrued from future Social Security wealth is not problematic. As we show in Section 3.7.3, quantifying the value of Social Security based on the present value of expected benefits and doing so based on benefits that workers have already accrued result in substantially similar estimates. Essentially, to a first approximation, going forward, Social Security is a zero NPV investment for workers.[56]

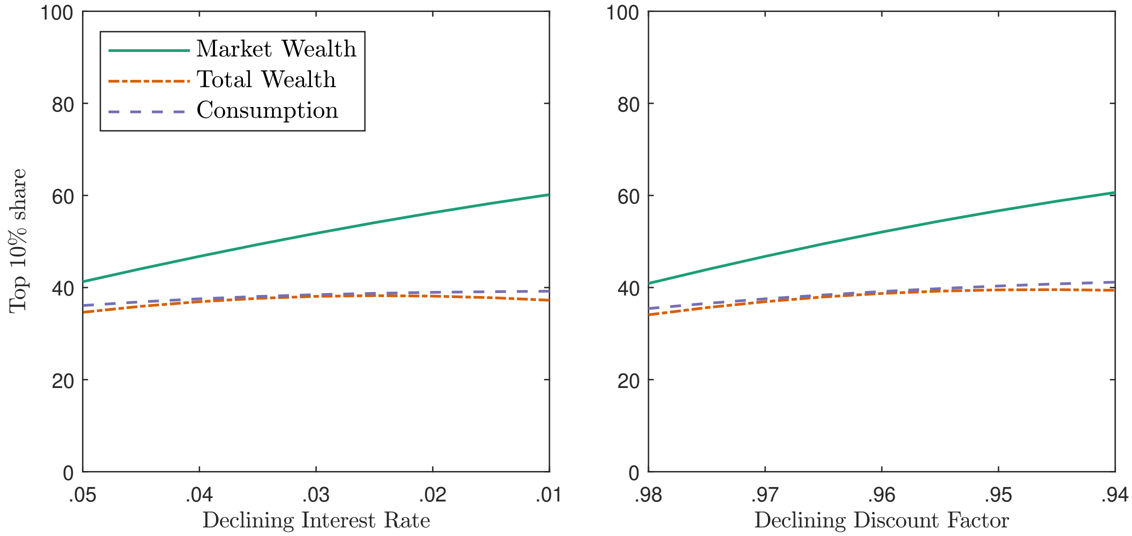
3.2.3. Wealth inequality and interest rates

Saez and Zucman (2016) argue that falling saving rates explain most of the drop in the bottom 90% share, whereas Greenwald et al. (2021) show that the evolution of top wealth shares tracks that of interest rates.

As Figure 3.1 shows, in the absence of Social Security, a lower interest rate or discount factor depresses wealth accumulation across the entire income distribution. However, in the presence of Social Security, the private saving rate of low and middle-class earners falls proportionally more

[56]If anything, the value of accrued benefits *exceeds* the net present value concept by roughly 10% (Nickerson and Burkhalter, 2019) since contributions made in the later years of a worker's career are actually negative NPV investments because the benefit formula is concave and only considers the best 35 years of earnings.

Figure 3.2: Top 10% shares at age 66



because contributions to Social Security do not change. If anything, workers accumulate *more* Social Security wealth in the low rate environment because their contribution buy annuities with higher market value. Overall, in the low interest rate environment, private savings are much more concentrated in the upper part of the income distribution. Yet, if anything, total savings are more equally distributed.

To further illustrate the role of Social Security, Figure 3.2 reports three measures of inequality at retirement age when the model is simulated with interest rates ranging, from left to right, between $r = .05$ and $r = .01$ and discount factors between $\beta = .98$ and .94. These measures are the top 10% share of wealth (W), total wealth ($\overline{W} = W + S$), and consumption. Figure 3.2 shows that a decline in interest rates or patience leads to an increase in market wealth inequality: a 4 pp drop in rates or the discount factor translates into a 20 point increase in the top 10% share of market wealth among new retirees. Lower incentives to save have a disproportionate effect on the *private* savings rates of low-earners, because they contribute a larger share of their income towards Social Security. On the other hand, the top 10% share of total wealth or consumption is barely changed. Moreover, measures of wealth inequality are more distorted by the omission of Social Security in a low rate environment or when households save less.

Hence, in a simple consumption model, when interest rates fall, measures of wealth concentration that overlook Social Security suggest an increase in wealth inequality that is not representative of the allocation of resources across the population.

3.3. Data

We use the triennial SCF for two main purposes: (i) measuring marketable wealth shares, and (ii) estimating aggregate Social Security wealth, and determining the share of Social Security wealth going to the wealthy. The SCF is well suited to measure marketable wealth shares as it covers most asset classes and provides detailed information on liabilities. We measure marketable wealth using the net worth variable: the sum of assets net of liabilities. One caveat is that the SCF does not survey extremely wealthy households. To fill this gap, we follow Saez and Zucman (2016) by adding the Forbes 400 list to the richest 0.01%.

We compute the Social Security wealth of retirees using detailed data on retirement and survivor benefits. The ability to observe benefits at the household level allows us to observe the joint distribution of Social Security and marketable wealth among retirees. We rely on this joint distribution to assign Social Security wealth between the top and bottom of the distribution.

Yield curve data come from the Federal Reserve. These data provide estimates of the zero-coupon yield curve using off-the-run Treasury coupon securities for horizons up to 30 years. To obtain interest rates at horizons greater than 30 years, we extend this series by repeatedly applying the 29-to-30 year forward rate to the annualized spot rate at 30 years. Hence, the annualized spot rate at $30 + h$ is $r_{t,t+30+h} = \left((r_{t+29,t+30})^h (r_{t,t+30})^{30}\right)^{\frac{1}{30+h}}$. Our assumption is that this forward rate represents the long-run interest rate on nominal government claims.

We use historical inflation, wage growth, and discount rate projections from past SSA Annual Reports to calibrate and validate our valuation model. We also collect Social Security parameters such as the time series of the Social Security bend points, national wage index, maximum taxable earnings, and cost-of-living index from the SSA website.

3.4. Valuing Social Security

In this paper, we trace out how accounting for Social Security impacts trends in wealth concentration. To do so, we estimate the evolution of Social Security wealth by cohort. Then, we distribute this wealth between the top 10% or 1% and the rest of the population. We proceed differently for retirees and workers.

3.4.1. Retirees

For retirees, calculating Social Security wealth is relatively straightforward, since we observe their marketable wealth and Social Security benefits in the SCF. As there are no more taxes to be paid, Social Security wealth is

$$S_{it} = \sum_{s=t}^{T} \left(\prod_{k=t}^{s-1} (1 - m_{itk}) \right) \frac{B_{it}}{(1 + r_{t,s})^{s-t}} \frac{\mathbb{E}[P_s]}{P_t} \tag{3.4.1}$$

where nominal benefits are indexed to the consumer price index P_t. We also include survivor benefits in this calculation. Survivor benefits are paid to the surviving spouse and can represent up to 100% of the benefits of the deceased husband or wife (see details in Appendix C.2.4).

3.4.2. Workers

For working-age cohorts, we simulate the distribution of income paths of their members using the income process laid out in Equations (10.1)-(10.7). Then, we apply the Social Security benefit and tax formulas to compute average Social Security wealth by cohort, gender, and year.

Taxes As in Equation (3.2.3), we assume that workers will keep paying 10.6% of their earnings below the Social Security wage base.

Benefits For simplicity, we assume that workers retire at the cohort-specific full retirement age. We compute the AIYE as the average of the best 35 years of indexed earnings $\mathrm{L}_{it}^{\text{indexed}}$, as defined in equation (3.2.4). Yearly benefits depend on year of birth c, and the marginal replacement rate drops at two cohort-specific bend points, $b_{1,c}$ and $b_{2,c}$. Hence, benefits are concave and piece-wise linear function of AIYE:

$$B_{it} = \begin{cases} \frac{P_t}{P_{c_i+60}} \cdot .9 \cdot \text{AIYE}_i & \text{if AIYE}_i < b_{1,c_i} \\ \frac{P_t}{P_{c_i+60}} \left[.9 \cdot b_{1,c_i} + .32(\text{AIYE}_i - b_{1,c_i})\right] & \text{if } b_{1,c_i} \leq \text{AIYE}_i < b_{2,c_i} \\ \frac{P_t}{P_{c_i+60}} \left[.9 \cdot b_{1,c_i} + .32(b_{2,c_i} - b_{1,c_i}) + .15(\text{AIYE}_i - b_{2,c_i})\right] & \text{if } b_{2,c_i} \leq \text{AIYE}_i. \end{cases} \tag{3.4.2}$$

where $\frac{P_t}{P_{c_i+60}}$ is an adjustment for the increase in the consumer price index since the retiree turned 60.[57]

3.4.3. Aggregate Social Security wealth by cohort

For households below the minimum retirement age, we aggregate the Social Security wealth of workers at the cohort level using the SCF demographics weights ω_{it}. Specifically, we have:

$$S_{ct}^{\text{total}} = \sum_{i \in c} \omega_{it} \cdot S_{gct}^{\text{mean}}, \tag{3.4.3}$$

where S_{gct}^{mean} is the mean social security wealth by gender and cohort. We account for the estimated 20% of women and 10% of men do not contribute to Social Security, as detailed in Appendix C.2.5.

For respondents from 62 to 66, the simulated data and SCF overlap. For those whose benefits are reported in the SCF, we rely on these estimates. For individuals without benefits, we fill in the average simulated Social Security wealth, adjusting for the non-contributing share of the population. For individuals aged 66-69 who have not yet claimed their benefits, we backfill average benefits and wealth from the succeeding survey for respondents from 70 to 73 years of age (see details in Appendix C.3). Finally, for cohorts above age 69, we simply aggregate individual-level estimates of Social Security wealth using SCF survey weights.

3.4.4. Calibration

Lifetime income profiles We assume $g(t)$ to be cohort and gender-specific. Guvenen, Kaplan, Song, and Weidner (2018) report the average earnings of each cohort c

[57] In practice, workers can claim benefits as early as age 62 or as late as age 70. However, this option is relatively fairly priced as retiring earlier (later) reduces (increases) benefits in a proportion consistent with life expectancy at retirement, such that overall the total present value of benefits remains the same (Auerbach et al., 2017).

and gender g by year from 1957 to 2013. First, we divide these time series by the wage index $L_{1,t}$ to get the average realization of L_2 of each cohort-gender group: $\mathrm{L}_{2,cgt}$. Then, we estimate $g_{cg}(t)$ by regressing $\ln(\mathrm{L}_{2,cgt})$ on a cubic polynomial of age. The data includes workers who enter the labor force from 1949-2016. For cohorts where there is insufficient labor market data to estimate $g(t)$ directly, we rely on estimates for nearby cohorts, whose earnings trajectories follow similar paths.

Social Security parameters To obtain Social Security wealth for a given year, we use actual Social Security parameters up to that year as they are stated on the SSA website. We then assume that future Social Security parameters will scale up with the wage index, which has been the case over our sample period. Hence, we assume that the Social Security wage base will remain 2.5 times the wage index ($\mathrm{SSWB}_t = 2.5 \cdot L_{1,t}$), and the bend points of the benefits formula will remain .21 and 1.25 the wage index ($b_{1,t} = 0.21 \cdot L_{1,t}$ and $b_{2,t} = 1.25 \cdot L_{1,t}$). We assume that Social Security respectively covers 90% and 80% of the male and female populations (see Appendix Figure C.4).

Macroeconomic assumptions Because they are inflation-indexed, Social Security cash flows should be discounted using the real yield curve. In our baseline specification, we use the nominal yield curve for Treasury notes with data coming from the Federal Reserve, described in Section 3.3. Therefore, we let cash flows grow with the consumer price index. We use inflation projection from SSA reports, as we are not aware of another source for long-term inflation projections since 1989. Wage growth projections also come from the SSA reports. We discuss alternative growth scenarios in Section 3.8.1.

Mortality and differences in life expectancy Survival probabilities are calibrated to the historical mortality rates by gender from 1989–2017 coming from the Human Mortality Database (HMD), which we adjust for differences in life expectancy by income. Individuals with higher earnings live longer: life expectancy for men in the top 1% by income is nearly 15 years longer than average life expectancy for the bottom 1% (Chetty et al., 2014). We adjust for these differences using data from the Health Inequality Project (HIP) by allowing survival probabilities of SCF respondents receiving Social Security retirement benefits to differ by income.[58] Our adjustment

[58] We proxy for the permanent income distribution using the Social Security benefits distribution because benefits are, by construction, a proxy for lifetime earnings.

effectively makes high income retirees younger and low income retirees older, a procedure discussed in Appendix C.2.3.

3.4.5. Validation

To validate our methodology, we check (i) that the benefits predicted by our simulation match the data, (ii) that, when using the same discount rates as the SSA, we obtain similar estimates of the evolution of aggregate Social Security wealth, and (iii) that the use, due to data availability, of a nominal rather than real yield curve is not driving our results.

Matching observed benefits at retirement age In Figure 3.3, we compare simulated and observed benefits for retirees between ages 62 and 67. For those who did not retire at full retirement age, we use Social Security rules to determine what their full retirement age benefits would be if they had (see Appendix C.2.2). The simulated data track observed benefits closely.

Matching SSA estimates of aggregate Social Security wealth Every year, the SSA estimates the present value of expected benefits to current participants, net of their expected payroll taxes. Our goal is not to replicate the SSA estimates, as the SSA actuaries' assumptions regarding the level and slope of the yield curve are inaccurate. Rather than using a market-implied spot rate to discount future cash flows, the SSA projects rates based on interest rate movements in prior business cycles, which drastically understates the decline in interest rates. As reported in Appendix Figure C.3, the SSA discount rates fell by only 2 pp between 1989 and 2019 while the market yield curve fell by three times that amount.

Nevertheless, Figure 3.4 shows that, if we choose to use the SSA's discount rates, the evolution of aggregate Social Security wealth reported by the SSA tracks our estimates. This gives us confidence in our simulated estimate of workers' lifetime earnings histories, from which we derive their Social Security wealth. For comparison, we also include our estimate of aggregate Social Security wealth discounting based on the market-implied yield curve. The deviations between discounting based on SSA projections and Treasury reported rates is fairly small in the first decade of our sample, but it grew substantially in the last 15 years. In 2019, SSA-implied aggregate Social Security wealth was nearly \$32 trillion, compared to over \$50 trillion when using market rates.

Figure 3.3: Simulated and actual full retirement age benefits

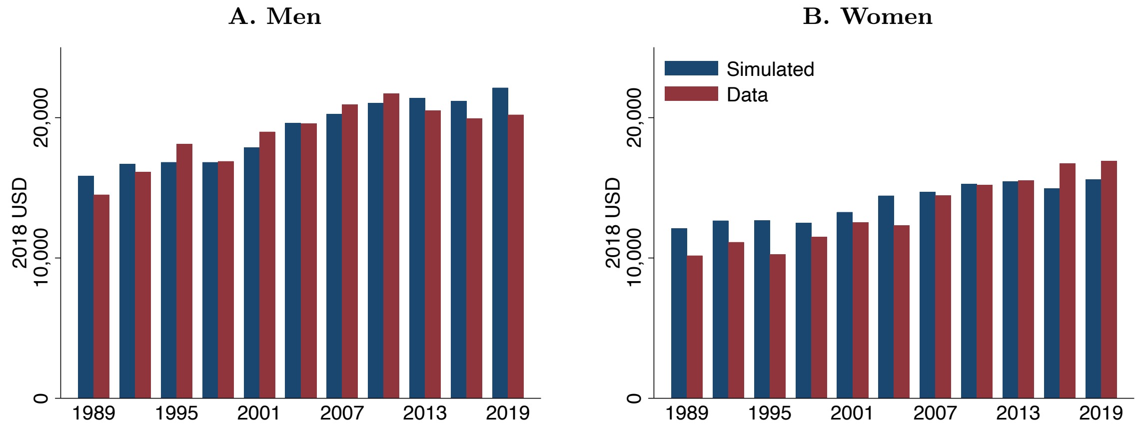
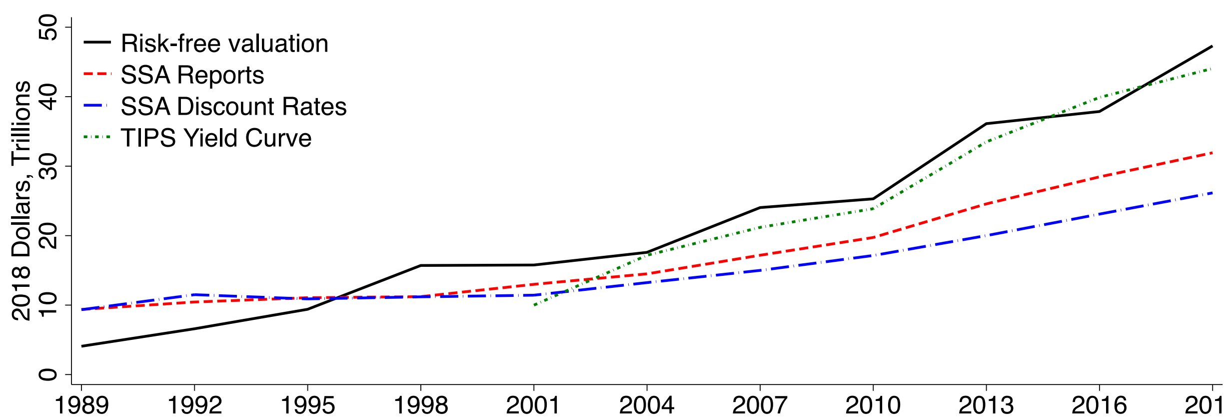


Figure 3.4: Aggregate Social Security wealth under alternative discount rates

This figure reports estimates of the aggregate present value of Social Security. The "SSA Reports" line reports estimates by the Office of the Chief Actuary (OACT). We subtract the value of the Disability Insurance program by assuming that it represents 1.8/12.4 of the total, which is consistent with the allocation of payroll tax revenues. The "SSA Discount Rates" line reports our estimates using OACT discount rates. The "TIPS Yield curve" line reports our estimates when we assume no inflation and use the real yield curve implied by treasury inflation-protected securities. The "Risk-free valuation" line reports our estimates using the nominal market yield curve.



Using the real yield curve to validate inflation forecasts Finally, because we discount future cash flows using the nominal yield curve, our findings are sensitive to inflation forecasts, which we take from SSA annual reports. To make sure that our results are not driven by these assumptions, we also discount future cash flows using the real yield curve implied by the price Treasury Inflation Protected Securities (TIPS) and assume no inflation. This exercise can only be done for the 1999-2016 period. As reported in Figure 3.4, this alternative methodology implies a faster increase in aggregate Social Security wealth than ours;[59] as such, our findings are not driven by challenges with forecasting inflation.

3.4.6. Assigning Social Security wealth to the top

With an aggregate value of Social Security wealth, we now turn to allocating this wealth across the distribution. Our strategy to assign each cohort's Social Security wealth to the top and bottom of the distribution depends on whether households have already claimed their benefits.

Retirees For retirees, we compute Social Security wealth at the individual level. Hence, we can precisely estimate the share of Social Security wealth that is captured by each centile of the overall marketable wealth distribution.

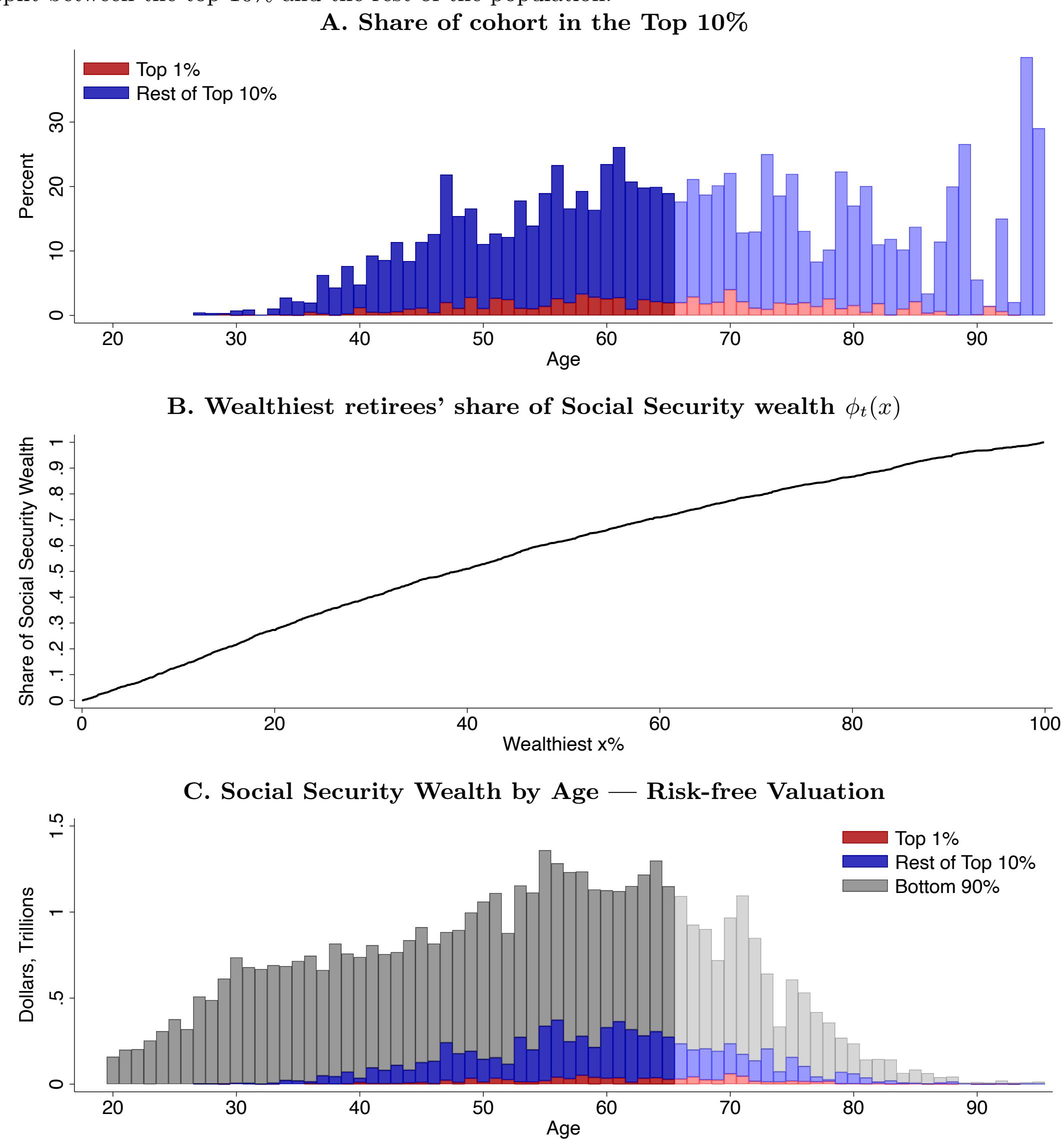
[59]There is an economically significant deviation between the nominal and TIPS discounted valuations in 2001. However, TIPS rates were not representative of the real risk-free rate in the early part of the sample from 1999-2003 (Fleming and Krishnan, 2004).

Figure 3.5: Assignment of Social Security wealth by working-age cohort in 2019

This figure illustrates how we allocate the Social Security wealth of working-age cohorts between the top 10% and bottom 90% of the wealth distribution. Following Equation (3.4.4), the Social Security wealth of cohort c going to the top 10% is:

$$S^{10\%}_{c,2019} = \phi_{2019}(\text{Share in Top }10\%_{c,2019}) \cdot S^{\text{total}}_{c,2019}$$

In Panel A, we report the share of households falling in the top 10% of the overall wealth distribution, that is Share in Top $10\%_{c,2019}$. In Panel B, we estimate the share of the Social Security wealth of young retirees (65–75) that goes to the richest x% of that group, that is the function $\phi_t(x)$. Panel C reports total social security wealth by cohort, split between the top 10% and the rest of the population.



Workers For workers, we need to divide our cohort-level estimates of aggregate Social Security wealth between the top 10% or 1% and the rest of the population.We assume that the share of

Social Security Wealth going to the top 10% is:

$$S_{ct}^{10\%} = \phi_t(\text{Share in Top } 10\%_{ct}) \cdot S_{ct}^{\text{total}} \tag{3.4.4}$$

where Share in Top $10\%_{ct}$ is the share of cohort c in the overall top 10% of the market wealth distribution in year t. The function $\phi_t(x)$ represents the share of Social Security wealth held by the wealthiest $x\%$ of young retirees in the same year. Both Share in Top $10\%_{ct}$ and $\phi_t(.)$ are readily observable in the SCF.

A numerical example illustrates our approach.

1. In 2019, 60 year-olds had $1.2 trillion in Social Security wealth.

2. As Panel A of Figure 3.5 shows, in 2019, 23% of 60-year old households were in the top 10% of the overall marketable wealth distribution.

3. Panel B shows that, within the population of young retirees (65-75), the wealthiest 23% held 30% of the Social Security wealth of this age group.

4. Hence, we allocate $360 billion ($1.2 trillion x .30) of 60 year-olds' Social Security wealth to the top 10% in 2019.

By repeating this exercise for all working-age cohorts, we determine the overall amount of simulated Social Security wealth owned by the top 10% and bottom 90% in 2019. We use the same procedure for other survey years and the top 1%.

In this exercise, our key assumption is that the share of Social Security wealth that accrues to different centiles of the market wealth distribution is constant across ages. There are several reasons why this assumption is reasonable for our exercise. First, this assumption is most tenuous for the youngest workers. However, as illustrated by Panel A of Figure 3.5, the implications of any potential mis-allocation of Social Security wealth for these cohorts are quantitatively irrelevant to our exercise, because their chances of being in the top 10% of the overall population are negligible. Relatedly, the

Social Security wealth of current workers is concentrated among those approaching retirement, who are nearly finished paying into Social Security and have yet to claim their benefits. As illustrated by Panel C of Figure 3.5, 79% of the Social Security wealth of the top 10% goes to households above age 55 and the share going to those below 45 is close to zero. For workers above 55, relying on the relationship between marketable wealth and Social Security wealth observed for retirees is sensible.

If anything, our assumption overstates the share of Social Security wealth that accrues to the top 10% because the value of Social Security is low and perhaps even negative for the wealthiest individuals in younger cohorts. Social Security is progressive, and so it offers higher replacement rates to low earners. Though high earners who recently retired have more Social Security wealth than low earners, each dollar has been bought at a higher price. At retirement, this price is sunk and does not change their Social Security wealth. However, for younger cohorts, a large fraction of this cost remains to be paid, which reduces the net present value of Social Security disproportionately for high earners.

3.4.7. Baseline top wealth shares

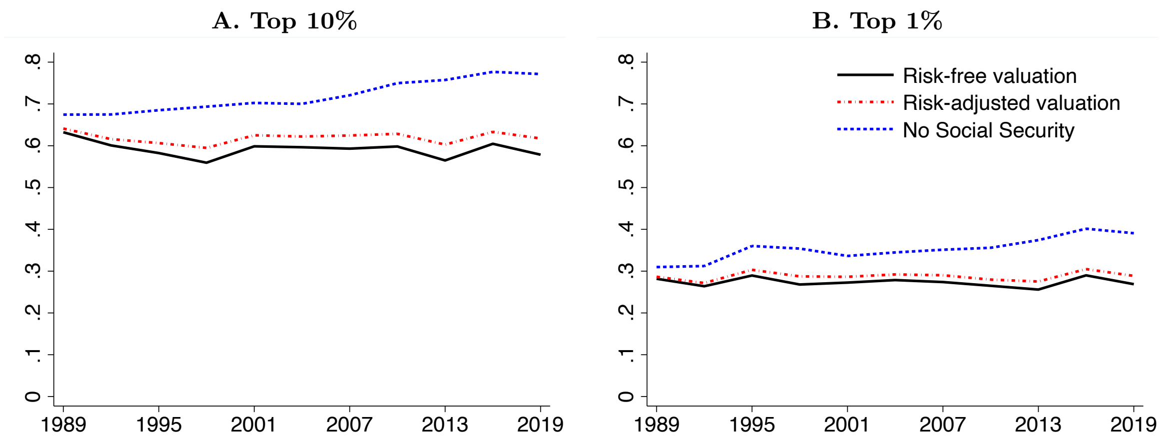
Figure 3.6 reports the levels and trends of top wealth shares with and without Social Security wealth.[60] We define top wealth shares based on the top 10% and top 1% of the population by measures of marketable wealth. This allows for comparison of how previously documented inequality trends are impacted by the inclusion of Social Security.

Panel A focuses on the top 10%. The top 10% share of market wealth grew by 10 pp between 1989–2019. Once Social Security wealth is included, this trend is reversed. Rather than rising, the top 10% wealth share falls by 5.4 pp over this period. Panel B shows the impact of Social Security wealth on top 1% wealth share. When Social Security wealth is excluded, the top 1% share has grown by 10 pp over our sample period. Once it is included, the top 1% share has risen by 1.3 pp.

[60] We find similar declines in top wealth shares when other data on marketable wealth are used, as discussed in Appendix Section 3.8.5.

Figure 3.6: Top 10% and Top 1% Wealth Shares with and without Social Security

This figure reports the evolution of the top 10% and 1% wealth shares with and without Social Security wealth. In the risk-free valuation, cash flows are discounted using the yield curves implied by the price of government bonds. In the risk-adjusted valuation, we adjust discount rates to account for the long-run cointegration between the labor and stock markets, as detailed in Section 3.5.1.



3.5. Accounting for macroeconomic risk

The rate of return of pay-as-you go systems is the sum of the growth rates of the population and per capita earnings (Samuelson, 1958). For U.S. Social Security, the relationship between returns on contributions and the long-run growth in earnings is explicitly achieved through wage-indexation. Wage-indexation exposes Social Security participants to long-run macroeconomic risk, and discount rates should reflect this systematic risk.

Social Security cash flows perfectly scale up with the national wage index. Since 1980, the Social Security wage base and bend points have been growing at the same rate as earnings. In Section 3.4.2, we show that tax payments are proportional to the wage index, whereas benefits are proportional to the wage index the year a worker turns 60. Therefore, a diversified investor would discount these cash flow using the expected return on an asset delivering a single wage-indexed coupon with the same years of indexation and payment. In this Section, we determine the expected return for such a security.

3.5.1. Market beta of Social Security cash flows

At what rate should we discount a cash flow that is proportional to the average level of earnings $L_{1,t+n}$ in n years? To answer this question, we assume that the stock and labor markets are cointegrated as documented in Benzoni et al. (2007) and as would be expected if the shares of labor and profits are stable over long periods. Specifically, we assume that the log of L_1 evolves as follows:

$$dl_{1,t} = \left((\phi - \kappa) y_t + \mu - \delta - \frac{\sigma_l^2}{2} \right) dt + \sigma_l dz_{1,t}, \tag{3.5.1}$$

where $\mu - \delta$ determines the unconditional log aggregate growth rate of earnings and σ_l its volatility. Log stock market gains follow:

$$ds_t = \left(\mu + \phi y_t - \frac{\sigma_s^2}{2} \right) dt + \sigma_s dz_{2,t}, \tag{3.5.2}$$

where μ and σ_s represent expected stock market log returns and their volatility. The state variable y_t keeps track of whether the labor market performed better or worse than the stock market relative to expectations. Specifically, y_t evolves as follows:

$$dy_t = -\kappa y_t + \sigma_l dz_{1,t} - \sigma_s dz_{2,t}, \tag{3.5.3}$$

where κ determines the strength of the cointegration. If the two markets are cointegrated, y_t should mean revert to zero. Mean reversion takes two forms. If stock markets gains are caused by higher long-run economic growth, wages will catch up. If stock market returns have nothing to do with future economic growth, we should expect them to mean revert. The parameter ϕ controls the fraction of the mean reversion in y_t caused by mean reversion in stock market returns.

In Appendix C.4, we show that the market beta of a security delivering a single coupon proportional to $L_{1,t+n}$ is:

$$\beta_t^{L_1,n} = \left(1 - \frac{\phi}{\kappa} \right) \left(1 - e^{-\kappa n} \right) \ , \tag{3.5.4}$$

and we demonstrate that, under the no-arbitrage condition, the expected return on this security is:

$$\mathrm{E}_t\left[r_t^{L_1,n}\right] = \beta_t^{L_1,n}\left(\mu - r\right) + r \tag{3.5.5}$$

where r is the risk-free rate. Note that, assuming policy risk away, any Social Security payment proportional to L_{t+n} would deliver the same expected return if it were publicly traded, as all other sources of risk are purely idiosyncratic.

Our empirical exercise is in discrete time, so we approximate our results by assuming that the discount factor for a cash flow proportional to $L_{1,n}$ paid in year k is:

$$\chi_{t,n,k} \approx \left[\prod_{s=t}^{n}\left(1 + \beta_s^{L_1,n}\left(\mu - r\right) + r_{ts}\right)\prod_{s=n+1}^{k}\left(1 + r_{ts}\right)\right]^{-1}, \tag{3.5.6}$$

and the risk-adjusted present value of Social Security is:

$$\text{Adj. S}_{it} = \sum_{s=t+1}^{T}\left(\prod_{k=t}^{s-1}(1 - m_{itk})\right)\left(\mathbb{E}\left[\mathrm{B}_{it}\right]\cdot\chi_{t,c_i+60,s} - \mathbb{E}\left[\mathrm{T}_{it}\right]\cdot\chi_{t,s,s}\right) \tag{3.5.7}$$

where real benefits are indexed on L_1 in the year in which the worker turns 60.

We calibrate the model as in Benzoni et al. (2007) who estimate $\kappa = .16$ and $\phi = .08$ using U.S. data from 1929 to 2004. This implies a market beta of 0.5 for for the most distant indexed cash flows. The equity premium is $\mu - r = .06$.

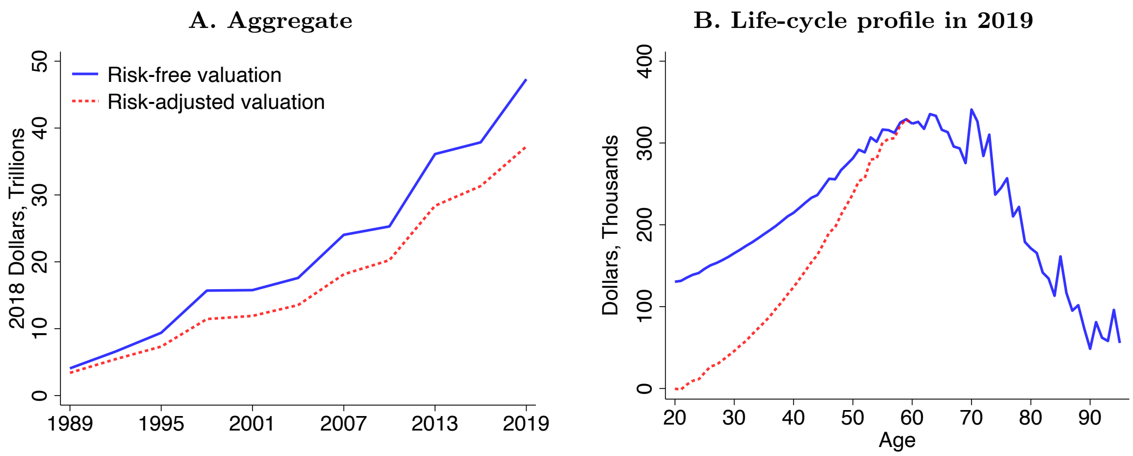
3.5.2. Risk-adjusted results

Panel A of Figure 3.7 reports aggregate Social Security wealth with and without adjusting for systematic labor market risk. Panel B shows that the adjustment is larger for young workers: it cuts the Social Security wealth of a 25-year old's benefits by 60%.

Once macroeconomic risk associated with Social Security cashflows is factored in, Figure 3.6 shows that the share of the top 10% decreased by 2.4 pp and that the top 1% share has increased by 0.2 pp. This finding differs from our baseline risk-free specification because Social Security wealth is

Figure 3.7: Risk-adjusted valuation

Panel A presents aggregate Social Security wealth in 2018 dollars. Panel B presents average Social Security wealth by age in 2019.



smaller, and therefore plays a lesser role in the evolution of wealth inequality. The risk-adjusted results primarily decrease Social Security wealth for younger workers, who are rarely in the top 10%. Consequently, our risk adjustment decreases the wealth of the bottom 90%, with only a small impact on the Social Security wealth of the top 10%. Regardless, top wealth shares remain substantially attenuated relative to prior work.

3.6. Role of interest rate environment

The decline in interest rates has important implications for wealth inequality. Over the last 30 years, long-duration assets have dramatically outperformed short-duration assets (Binsbergen, 2020). Because rich households invest in longer-duration assets such as stocks and private businesses, the decline in interest rates can explain most of the increase in market wealth inequality (Greenwald et al., 2021). However, the focus on market wealth overlooks the largest long-duration investment of most households: their Social Security contributions.

3.6.1. Social Security as a leveraged exposure to duration

For working-age households, Social Security benefits are disbursed years into the future, while taxes are paid into the program today. Essentially, the exposure to rates through future tax payments can be replicated by selling short- and medium-term bonds, and the exposure through benefits can be

Table 3.1: Impact of leverage and interest rates on Social Security wealth

This tables decomposes the increases in Social Security wealth between 1989 and 2019. Columns (a) and (b) report the weights of future benefits and taxes in the net present value of Social Security in 1989. Columns (c) and (d) report the increase in the present values of benefits and taxes. The last column reports the percentage change in Social Security wealth.

	Share of Social Security wealth in 1989		Change since 1989		
	Benefits (a)	Taxes (b)	Benefits (c)	Taxes (d)	NPV (a)·(c)+(b)·(d)
Bottom 99%	248%	-148%	491%	136%	1015%
Top 1%	123%	-23%	436%	142%	504%
Entire population	243%	-143%	490%	136%	994%

replicated by buying long-term bonds. In Table 3.1, we report the weights of benefits and taxes in the overall net present value of Social Security and the change in the value of each component since 1989. Because benefits (the long position) have a longer duration, when rates fall, their present value rises faster than that of taxes (the short position). The result is a rapid increase in the net present value of Social Security.

This increase is especially important for the bottom of the wealth distribution for two reasons. First, Social Security represents a larger share of their total wealth. Second, as the last column of Table 3.1 shows, the value of Social Security has increased 511 pp more for the bottom 99% than for those in the top 1%.

3.6.2. Why low rates make Social Security more valuable

Even if it is primarily driven by interest rate changes, the evolution of Social Security wealth matters to our understanding of inequality trends. Consider a household that is 20 years away from retirement and seeks to save enough to finance one dollar of consumption for 20 retirement years. Assuming an interest rate of 5%, as in 1989, this household needs to save \$0.38 per year over the next two decades. If the interest rate is 0.8%, as in 2019, this household needs to save \$0.85 annually. Now, if the rate of returns on Social Security contributions has been constant at 2.5%, which it has since 1989, the same can be achieved by contributing approximately \$0.61 every year to Social Security. So, in effect, from this household's point of view, one dollar of Social Security contribution was equivalent to \$0.62 (\$0.38/\$0.61) of private saving in 1989, but is equivalent to \$1.39 (\$0.85/\$0.61) in 2019. Said another way, the future purchasing power of \$1 of Social Security contributions corresponds to more private savings when rates are low. Discounting based on market rates is important as it captures the fact that the attractiveness of Social Security depends on its rate of returns relative to comparable investment opportunities on the private market.[61]

Incidentally, whether the increase in Social Security wealth corresponds to a real improvement in well-being is not key to our choice of an appropriate discount rate. This same question can be asked of marketable assets whose value has increased faster than the cashflows they generate (Greenwald et al., 2021). To compute shares correctly, one dollar of Social Security wealth must afford the same consumption as one dollar of marketable wealth in any given year. To do this, we need to discount cash flows using the prevailing cost of exchanging current and future dollars—the contemporary yield curve.

To grasp the importance of consistency, consider a household who just retired in 2019 and bought an annuity that exactly matches their Social Security benefits. It is difficult to understand why we would value this annuity using 1989 market prices. It is equally difficult to see why Social Security benefits would not have the same value as this annuity. If the annuity—and other marketable assets—are valued using contemporary prices, the same should be done for Social Security benefits.

Similarly, if a household has mortgage payments representing 10.6% of their earnings, then from a consumption standpoint, these cashflows have the same implications as Social Security taxes. But they correspond to a smaller mortgage in 1989, when rates were high, than in 2019. Therefore, they register as a greater liability in the computation of marketable net wealth in 2019 than in 1989. This mechanical upsurge in debt contributes to the rise in marketable wealth inequality documented by Saez and Zucman (2016). It is inconsistent that an increase in the present value of the mortgage payments would be accounted for in wealth inequality estimates, but not an equivalent increase in the value of Social Security taxes.

[61] In work following ours, Sabelhaus and Volz (2020) instead apply a constant discount rate to Social Security cashflows. This is a mistake because it ignores the effect of interest rates on asset prices, one of the main causes of rising marketable wealth inequality.

Table 3.2: Decomposing the increase in Social Security wealth

This table lays out the relative importance of changes in interest rates, the aging of the population, life expectancy, the scope of the Social Security retirement program, and the size of the population to the growth of aggregate Social Security wealth. The first row is calculated as the difference between log per capita Social Security wealth in 2019 and log per capita Social Security wealth in 2019 under the 1989 yield curve. The second row is computed by subtracting log per capita Social Security wealth in 2019 under the 1989 yield curve from Social Security wealth in 2019 under the 1989 age distribution and yield curve. The third row is computed by subtracting log per capita Social Security wealth in 2019 under the 1989 age distribution and yield curve from log per capita Social Security wealth in 2019 under the 1989 age distribution, yield curve, and survival probabilities. The fourth row is computed by subtracting log per capita Social Security wealth in 2019 under the 1989 age distribution, yield curve, and survival probabilities from log per capita Social Security wealth in 1989. The total log per capita wealth change is given by $\log(SSW^{2019})-\log(SSW^{1989})$ where both terms are calculated under the 2019 and 1989 populations, life expectancies, benefit policies, and yield curves, respectively.

	Valuation method	
	Risk-free	Risk-adjusted
Change in yield curve	1.239	1.099
Shift in age distribution	0.290	0.348
Life expectancy	0.262	0.254
Social Security expansion & other	0.338	0.369
Log total per capita	2.128	2.069
Population growth	0.323	0.323
Log total	2.451	2.392

3.7. Discussion

3.7.1. Factors contributing to Social Security's growth

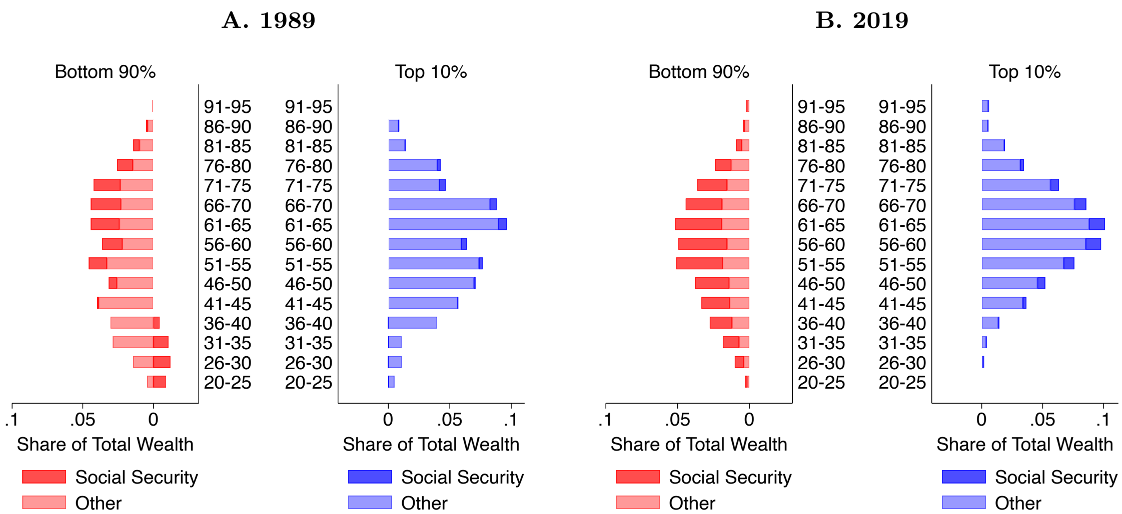
Table 3.2 lays out the several contributors to Social Security's growth. These include changes in demographics (Social Security wealth is highest for those nearing retirement, who are a larger share of the population today), increasing life expectancy (average life expectancy increased by 3.5 years since 1989), and the expansion of the program (the share of earnings subject to Social Security taxes increased from 1.25 times average earnings to 2.5 times), as well as the interest rate environment. But by far the largest contributor is changes in the yield curve previously discussed, which drives 46% of Social Security's growth (51% with risk-free valuation).

3.7.2. Shifts in the composition of wealth

Figure 3.8 reports how total wealth is distributed by age and between the top 10% and the rest of the population. The overall share of the top 10% has not changed much between 1989 and

Figure 3.8: Total Wealth Distribution by Age — Risk-adjusted valuation

This figure plots the shares of total wealth by age group for Social Security and non-Social Security wealth for 2019 and 1989 using the risk-adjusted valuation method.



2019, nor has its composition. On the other hand, the composition of the wealth of the bottom 90% has changed dramatically. In 1989, Social Security only represented 17.2% of the total wealth of the bottom 90%. Because the rate of return on Social Security contributions was lower than risk-adjusted discount rates, Social Security wealth was even negative for households below age 40. In 2019, Social Security represents 56.9% of the wealth of the bottom 90%. The constituents of wealth held by the bottom and top of the distribution have diverged, making clear why a focus on marketable wealth inequality alone is misleading.

3.7.3. Comparing Social Security and private wealth

Some suggest Social Security should be excluded from wealth concentration estimates based on a few arguments: first, that Social Security wealth is uncertain, without a readily available market value; second, that Social Security benefits cannot be passed down to heirs like private wealth; and third, that Social Security wealth is illiquid and cannot be used to absorb shocks today (Zucman, 2019).

None of these arguments are compelling. First, many sources of wealth included in existing estimates, for example pension wealth, are also illiquid. It is true that, relative to retirement accounts, there is more uncertainty in Social Security's value given policy risk (which we address in Section 3.8.1). But a significant contributor to rising top wealth shares—private business wealth—is similarly illiquid, and of much more uncertain value than Social Security (Bhandari et al., 2020). Further, unless beneficiaries die prematurely, retirement benefits not used to finance consumption in retirement are bequestable. Finally, the illiquidity of Social Security is in and of itself a policy choice so that the program can provide longevity insurance to retirees and guarantee a minimum level of wealth to those who may otherwise save too little. This means it can be relaxed (Catherine et al., 2020). But this choice does not detract from the fact that Social Security is the primary source of income for all but the very wealthiest retirees, and so is relevant to our understanding of inequality.

A significant share of Social Security wealth accrues to households who are not liquidity constrained. Households above age 50 own 79% of Social Security wealth. As Figure C.6 shows, of those 50 and older, 64% have more than $10,000 in accessible wealth and 44% have more than $100,000.

3.8. Robustness

We next consider the extent to which our baseline results are sensitive to alternative assumptions that impact our estimates of aggregate Social Security wealth, including policy risk that beneficiaries will not receive all promised benefits or that taxes will rise to replenish a depleted trust fund; weak economic growth; and differences in mortality between the rich and the poor. Table 3.3 presents results using alternative assumptions, which we discuss in turn below.

Our overall conclusion—that the inclusion of Social Security substantially attenuates the growth in top wealth shares—is not sensitive to the specification chosen. The top 10% and 1% shares of marketable wealth (excluding Social Security) rose by 9.7 and 8.1 pp respectively between 1989–2016. Once Social Security is included, using our most conservative set of assumptions, the top 10% and 1% shares grow by only a small fraction of that over this horizon.

3.8.1. Accounting for Social Security policy risk

An important caveat to our baseline calculations is the imminent depletion of the Social Security trust fund: within the next 15 years, absent entitlement reform, the SSA will not be able to meet

Table 3.3: Robustness checks

Panel A reports our baseline results. First, we report top shares of marketable wealth in the SCF+Forbes 400. We then report top wealth shares including our risk-free and risk-adjusted valuations of Social Security. In Panel B, we address the projected funding gap by cutting Social Security benefits or increasing taxes. We calibrate our wage growth assumptions and benefits cuts/tax increases based on the baseline ("Intermediate cost") and pessimistic scenarios ("High cost") used by the SSA. Under the high cost assumptions, the trust fund is depleted earlier and wages grow less than under the intermediate cost assumptions. Panel C shows additional robustness tests. First, we assume that expected wage growth declined linearly from 1% in 1989 to 0% in 2019. Second, we report three specifications which adds 1, 2 or 3 percentage points to our discount rates to reflect a hypothetical liquidity premium. All specifications in Panels B and C use the risk-adjusted valuation method.

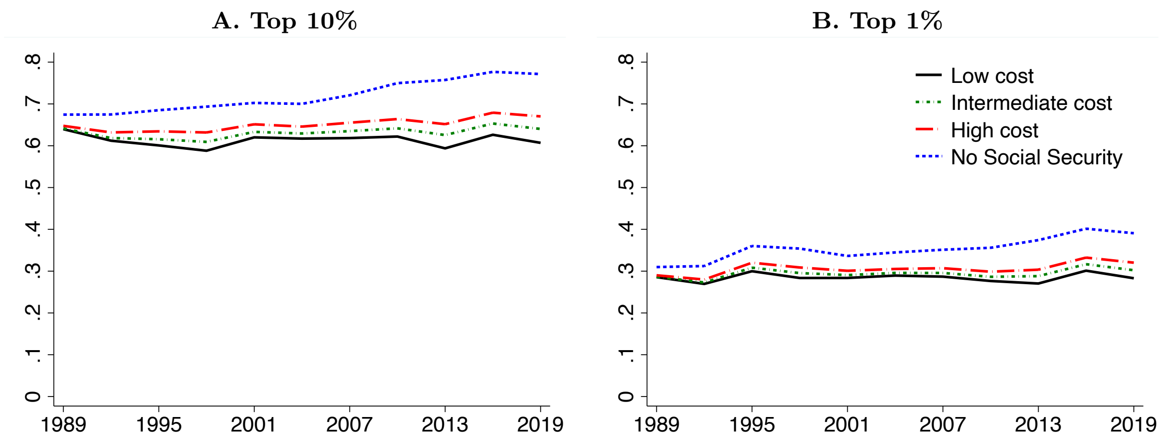
	Share of Top 10%			**Share of Top 1%**		
	1989	2019	Change	1989	2019	Change
Panel A: Baseline results						
Marketable wealth	67.5%	77.2%	9.7%	31.0%	39.1%	8.1%
Risk-free valuation	63.2	57.9	-5.4	28.2	26.9	-1.3
Risk-adjusted valuation	64.1	61.7	-2.4	28.6	28.9	0.2
Panel B: Funding gap						
Benefit cut (Intermediate Cost)	64.1	64.0	-0.1	28.7	30.2	1.6
Benefit cut (High Cost)	64.8	67.0	2.3	29.0	32.0	3.0
Tax hike (Intermediate Cost)	64.1	62.1	-2.0	28.6	29.1	0.4
Tax hike (High Cost)	64.3	63.7	-0.6	28.8	29.9	1.1
Panel C: Robustness						
Declining wage growth	64.3	63.6	-0.7	28.8	29.9	1.1
Liquidity Premium (1%)	64.8	64.1	-0.6	29.0	30.3	1.3
Liquidity Premium (2%)	65.6	66.9	1.3	29.5	31.9	2.4
Liquidity Premium (3%)	66.2	69.0	2.7	29.9	33.3	3.3

their full obligations to beneficiaries. To ascertain the impact of policy risk on our results, we modify our estimates of Social Security wealth by directly adjusting the cashflows that beneficiaries will receive or the taxes they will pay. Even under the most conservative assumptions—that beneficiaries will receive only benefits that are payable at current tax rates (eventually cutting benefits by up to 40%), or that taxes will rise for all but the top of the wealth distribution— our conclusion regarding the substantial impact Social Security has on estimates of wealth inequality is unchanged.

Balancing the budget by cutting benefits The SSA provides benchmark estimates of the extent to which the trust fund's bankruptcy will impair its obligations under three scenarios: low cost, intermediate, and high cost. Appendix Figure C.5 reports the proportion of payable benefits under each of the SSA's 1989 and 2019 cost scenarios. We assume that benefits will decrease across

Figure 3.9: Top 10% and Top 1% wealth shares — Funding gap adjustment

This figure presents top 10% and 1% wealth shares under four, risk-adjusted specifications. The "Low cost" specification refers to the SSA's high economic growth scenario in which benefits are fully paid. In the "Intermediate cost" and "High cost" specifications, benefits are cut to match expected tax revenues under the baseline and worst-case economic growth scenarios. All specifications is the risk-adjusted valuation method.



the board to the payable amounts reported by the SSA in each scenario, despite potential political pressure for more progressive entitlement reform.

To understand the impact of insolvency risk on our estimates, we collect annual data from the SSA on the year that the trust fund is projected to run out, the total revenue generated from Social Security payroll taxes, and the total obligations to beneficiaries. Once the Social Security fund is extinguished (estimated to be between 2030-2035), benefits paid in a year must be less than or equal to total tax revenue going forward.

Assuming maximal cuts to expected Social Security benefits decreases the bottom 99% wealth share by 3.2 pp, wiping out a quarter of Social Security's impact. But as Figure 3.9 shows, top wealth shares are still significantly attenuated. This is for two reasons. First, for people close to retirement, the impact of the fund's depletion is small, since benefits will pay out as normal for the first 10-15 years. Second, even for cohorts impacted, 60% of expected Social Security benefits represents a sizable sum relative to their marketable wealth.

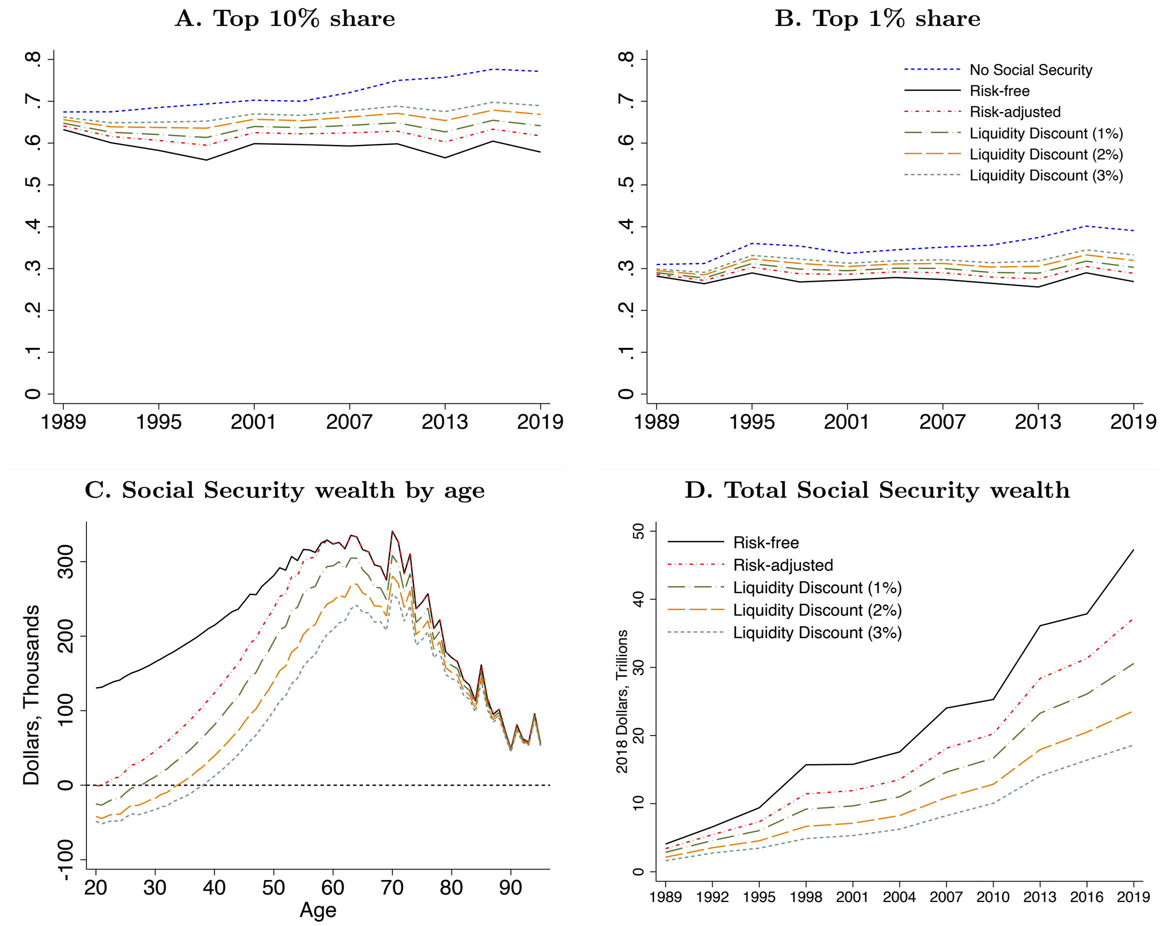
Balancing the budget by raising taxes Alternatively, taxes could be raised to avoid cutting benefits. To assess this possibility, we adopt the most conservative assumption from the perspective of our baseline results: that the additional tax burden will be borne entirely by the bottom 90%, or bottom 99%. Nonetheless, the top 10% share still declines by 0.6 pp; the top 1% share rises slightly, by 1.1 pp. Interestingly, raising taxes has less of an impact on aggregate Social Security wealth than cutting benefits. This is because raising taxes pushes a greater portion of the funding gap to future generations.

3.8.2. Adjusting for the illiquidity of Social Security

While excluding Social Security wealth based on its illiquidity is misguided for the reasons described in Section 3.7.3, one may be interested in how the results would change if discount rates account for an illiquidity premium. Who should be covered by this illiquidity premium is not clear since a significant share of Social Security wealth goes to households with ample liquid wealth. Nevertheless, we apply liquidity discounts of a 1%, 2%, and 3% to Social Security wealth for all households and report the effect on our main results in Figure 3.10.

Figure 3.10: Liquidity premium adjustment

This figure reports the evolution of the top 10% and 1% wealth shares, average Social Security wealth by age in 2019, and the aggregate value of Social Security wealth when we add 1, 2 or 3 percentage points to our discount rates to reflect a hypothetical liquidity premium. All specifications is the risk-adjusted valuation method.



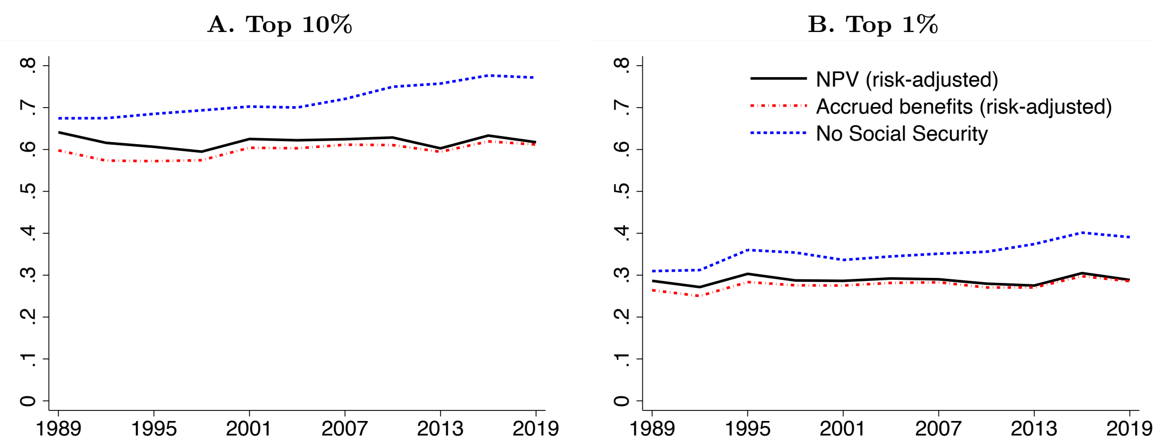
Panels C and D show that applying a 3% illiquidity premium reduces aggregate Social Security wealth by half, and Social Security wealth even becomes negative for young households. Yet, even under such a drastic adjustment, the rise in top wealth shares remains substantially attenuated when we include Social Security, as shown in Panels A and B. The top 10% and top 1% wealth shares only rise by 2.7 and 3.3 pp respectively, instead of 9.7 and 8.1 pp when Social Security is not included. These computations do not take into account that, for such an exercise to make sense, some important components of market wealth, such as the private business wealth of wealthy entrepreneurs, would also need to be adjusted for their illiquidity.

3.8.3. Decline in productivity growth

The decline in interest rates could be symptomatic of lower future long-run economic growth, which reduces the value of wage-indexed Social Security benefits. Our baseline estimates already assume a decline in the growth rate of wages: we rely on assumptions from SSA reports, which, as of 2019, assumed a 1.2% long-term annual wage growth rate, down from 1.7% in 1989. When we consider a more pessimistic scenario in which the real growth rate of wages declines linearly from 1% to 0% between 1989 and 2019, our main result is qualitatively unchanged: the top 10% decreased by 0.7 pp whereas the top 1% share increased by 1.1 pp.

Figure 3.11: Top 10% and Top 1% wealth shares — Accrued benefits

This figure shows the top 10% and top 1% wealth shares with and without the risk-adjusted value of accrued Social Security benefits.



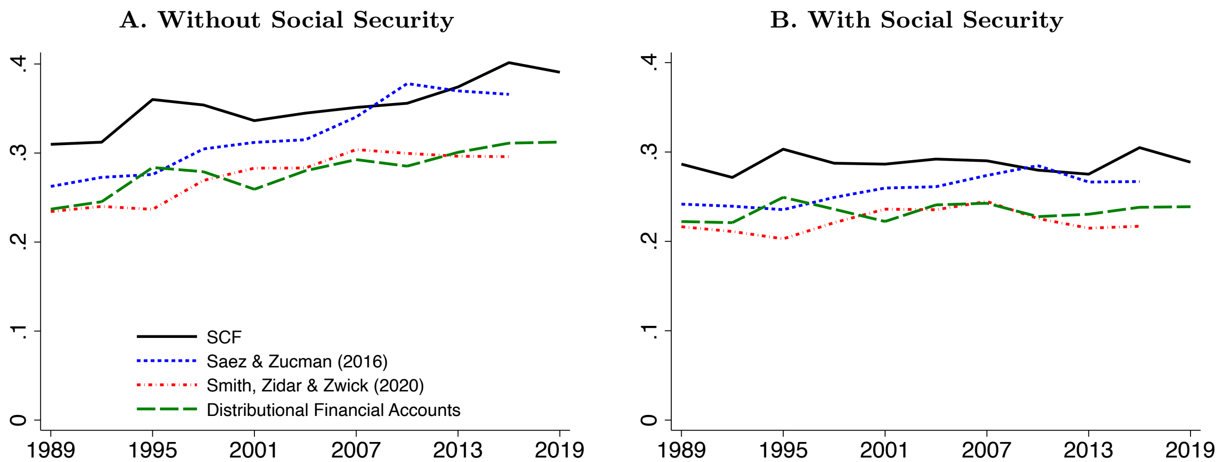
3.8.4. Restricting Social Security wealth to accrued benefits

An alternative definition of Social Security wealth is the value of expected benefits based on past contributions only. As shown in Figure 3.11, our findings are similar when we use this definition. This should be expected if the present value of future taxes are close to the corresponding increase in benefits.

3.8.5. Adjusting previous studies on wealth inequality

Previous studies compute top wealth shares using other datasets than the SCF . In Figure 3.12, we adjust these studies to include our estimates of the Social Security wealth of the top 1% and bottom 99%. Our main results remain unchanged.

Figure 3.12: Top 1% from previous studies



3.9. Conclusion

Prior studies find large increases in U.S. wealth inequality over the last three decades based on measures of wealth concentration that exclude Social Security. We find that, when Social Security is incorporated into inequality estimates, top wealth shares have not increased since 1989. Our top wealth estimates may still be overstated because we exclude programs like disability insurance and Medicare, which accrue disproportionately to the bottom of the wealth distribution. Overall, our paper shows that public transfer programs like Social Security make the U.S. economy more progressive, and it is important for inequality estimates to reflect this. Much more work is needed to arrive at a fuller understanding of wealth concentration in America.

APPENDIX A

Supplementary Material for Chapter 1

A.1. Data appendix

This section provides information on how each data series used in the paper is constructed. Additional information on the specific data used for each country can be found in the Supplemental Online Appendix.

Equity returns and dividend yields I construct the longest possible equity return series by combining data from five main sources: the Global Financial Data (GFD) Main Dataset, the Jorda-Schularik-Taylor Macrohistory Database (JST), the GFD London Stock Exchange (GFD-LSE) Dataset, IBES Global, and Factset International Annual Fiscal data. From both the main GFD dataset and the GFD-LSE dataset, I obtain the dividend yield, total return index, and price index for each country. Cum-dividend returns are therefore given by

$$R_{c,t}^{tot} = \frac{\text{Total Return Index}_{c,t}}{\text{Total Return Index}_{c,t-1}}$$

and ex-dividend returns (capital gains) by

$$R_{c,t}^{cap.\ gains} = \frac{\text{Price Index}_{c,t}}{\text{Price Index}_{c,t-1}}.$$

The GFD return series are in downloaded in U.S. dollars and then adjusted for expected U.S. inflation, which is calculated by fitting an AR(1) process to realized inflation, to put them in real terms. The JST returns data must be converted to U.S. dollars, which is done using the `xusd` variable they provide. Using the dividend yield and capital gains, I also construct a dividend growth series, which is given by

$$\frac{D_{c,t}}{D_{c,t-1}} = \frac{\text{Dividend Yield}_{c,t}}{\text{Dividend Yield}_{c,t-1}}(R_{c,t}^{cap.\ gains})^{-1}.$$

To obtain the series from IBES Global, I use the Actuals file which contains price and dividend yield information for several country-specific stock indices from 1985 to the present. For these data, the index with the longest possible time series in each country is used. All series with a dividend yield equal to 0 or above 0.50 are dropped. For data from Factset Annual Fiscal file, I obtain a market capitalization weighted average of dividends per share and the price per share by country-year and divide them to obtain the dividend yield.

Using these data, I construct the longest possible cum-dividend returns series possible. To do this, I create an all cum-dividend returns variable and populate it with returns from GFD's Main Dataset and then fill in missing observations with the JST data. The JST data only covers 17 countries compared to the 64 countries the main GFD dataset covers. This adds 438 additional observations. I then fill in the remaining missing observations with capital gains data from the main GFD data added to the dividend yield from either the main GFD data or the GFD-LSE data, which adds 1,093 observations. Next, the remaining missing data are filled in using returns from IBES and Factset which are obtained by adding the capital gains to the dividend yield. This yields an additional 430 observations. Finally, I fill in any remaining missing observations with cum-dividend returns from the GFD-LSE data, which adds 2,880 observations. The total cum-dividend returns data have 8,845 observations. The GFD-LSE data covers a different universe of companies than the other two datasets, usually large, global firms. One may be concerned with using this series, as it is not explicitly traded by home market participants. However, it has a fairly strong correlation (0.44) with the main GFD data.

I use the same procedure to combine the ex-dividend returns data and dividend growth data and to combine the *changes* in the dividend yield, rather than the levels. Indeed, the levels vary somewhat across data sources, so combining them would lead to arbitrary jumps in the series. Using the changes, on the other hand, does not have this issue. The change in the log dividend yield series covers 90 countries over 201 years.

Fixed income and inflation Results for corporate bond yields are reported in Appendix A.2. Corporate bond yields data only come from one data source, the GFD main dataset. This series covers 21 countries over 164 years. U.S. government bond and bill yields come from the GFD main dataset.

Inflation data come from the GFD main dataset, the JST data, and the Varieties of Democracy (V-Dem) database. The aggregate series is created by taking an equal weighted average over all these series.

Episodes of Regime Transformation data The main source used to locate democratization episodes of the Episodes of Regime Transformation (ERT) data. These data use changes in electoral democracy index (EDI) from the Varieties of Democracy (V-Dem) project to determine the start and end years of democratizations. V-Dem creates the EDI by surveying over 3,500 country-level experts and asking "to what extent is the ideal of electoral democracy in its fullest sense achieved." This is done in practice by combining information on the level of freedom of association, to what extend elections are free and fair, the level of freedom of expression, to what extent government officials are elected, and by examining the proportion of individuals in the country with voting rights. V-Dem then combines these 5 index categories both additively and using a five-way multiplicative interaction to produce a continuous index from 0 to 1.

The ERT data locate democratization episodes using the EDI according to two main criteria. First, a democratization episode must begin with at least a 0.01 increase in the EDI. Second, the episode must have at least a 0.10 increase in the EDI before experiencing (1) an annual drop in the EDI of 0.03, (2) a cumulative drop in the EDI of 0.10, or a stasis period of 5-years or longer. A stasis period is defined as a period where no years see at least a 0.01 increase in the EDI. The end year of a democratization is determined as the final year prior to when the annual or cumulative decline threshold or the stasis period condition is met. V-Dem produces these data from 1900– 2018. To extend the data to cover my full sample, I use an identical procedure on the subset of countries V-Dem provides the EDI prior to 1900. This yields 10 additional democratization episodes. In addition to providing democratization dates, the ERT data also provide information on autocratization episodes too. This is done by using an identical procedure to create the democratization indicators, but using 1 minus the EDI.

Successful and failed democratizations are determined using the aggregate democratization outcome (dem_ep_outcome_agg) variable. This measure yields four potential outcomes: (1) democratic transition, (2) no democratic transition, (3) deepened democracy, or (4) outcome censored. A democratization is coded as a democratic transition if "the episode resulted in a change from autocracy to democracy on the [regimes of the world] measure followed by a democratic founding election." A democratization is coded with no democratic transition if "the episode did not result in a change from autocracy to democracy on the [regimes of the world] measure; or it did result in a change between democracy and autocracy on the [regimes of the world] measure, but the political unit did not hold a democratic founding election before reverting to autocracy." A democratization is coded as a democratic deepening if "the episode resulted in further liberalization or democratization of a political unit that was already classified as democracy in the pre-episode year." A democratization is coded as censored if the episode is ongoing in the final year of the data. Both democratic transition and democratic deepening episodes are coded as successful democratizations whereas no democratic transition episodes are coded as failed.

A list of the democratization episodes used for the asset pricing results is presented in Table A.22. Alongside this table is a discussion of 2 case studies of the democratization process, subsequent redistribution, and stock market impact of the democratization events. These case studies focus on the democratic transition in Sweden from 1917–1924 (Appendix A.6.1) and the failed democratization in France from 1847–1852 (Appendix A.6.2).

A.2. Stylized facts appendix

This section presents additional evidence that risk premia are elevated during periods of democratization and robustness results on the stylized facts included in the paper.

A.2.1. Democratizations during defaults and wars

As discussed in Section 1.3 the change in the dividend yield will be a strongly downward biased proxy for the change in the risk premium if there are transitory shocks to the level of dividends that quickly rebound. Table A.1 shows that this condition holds for the subset of democratizations that occur when a war is happening within that country or during a sovereign default episode. The three-year change in log dividends imply a nearly 50% drop in dividends at the start of these "adverse" democratizations. However, this drop is reversed over the remainder of the democratization, which sees 5.3% higher average dividend growth annually during the remainder of the democratization.[62] As such, the change in dividend yields during these episodes cannot solely be interpreted as the change in the risk premium, but is a stronger statement of the speed with which investors expect cashflows to rebound.

However, we can do some back-of-the-envelope math to better understand how discount rates change during these episodes. Recall that the price-dividend ratio can be expressed as a weighted-sum of dividend strips (Gonçalves, 2022). According to Gonçalves (2022), approximately 50% of the price-dividend ratio comes from the first 20 dividend strips in the United States from 1953–2019. Over that time, the United States had a price dividend ratio of 38.5, considerably larger than the PD ratio of 30 of the average country three years prior to a democratization. Adjusting his numbers would imply that approximately 40% of the price-dividend ratio comes from the first 11 years of dividend strips in countries undergoing an adverse democratization. This implies that the expected growth rate at the start of an adverse democratization is approximately $0.40 \times 5.3\% = 2.1\%$ higher than it is in normal times. Given an average dividend yield of 5% prior to a democratization, this implies that, in the absence of a change in discount rates, that the log dividend yield should have fallen by 0.55, much larger than the 0.25 decline observed in the data over 5 years. This difference leaves room for an increase in discount rates of 1-1.5 percentage points, similar in magnitude to the other democratizations reported in the main text.

A.2.2. Other indicators for democratizations

This section presents results for regressions of the 5-year change in the log dividend yield on various indicators for democratization episodes. Table A.2 presents the results without fixed effects, Table A.3 with country and year fixed effects, and Table A.4 with country and region-by-year fixed effects. Column (1) presents the results for the ERT data without the extension to the 19th century

[62] It is also worth noting that wars inside of a country and sovereign default episodes, both inside and outside democratizations, display a similar pattern, albeit with a lower magnitude on decline and subsequent rebound than those seen in democratizations that co-occur with these events.

Table A.1: Dividend growth in adverse democratizations

This table presents regressions for the cumulative 3 year change in log dividends and log prices around adverse democratizations, defined as democratizations that begin in a country fighting in a war on their own continent or are engaged in a sovereign default. Results are shown in a three-year window around the adverse democratization start and then reported for the remainder of the democratization after the start in the final column. Standard errors are clustered by country and year and are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Three-year change in log dividends		Three-year change in log prices	
	(1)	(2)	(3)	(4)
F.Adverse Democratization Start	-50.59**	-42.84**	-28.60**	-29.02***
	(22.66)	(19.51)	(14.33)	(10.67)
Adverse Democratization Start	-52.53***	-38.71**	-14.00	-7.75
	(18.08)	(16.01)	(17.05)	(12.25)
L.Adverse Democratization Start	-33.31*	-22.95	-5.10	5.80
	(18.70)	(19.79)	(19.35)	(15.49)
Adverse Democratization After Start	15.90**	16.52**	9.43	5.64
	(6.25)	(7.51)	(7.65)	(6.84)
Country FE	No	Yes	No	Yes
Year FE	No	Yes	No	Yes
R^2	0.01	0.21	0.00	0.40
Observations	5,668	5,666	5,668	5,666

that I reconstruct. Without the 19th century data, the results are even stronger, statistically significant at the 1% level across all specifications, and point to a 20.5–27.2% rise in the risk premium. Columns (2) presents the results for democratizations constructed using episodes where the growth rate of the electoral institutions index is in excess of the 95th percentile. Using the growth, as opposed to the raw difference overweights democratizations occurring in less democratic countries. This measure permits an additional 100 event years, and the results are quantitatively similar, significant at either the 1% or 5% level, and point to a 16.7–20.9% increase in the risk premium. Column (3) presents democratic jumps, determined as periods where the electoral democracy index rises in excess of 0.01, the threshold value for the beginning of an ERT democratization. In total, there are 845 democratic jumps in the sample for which there are dividend yield data. The results point to a 5.6-6.5% change in the risk premium over 5-years per democratic jump and are statistically significant at either the 1% or 5% level. The smaller magnitudes here make sense, as these are generally episodes without a large increase in possibility of a full democratic transition. Column (4) presents the results for the democratization start year in the Lindberg et al. (2018) data, which

Table A.2: Other measures of democratization, no fixed effects

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of a democratization for 5 different potential measures of democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents the coefficient on a vector of ones. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The Lindberg et al. data have fewer observations because they begin in 1900. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields				
	(1)	(2)	(3)	(4)	(5)
F.Democratization start	7.29	3.53	2.80	11.91	-2.72
	(9.45)	(7.03)	(2.42)	(7.82)	(12.64)
Democratization start	20.52***	10.68*	6.12***	28.59**	19.86**
	(6.07)	(5.97)	(2.14)	(10.97)	(9.66)
L.Democratization start	17.41**	16.76**	2.06	-10.86	20.20
	(8.04)	(6.40)	(2.49)	(14.92)	(12.68)
Democratization type	ERT Only	Growth Rate	Dem. Jump	Lindberg	Acemoglu et al
Country FE	No	No	No	No	No
Year FE	No	No	No	No	No
Region × Year FE	No	No	No	No	No
Continent × Regime × Year FE	No	No	No	No	No
Event Controls	Yes	Yes	Yes	Yes	Yes
Episode obs.	74	162	917	43	32
R^2	0.02	0.02	0.02	0.02	0.02
Observations	5,972	5,972	5,972	4,509	5,972

display the largest point estimate of any of the measures shown, are significant at either the 1% or 5% level, and indicate a 27.7–38.9% rise in the risk premium.

Finally, Column (5) presents the results for democratizations from Acemoglu et al. (2019) from 1960–2010. For episodes prior to 1960, I fill in these data using a similar methodology. Most of the variation in the Acemoglu et al. measure comes from transition years as given by the Polity and Freedom House regime type datasets.[63] Since both Polity and V-Dem provide data back to the 1800's, I extend the Acemoglu dataset using consensus transition years in both dataset. This procedure provides 32 total transition years for which asset pricing data are available. As Column (5) of Table A.2 shows, the change in dividend yields around these transition years is large and statistically significant at the 1% or 5% levels, pointing to a 19.9–24.6% rise in the risk premium.

[63] Prior to 1972, when the Freedom House data end, these authors rely on other regime type measures and independent historical research.

Table A.3: Other measures of democratization, country and year fixed effects

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of a democratization for 5 different potential measures of democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents country and year fixed effects. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The Lindberg et al. data have fewer observations because they begin in 1900. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields				
	(1)	(2)	(3)	(4)	(5)
F.Democratization start	12.83	7.86	3.79	14.89**	3.48
	(9.69)	(7.13)	(2.42)	(7.13)	(11.69)
Democratization start	22.29***	8.75	5.60**	27.73***	22.54**
	(5.98)	(7.10)	(2.26)	(9.76)	(9.19)
L.Democratization start	14.07*	16.66***	0.84	-5.81	21.92*
	(7.94)	(6.22)	(2.68)	(13.72)	(12.56)
Democratization type	ERT Only	Growth Rate	Dem. Jump	Lindberg	Acemoglu et al
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Region × Year FE	No	No	No	No	No
Continent × Regime × Year FE	No	No	No	No	No
Event Controls	Yes	Yes	Yes	Yes	Yes
Episode obs.	74	162	917	43	32
R^2	0.15	0.15	0.15	0.16	0.15
Observations	5,968	5,968	5,968	4,508	5,968

A.2.3. Alternative measures for change in risk premia

This section presents other measures for changes in risk premia as proxied by changes in dividend yields. One potential concern comes from using the 5 year difference in log dividend yields as the main measure. While this methodological choice is mainly made to stay in line with the prior literature, Table A.5 shows that all differences in dividend yields from 1 to 4 years provide very similar results. In particular, the 3 and 4 year change in log dividend yields provides nearly identical quantitative results to the 5 year change, while the 1 and 2 year change provide results that are smaller in magnitude. This potentially indicates, however, that financial markets begin to react to democratization risk earlier than the political scientists labeling these episodes.

The level of dividend yields are also elevated at the start of democratizations even relative to their country-specific long-run mean and the average dividend yield in a given year, region-year, or

Table A.4: Other measures of democratization, country and region-year fixed effects

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of a democratization for 5 different potential measures of democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents country and region-year fixed effects. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The Lindberg et al. data have fewer observations because they begin in 1900. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields				
	(1)	(2)	(3)	(4)	(5)
F.Democratization start	10.15	12.99	6.16**	36.57***	5.52
	(10.06)	(8.14)	(2.73)	(11.29)	(11.23)
Democratization start	27.24***	11.33	6.50***	38.91***	24.62***
	(6.66)	(8.39)	(2.47)	(14.65)	(8.22)
L.Democratization start	25.09***	20.93***	0.02	3.32	18.54
	(9.42)	(7.07)	(2.64)	(12.25)	(11.65)
Democratization type	ERT Only	Growth Rate	Dem. Jump	Lindberg	Acemoglu et al
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No
Region × Year FE	Yes	Yes	Yes	Yes	Yes
Continent × Regime × Year FE	No	No	No	No	No
Event Controls	Yes	Yes	Yes	Yes	Yes
Episode obs.	73	149	895	40	32
R^2	0.36	0.36	0.36	0.37	0.36
Observations	5,591	5,591	5,591	4,306	5,591

continent-regime-year as shown in Table A.6.

Additionally, as shown in some of the case studies below in Appendices A.6.1 and A.6.2, the dividend yield in democratizations is not always highest at the start of the episode. To account for this, Table A.7 provides two additional measures for the change in log dividend yields. The first takes, for any given t, the maximum dividend yield from $t-3$ to $t+5$ and subtracts it from the minimum dividend yield from $t-8$ to $t-4$. In this way, this change in the dividend yield is a peak-to-trough measure that assures the higher dividend yield occurs around the event of interest. The reason $t-3$ is chosen is because this is when dividend yields begin to rise in the event study plot shown in Figure 1.1, but results are similar using other windows. Also reported are the same regressions on the maximum 5-year change in log dividend yields observed in from $t-2$ to $t+2$ for any given t. Each of these measures point to a large and statistically significant rise in dividend yields around

Table A.5: Shorter changes in log dividend yields

This table presents regressions of the 1, 2,3, and 4 year change in log dividend yields on indicator variables representing the start of a democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-h} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield, $h \in \{1, 2, 3, 4\}$ represents the period over which the difference is taken, and α represents either the coefficient on a vector of ones or the fixed effects specification. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	One-year change				Two-year change			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratization Start	9.28**	6.86*	9.01**	11.02***	12.41**	11.64**	12.76***	15.59***
	(4.18)	(3.84)	(4.20)	(4.04)	(5.80)	(5.15)	(4.54)	(4.00)
Country FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	No	Yes	No	No
Region × Year FE	No	No	Yes	No	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	72	72	71	68	69	69	68	65
R^2	0.01	0.11	0.28	0.26	0.01	0.13	0.30	0.28
Observations	6,334	6,334	5,969	6,122	6,208	6,208	5,839	5,992

Dependent variable:	Three-year change				Four-year change			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratization Start	17.12***	14.98***	17.42***	22.12***	18.61**	15.98**	16.22**	18.40**
	(5.81)	(5.53)	(6.28)	(6.62)	(8.47)	(7.48)	(7.42)	(8.64)
Country FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	No	Yes	No	No
Region × Year FE	No	No	Yes	No	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	67	67	66	63	65	65	64	61
R^2	0.01	0.13	0.32	0.30	0.01	0.14	0.35	0.33
Observations	6,083	6,080	5,707	5,855	5,961	5,960	5,583	5,730

democratizations start years.

Finally, the increase in dividend yields in democratizations are almost entirely driven by price declines as shown in Figure A.1, which shows the combined log capital losses around democratizations and financial crises. Prices decline substantially in both events, corresponding to a 23.0% decline over 5 years around democratizations and a 32.4% decline around financial crises at the trough of each episode.

Table A.6: Periods of democratization and log dividend yields

This table presents regressions of the level of the log dividend yield on indicator variables representing the start of a democratization. The specification estimated is

$$dp_{c,t} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects specification. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The estimated coefficients on financial crises are also reported for purpose of comparison. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Level of log dividend yields			
	(1)	(2)	(3)	(4)
Democratization start	21.36**	13.42*	14.89**	12.72**
	(9.34)	(6.77)	(7.26)	(5.94)
Financial crisis start	14.23***	12.49**	7.07	6.29
	(4.50)	(4.88)	(5.23)	(5.20)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	57	57	57	52
R^2	0.02	0.40	0.57	0.57
Observations	5,184	5,183	4,817	4,884

A.2.4. Alternative standard errors

In addition to being robust to other democratization indicators, the results are also robust to alternative choices for standard errors. In particular, one may be concerned that the overlapping observations introduced by using 5-year change in the log dividend yield may yield standard errors that are too small. To account for this, Table A.8 reproduces the exercise from Table 1.1 with with Driscoll-Kraay standard errors with a lag of 5 and clustered at the year level. The results remain significant at the 1% level.

A.2.5. VAR decomposed shocks

This section presents a vector autoregression (VAR) based estimate of discount rate shocks via the Campbell (1991) methodology. Realized returns can be decomposed into expected returns and

Table A.7: Peak-to-trough and maximum 5 year change

This table presents regressions of two peak-to-trough measures of the change in the log dividend yield on indicator variables representing the start of a democratization. The specification estimated is either

$$\max\{dp_{c,s}\}_{s=t-3}^{t+5} - \min\{dp_{c,s}\}_{s=t-8}^{t-4} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

or

$$\max\{dp_{c,s} - dp_{c,s-5}\}_{s=t-2}^{t+2} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects specification. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The estimated coefficients on financial crises are also reported for purpose of comparison. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Peak-to-trough				Maximum 5-year change			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratization start	20.39*	22.53***	22.55***	15.78**	11.52*	13.84**	15.76**	16.52***
	(11.97)	(7.97)	(6.96)	(6.88)	(6.92)	(5.43)	(6.35)	(6.25)
Country FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	No	Yes	No	No
Region × Year FE	No	No	Yes	No	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	54	54	54	50	67	67	66	64
R^2	0.03	0.42	0.59	0.59	0.01	0.24	0.41	0.40
Observations	5,240	5,240	4,892	4,940	6,385	6,385	6,022	6,172

innovations to future expected cash flows and discount rates using the decomposition:

$$r_{t+1} = \mathbb{E}_t r_{t+1} + v_{t+1}^r \tag{A.2.1}$$

$$v_{t+1}^r = \eta_{t+1}^d - \eta_{t+1}^r \tag{A.2.2}$$

where

$$\eta_{t+1}^r \equiv (\mathbb{E}_{t+1} - \mathbb{E}_t) \sum_{j=1}^{\infty} \rho^j r_{t+1+j} \tag{A.2.3}$$

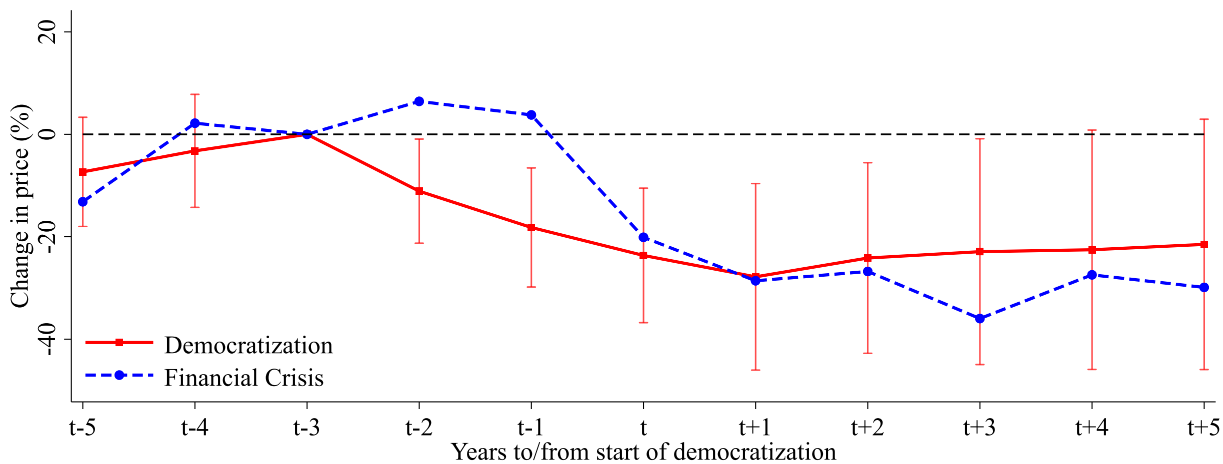
are discount rate shocks,

$$\eta_{t+1}^d \equiv (\mathbb{E}_{t+1} - \mathbb{E}_t) \sum_{j=1}^{\infty} \rho^{j-1} \Delta d_{t+j} \tag{A.2.4}$$

are cash flow shocks and $\rho \equiv \frac{\overline{pd}}{1+\exp\{\overline{pd}\}}$ as in Campbell and Shiller (1988) where $\overline{pd}$ is the average log price-dividend ratio. The discount rate and cash flow shocks given in Equations (A.2.3) and (A.2.4) can be estimated directly by assuming a process for discount rates and cashflows. To do this,

Figure A.1: Change in log prices in democratizations

This figure presents an event study of log prices around the start of a democratization and a financial crisis. Estimates are relative to the value three years prior to the event start to allow for the possibility that financial markets incorporate information about the events earlier than the start. Endpoints (not shown) are binned. To be sure the series is consistent across observations, only prices from GFD's main data series are plotted. The red bars on the democratization line represents a 90% confidence interval of the point estimates with standard errors clustered by country and year.



I assume a first-order VAR structure for log cum-dividend returns, dividend growth, consumption growth, government bond yields, and capital gains given by

$$\tilde{\mathbf{X}}_{t+1} = \mathbf{\Phi}\tilde{\mathbf{X}}_t + \mathbf{w}_{t+1} \tag{A.2.5}$$

where $\tilde{\mathbf{X}}_t = \mathbf{X}_t - \bar{\mathbf{X}}$ and $\mathbf{X}_t$ is the data vector with cum-dividend returns, r_t, in the first position.[64] Now, define $\mathbf{e}_1$ as an elementary column vector with a 1 in the first position and 0s elsewhere, meaning that Equation (A.2.2) can be written as $v^r_{t+1} = \mathbf{e}_1'\mathbf{w}_{t+1}$. Under the assumed VAR structure, Equation (A.2.3) becomes

$$\eta^r_{t+1} = \lambda'\mathbf{w}_{t+1}. \tag{A.2.6}$$

[64] To estimate the vector autoregression, I use the combination of control variables that give the largest sample. For example, if I have 100 cum-dividend returns observations, 100 dividend growth observations, and 80 riskfree rate observations, I will estimate the VAR using only cum-dividend returns and dividend growth.

Table A.8: Democratizations and the change in log dividend yields, Driscoll-Kraay Standard Errors

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of a democratization. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors correct for autocorrelation in the dependent variable using the method of Driscoll and Kraay (1998) and are clustered by year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The same results for financial crises are included for purpose of comparison. In Columns (3) and (4) some observations are lost due to there only being one observation in a region-year or in a continent-regime-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields			
	(1)	(2)	(3)	(4)
Democratization start	17.97***	19.79***	23.13***	26.34***
	(6.22)	(5.39)	(5.54)	(8.18)
Financial crisis	13.67***	16.07***	10.23*	10.96*
	(5.15)	(4.42)	(5.35)	(5.98)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	64	64	63	60
R^2	0.01	0.01	0.01	0.01
Observations	6,040	6,040	5,663	5,813

where $\lambda' \equiv \mathbf{e}_1' \rho \mathbf{\Phi}(\mathbf{I} - \rho\mathbf{\Phi})^{-1}$. Combining Equations (A.2.2) and (A.2.6) gives the cashflow shock as

$$\eta^d_{t+1} = (\mathbf{e}_1' + \lambda')\mathbf{w}_{t+1}. \tag{A.2.7}$$

The cashflow and discount rate shocks are, therefore, immediately given after estimating the VAR coefficients and residuals.

The first three columns of Table A.9 shows that large cumulative discount rate shocks are concentrated around democratization starts. Column (1) shows that the combined discount rate shock in the 3-years before, year of, and 3-year after a democratization start is 4.6 percentage points, in line with the findings from dividend yields. Adding year and country fixed effects reduces the cumulative rise in discount rates to 3.7 percentage points, but rises to 5.8 percentage points after region-year fixed effects are added.

Table A.9: Democratizations, discount rate shocks, and cashflow shocks

This table presents regressions of cumulative discount rate and cash flow shocks estimated from a vector autoregression on indicator variables for democratization starts. The regressions estimated take the form

$$\sum_{j=-3}^{3} \eta^{r}_{c,t+j} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start}\} + \epsilon_{c,t}.$$

The discount rate and cash flow shock variables are winsorized at the 1% and 99% levels. Standard errors are clustered by country and year are in parentheses. All coefficients have been multiplied by 100.

Dependent variable:	Discount rate shocks			Cashflow shocks		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization Start	4.64^{**}	3.73^{*}	5.84^{**}	-12.68	-4.51	-0.05
	(2.04)	(2.22)	(2.60)	(7.70)	(7.26)	(5.71)
Country FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No
Region × Year FE	No	No	Yes	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	68	68	66	68	68	66
R^2	0.01	0.20	0.33	0.01	0.34	0.54
Observations	6,273	6,273	6,011	6,273	6,273	6,011

Supporting the view that changes in discount rates drive the changes in prices that occur during democratizations, Table A.9 also shows the results for the VAR decomposed cash flow shocks. The cashflow shocks decomposed from the VAR are more volatile than the discount rate shocks, and therefore are accompanied by less precise estimates. None of the columns indicate a statistically significant change in expected cashflows. Column (4) comes along with large point estimates, but this is mostly driven by region-year variation, as the Column (6) estimate is very close to 0. These results highlight that discount rate shocks seem to be driving the results in Section 1.3.

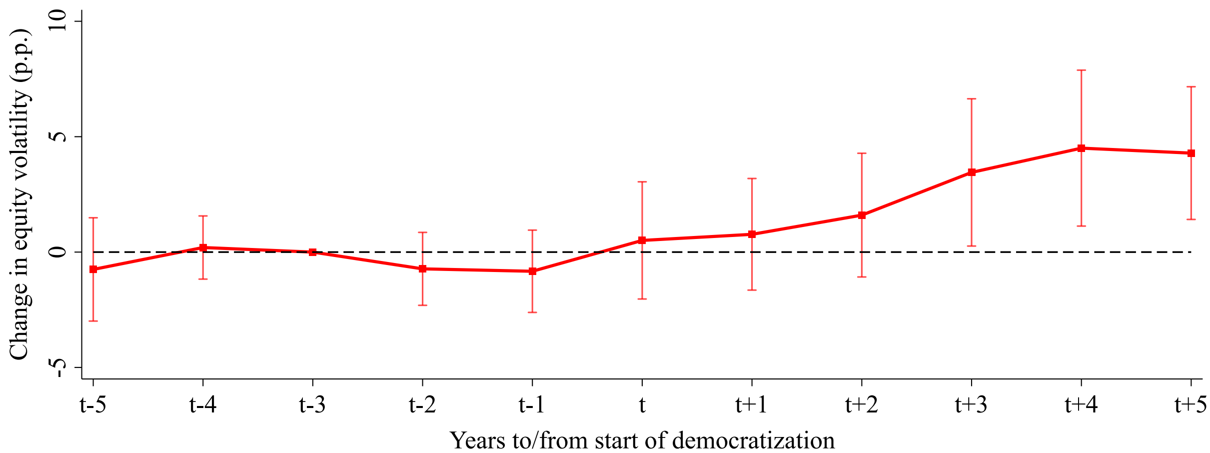
A.2.6. Equity volatility

Figure A.2 shows that equity volatility is elevated during democratizations, where equity volatility is the 10-year moving standard deviation of realized equity returns. While the model presented in Section 1.6 does not produce this result, this provides additional support that democratizations are risky periods for investors.[65]

[65] The model could produce increased equity volatility by adding time-variation in either the cost of revolution, μ, or the redistribution in democracy, Z, within the democratization state.

Figure A.2: Democratizations and equity volatility

This figure shows an event study plot of the 10-year moving standard deviation of realized returns around democratization starts. Country and year fixed effects are included and standard errors are clustered by country and year. A



A.2.7. Corporate bond yields

The final proxy for changes in discount rates I employ is corporate bond yields, which is also used by Muir (2017). When risk premia rise, so do corporate bond yields, making them a valid proxy for risk premia. These results should be interpreted carefully, however, as they come from only 11 democratization episodes. Table A.10 shows that the five-year change in log corporate bond yields is also large, statistically significant, and similar in magnitude to the estimates from Section 1.3.

A.2.8. Additional evidence on cashflow stability

Section 1.3.2 presented evidence on log GDP per capita growth for all countries whereas Section 1.3.1 presented evidence for only countries with data on dividend yields. To show the growth results are identical in both all countries and the subset of countries with data on dividend yields, Table A.11 presents the growth results either 5 or 10 years after a democratizations start. They are similar to those presented in the main text and are, if anything, tilted toward higher, not lower, growth.

Figure A.3 presents an event study plot of a three-year moving average of log dividend growth and log GDP per capita around democratization starts. Log dividend growth displays negative point estimates after the start of a democratization, but none are statistically different than 0 at

Table A.10: Democratizations and corporate bond yields

This table presents regressions of the 5-year change in log corporate bond yields on indicator variables representing the start of a democratization. The specification estimated is

$$y_{c,t} - y_{c,t-5} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where y is the log corporate bond yield and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year and are in parentheses. All coefficients have been multiplied by 100 for presentation. In Columns (3) and (4) some observations are lost due to there only being one observation in a region-year or in a continent-regime-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log corporate bond yields			
	(1)	(2)	(3)	(4)
Democratization start	12.99*	10.91***	14.08**	20.00*
	(7.05)	(3.33)	(6.52)	(11.16)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	11	11	10	8
R^2	0.02	0.54	0.62	0.61
Observations	1,422	1,407	1,311	1,211

Table A.11: Democratizations and growth, Dividend yield sample

This table presents regressions of log GDP per capita growth on indicator variables denoting if the year is in the first 5 or 10 years of a democratization. The regressions estimated take the form

$$\text{log GDP per capita growth}_{c,t} = \alpha + \beta \mathbb{1}_{c,t}\{\text{First 5 or 10 years of Democratization}\} + \epsilon_{c,t}$$

where α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. In Columns (3) and (6) some observations are lost due to there only being one observation in a region-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Democratization window:	5 years			10 years		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization, 5 or 10 years	0.82*	0.54	0.49	0.86***	0.39	0.24
	(0.45)	(0.41)	(0.41)	(0.33)	(0.31)	(0.33)
Country FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No
Region × Year FE	No	No	Yes	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	363	363	358	732	732	721
R^2	0.04	0.24	0.40	0.04	0.23	0.40
Observations	6,082	6,082	5,693	6,082	6,082	5,693

conventional levels of significance. This stands in contrast to large declines in dividend growth during financial crises with point estimates indicating average dividend growth of -12% in the three

Table A.12: Democratizations, growth, and cash flows

This table presents regressions of log GDP per capita and dividend growth on indicator variables denoting if the year is in the first 10 years of a democratization. The regressions estimated take the form

$$\text{Cash Flow Growth}_{c,t} = \alpha + \beta \mathbb{1}_{c,t}\{\text{First 10 years of Democratization}\} + \epsilon_{c,t}$$

where α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. In Columns (3) and (6) some observations are lost due to there only being one observation in a region-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Log GDP per capita growth			Log dividend growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization, 10-years	0.49**	0.20	0.11	-0.79	-2.43	-1.57
	(0.21)	(0.22)	(0.21)	(1.60)	(1.70)	(1.58)
Financial Crisis, 10-years	-0.11	-0.25	0.03	-2.32*	-2.52**	-1.38
	(0.19)	(0.19)	(0.18)	(1.31)	(1.15)	(1.11)
Country FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No
Region × Year FE	No	No	Yes	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes	Yes	Yes
Episode obs.	2313	2313	2303	742	742	726
R^2	0.02	0.11	0.23	0.01	0.12	0.29
Observations	18,222	18,222	18,017	6,221	6,221	5,733

years after the financial crisis start.[66]

GDP per capita also declines slightly prior to the democratization start, but all of the effects are offset 5-years into the episode and the decline is not statistically different than 0. This stands in contrast to financial crises, which see lower GDP per capita for at least 5-years after the episode start.

We can also see similar results to those reported in Section 1.3.2 when examining realized GDP growth and cashflow growth in the 10 years after a democratization start, as shown in Table A.12. GDP growth is significantly positive now in the specification without fixed effect, but not statistically different than 0 once fixed effects are added. Dividend growth shows negative point estimates across specifications, but are also not statistically different than 0.

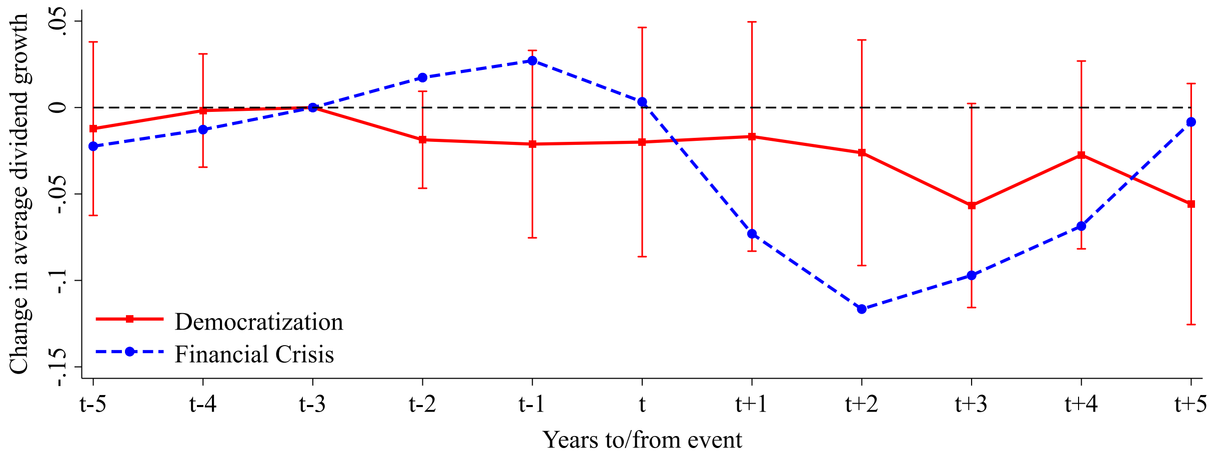
For comparison to the results in the main text, Figure A.4 presents the distribution of GDP growth

[66]Note, these values are benchmarked to the average growth 3 years prior to the episode start, which is 2.8% for democratizations and 3.2% for financial crises. Both these numbers are slightly above average log dividend growth in the data.

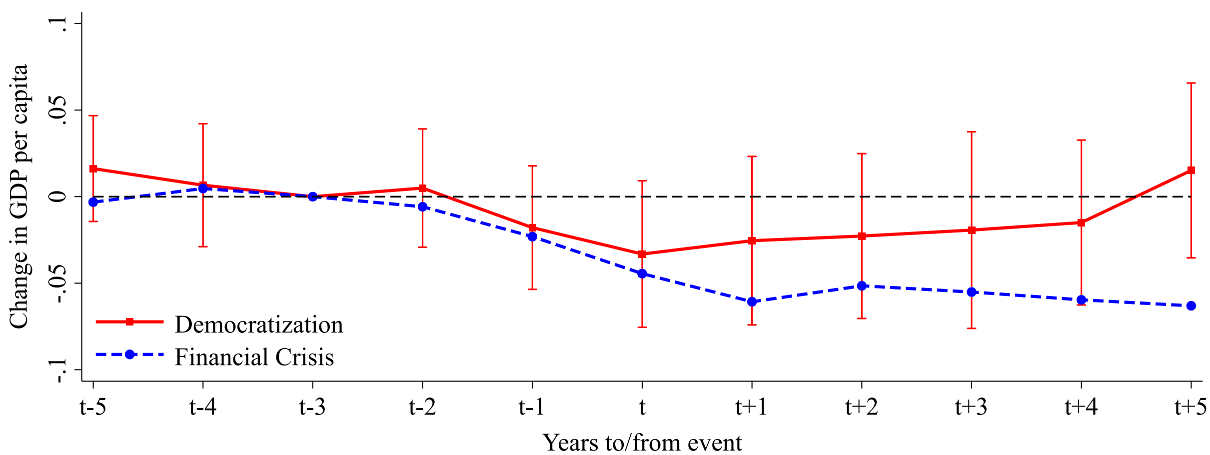
Figure A.3: Log dividend growth and GDP growth in democratizations

This figure presents an event study of a three-year moving average of log dividend growth and log GDP per capita around the start of a democratization and a financial crisis. Estimates are relative to the value three years prior to the event start. Endpoints (not shown) are binned. The red bars on the democratization line represents a 90% confidence interval of the point estimates with standard errors clustered by country and year.

A. Log dividend growth



B. Log GDP per capita



and consumption growth inside and outside of financial crises. Both series are substantially negative and left-skewed. This stands in contrast to the results with respect to democratization.

Finally, Figure A.5 shows net foreign direct investment (FDI) divided by GDP in an event study around the start of a democratization. Net FDI is not substantially changed around democratizations. This provides evidence that the equity market results are not driven by extreme outflows of

Figure A.4: Distribution of GDP and consumption in financial crises

Log GDP and consumption growth are winsorized at the 0.25% and 99.75% level. GDP data come from the Maddison Historical Statistics database. Consumption data come from the Penn World Tables and represent the period from 1945 to 2018.

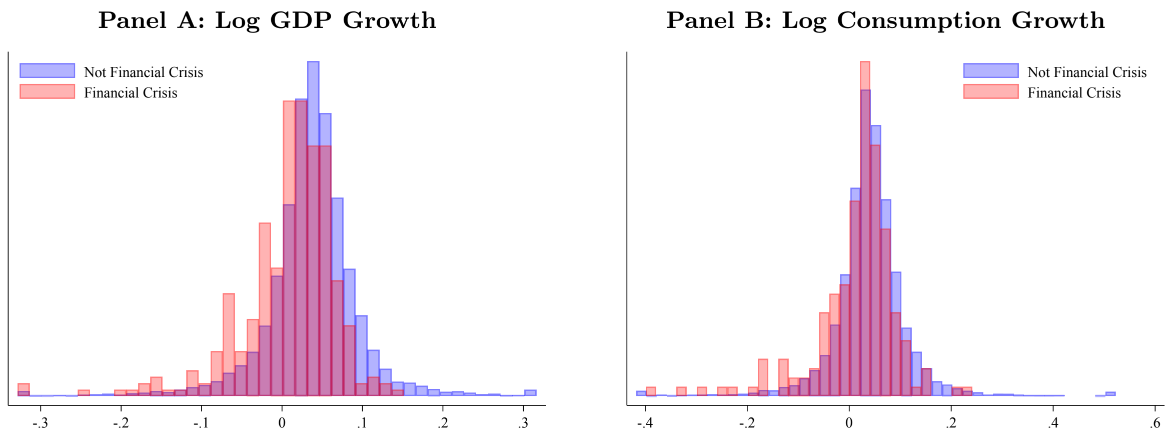
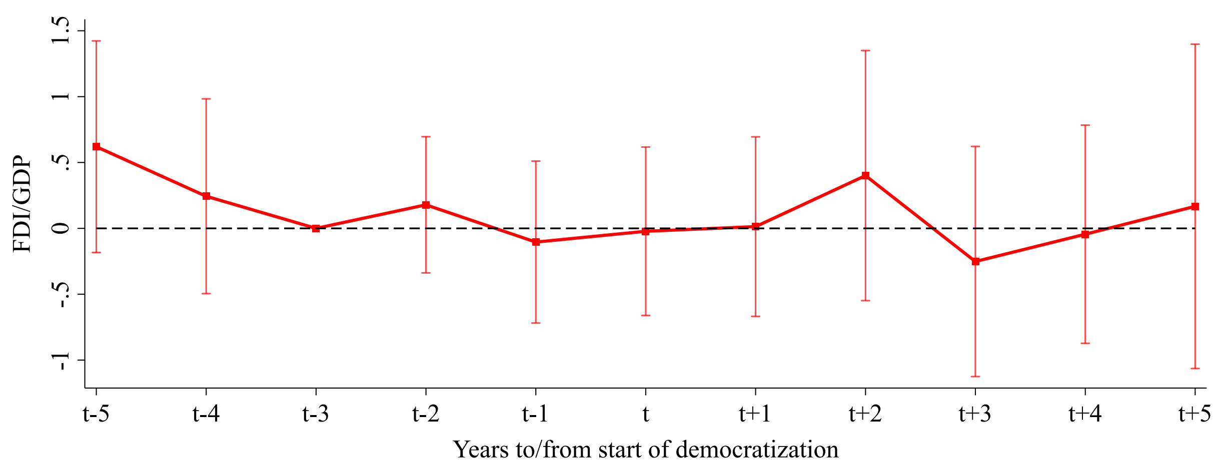


Figure A.5: Net foreign direct investment in democratizations

This figure presents an event study of net foreign direct investment scaled by GDP around the start of a democratization. Estimates are relative to the value three years prior to the event start. Endpoints (not shown) are binned. The red bars on the democratization line represents a 90% confidence interval of the point estimates with standard errors clustered by country.



foreign capital in democratizations.

Table A.13: Democratizations in ICB Crises

This table presents the 5-year change in log dividend yields in the sample of ICB crises. Fixed effects are applied first to the 5-year change in log dividend yields, and then the following regression is run: Standard errors are heteroskedasticity robust.***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	5-year change in log dividend yields			
	(1)	(2)	(3)	(4)
Democratization	25.74**	23.67***	24.16***	22.66**
	(10.32)	(8.46)	(8.73)	(9.34)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Episode obs.	20	20	18	20
R^2	0.02	0.02	0.04	0.03
Observations	150	150	129	147

A.2.9. Additional evidence on political risk

This subsection presents additional evidence that an increase in generic political risk is not driving the increase in risk premia observed in democracy. In particular, Table A.13 examines the increase in log dividend yields in democratization years that occur in the subsample of ICB crisis years. Democratizations within ICB crises display approximately a 22.7–25.7% larger increase in dividend yields that other ICB crisis events depending on the fixed effects employed. This provides additional evidence that, indeed, there is something special about democratizations above and beyond a generic increase in political risk or uncertainty.

A.2.10. Additional evidence addressing uncertainty

Evidence that a generic increase in uncertainty cannot explain the results is supported by Table A.14 which shows the evolution of investment-capital ratios and the human capital index from the Penn World Tables around democratization starts. Consistent with an increase in discount rates for investors, investment-capital ratios decline at the start of democratizations and then slowly rebound. Conversely, human capital, which is the primary asset of the lower and middle classes, rises as the prospect of democracy becomes more likely. The potential for democracy represents a positive shock to the value of human capital for these groups, not a period of uncertainty, leading to an increase in their investment in skills.

Table A.14: Democratizations, physical capital, and human capital

This table presents regressions of the 3 year change in log investment-capital ratios and log human capital index from the Penn World Tables on indicator variables representing the start of a democratization. Both variables have been standardized by subtracting the mean 3 year change and dividing by the standard deviation of the 3 year change of each variable. The specification estimated is either

$$y_{c,t} - y_{c,t-3} = \alpha + \beta \mathbb{1}_{c,t}\{\text{Democratization Start Year}\} + \epsilon_{c,t}$$

where y is either the standardized log investment-capital or human capital index and α represents either the coefficient on a vector of ones or the fixed effects specification. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The estimated coefficients on financial crises are also reported for purpose of comparison. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

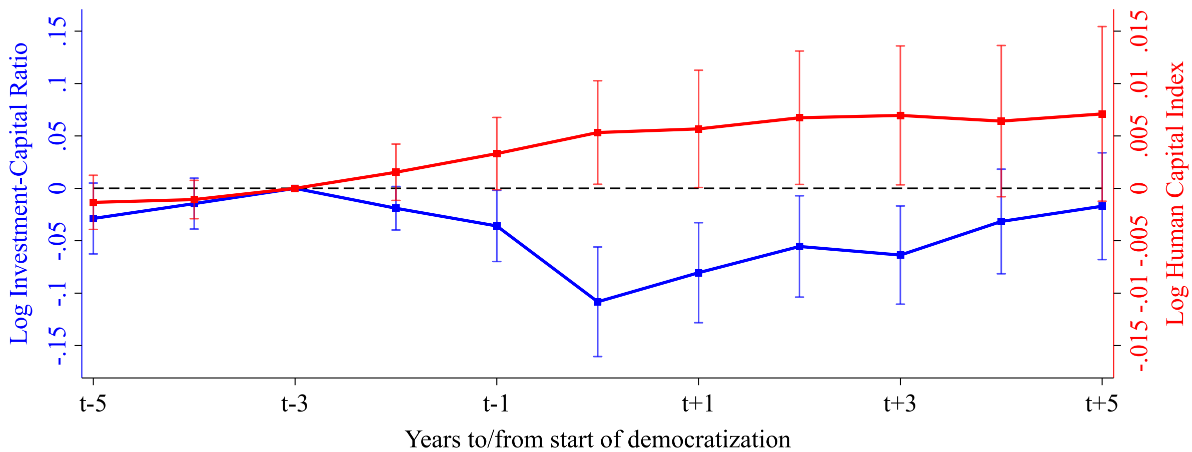
Dependent variable:	Three-year change in log investment-capital ratio			
	(1)	(2)	(3)	(4)
Democratization start	-0.16*	-0.18**	-0.15**	-0.23***
	(0.09)	(0.08)	(0.07)	(0.07)
Autocratization Start	0.00	-0.03	-0.03	-0.01
	(0.09)	(0.09)	(0.10)	(0.10)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	155	155	151	155
R^2	0.01	0.07	0.17	0.16
Observations	8,798	8,798	8,728	8,786

Dependent variable:	Three-year change in log human capital index			
	(1)	(2)	(3)	(4)
Democratization start	0.25***	0.27***	0.18**	0.20**
	(0.09)	(0.08)	(0.07)	(0.09)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Episode obs.	127	127	123	127
R^2	0.02	0.29	0.39	0.36
Observations	8,061	8,061	7,990	8,049

Moreover, Figure A.6 shows the separate evolution of these two series in an event study plot. In the years prior to the democratization, human capital and physical capital were trending closely and were relatively stable. After the democratization begins, however, the two series begin to diverge, with investment-capital ratios falling by 10%. This decline, is the primary cause of the small decline in GDP per capita observed at the start of the democratization. However, this is offset to some

Figure A.6: Physical and human capital in democratizations

This figure shows an event study plot of investment-capital ratios and the human capital index around democratization starts. Estimates are relative to the value three years prior to the democratization start. Endpoints (not shown) are binned. The red bars on the democratization line represents a 90% confidence interval of the point estimates with standard errors clustered by country and year.



degree by the rise in human capital.

A.2.11. Additional evidence on other risk

This subsection presents additional evidence that an increase in the likelihood of other adverse macroeconomic events does not seem to explain increased risk premia during democratizations. Table A.15 presents the coefficient estimates from a linear probability model assessing the likelihood that adverse events, defined as sovereign defaults, wars, financial crises, and recessions, showing that these are no more likely to start in democratizations than in normal times. This stands in contrast to autocratizations, which have a higher likelihood of experiencing a sovereign default or war. Additionally, in the panel dataset employed in this paper, data on equity prices is sometimes lost. It is possible that this missing data could bias the results if it represents a market shutdown and these shutdowns are more likely in democratizations and/or autocratizations. Column (6) shows that this also does not seem to be a concern, as missing data is no more likely in either episode.

Table A.15: Democratizations and probability of adverse events

This table presents regressions of the form

$$\mathbb{1}_{c,t}\{\text{Event Start}\} = \alpha + \beta_1 \mathbb{1}_{c,t}\{\text{Democratization}\} + \beta_2 \mathbb{1}_{c,t}\{\text{Autocratization}\} + \epsilon_{c,t}$$

. No fixed effects are added. Standard errors are clustered by country.***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Adverse Event	Default	War	Financial Crisis	Recession	Market Loss
	(1)	(2)	(3)	(4)	(5)	(6)
Democratization	0.60	0.37	-0.28	0.45	-0.04	0.25
	(0.51)	(0.30)	(0.49)	(0.31)	(0.33)	(0.22)
Autocratization	2.27^{**}	1.70^{***}	2.43^{**}	0.34	0.40	0.46
	(0.95)	(0.62)	(0.97)	(0.48)	(0.55)	(0.65)
Democratization obs.	2790	2790	2520	2790	2790	1607
R^2	0.02	0.00	0.00	0.00	0.05	0.00
Observations	18,482	18,482	16,346	18,482	18,482	9,102

A.3. Quasi-natural experiment appendix

A.3.1. Anti-regime Civil Society Organizations and democratizations

Figure A.7 shows that anti-regime civil society organization (CSO) activity is a key predictor of future democratizations. In particular, this figure estimates a linear probability model including both lagged anti-regime CSO activity and democratic mobilizations where the dependent variable is equal to 1 if it is a democratization year for a given country. The number of lags is shown on the x-axis. Here, we see that anti-regime CSO activity is (1) a significant predictor of future democratizations and (2) outperforms democratic protests substantially at longer horizons between 5 to 20 years. This is important as anti-regime CSO activity spikes during the treatment period in majority Catholic autocracies from 1959–1963.

A.3.2. The First Vatican Council

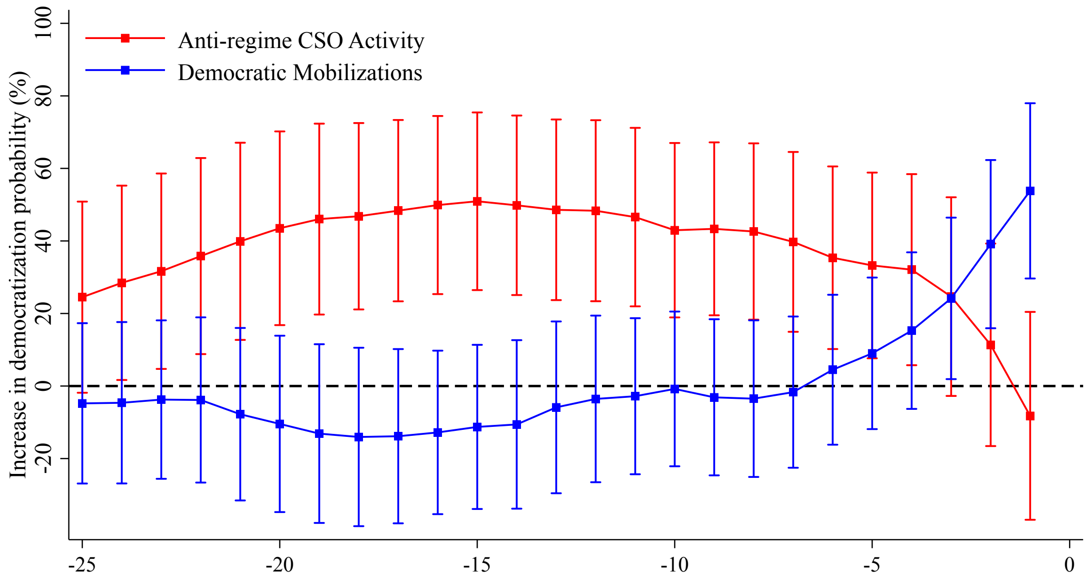
One potential concern is that the results are driven by the change in Catholic church doctrine, and have nothing to do with an increases probability of democratization. To assess the validity of this challenge, I estimate the difference-in-differences specifications on another major change in Catholic church doctrine: the First Vatican Council of 1868–1870 (Vatican-I). Vatican-I is distinct from Vatican-II in that it reaffirmed the Church's rejection of liberalism and democratic principles.

Figure A.7: Predicting democratizations with anti-regime CSO activity vs. democratic mobilizations

This figure presents the the coefficients from a linear probability model of the form

$$\mathbb{1}\{\text{Democratization}\}_{c,t} = \gamma_t + \eta_c + \beta_1 \text{Anti-regime CSO}_{c,t-h} + \beta_2 \text{Democratic Mobilization}_{c,t-h} + \varepsilon_{c,t} \quad \text{(A.3.1)}$$

estimated on the post-1960 sample. Each coefficient is scaled by the unconditional probability of being in a democratization year. Standard errors are clustered by country and year.



As such, it serves as an excellent test of whether changes in religious doctrine, in general, lead to high risk premia. For the estimation window, I use all years from 1864–1870, as Vatican-I was announced in 1864. Moreover, the affirmation of the Church's stance against liberalism began with the Syllabus of Errors in 1864, which Luebbert (1991) calls a "declaration of war on liberalism."

Once again, two sample windows are estimated: one 15 year symmetric window from 1849–1885, as to place the Revolutions of 1848 outside the sample, and one symmetric 20 year window from 1844–1890. All countries that are not majority Catholic autocracies are used as the control group. The results, reported in Table A.16, display negative, insignificant point estimates in both specifications. This is consistent with the theory underlying the shock: The Vatican-I likely reduced the probability of democratization, thereby reducing risk premia. It also supports that changes in Catholic doctrine do not generally raise risk premia.

Table A.16: Difference-in-Differences — First Vatican Council

This table shows the regression coefficients of a difference-in-differences regression given by Equation (1.4.1). In each regression, 1864–1870 are the years of treatment and are excluded. Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1844–1890. All coefficients have been multiplied by 100, and standard errors are in parentheses.

	All Countries	
	(1)	(2)
Majority Catholic Autocracy × Post	-4.89	-5.49
	(4.50)	(3.70)
Country FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
Sample	1849–1885	1844–1890
R^2	0.15	0.12
Observations	499	644

A.3.3. Shifting the treatment window

This section presents an additional falsification test coming from shifting the treatment window. Figure A.8 presents the results. This falsification test indicates that estimating the difference-in-differences specifications would only have yielded significant results in a narrow range of years. Moreover, the results shifting forward by 1 to 4 years are made stronger by the realized negative returns entering the pre-period estimation, as shown in Figure 1.5. Without this, shifting the treatment window forward would have yielded insignificant results more quickly.

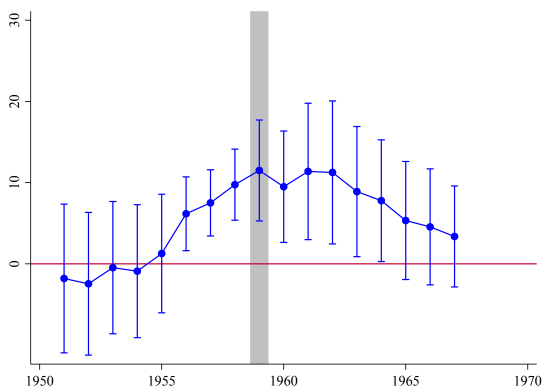
A.3.4. Estimation end date

The end year of the estimation window in Section 1.4 is chosen such that the sample is symmetric about the treatment window. However, other choices for end years may be reasonable. To show that the results for each specification are robust to different choices, I provide the point estimate and 95% confidence interval for each specification with the estimation window ending in each year from 1970–1983, shown in Figure A.9. For the specification where all countries are included, all of the point estimates are significant at the 95% level, and decline as the post-treatment window moves further in the future, suggesting a gradual resolution of the increased risk premia over time. In the autocracies only sample, the results become significant in 1976 as more observations enter the sample and the post-treatment effects become more precisely estimated. Moreover, the treatment

Figure A.8: Dropping every country pair, 1946–1976

This figure estimates the specification from Equations (1.4.1) on different treatment windows. The x-axis represents the treatment start year. The treatment years contain the start year plus 4 additional years, and are excluded from each regression The sample period is always a symmetric 13-year window around the treatment years and estimates for two different samples, one for all countries (Panel A) and the other on autocracies only (Panel B), are reported. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100, and standard errors are in parentheses. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita.

Panel A: All Countries



Panel B: Autocracy Only

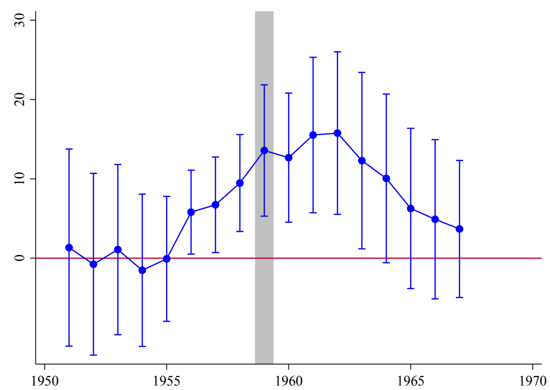
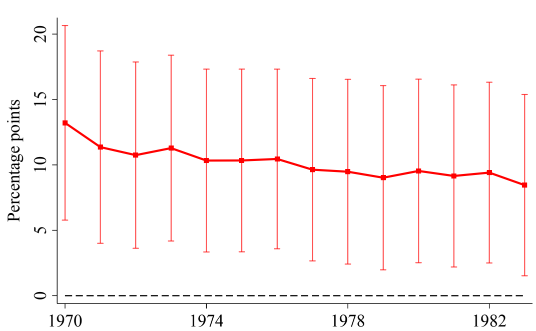


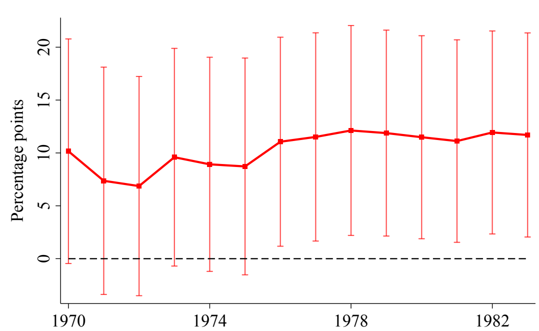
Figure A.9: Different estimation window end dates

This figure estimates the specification from Equations (1.4.1) on different window end dates and reports the point estimates and 95% confidence interval for the treatment effect. The sample period starts in 1946 and the x-axis denotes the end year. Two samples, one for all countries (Panel A) and the other on autocracies only (Panel B), are reported. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita.

Panel A: All countries



Panel B: Autocracies only



effect seems to be stable at more years are included.

Different estimation windows

A.3.5. Dropping every country pair

To assure the results are not driven by any one or two countries, I estimate all specifications excluding every possible combination of countries. This means that each regression is estimated on 41 countries from 1946–1976. Figure A.10 shows the results. No pairs of countries drive the results, which are strongly significant in every specification. For the all countries specification, the point estimates range from 6 to 12 percentage points and all are significant at the 5% level. Similar results hold for the autocracies only specifications with estimates between 7 to 15 percentage points and all significant at the 5% level.

A.3.6. Extreme values driving the results

The results reported in Section 1.4 are somewhat large when compared to the results found in the panel regressions. Some of this could be due to anomalously high returns in the post period, in particular in the years 1967–1969. To show how removing these outliers affects the results, I use three different methods: (1) winsorizing at the 5% and 10% levels, (2) removing the three highest return years from 1967–1969, and (3) using outlier robust regression via Li (2006).

In the winsorized results, the global and continental risk-adjusted returns are truncated at the 5th and 95th percentiles and the 10th and 90th percentiles. The results are shown in Table A.17. The results are still highly statistically significant and have lower point estimates, pointing to a 4 to 7.5 percentage point treatment effect, indicating that about one-third of the treatment effect reported in the main text is coming from anomalously large observations. Table A.17 also shows the results from excluding the high return years. These three years do not seem to be driving the results and, when removed, the estimated treatment effect is between 4 to 10 percentage points.

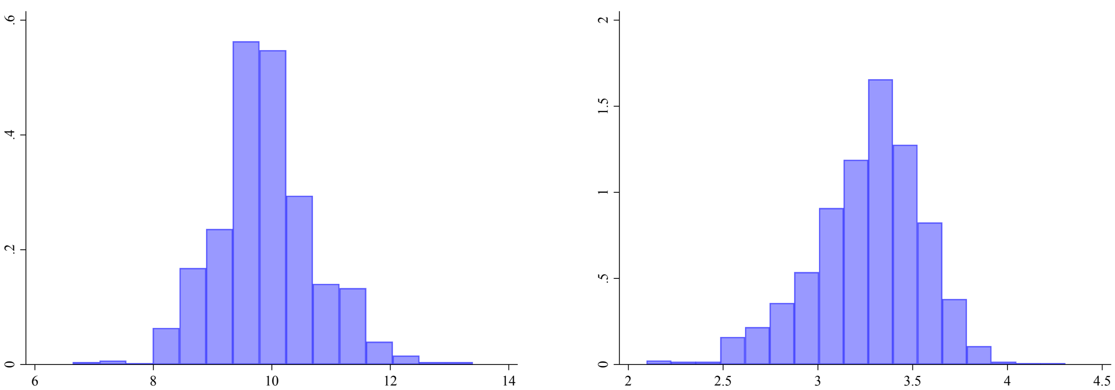
Table A.18 uses outlier robust regression weights via Li (2006) and finds, similar to the results above, a treatment effect of 6–7.5 percentage points. These results indicate that approximately one-third of the results above can be ascribed to anomalously large observations.

Figure A.10: Dropping every country pair, 1946–1976

This figure estimates the specification from Equations (1.4.1) excluding each possible country pair. The sample period is from 1946–1976 and estimates for two different samples, one for all countries (Panel A) and the other on autocracies only (Panel B), are reported. In each regression, 1959 to 1963 are the years of treatment and are excluded. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita.

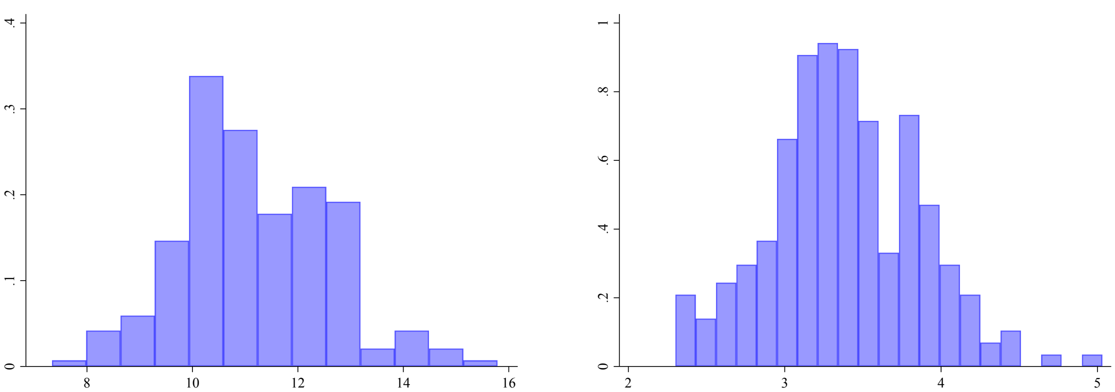
Panel A: All countries

Point Estimates **t-Statistics**



Panel B: Autocracy only

Point Estimates **t-Statistics**



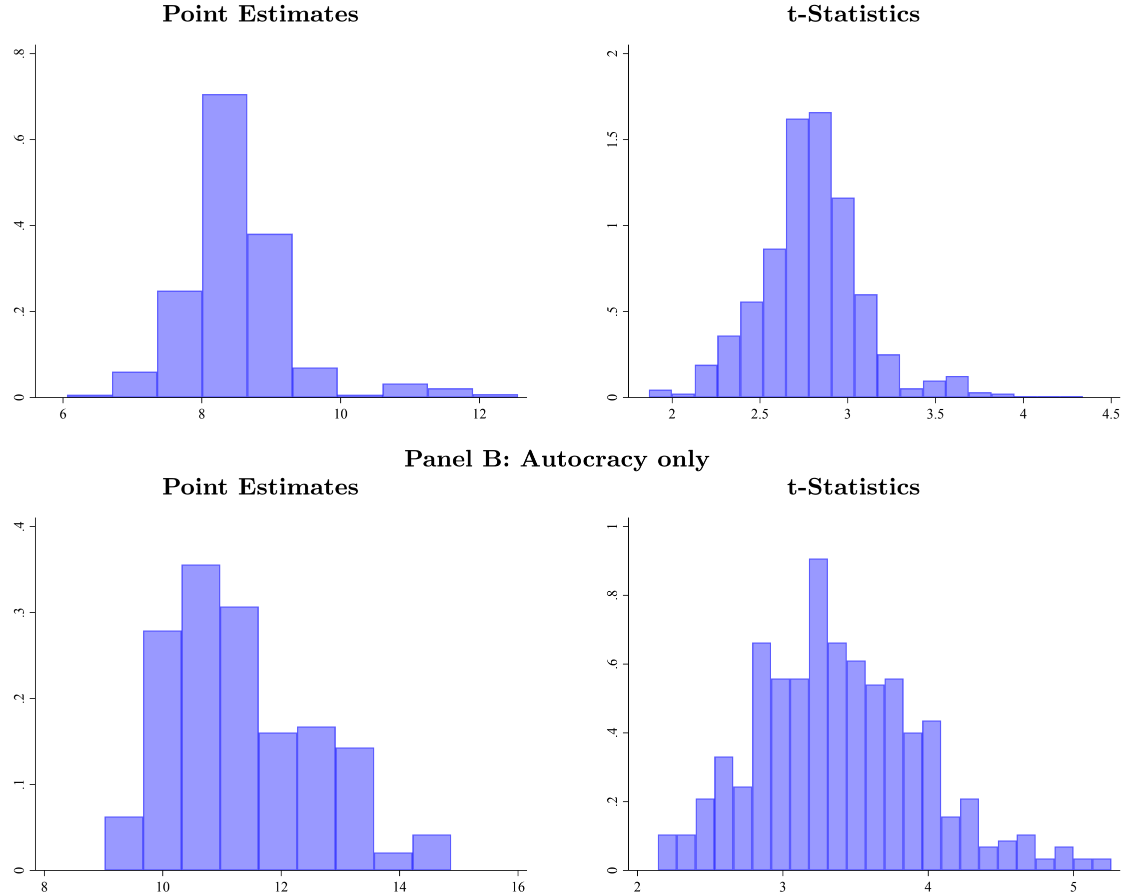
A.3.7. Other methods of adjusting for orthogonal sources of risk

Another potential concern is that factor model used to adjust average excess returns for time-varying global and continental risk could be absorbing some of the treatment variation, especially for the majority Catholic autocracies in Latin America. To assure this is not driving the results, this subsection presents the results adjusting average excess returns for global risk only, estimating

Figure A.11: Dropping every country pair, 1939–1983

This figure estimates the specification from Equations (1.4.1) excluding each possible country pair. The sample period is from 1939–1983 and estimates for two different samples, one for all countries (Panel A) and the other on autocracies only (Panel B), are reported. In each regression, 1959 to 1963 are the years of treatment and are excluded. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita.

Panel A: All countries



a one factor model of the form

$$R^e_{c,j,t} = \alpha_{c,t} + \beta_{1,t} R^{e,global}_t + \varepsilon_{c,j,t} \tag{A.3.2}$$

where $R^{e,global}_t$ denotes the total return in excess of the return on U.S. treasury bills on a GDP-weighted global market portfolio, c denotes the country, and t denotes the year. Once again, the β's

Table A.17: Difference-in-differences — Removing outliers

This table shows the regression coefficients for the difference-in-differences specification in Equation (1.4.1) on two sample windows, one from 1946–1976 and the other from 1939–1983, and for two different samples, one for all countries and the other on autocracies only. In each regression, 1959 to 1963 are the years of treatment and are excluded. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100, and standard errors are in parentheses. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita. The first two columns present results winsorized at the 5% threshold. Columns (3) and (4) present results winsorized at the 10% threshold, and Columns (5) and (6) present results with 1967–1969 excluded. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Winsorized at 5% and 95%		Winsorized at 10% and 90%		Excluding 1967–1969	
	(1)	(2)	(3)	(4)	(5)	(6)
Majority Catholic × Post	7.47***	6.01**	6.55***	5.35**	8.84***	10.15**
	(2.50)	(2.39)	(2.02)	(2.05)	(3.13)	(3.59)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	1946–1976	1946–1976	1946–1976	1946–1976	1946–1976	1946–1976
R^2	0.13	0.15	0.13	0.15	0.14	0.18
Observations	1,069	557	1,069	557	940	449

	Winsorized at 5% and 95%		Winsorized at 10% and 90%		Excluding 1967–1969	
	(1)	(2)	(3)	(4)	(5)	(6)
Majority Catholic × Post	4.24**	6.61***	4.06**	5.75***	4.24*	7.95**
	(1.84)	(1.94)	(1.62)	(1.72)	(2.50)	(2.88)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	1939–1983	1939–1983	1939–1983	1939–1983	1939–1983	1939–1983
R^2	0.09	0.14	0.09	0.14	0.09	0.15
Observations	1,592	736	1,592	736	1,463	673

are estimated on a rolling basis over 10-years, and require a minimum of 5-years to be estimated. This risk model also has good explanatory power for returns in the cross-section of countries, with an average (median) coefficient of determination, or R^2, of 0.39 (0.37). The results remain large and statistically significant, albeit with larger standard errors, potentially coming from measurement error in the dependent variable when using only a one factor model.

Table A.18: Difference-in-differences — Outlier robust weights

This table shows the regression coefficients for the difference-in-differences specification in Equation (1.4.1) on two sample windows, one from 1946–1976 and the other from 1939–1983, and for two different samples, one for all countries and the other on autocracies only. Robust regression weights are constructed as suggested in Li (2006) using a biweight tuning constant equal to 7, meaning observations in excess of seven times the median absolute deviation from the median residual are down-weighted. In each regression, 1959 to 1963 are the years of treatment and are excluded. Excess returns are adjusted for global and continental risk using the two-factor risk model described by Equation (1.4.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100, and standard errors are in parentheses. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	All countries		Autocracies only	
	(1)	(2)	(3)	(4)
Majority Catholic × Post	6.40**	6.03***	6.73**	7.43***
	(2.42)	(2.02)	(3.17)	(2.09)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	1946–1976	1939–1983	1946–1976	1939–1983
R^2	0.21	0.18	0.26	0.24
Observations	1,062	1,293	503	594

A.4. Mechanism appendix

A.4.1. Event study plots

This section presents an event study comparing successful democratizations to failed democratizations for a 5-year moving average of the change in government revenue-GDP ratios and the Gini coefficient. The results indicate that government revenue-GDP ratios begin increasing and inequality begins declining quickly after a successful democratization end.

A.4.2. Prices at democratization ends

The results above contend that successful democratizations are a risk to investors. As such, we should see evidence in the data that investors view the realization of a successful democratization as negative in the data. Consistent with this idea, Figure A.13 shows a 5-year moving average of log capital gains around successful and failed democratization end years. Prices fall significantly around the end of successful democratizations with an F-test indicating that the coefficients are statistically different at the 5% level from what is seen in failed democratizations. To put this in

Table A.19: Difference-in-differences, 1959–1963 treatment window

This table shows the regression coefficients for the difference-in-differences specification in Equation (1.4.1) on two sample windows, one from 1946–1976 and the other from 1939–1983, and for two different samples, one for all countries and the other on autocracies only. In each regression, 1959 to 1963 are the years of treatment and are excluded. Excess returns are adjusted only for global risk using a one-factor risk model from Equation (A.3.2). Standard errors are clustered by country and year. Included countries must have at least 20 observations from 1946–1983. All coefficients have been multiplied by 100, and standard errors are in parentheses. The controls used are a series of "event controls" meaning indicator variables for whether there is a war, financial crisis, recession, first 5-years of a sovereign default, a head of government death, and ICB political crisis, democratization, or coup d'etat. In addition to the event controls, I also control for log GDP per capita. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: No Fixed Effects

	All Countries		Autocracies Only	
	(1)	(2)	(3)	(4)
Majority Catholic Autocracy × Post	10.63***	10.77***	11.75**	11.47***
	(3.45)	(1.98)	(5.13)	(2.64)
Country FE	No	No	No	No
Year FE	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Sample	1946–1976	1946–1983	1946–1976	1946–1983
R^2	0.04	0.03	0.06	0.04
Observations	1,069	1,309	512	608

Panel B: Two-Way Fixed Effects

	All Countries		Autocracies Only	
	(1)	(2)	(3)	(4)
Majority Catholic Autocracy × Post	13.61***	12.35***	4.45	7.09**
	(3.52)	(2.03)	(5.84)	(3.35)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	1946–1976	1946–1983	1946–1976	1946–1983
R^2	0.14	0.11	0.19	0.16
Observations	1,069	1,309	512	608

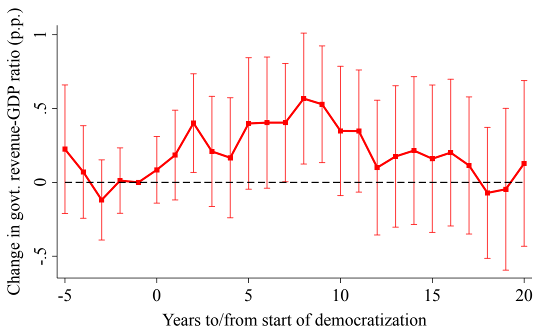
perspective, investors in the market of a country undergoing a successful democratization see the price of their investment fall by 16.1% (after subtracting the intercept) over 5-years.

Further, as shown in Panel B, there is substantial heterogeneity in the effect, with countries experiencing deeper democratizations seeing prices fall by 36.1% over 5 years. These results are consistent with the Elite disaster interpretation of successful democratizations presented in the model in Section 1.6. Moreover, democratizations that are reversed or co-opted after the democratization end see high realized capital gains, as evidenced by the reversed democratization line in Panel B.

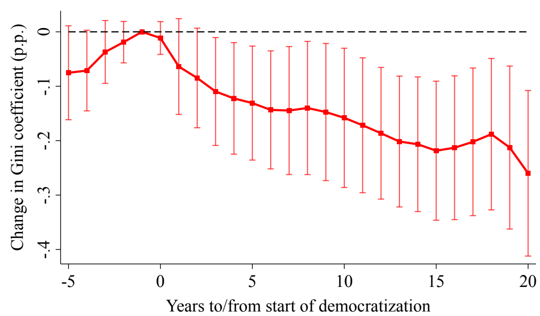
Figure A.12: Explicit redistribution event study

This figure shows an event study plot of a 5-year rolling average of the change in government revenue-GDP growth and the Gini coefficient around successful democratization ends compared to failed democratization ends. This is done by estimating additional event dummies for failed democratizations, and estimating the successful democratization coefficients relative to them. Country and year fixed effects are included and standard errors are clustered by country and year. A 90% confidence interval is reported.

Panel A: Government Revenue-GDP Ratios



Panel B: Gini Coefficient



A.4.3. Asset prices and redistribution

This section presents additional evidence that redistribution risk drives the rise in risk premia during democratizations. The prediction of the model is that democratizations see a rise in risk premia due to wealth elites' fear of future redistribution. This would imply that democratizations in which this ingredient were not present should see a smaller effect on risk premia.

To test whether democratizations where the elites are the most affected are driving the asset pricing results, I use data from V-Dem that denotes the most powerful socioeconomic group in every country in each year. I then group all democratizations into whether the most powerful group are the elites[67] or some other group[68].

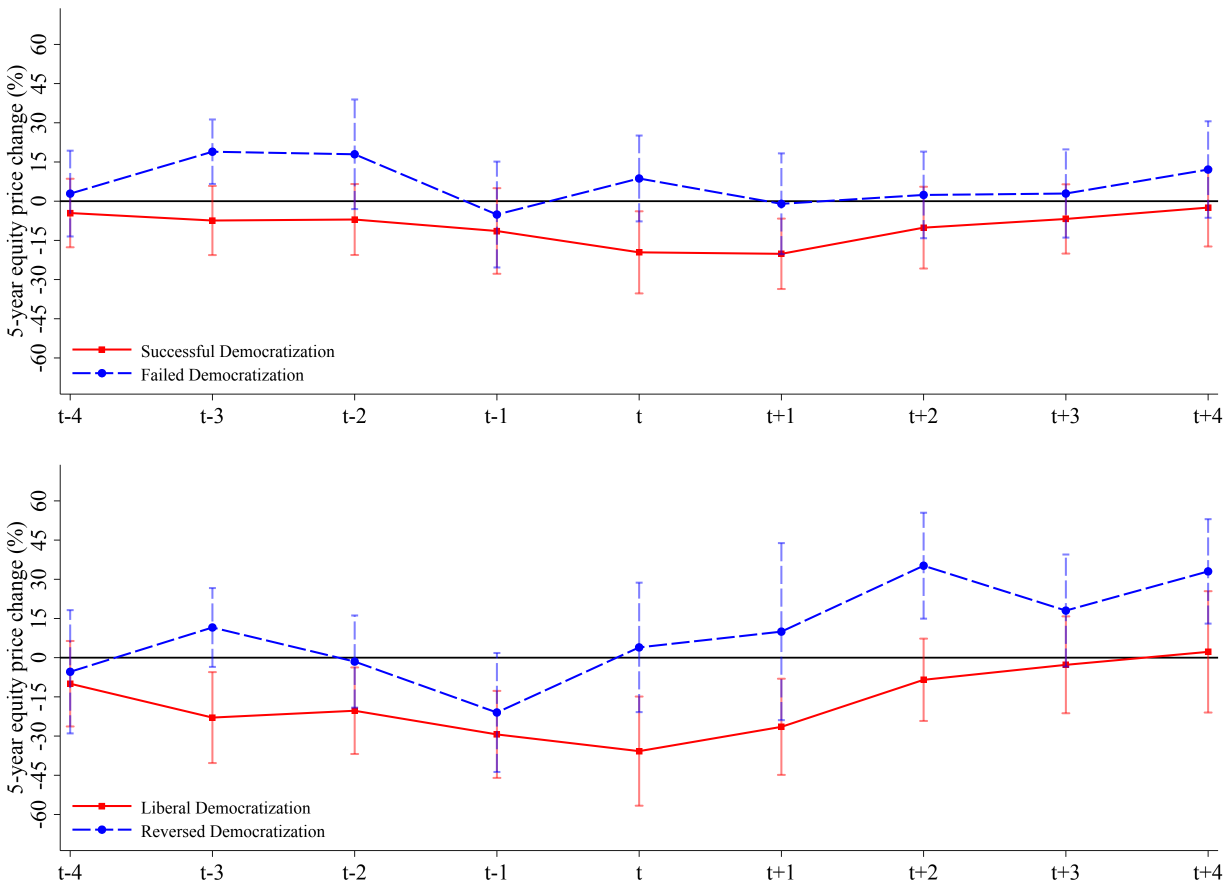
Table A.20 presents these results. Consistent with the model, elite democratizations see a substantially larger asset pricing effect. In all specifications, elite democratizations have a 2.5 larger rise in dividend yields than non-elite democratizations. Moreover, in Columns (3) and (4), elite democratizations so a statistically larger rise in dividend yields according to an F-test.

[67]This is done by combining the aristocracy, business elites, political elites, and local elites into one group called "the elites."

[68]This is done by combining the middle class, lower class, and foreign powers into a group called the "non-elites."

Figure A.13: Price response to successful vs. failed democratizations

This figure presents the coefficients of the 5-year change in log prices on indicator variables for the on each year in a 9-year window around the end of "successful" and "failed" democratizations (Panel A) and "liberal" or "reversed" democratizations (Panel B). Successful and failed democratizations are determined using the designation in the ERT data. Namely, successful democratizations are ones in which there is a democratic transition or deepened democracy. Failed democratizations are ones in which there is no democratic transition. Liberal democratizations are ones in which the ending regime is a "liberal democracy" as determined by the V-Dem regime type variable. A reversed democratization is one in which the country reverts to a closed autocracy or the business or political elites become the most powerful group in the regime, also determined by the V-Dem regime indices, in the 5 years after the end of a democratization. The bars represent a 90% confidence interval of the point estimates with standard errors clustered



Moreover, Table A.21 shows that democratizations with deeper price declines see larger future declines in inequality. In particular, this table estimates a regression of the change in the Gini coefficient five or ten years in the future (e.g. $t+1$ to $t+5$) on the prior three years price decline interacted with whether a country is in a democratization. The positive coefficients indicate that the two series move in the same direction. This means that deeper price declines during democratizations are associated with significantly lower Gini coefficients 5 or 10 years in the future. This is not entirely

Table A.20: Elite democratizations and changes in log dividend yields

This table presents regressions of the 5-year change in log dividend yields on indicator variables representing the start of an elite democratization or non-elite democratization. An elite democratization is a democratization in which the business elites, political elites, or aristocracy were the most powerful political group in the year prior to the democratization start according to the V-Dem regime data. The specification estimated is

$$dp_{c,t} - dp_{c,t-5} = \alpha + \beta_1 \mathbb{1}_{c,t}\{\text{Elite Democratization Start Year}\} + \beta_2 \mathbb{1}_{c,t}\{\text{Non-Elite Democratization Start Year}\} + \epsilon_{c,t}$$

where dp is the log dividend yield and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. The same results for financial crises are included for purpose of comparison. In Columns (3) and (4) some observations are lost due to there only being one observation in a region-year or in a continent-regime-year. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in log dividend yields			
	(1)	(2)	(3)	(4)
Elite Democratization	24.43**	28.31***	34.78***	35.52***
	(9.57)	(9.73)	(10.21)	(10.25)
Non-Elite Democratization	10.86	8.57	8.64	12.02
	(8.37)	(7.33)	(8.80)	(8.83)
Country FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	No
Region × Year FE	No	No	Yes	No
Continent × Regime × Year FE	No	No	No	Yes
Event Controls	Yes	Yes	Yes	Yes
Elite Episode obs.	34	34	34	34
R^2	0.01	0.15	0.36	0.34
Observations	5,857	5,854	5,508	5,602

surprising: it is, in essence a combination of the results from Section 1.5 and Appendix Section A.4.2. However, the provides additional evidence that the two series are indeed connected.

A.5. Model calculations and proofs

A.5.1. Value functions of the Citizens and the Elites

Both the Elites and the Citizens have Epstein and Zin utility over coconuts. For the Citizens, in autocracy, their only decision is over whether to revolt; in democracy their only decision is the tax rate to implement. To understand the former, we need to understand the solution to the Citizens' value function. We can solve this in three case: (1) by solving for their value function in the revolution, (2) by solving for their value function in democracy, and (3) by solving for their value function in autocracy as a function of the cost of revolution μ. For the Elites, they must decide the

Table A.21: Future inequality and price declines

This table presents the relationship between price declines in democratizations and future declines in inequality. The specification estimated is

$$\begin{aligned}\text{Gini}_{c,t+h} - \text{Gini}_{c,t+1} = \alpha + \beta_1 \mathbb{1}_{c,t}\{\text{Democratization}\} \\ + \beta_2(p_{c,t} - p_{c,t-3}) + \beta_3 \mathbb{1}_{c,t}\{\text{Democratization}\} \times (p_{c,t} - p_{c,t-3}) + \epsilon_{c,t}\end{aligned}$$

where h is either 5 or 10, p is the log price of the aggregate stock market index, and α represents either the coefficient on a vector of ones or the fixed effects denoted at the bottom of the table. Standard errors are clustered by country and year. All coefficients have been multiplied by 100 for presentation, and standard errors are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Five-year change in Gini coef.		Ten-year change in Gini coef.	
	(1)	(2)	(3)	(4)
Democratization × 3-year Price Change	0.24**	0.23**	0.54**	0.37*
	(0.10)	(0.11)	(0.24)	(0.20)
Democratization	-0.17	-0.01	-0.68**	-0.13
	(0.12)	(0.13)	(0.26)	(0.30)
3-year Price Change	-0.07	-0.14**	-0.22	-0.09
	(0.08)	(0.05)	(0.24)	(0.10)
Country FE	No	Yes	No	Yes
Year FE	No	Yes	No	Yes
Region × Year FE	No	No	No	No
Continent × Regime × Year FE	No	No	No	No
Controls	No	No	No	No
Episode obs.	330	330	306	304
R^2	0.01	0.40	0.02	0.49
Observations	1,807	1,804	1,491	1,487

tax rate to set in autocracy, and make no decisions of consequence for the political environment in democracy. In all periods, they must choose their portfolio in financial markets.

Value functions in the revolution If the Citizens decide to revolt, their value function can be written as

$$V^p(R,\mu_t)^{1-1/\psi} = (1-\beta)(Y_t^R)^{1-1/\psi} + \beta\left(\mathbb{E}_t\left[V^p(R,\mu_t)^{1-\gamma}\right]\right)^{\frac{1-1/\psi}{1-\gamma}}$$

where $Y^R \equiv \left(\frac{1-\mu}{1-\delta}\right)Y$ and the expectation is taken over the next period value of Y. Because Y is independent and identically distributed and the value function is homogeneous, we can scale the value function by Y, which yields $v^p(R,\mu_t)Y_t \equiv V^p(R,\mu_t)$. The scaled value function is then equal to:

$$v^p(R,\mu_t)^{1-1/\psi} = (1-\beta)\left(\frac{1-\mu}{1-\delta}\right)^{1-1/\psi} + \beta^\star\left(v^p(R,\mu_t)^{1-\gamma}\right)^{\frac{1-1/\psi}{1-\gamma}}$$

where $\beta^\star = \beta e^{(1-1/\psi)\bar{y}+\frac{1}{2}(1-\gamma)(1-1/\psi)\sigma_y^2}$. Solving for the value function yields the solution,

$$v^p(R,\mu_t) = \left(\frac{1-\beta}{1-\beta^\star}\right)^{\frac{1}{1-1/\psi}}\left(\frac{1-\mu}{1-\delta}\right).$$

The Elites conversely are assumed to have a large negative payoff in the revolution state $-v^r(R)$ such that they would always rather concede democracy.

Value functions in democracy The value function of the Citizens in democracy can be solved for using an identical logic to the solution in the revolution. Since the Citizens set the tax rate in democracy, their income is given by $\hat{Y}^p(\tau^{p*}(\theta^D)) = \left(\frac{1-\theta^D}{1-\delta} + \frac{1}{2}\left(\frac{\theta^D-\delta}{1-\delta}\right)^2\right)Y_t$. Since the economy remains a democracy forever after a successful democratization, the value function can be written as

$$v^p(D)^{1-1/\psi} = (1-\beta)\left(\frac{1-\theta^D}{1-\delta} + \frac{1}{2}\left(\frac{\theta^D-\delta}{1-\delta}\right)^2\right)^{1-1/\psi} + \beta^\star\left(v^p(D)^{1-\gamma}\right)^{\frac{1-1/\psi}{1-\gamma}}.$$

Solving yields

$$v^p(D) = \left(\frac{1-\beta}{1-\beta^\star}\right)^{\frac{1}{1-1/\psi}}\left(\frac{1-\theta^D}{1-\delta} + \frac{1}{2}\left(\frac{\theta^D-\delta}{1-\delta}\right)^2\right).$$

In equilibrium, the value function of the Elites in democracy can be solved for in the same way. Their income in democracy is given by $\hat{Y}^r(\tau^{p*}(\theta^D)) = \left(\frac{\theta^D}{\delta} - \frac{(\theta^D-\delta)^2}{\delta(1-\delta)} - \frac{1}{2}\left(\frac{\theta^D-\delta}{1-\delta}\right)^2\right)Y_t$. Again, since the economy remains a democracy forever after a successful democratization, the value function can be written as

$$v^r(D) = \left(\frac{1-\beta}{1-\beta^\star}\right)^{\frac{1}{1-1/\psi}}\left(\frac{\theta^D}{\delta} - \frac{(\theta^D-\delta)^2}{\delta(1-\delta)} - \frac{1}{2}\left(\frac{\theta^D-\delta}{1-\delta}\right)^2\right).$$

Value to the Citizens in autocracy In autocracy, there will be a solution to the value function for each value μ takes. This means we can write the value function of the Citizens in autocracy as

$$v^p(A,\mu_t)^{1-1/\psi} = (1-\beta)\hat{y}_t^p(\tau_t)^{1-1/\psi} + \beta^\star\left(\mathbb{E}_t\left[v^p(\mu_{t+1})^{1-\gamma}\right]\right)^{\frac{1-1/\psi}{1-\gamma}} \tag{A.5.1}$$

where the continuation value is given by

$$v^p(\mu_{t+1})^{1-\gamma} = \begin{cases} v^p(D)^{1-\gamma} & \text{if } \phi_{t+1} = 1 \\ v^p(A, \mu_{t+1})^{1-\gamma} & \text{if } \phi_{t+1} = 0 \cdot \\ v^p(R, \mu_{t+1})^{1-\gamma} & \text{if } \rho_{t+1} = 1 \end{cases}$$

A.5.2. Solution to more general cases of the model

In the main text, the model is calibrated such that upon reaching the third state, society transitions to democracy. In general though, for higher values of μ in the third state, the outcomes will be different. This section solves for the cutoff values of μ that achieve the different equilibrium outcomes in the third state, in particular, the three thresholds, $\underline{\mu}$, μ^*, and μ^{**}. In this example, for simplicity I take the case where $\mu^1 = \mu^2 = 1$ and $\mu^3 = \mu$, and I will characterize the solution for the threshold points in the third state. The transition matrix is given by

$$\mathbf{P} = \begin{pmatrix} p_{11} & p_{12} & p_{13} \\ p_{21} & p_{22} & p_{23} \\ p_{31} & p_{32} & p_{33} \end{pmatrix}$$

where all of the rows must sum to 1. The optimized value function (scaled by Y) of the citizens can be expressed compactly as

$$\mathbf{V}^p = \mathbf{Y} + \beta^\star \mathbf{P} \mathbf{V}^p$$

and implies the solution

$$\mathbf{V}^p = (\mathbf{I} - \beta^\star \mathbf{P})^{-1} \mathbf{Y} \tag{A.5.2}$$

where $\mathbf{I}$ is the identity matrix. The solutions in this case are pinned down by the cashflows in the final state and the transition probabilities.

To obtain the first threshold, $\underline{\mu}$, notice that the present value of coconuts when the Citizens receive no transfers in any period is

$$V^p(A, \mu_t; \tau_t = 0 \ \forall t) = \frac{1-\theta^A}{(1-\delta)(1-\beta^\star)}. \tag{A.5.3}$$

Equating Equation (1.6.7) with Equation (A.5.3) shows that

$$\underline{\mu} = \theta^A. \tag{A.5.4}$$

The second threshold, μ^*, is given by

$$\mu^* = \theta^A - \frac{\varpi(\theta^A - \delta)^2}{2(1-\delta)}, \tag{A.5.5}$$

where

$$\varpi = \mathbf{e}_3'(\mathbf{I} - \beta^\star \mathbf{P})^{-1}\mathbf{e}_3(1-\beta^\star)$$

where $\mathbf{e}_3$ is a column vector with a 1 in the third position and zeros elsewhere, $\mathbf{I}$ is a 3×3 identity matrix. In addition, when μ is in the range $\mu \in [\mu^*, \underline{\mu})$, the minimum tax the Elites can offer to avoid revolution is given by

$$\hat{\tau}(\mu) = \frac{\theta^A - \delta}{1-\delta} - \frac{\sqrt{(\theta^A-\delta)^2 - 2\left(\frac{\theta^A - \mu}{\varpi}\right)(1-\delta)}}{1-\delta}. \tag{A.5.6}$$

The final threshold, μ^{**} is described above in Equation (1.6.14).

Proposition 1. *If the transition matrix for μ follows Equation (1.6.15) and $\mu^1 = \mu^2 = 1$ and $\mu^3 = \mu$, and the regularity conditions $\beta^\star < 1$ and $\theta > \delta$ hold, then:*

- *For $\mu \in [\underline{\mu}, 1]$, the economy is an autocracy and taxes are set to 0 in all periods;*

- *For $\mu \in [\mu^*, \underline{\mu})$, the economy is an autocracy in all periods and taxes are set to 0 in the autocracy state and the democratization state, and to $\hat{\tau}(\mu)$, as specified in Equation (A.5.6),* *in the third state;*

- *For $\mu \in [\mu^{**}, \mu^{*})$, the economy is an autocracy and taxes are set to 0 in the autocracy state and the democratization state, and the economy becomes a democracy in the third state and taxes are set to τ^{p*}. Once the third state is reached, the economy remains a democracy forever;*

- *For $\mu \in [0, \mu^{**})$, the economy is an autocracy and taxes are set to 0 in the autocracy state and the democratization state, and the Citizens revolt in the third state;*

is a Markov perfect equilibrium with the threshold points $\underline{\mu}$, μ^{*}, and μ^{**} described by Equations (A.5.4), (A.5.5), and (1.6.14).

A.5.3. Asset pricing algebra

The solution for the pricing kernel revolves around the growth rate of the consumption of the Elites. This can be decomposed as

$$\frac{C^r_{t+1}}{C^r_t} \equiv \left(\frac{Y_{t+1}}{Y_t}\right)\left(\frac{c^r_{t+1}}{c^r_t}\right) \tag{A.5.7}$$

where c_{t+1} is consumption scaled by aggregate income. The growth rate of scaled consumption is given by

$$\frac{c^r_{t+1}}{c^r_t} \equiv \begin{cases} Z & \text{if } \phi_t = 1; \phi_{t-1} = 0 \\ 1 & \text{otherwise} \end{cases} \tag{A.5.8}$$

where $Z < 1$ represents the penalty the Elites face to their consumption upon a successful transition to democracy, given by

$$Z = \frac{\hat{y}^r_t(\tau^{p*}(\theta^D))}{\hat{y}^r_t(\tau_t)}. \tag{A.5.9}$$

Under Epstein-Zin utility, the stochastic discount factor of the Elites is

$$M_{t+1} = \beta^{\alpha}\left(\frac{C^r_{t+1}}{C^r_t}\right)^{-\frac{\alpha}{\psi}} R^{(\alpha-1)}_{W,t+1}$$

where $\alpha \equiv \frac{1-\gamma}{1-\frac{1}{\psi}}$. The return on wealth can be written as

$$R_{W,t+1} = \left(\frac{\kappa_{t+1}}{\kappa_t - 1}\right)\left(\frac{C^r_{t+1}}{C^r_t}\right) \tag{A.5.10}$$

where $\kappa \equiv W/C$ is the cum-dividend wealth-consumption ratio. Conjecture that κ is constant in each state of μ. This means that the solution is given by the solution to the system of equations

$$\kappa(\mu^j) = 1 + \beta e^{(1-\frac{1}{\psi})\bar{y}+\frac{1}{2}(1-\gamma)(1-\frac{1}{\psi})\sigma_y^2}\left[\mathbf{e}'_j \mathbf{P} \boldsymbol{\kappa}^{\alpha}\right]^{\frac{1}{\alpha}} \tag{A.5.11}$$

in states 1 and 2, where

$$\boldsymbol{\kappa}^{\alpha} \equiv \begin{pmatrix} \kappa(\mu^1)^{\alpha} \\ \kappa(\mu^2)^{\alpha} \\ \kappa(\mu^3)^{\alpha} Z^{1-\gamma} \end{pmatrix}. \tag{A.5.12}$$

In state 3, the wealth-consumption ratio is

$$\kappa(\mu^3) = \frac{1}{1 - \beta e^{(1-\frac{1}{\psi})\bar{y}+\frac{1}{2}(1-\gamma)(1-\frac{1}{\psi})\sigma_y^2}}.$$

This system of equations can be solved numerically.

The riskfree rate, similar to the wealth-consumption ratio, varies only with the state of μ and is given by

$$R_f(\mu_t) = \mathbb{E}\left[\beta^{\alpha}\left(\frac{C^r_{t+1}}{C^r_t}\right)^{-\gamma}\left(\frac{\kappa(\mu_{t+1})}{\kappa(\mu_t) - 1}\right)^{\alpha-1}\right]^{-1}.$$

This once again yields a system of 3 equations for the riskfree rate, which are characterized by

$$R_f(\mu^j) = \beta^{-\alpha} e^{\gamma\bar{y}-\frac{1}{2}\gamma^2\sigma_y^2}\left(\kappa(\mu^j) - 1\right)^{\alpha-1}\left[\mathbf{e}'_j \mathbf{P} \boldsymbol{\kappa}^{\alpha-1}\right]^{-1} \tag{A.5.13}$$

in states 1 and 2, where

$$\boldsymbol{\kappa}^{\alpha-1} \equiv \begin{pmatrix} \kappa(\mu^1)^{\alpha-1} \\ \kappa(\mu^2)^{\alpha-1} \\ \kappa(\mu^3)^{\alpha-1} Z^{-\gamma} \end{pmatrix}. \tag{A.5.14}$$

This riskfree rate in the 3rd state is given by

$$R_f(\mu^3) = \beta^{-1} e^{\frac{1}{\psi}\bar{y} - \frac{1}{2}(\gamma - \frac{1}{\psi}(1-\gamma))\sigma_y^2}.$$

The equity claim is modeled as a levered claim to Elite consumption as in Wachter (2013), such that $D_t = C_t^{\lambda}$. This implies the Euler equation

$$1 = \mathbb{E}_t\left[\beta^{\alpha}\left(\frac{C_{t+1}^r}{C_t^r}\right)^{\lambda - \frac{\alpha}{\psi}} R_{W,t+1}^{(\alpha-1)}\left(\frac{pd_{t+1}+1}{pd_t}\right)\right] \tag{A.5.15}$$

where *pd* is the ex-dividend price-dividend ratio. In democracy, the price-dividend ratio is given by

$$pd(D) = \beta e^{(\lambda - \frac{1}{\psi})\bar{y} + \frac{1}{2}((\lambda-\gamma)^2 + (1-\gamma)(\gamma - \frac{1}{\psi}))\sigma_y^2}\left(pd_{t+1} + 1\right) \tag{A.5.16}$$

which implies that

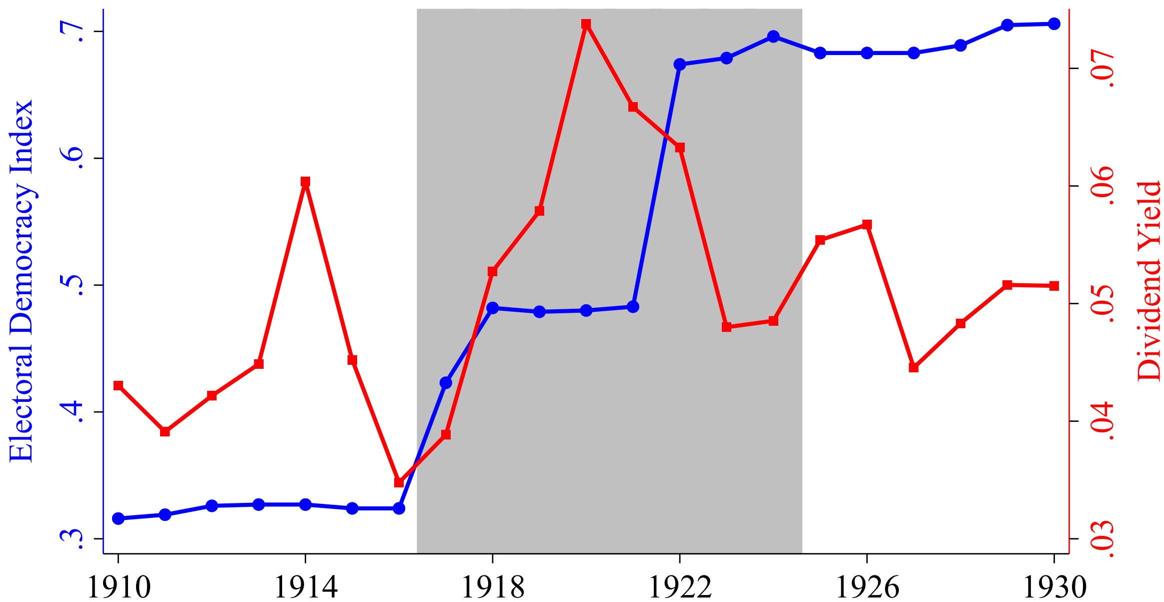
$$pd(D) = \frac{\beta e^{(\lambda - \frac{1}{\psi})\bar{y} + \frac{1}{2}((\lambda-\gamma)^2 + (1-\gamma)(\gamma - \frac{1}{\psi}))\sigma_y^2}}{1 - \beta e^{(\lambda - \frac{1}{\psi})\bar{y} + \frac{1}{2}((\lambda-\gamma)^2 + (1-\gamma)(\gamma - \frac{1}{\psi}))\sigma_y^2}}. \tag{A.5.17}$$

A.6. Case studies of democratization

A.6.1. Sweden, 1917–1924

The fall of the monarchy in Sweden offers an excellent example of a democratizations associated with a large stock market response combined with subsequent redistribution. Relative to its Scandinavian neighbors, Sweden was slow to democratize. This changed in 1917. The year began with a conservative government in power. By autumn, however, this government had been forced from office due to "food riots and the unreliability of the army (Luebbert, 1991)." Worker and soldier unrest continued into 1918 and by October the decisive democratic breakthrough had occurred.

Figure A.14: Electoral Democracy Index and dividend yield, Sweden 1917–1924



This victory brought with it a coalition Liberal-Social Democrat government from 1918–1920 which instituted several pro-labor policies through strengthening the already strong trade unions and instituting the 8 hour work day (Bengtsson, 2014). Universal suffrage was also established during this time, with the first elections under universal suffrage taking place between September 10th and 26th in 1921. V-Dem's Electoral Democracy Index tracks this progress well, as shown in Figure A.14, showing an initial increase in 1918 and final increase in 1922 as the newly elected government takes power.

While these policy changes did not immediately bring forth the famed Swedish welfare state—that would come about during and after the Great Depression—they did alter the bargaining power between labor and capital tremendously. For example, Bengtsson (2014) finds a structural break in the capital share of income in 1920, with the capital share going from a high of 40% in 1916 down to 20% just after 1920. Moreover, this effect seemed to permanent; from 1920–2000, it would not reach above 30% again.

Additional support for a nearly immediate reduction in inequality comes from examining top income shares. While exact numbers on how much inequality declined after the democratization are somewhat contested, recent research by Bengtsson et al. (2021) on a random sample of tax returns in Stockholm indicate that the Gini coefficient fell by at much as 20 percentage points and the top 10% share of income by 15 percentage points from 1920 to 1940. For comparison, the World Inequality Database (WID) reports that the top 10% share in the United Kingdom and United States remained flat over this period, and in France only declined by 5 percentage points. Similarly, the WID reports that the top 1% income share in Sweden fell by 8 percentage points, compared to 5 percentage points in the UK and France from 1919–1941. It remained flat in the U.S. over this period. Bengtsson (2019) also notes the discontinuity in Swedish income inequality post-democratization.

Finally, the Swedish democratization brought with it asset pricing effects consistent with the main message of the paper, as shown by Swedish dividend yield in Figure A.14. The dividend yield began to rise in 1917 with the labor unrest and calls for increased political rights. In the year of the democratic breakthrough, the dividend yield rose further with the onset of the democratic breakthrough. From 1917–1920, the outcome of the democratization remained highly uncertain. However, as 1920 came to an end, the shift of power toward the left became complete, and brought with it large declines to inequality. With this uncertainty resolved, the dividend yield began to fall to it's pre-democratization levels.

A.6.2. France, 1847–1848

The establishing of the Second French Republic in the wake of the revolution of 1848 presents an excellent example of a failed democratization. The movement toward the 1848 began in 1847 with the beginning of the Reformist "banquets" at which toasts were drunk to the *République française* (Marx, 1850). This *Campagne des banquets* was constructed to circumvent the restriction on political gatherings levied by the monarchy. While mostly liberal in nature, these banquets were also attended by reformists of all kinds; for example, a young Friedrich Engels attended some of these banquets starting in October 1847. King Louis Philippe allowed for these Reformist meetings to continue, resulting in an increase in free expression in the Electoral Democracy Index, as shown in Figure A.15.

As the banquets became more revolutionary in nature, however, the Prime Minister of France, François Guizot, outlawed them in January, 1848. Despite this ban, the gatherings continued. Things came to a head on February 22nd, when the French government banned the banquets for the second time, leading the organizing committee to cancel the events. Workers and students, however, had been mobilizing prior to the ban, and they did not plan to cancel their demonstration. It was with these demonstrations that a second "Three Glorious Days" began, leading to the ousting of King Louis Philippe on February 24th.

Shortly after the abdication of King Louis Philippe, the Second French Republic was declared. However, the democratic progress was short lived. Infighting in the proto-socialist groups made them politically ineffectual and ultimately led to the election of Louis Napoleon Bonaparte in the election of 1848. Bonaparte, a man viewed as the arch-ally to the *bourgeoisie* by Marx, ultimately fully reversed the democratic progress in his famed 1851 *coup d'état*, which established the Second French Empire.

Also shown in Table A.15 is the movement in the dividend yield across the failed democratization. Dividend yields spike in 1848 with the initial unrest and fall of the monarch. They then drop after the election of Louis Napoleon, but remain elevated until 1851, and the establishment of the Second Empire. In 1851 and 1852, stock prices rose 27% and 53%, respectively, signaling the end of the episode.

A.7. List of democratizations

Table A.22 shows the list of democratizations used in the asset pricing results.

Figure A.15: Electoral Democracy Index and dividend yield, France 1847–1848

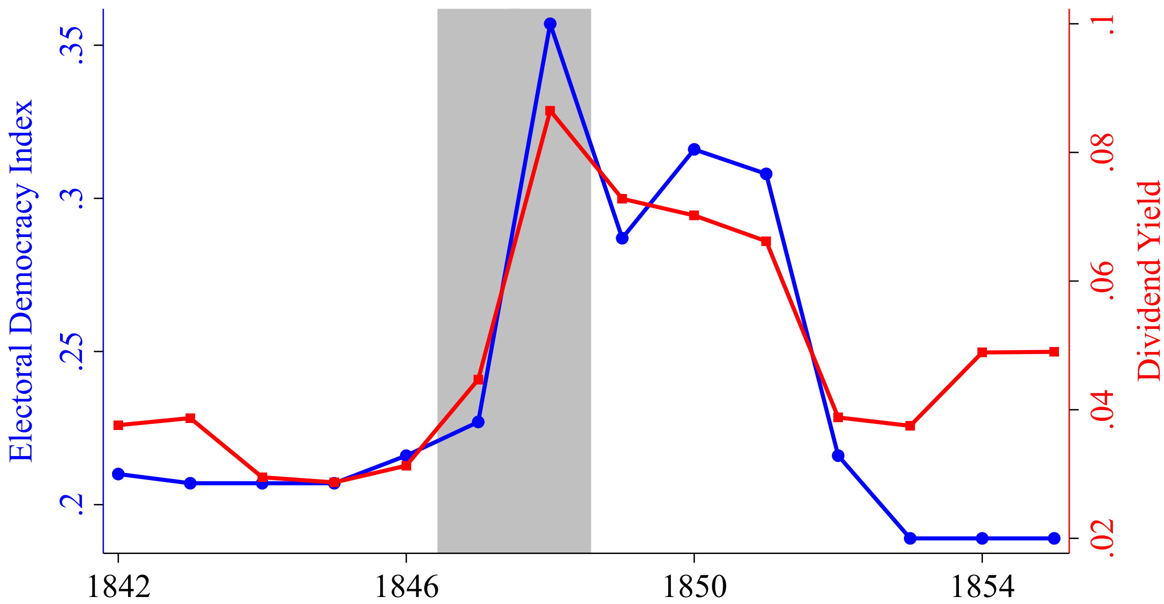


Table A.22: List of democratizations and democratic jumps

Country	**Democratizations**	**Major Events**
Argentina	1916–1926	1916: First Presidential Election with universal male sufferage 1921: Passage of Labor Codes 1922: Successful transition of power to Alvear Administration
Argentina	1932–1940	1932: Removal of Jose Felix Uriburu after turn toward fascism 1932: (Fraudulent) election after coup 1933: Survival of attempted coups 1937: General strike in support of construction workers 1938: Ortiz administration attempts to curtail electoral fraud

(Continued on next page)

Country	Democratizations	Major Events
Argentina	1946–1948	1946: Presidential election which Peron won in a landslide 1947: Suffrage extended to women 1948: Successful legislative election
Argentina	1972–1974	1972: Peronists begin general strikes and protests 1972: Return of Juan Peron from exile 1973: First elections in 10 years 1973: Juan Peron second presidency 1974: Death of Juan Peron 1974: Beginning of Isabel Peron administration
Australia	1843–1844	1843: First parlimentary election
Australia	1856–1858	1856: Beginning of Responsible Government 1856: Eight hour workday introduced 1856: Manhood suffrage introduced 1856: South Australian Constitution 1858: Secret Ballot introduced 1858: Women granted right to divorce
Australia	1901–1904	1901: Formation of the Australian federation 1901: Commonwealth of Australia proclaimed 1901: Australian Labor Party becomes official federal party 1901: First federal election 1902: Women receive right to vote 1903: High Court of Australia established 1903: Women vote in first election 1904: First Labor government

(Continued on next page)

Country	Democratizations	Major Events
Australia	1918–1923	1918: Beginning of industrial unrest 1918: End of WWI bring end to conscription of troops 1919: Preferential voting introduced 1921: First Woman elected to parliament 1922: Queensland abolishes upper house
Belgium	1894–1900	1893: General strike for suffrage 1894: First election under universal manhood sufferage 1894: Beginning of welfare net 1896: Beginning Liberal-Labor alliance 1900: Election of 1900
Belgium	1919–1922	1919: End of German occupation 1919: Beginning of Labor-Catholic Party coalition 1919: Introduction of graduated income tax 1919: First election with universal single-vote suffrage 1921: General election
Belgium	1944–1950	1944: End of German occupation 1944: Social Pact between labor party and trade unions 1945: Return of government in exile 1946: General election 1949: Introduction of women's sufferage 1950: General strike and abdication of King Leopold

(Continued on next page)

Country	Democratizations	Major Events
Belgium	1961–1965	1961: “Strike of the Century” 1961: Linking of Walloon nationalism with syndicalism 1961: Decolonization of Congo 1965: End of Congo Crisis
Bahrain	2000–2003	1999: Death of Shaikh Isa bin Salman Al Khalifa 2000: Creation of Supreme Judicial Council 2001: National Action Charter 2002: New constitution 2002: Legislative Election 2002: Women’s right to vote
Brazil	1945–1950	1945: End of the Estado Novo 1945: Beginning of Social Democratic Party dominance 1946: Fifth constitution of Brazil 1947: Legislative election 1950:: General election
Canada	1867	1867: Creation of the Dominion of Canada

(Continued on next page)

Country	Democratizations	Major Events
Canada	1920–1938	1920: Dominion Elections Act 1920: Formation of Progressive Party of Canada 1921: Election of first woman to House of Commons 1922: Full suffrage to black and white women in most provinces 1925: Extension of suffrage in Newfoundland and Labrador 1925: Election with continued Progressive Party success 1926: King-Byng affair
Canada	1942–1954	1942: A national plebiscite is held on the issue of conscription 1942: Income War Tax Act brings increased labor mobilization 1949: End of Judicial Committee of the Privy Council appeals in Canada
Switzerland	1970–1972	1971: First National Election with Women Voting
Ivory Coast	2001–2002	2001: Ivorian Popular Front (FPI) win majority
Colombia	1990–1995	1990: Colombian Constitutional Assembly election 1991: Enacting of the Constitution of Human Rights 1993: Death of Pablo Escobar 1995: Downfall of Cali Cartel

(Continued on next page)

Country	Democratizations	Major Events
Denmark	1901–1902	1901: Introduction of parlimentary sovereignty 1901: Folketing election 1902: Landsting election
Denmark	1916–1920	1915: Women granted right to vote 1916: Beginning of the Danish welfare state 1918: First elections under women's suffrage 1920: Easter Crisis
Denmark	1945–1948	1945: End of German Occupation 1945: Folketing and Landsting elections 1945: Beginning of Social Democrat dominance 1946: October Note 1948: Faroe Island given "home rule"
Spain	1931–1934	1931: Deposition of King Alfonso XIII 1931: Beginning of Second Spanish Republic 1931: New constitution 1933: General election
Spain	1976–1980	1975: Death of Francisco Franco 1977: First parliamentary election since 1936 1978: Approval of 1978 Constitution 1979: First general election under new constitution 1981: Survival of attempted coup

(Continued on next page)

Country	Democratizations	Major Events
Finland	1917–1921	1917: Independence from Russia 1918: End of Finnish Civil War 1919: New Constitution enacted 1919: Parliamentary election 1919: Social Democrat victory 1921: Official completion of Finnish Independence
Finland	1945–1946	1945: End of alliance with Nazi Germany 1945: Parliamentary election 1946: Beginning of Mauno Pekkala administration
Finland	1948–1950	1948: Parliamentary elections 1948: End of Pekkala administration 1949: Kemi strike; rejection of Communism 1950: Labor unrest and threat of general strike 1950: Start of a social reform era and welfare state
France	1847–1848	1847: Beginning of the Reform Movement and the banquets 1848: July Monarchy Ends 1848: Founding of Second French Republic 1848: Election of President Louis-Napoleon Bonaparte
France	1966	1966: Founding of Democratic Centre party 1966: Beginning of student movement toward May 68

(Continued on next page)

Country	Democratizations	Major Events
Hong Kong	1989–1992	1989: Tienanmen Square Protests 1989: Founding of Hong Kong Alliance in Support of Patriotic Democratic Movements of China 1990: Beijing ratifies Hong Kong's Basic Law 1991: Introduction of directly elected seats in legislature 1992: Governor Chris Patten announces reform package
Indonesia	1945–1957	1945: Beginning of Indonesian National Revolution 1946: Beginning of Republican government in Jakarta 1949: Independence 1950: Provisional Constitution of 1950 1951: Founding of Indonesian Communist Party 1955: First parlimentary elections 1957: System of Guided Democracy
Indonesia	1997–2004	1997: Indonesian legislative election 1998: Student demonstrations begin 1998: Collapse of Suharto regime 1999: First democratic elections 2000: Process of Constitutional reform 2004: Presidential election
India	1950–1957	1950: Adoption of Constitution of India 1950: First Republic Day 1951: General election 1952: Completion of General election 1957: General election

(Continued on next page)

Country	Democratizations	Major Events
India	1977–1979	1977: End of emergency powers 1977: Founding of Congress for Democracy 1977: General Elections; first loss for the Congress 1978: Appointment of Backward Classes Commission 1979: Fall of Janata Party
Kenya	1990–2003	1990: Increased congressional pressure for reform 1991: Founding of Forum for the Restoration of Democracy (FORD-Kenya) 1991: Repeal of one party amendment 1992: General election 1993: Successful transition to multiparty rule 2003: FORD-Kenya election victory
South Korea	1981–2000	1980: Gwangju Uprising 1981: Founding of Fifth Republic of Korea 1987: June Democracy Movement 1987: First democratic elections 1988: Founding of Sixth Republic of Korea 1988: New Constitution 1993: Reforms clamping down on corruption 1998: Inauguration of Kim Dae-jung 1998: First peaceful tranfer of power between parties
South Korea	2017–2018	2017: Park Geun-hye's removal from office 2017: President Moon Jae-in elected 2018: Park sentenced to 25 years in prison for bribery, coercion, and abuse of power

(Continued on next page)

Country	Democratizations	Major Events
Sri Lanka	1947–1949	1947: First elected parliamentary government 1947: New republican constitution replaced the Soulbury Constitution 1948: Discriminatory legislation passed 1949: Tamil congress splits; Federal Party is formed
Sri Lanka	2015–2017	2015: Presidential elections 2015: Vote for Mahinda Rajapaksa; does not belong to established political party 2015: Agenda to reverse near autocratic actions of last decade 2016: New president lifts ban on Tamil
Malaysia	2018	2018: Election of the Pakatan Harapan 2018: End of 60 year political reign by United Malays National Organisation 2018: Malay rights groups lead anti-ICERD rally reversing Mahathir's decision to ratify ICERD 2019: Partnership between UNMO and PAS is formalized
Namibia	2013–2016	2013: Push for gender equality 2014: Election with peaceful transfer of power 2014: Surveys indicate more citizens support democracy 2015: Local and regional elections held with electronic voting 2016: SWAPO power checked by High Court

(Continued on next page)

Country	Democratizations	Major Events
Nigeria	1976–1980	1976: Commander in Chief Muhammed killed in abortive coup 1976: General Olusegun Obasanjo, takes over 1976: Minorities vote for new president, Alhaji Shehu Shagari 1978: Obasanjo lifts ban on political parties
Nigeria	2010–2016	2010: Death of President Umaru Yar'Adua 2011: Election of 2011 (most transparent since 1999) 2015: Even more transparent general election 2015: Successful transition of power to Muhammadu Buhari
Netherlands	1917–1923	1917: Universal manhood suffrage implemented 1917: Women allowed to be elected, but not vote 1918: Unsuccessful socialist revolution in November 1919: Full suffrage granted to women 1920: Netherlands joins League of Nations
Netherlands	1945–1980	1945: End of German occupation 1946: Liberal State Party becomes Freedom Party 1946: Freeminded Democratic League joins Labor Party 1948: People's Party for Freedom and Democracy formed 1966: Democrats 66 formed

(Continued on next page)

Country	Democratizations	Major Events
Norway	1906–1910	1906: First parliamentary elections since the end of Union with Sweden 1907: Legislature allows women limited suffrage and ability to hold office 1909: Sorting passes Concessions Laws following much debate and split in Venstre
Norway	1914	1913: Universal suffrage established 1914: First elections with universal suffrage
Norway	1945–1998	1945: End of German Occupation 1945: Parliamentary election 1945: Labor wins for first time since 1915 1948: Break between Labor and Communist parties
New Zealand	1889–1897	1889: Abolition of plural votes for men of property 1890: First political party, Liberal Party, formed 1893: Universal suffrage granted 1894: Act of 1894 gave state power to repurchase land
Pakistan	2002–2017	2002: Referendum and General Election 2002: Beginning of multi-party politics after 1999 coup 2003: National assembly 2008: General election; end of Musharraf administration 2008: Official end of military rule 2013: General election 2017: Disqualification of Prime Minister Sharif by Supreme Court

(Continued on next page)

Country	Democratizations	Major Events
Peru	2001–2004	2001: Elections after fall of Fujimori 2001: Numerous reforms 2002: Regionalization Law 2002: National Accord 2004: Expansion of social safety net
Philippines	2010–2011	2010: Presidential election 2010: Introduction of electronic vote counting 2010: Aquino administration; politically stable and relatively clean
Portugal	1970–1984	1969: Transition to Caetono Regime 1969: Legislative election 1974: Carnation Revolution 1975: Elections for constitutional assembly 1975: Communist coup replaced by moderate coup 1976: Adoption of new constitution 1977: Beginning of European integration process 1979: First woman prime minister Maria de Lourdes Pintasilgo 1980: Legislative election 1983: Legislative election; Socialist party victory
Sweden	1917–1924	1917: Fall of conservative government 1918: Introduction of universal sufferage 1918: First Left-Social Democrat coalition government 1921: First election under universal suffrage 1922: Successful transition of power

(Continued on next page)

Country	Democratizations	Major Events
Sweden	1971–1974	1971: Abolished upper house of the Riksdag 1974: New constitution; principles of parliamentarianism incorporated
Thailand	1992–1993	1992: Black May Protests 1992: General Elections after Coup
Thailand	1997–2001	1997: Enactment of the "People's Consitution" 1997: Chuan Leekpai becomes prime minister 1998: Extension of public programs
Thailand	2008–2012	2008: Elections held after 2006 military coup 2011: General election; Pheu Thai Party wins in landslide
Tunisia	2011–2016	2011: Jasmine Revolution ousts Zine El Abidine Ben Ali 2011: Beginning of Arab Spring 2014: Constitution of 2014 2014: Parliamentary elections
United States of America	1893–1903	1892: Founding of the Populist Party 1893: Start of the Progressive Era 1893: Beginning of the Anti-Saloon League 1897: Organized labor gains steam with Mother Jones at helm 1903: March on Theodore Roosevelt home by Mother Jones

(Continued on next page)

Country	**Democratizations**	**Major Events**
United States of America	1920–1932	1920: Presidential elections; first where women vote
		1921: Washington Naval Conference
		1922: First woman senator Rebecca Felton
		1927: Reduction in Second Ku Klux Klan popularity
		1930: Start of social safety net
		1932: Election of President Roosevelt and New Deal
United States of America	1970–1977	1970: Post-civil rights era reforms
		1971: Voting age moved to 18
		1974: Watergate and resignation of Nixon
		1977: Transition to Carter administration
South Africa	1994–2010	1994: End of South African Apartheid
		1994: Election of Nelson Mandela to presidency
		1995: Enactment of new constitution
		1999: General election
		1999: Beginning of Mbeki presidency
		2004: General Election
		2005: National Party merges with ANC

APPENDIX B

Supplementary Material for Chapter 2

B.1. Data sources and construction of variables

B.1.1. Country characteristics

We collect data on GDP and population from the World Bank. Bilateral trade flows data come from CEPII (Gaulier and Zignago, 2010; Bailey, Gupta, Hillenbrand, Kuchler, Richmond, and Stroebel, 2021). Data relating to conflicts include total number of unrest episodes gathered from the Cline Center Historical Phoenix Event Data. We separate the number of times a foreign country was a source or a target.

Data on the use of physical force to achieve political objectives by non-state actors and the number of coups d'état during a given year come from the Varieties of Democracy Database (V-Dem). Political corruption index, which combines six distinct types of corruption and measures the level of corruption in a given year, political polarization capturing the extent to which political differences affect social relationships beyond political discussions, as well as the electoral democracy index (EDI) of a country all come from V-Dem.

Democratization events come from the Episodes of Regime Transformation (ERT) data. These data use changes in EDI to determine the start and end years of democratizations. V-Dem produces these data from 1900–2018.Finally, we rely on a time-varying measure of each country's political preferences based on how they vote relative to the US on resolutions in the UNGA as estimated by Bailey et al. (2017) and the US presidential diplomatic visits as measured by Malis and Smith (2021).

B.1.2. Legislator characteristics

Personal characteristics. Legislators' personal characteristics come from the Center for Effective Lawmaking, which identifies the gender of the legislator, whether he/she comes from an underrepresented minority group, or whether he/she is african-american. The dataset also includes information on the age of the legislator, the vote margin, and the seniority within his/her own party.

Ideology and lawmaker effectiveness. A congressperson effectiveness and ideology scores come from the Center for Effective Lawmaking. The lawmaker effectiveness scores were developed by Volden and Wiseman (2014, 2018), and capture the level of success that each Representative or Senator has in advancing their legislative agenda items through the lawmaking process. The lawmaker effectiveness score is calculated by first grouping their sponsored bills into three different categories capturing whether they are commemorative, substantive, or substantive and significant, and, second, assessing how far the bill progressed through the process of becoming a law. Therefore, higher LES scores are given to members with large portfolios, those who tackle significant issues (not just commemorative measures), and those whose bills advance further in the lawmaking process. The LES is normalized to an average value of one in each Congress. These data are then matched to the legislators found the in FARA data representing one of the fifty U.S states using a fuzzy matching algorithm.

To examine ideology, we use the dynamic weighted NOMINATE (DW-NOMINATE) ideology scores for members of Congress, which are the seminal measures of legislator ideology based on Congressional roll-call votes created by Poole and Rosenthal (1985) and later refined by Poole and Rosenthal (2011). DW-NOMINATE 1 captures the economic and governmental aspects of the ideological left-right spectrum. A second dimension of the score, DW-NOMINATE 2, captures differences within the major political parties on currency, nativism, civil rights, and lifestyle issues. A value close to 1 represents a more conservative congressperson, while a value close to -1 a more liberal congressperson.

Elections. For election data we rely on information from the MIT election lab which compile biennial documents from the Clerk of the US House of Representatives. In particular, we use state-level returns for elections to the US Senate and the US House of Representative until 2018. The data includes the election year, state, electoral stage (distinguishing between a general election, a runoff election, or a primary election), whether it was a special election, name of the candidates, their parties, details on votes, and the winner. These data give us a comprehensive dataset of all legislators seeking election to legislative office from 2000–2018.

Congressional committee assignment. Data on Congressional committees come from Stewart (2017) who provide detailed information on committee membership for each legislator serving in Congress from 1993 to 2019 and calculate the first and last time they were on a committee. We make some corrections to the data. For example, six congresspeople in the House of Representatives and for seven Senators are assigned the wrong state, which we manually adjust. Moreover, we adjust the incorrect Homeland Security and Governmental Affairs committee identifiers for Sen. Jeffrey Chiesa. These data are then matched to the legislators found the in FARA data representing one of the fifty U.S states using a fuzzy matching algorithm. All matches that are not perfect are manually assigned the correct legislator.

B.1.3. Main outcomes of interest

Foreign aid. Data on foreign aid comes from ForeignAssistance.gov which is a website hosted by the US Department of State and the US Agency for International Development (USAID). It provides a comprehensive overview about US foreign assistance on multiple dimensions. Detailed information on the funding and implementing agencies are provided, as is the purpose of the appropriated aid. In particular, aid is differentiated by purpose into several categories: Agriculture, Commodity Assistance, Economics Growth, Education, Governance, Health and Population, Humanitarian, Infrastructure, and Other, whereas the latter differentiates Peace and Security, Democracy, Human Rights and Governance, Health, Education and Social Services, Economic Growth, Humanitarian Assistance, and Program Development and Oversight. For each entry the name agency to which funds were appropriated is provided.

From the data we have dropped all observations where a transaction date was unavailable. Subsequently, we have collapsed the data on the country-executive department-year-month level, that is, for each country we obtain the amount of aid received from each US government agency for every month starting from October 2001. We also calculate the total aid for each year given to a country split by executive department. Note that some of the values we obtain from that process are negative. This is because aid is occasionally provided in the form of loans and for a given month or year a foreign country could be repaying more than it receives.

Tariffs bills. Data on tariff bills are taken from GovTrack.us by searching the bill text and bill subject line for the word "tariffs." We then searched each bill for mentions of specific trade agreements using the list of terms shown at the bottom of the paragraph. This list of terms was then matched to all countries affected by these trade agreements. This search yielded 469 bills over the period 2000–2018. We then went through the text of each bill to determine whether it increased or decreased tariffs or duties on products entering the United States. All bills that reduce tariffs or duties were categorized as "favorable"; all those that increase tariffs or duties were categorized as "unfavorable." Of the 469 bills, 244 were labeled favorable and 81 were labeled unfavorable, with the remainder being unclear on the direction they would alter tariffs. The 244 favorable bills yielded 2,969 unique country-bill observations, whereas the unfavorable bills yielded 298 unique country-bill observations, when matching countries to the trade agreements. Data were then collected on the sponsors and co-sponsors of these bills, the committees that oversaw them post-introduction, and the various actions that took place over the life-cycle of the bill. Data for sponsors and cosponsors were matched to the FARA meeting data by country, legislator and the year and month of bill introduction. Data for committees were matched to all senior legislators, where a senior legislator is defined as being in the top quartile of seniority within each party-committee pair. These data are then matched to the FARA meeting data by country, legislator and the year and month of all bill actions that took place in those committees. Committee bill actions are then categorized as "favorable" if the bill progresses through the legislative process or "unfavorable" if the bill does not pass that committee.

Trade agreement phrase list: free trade agreement implementation act; (cafta-dr); africa growth and opportunity act; (agoa); generalized system of preferences; (gsp); automotive products trade act; (apta); agreement on trade in civil aircraft; north american free trade agreement; nafta; caribbean basin initiative; (cbi); andean trade preference act; (atpa); andean trade promotion and drug eradication act; (atpdea); agreement on trade in pharmaceutical products; uruguay round concessions on intermediate chemicals for dyes; caribbean basin trade partnership act; (cbtpa); harmonized tariff schedule; caribbean basin economic recovery act; (cbera); united states-caribbean basin trade partnership act; united states-mexico-canada agreement implementation act; (usmca); trade agreement; trade act; trade partnership act.

Subsidies. Data on government subsidies come from Good Jobs Subsidy Tracker who provide data at the state and federal levels. Good Jobs provides data on the state and city issuing corporate subsidies along with the company name, ticker, and country of incorporation, where applicable. Good Jobs collects data from a variety of local, state, and federal sources, detailed here. To obtain data at the foreign principal level, we sum subsidies across the country of incorporation, state, and year.

Government contracts. We obtain information on government contracts awarded between 2002 and 2018 from USASpending.gov. The Federal Funding Accountability and Transparency Act of 2006 (FFATA) mandates that whenever a federal contract, grant, loan, or other financial assistance award exceeds $25,000, it must be displayed on a publicly accessible website. This legislation was amended twice, in 2008 and 2014, to require recipients and government agencies to disclose further information relating to sub-recipients, direct agency expenditures, and the linkage between federal spending and federal agency programs.

The data includes details on the funding amount, the date of the transaction, the awarding and funding agency, sub-agency, the recipient country, and the location of the performance of the contract. Moreover, it also includes information on the type of assistance, e.g., whether it is a grant or a loan, and the type of recipient, e.g., whether it is part of the government or a small business.

We extract information on all government contracts awarded to foreign recipient countries at the contract-level to match these data to foreign governments. We first drop all contracts awarded to US recipients from the universe of contracts, including those executed by US-owned businesses, and keep only contracts performed within the United States. We remove contract cancellations and terminations from our sample to ensure we capture new contract awards. We then aggregate the number of contracts and the total value awarded to each recipient country by the state of performance in each year.

B.1.4. Other data

Twitter. We obtain Twitter data of the official and personal accounts for all US legislators serving as of 07 April 2022 using version 2 of the Twitter API. We download all historical tweets, retweets, and quote tweets from the years 2010–2021. Given our meeting data goes from 2000–2018, not all House and Senate members can be matched to the meeting data. Of the 535 congresspeople, we can match 348 to our meeting data. Since many Senators were previous House of Representatives members or governors, they are often present in our sample before being elected to the Senate. In total, we collect 6,671,713 tweets, and in the text, we search for mentions of all countries in our sample. In this search, we exclude the country Jordan as it is too often matched with the popular American first and last name. Similarly, we dropped tweets containing the word "Turkey" in November to exclude mentions of the popular American Thanksgiving cuisine. This selection yields 96,689 tweets which we match to country mentions, approximately 2% of the total sample.

Official foreign trips. We obtain data on all official foreign travel undertaken by members of the House of representatives. These data are available in accordance with the Mutual Security Act of 1954 (Title 22 U.S. Code, Chapter 24, Section 1754) and the International Security Assistance Act of 1978. The disclosures contain detailed information on the arrival and departure dates, foreign country visited, and the expenditures incurred during the trip.

Privately-sponsored trips. We obtain data on privately sponsored trips taken by members of the House of representatives from 2008 onwards. These data are available in compliance with the House ethics rules which mandates disclosure of all privately sponsored trips and their sponsors to the Clerk of the House (Rosenson, 2009; McGee and Moniz, 2021). The disclosures contain detailed information on the arrival and departure dates, foreign country visited, and the private agency sponsoring the travel.

Political contributions. Our dataset contains summary financial information for all candidates who raised or spent money as reported by the U.S. Federal Election Commission (FEC) for the period between January 1, 2000 and December 31, 2018. Our data include contributions to all entities raising more than $5,000 for federal elections, whether they are candidates, parties or any other political action committees (PACs), as well as transfers from authorized committees to individual candidates.

B.2. Summary of semi-annual reports

Our new comprehensive dataset of meetings between US legislators and lobbyists working on behalf of foreign countries separates us from the previous empirical literature on foreign lobbying. In fact, given that the DOJ, in addition to the detailed FARA filings, also publishes summary reports semi-annually, which are easily accessible, prior work trying to understand broad trends in foreign lobbying has mostly used those reports. Each report describes information on the lobbyist including their activities, nature of services, and money received for their political activities undertaken on behalf of foreign clients as reported in question 12. Importantly, these reports do not have information on the meetings lobbyists have with US legislators on behalf of their clients. Therefore, these summary reports are only suited to study broad trends in foreign lobbying in the US, and cannot be used to shed light on the scope and nature of foreign influence.

Following Lee (2020), we use the information from these reports to classify lobbying activities into 12 broad topics. To identify frequently lobbied topics, we selected key words relevant to each topic and coded the topic of lobbying incidents according to whether the key words were used to describe the incidents. The exact key words are below:

- **Trade:** trade; export; import; fta; nafta; cafta; drcafta; ftaa; naftas; kfta; caftas; korus-fta; tpp; transpacific partnership; gsp; mcool; tariff; custom; agoa; african growth and opportunity act, tpl; tariff preferential level; wto; gatt; mfn; antidump; dump; caribbean & basin; traders; exporters; imports; importers; sanction; commerc; food and drug administration; fda; food label
- **Economy:** financi; financ; fdi; tax; taxat; busi; econom; economi; debt; invest; investment; monetari; imf; bank; antitrust; scal; internat & monetari & fund; world & bank; exchang & rate; government & bond; securities & tax; securities & taxat; securities & exchang; securities &exchanges; securities & regulation; securities & regulations; securities & financial; secur & finance; oil; energy; appropriation
- **Security:** defence;defens; militari; nato; disarm; terror; counterterror; terrorist; antiterror; extremism; troop; peacemak; peacekeep; international & security; national & security; regional & security; security & relations; security & relationship; peace & process; peace & treaty; arms & sales
- **Diplomacy:** government relations; government relationship; government relationships; bilateral relations; bilateral relationship; bilateral relationships; diplomatic relations; diplomatic relationship; diplomatic relationships
- **Policy legal issues:** polici & consult; polici & counsel; polici & servic; polici & advic; polici & analysi; legal & consult; legal & counsel; legal & servic; legal & advic; legal & analysi; legal; law; political; act; legislation; s.[0-9]1,4; hr.[0-9]1,5; s-[0-9]1,4; hr-[0-9]1,5; public policy; foreign policy; US policy; us policy; resolution; settlement; regulat
- **Publicity:** media; news; newspaper; newspapers; newsletter; newsletters; enewslett; press; public & relations

- **Tourism:** tourism; tourist; tour; travel
- **Nuclear:** nuclear; atom; uranium
- **Visa:** visa; immigr; immigrat; immigrant
- **Foreign aid:** aid; usaid; economi & assistanc; militari & assistanc
- **Human rights:** human & rights; education; women; food assistance
- **Secession:** selfdetermin; self determin; self-determin

Panel A of Figure B.1 presents the evolution of the 6 most frequently listed topics over the sample period. We find that approximately one in four activities each year relate to publicity while one in ten activities relate to security. Over the sample period, lobbyists increased their engagement in diplomacy, while their engagement in economy and trade trended downwards. In addition to lobbying topics, we also classify the description of services into 5 broad topics which are presented in Panel B of Figure B.1. Lobbying services saw a significant uptick in 2010 and surpassed services related to promoting investment, trade, and tourism. By the end of 2018, more than half of the foreign agents report lobbying as their only service. Interestingly, there is a concomitant decrease in the promotion of investment, trade, and tourism around the same time as the uptick noted above. Further, we do not find any changes in consulting or fundraising activities over the sample period. These results reveal substantial heterogeneity in the role of lobbyists.

A next natural question is whether lobbyists specialize in providing issue-specific information to legislators, as indicated by prior work in the context of domestic lobbying (Bertrand, Bombardini, and Trebbi, 2014). To this end, Figure B.2 shows that the majority of lobbyists engage with legislators on fewer than three topics, suggesting that most lobbyists concentrate on a small number of topics in the foreign lobbying space.

Finally, we relate the number of topics engaged by the lobbyist on behalf of the foreign principal to the characteristics of the geographical region, where available. Specifically, we assess the relationship between foreign countries that lobby and several macroeconomic characteristics using data from the Word Bank. Specifically, we include data on Gross Domestic Product (GDP) per capita to capture economic growth, total value of exports and imports to capture reliance on trade, and labour share as a fraction of GDP to capture the trend toward automation that may affect incentives of policymakers (Ramey and Ramey, 1995; Jones and Olken, 2005; Jones and Romer, 2010). Additionally, we include the annual average country conflict score from the Cline Center Historical Phoenix Event Data, which provides detailed information on the level of conflict within each country every year (Althaus, Bajjalieh, Carter, Peyton, and Shalmon, 2020). Finally, we include data on institutions and the electoral democracy index from the Varieties of Democracy Database.

Table B.1 presents the estimates from a regression of the natural logarithm of the number of topics on time-varying characteristics discussed above. In particular, we estimate

$$\log(\text{Number of topics})_{frt} = \gamma_f + \delta_{rt} + \beta \text{ Country Characteristics}_{ft} + \epsilon_{frt}, \tag{B.2.1}$$

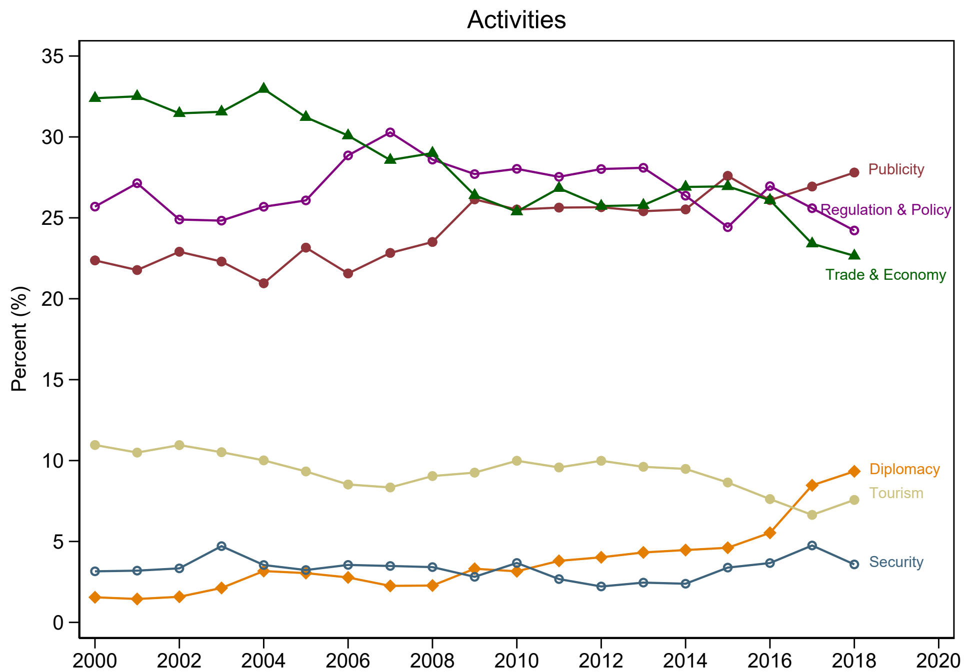
where f represents the country of the foreign principal, r represents the topic lobbied for, and t represents the year. The unit of observation is a foreign principal country-topic-year triad. The empirical specification includes country fixed effects to control for unobserved time-invariant regional characteristics in addition to topic-by-year fixed effects to allow for the importance of topics to vary over time. Our results suggest no statistically significant and economically meaningful association between foreign country characteristics and the number of topics except for the share of labor compensation as a fraction of the GDP. Note that the number of observations vary across specifications because of missing values of country characteristics.

In summary, the associations between topics and country characteristics from the semi-annual reports are informative of the broad trends in foreign lobbying activities. However, there are two major drawbacks. First, the summary reports do not contain information on the identities of individual US legislators, also a key issue in the broader literature on domestic lobbying using LDA data. Second, there is no information on the individual meetings between lobbyists and legislators. Both these drawbacks render summary reports unsuitable to study foreign influence in the US.

Figure B.1: Activities and services provided by lobbying firms

The figure shows the fraction of activities belonging to each specific topic (Panel A) and each type of services (Panel B). The twelve lobbying topics are identified following the procedure outlined in Appendix B.2.

A) Activities provided by the lobbying firm



B) Nature of services provided by the lobbying firm

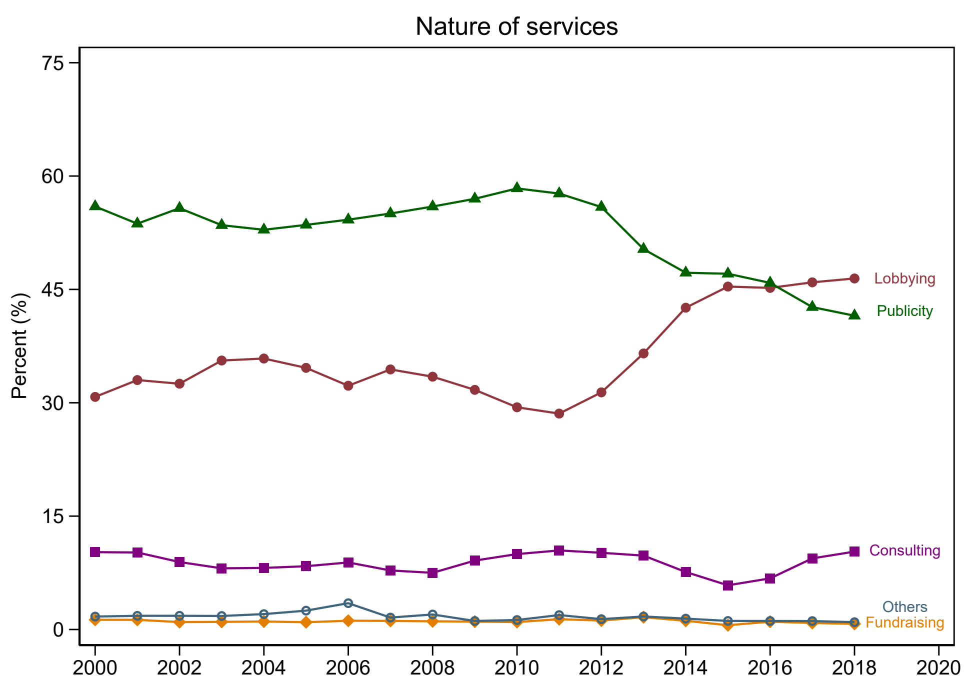


Figure B.2: Lobbyist specialization

The histogram shows the number of different topics each lobbying firm has worked on from 2000 to 2018 (horizontal axis), and the corresponding number of lobbying firms that have worked on a given number of topics (vertical axis). The twelve lobbying topics are identified following the procedure outlined in Appendix B.2.

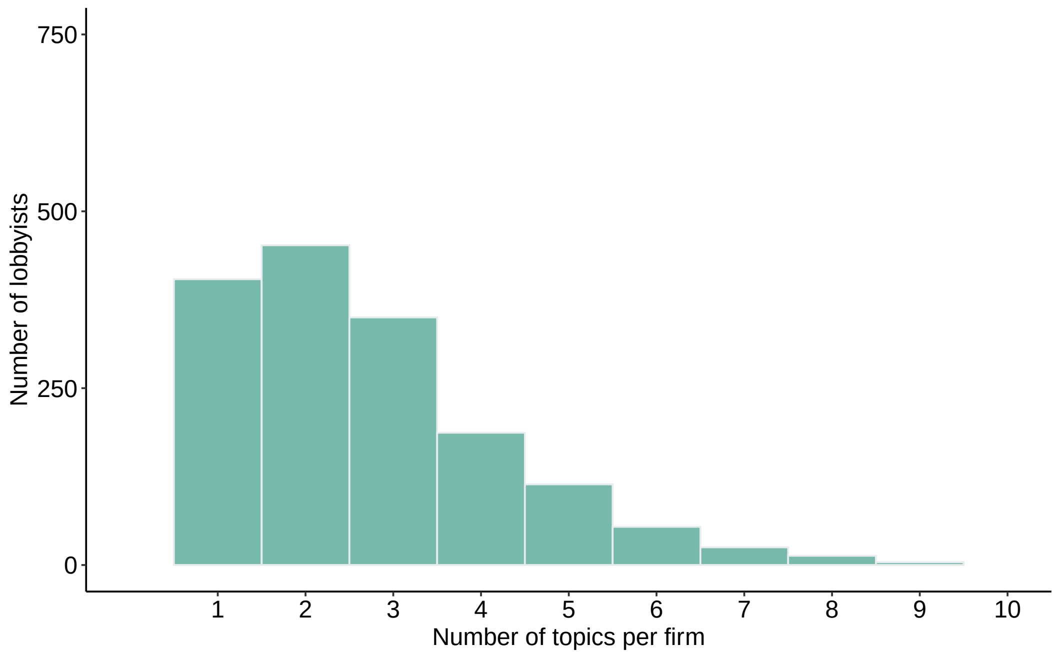


Table B.1: Lobbying topics and foreign country characteristics: Report-level analysis

This table relates lobbying topics extracted from FARA semi-annual reports to foreign country characteristics. The unit of analysis is a country-topic-year triad. The dependent variable is *Log (Number of topics)*, i.e., the natural logarithm of the number of topics. We relate this to the following foreign country characteristics, namely: *Economic* (column 1), *Conflict* (column 2), and *Institutional* (column 3). Column 4 includes all the characteristics. *Economic* characteristics include: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), share of labour compensation in GDP (*Labour share*); *Conflict* characteristics include: total number of unrest events in the source country (*Total unrest (source)*), total number of unrest events in the target country (*Total unrest (target)*). *Institutional* characteristics include the extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *Topic* $\times$ *year* and *Country* fixed effects and are estimated using ordinary least squares (OLS). Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	Log (Number of topics)			
Characteristics	Economic	Conflict	Institutional	All
	(1)	(2)	(3)	(4)
Gross Domestic Product (GDP)	-0.002			-0.011
	(0.026)			(0.027)
Population	0.103			-0.005
	(0.123)			(0.135)
Imports	0.041			0.017
	(0.096)			(0.100)
Exports	0.063			0.083
	(0.125)			(0.124)
Labour share	0.630*			0.792**
	(0.369)			(0.362)
Total unrest (source)		0.024		0.005
		(0.028)		(0.034)
Total unrest (target)		0.019		0.040
		(0.027)		(0.033)
Electoral democracy index			0.160	0.250
			(0.155)	(0.200)
Topic $\times$ year fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
R^2	0.41	0.41	0.40	0.42
Observations	4,412	5,696	6,011	3,887

B.3. Validation

We further probe the validity of the meeting intensity as a measure of connection/interest in the foreign country by examining how it correlates with foreign official trips of the legislator to that foreign country or the frequency with which the legislator publicly mentions the foreign country on Twitter.[69] Results for both the official foreign trips and Twitter activity are in Table B.2.

We study both the extensive and intensive margins of official foreign trips and Twitter activity. Regarding foreign trips, we find that more meetings are significantly related to (a) a larger probability of trips to that foreign country whose representatives the legislator meets more often, and (b) a longer stay in the country. Regarding Twitter, we find that more meetings are also positively associated with (a) a higher probability of a mention of the foreign country in the legislator's tweets and (b) more mentions of the foreign country in the legislator's tweets.

[69]Recent work points to the important role of social media platforms in affecting political outcomes and even financial investors (Fujiwara, Müller, and Schwarz, 2021; Muller and Schwarz, 2021; Enikolopov, Makarin, and Petrova, 2020; Bursztyn, Egorov, Enikolopov, and Petrova, 2019; Acemoglu, Hassan, and Tahoun, 2018; Allcott and Gentzkow, 2017; Bianchi, Cram, and Kung, 2021).

Table B.2: Legislator foreign trips and Twitter mentions of foreign countries around meetings

This table presents panel regressions examining foreign trips and Twitter mentions of foreign countries by legislators around meetings. Panel A captures official trips by legislators to foreign countries around meetings. Official travels are trips undertaken by legislators to perform their official and representational responsibilities, and the trips are paid for by government sources. Panel B focuses on Twitter activity from their official and personal Twitter accounts. We consider all tweets, re-tweets, and quote tweets that mention a foreign country. In panel A of columns 1 and 2, the dependent variable is $\mathbb{1}_{Trip>0}$, defined as an indicator for whether the legislator undertook an official travel to the foreign country with whose representatives they met with. The dependent variable in columns 3 and 4 is *Log (1+# days)*, defined as the natural logarithm of one plus of total number of days of official trips to the foreign country with whose representatives they met with. In panel B, the dependent variable in columns 1 and 2 is, $\mathbb{1}_{Tweet>0}$, defined as an indicator for whether the legislator wrote a tweet mentioning a given foreign country in the same year-month of the meeting. The dependent variable in columns 3 and 4 is, *Log (1+# tweets)*, defined as the natural logarithm of one plus of total number of tweets mentioning a foreign country in the same year-month of the meetings. The independent variable of interest is *Log (1+meetings$_t$)*, the natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators. All regressions include *Country* fixed effects to account for time-invariant country characteristics, and *State* × *year-month* fixed effects to control for regional trends. Additionally, specifications 2 and 4 in both panel include *Legislator* fixed effects to account for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Official foreign trips			
Dependent variable:	$\mathbb{1}_{Trip>0}$		Log (1+# days)	
	(1)	(2)	(3)	(4)
Log (1+Meetings$_t$)	0.002***	0.002***	0.003***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Legislator fixed effects	No	Yes	No	Yes
Country fixed effects	Yes	Yes	Yes	Yes
State × year-month fixed effects	Yes	Yes	Yes	Yes
R^2	0.00	0.01	0.00	0.01
Observations	4,045,632	4,045,632	4,045,632	4,045,632

	Panel B: Twitter activity			
Dependent variable:	$\mathbb{1}_{Tweet>0}$		Log(1+# tweets)	
	(1)	(2)	(3)	(4)
Log (1+Meetings$_t$)	0.004**	0.004**	0.004**	0.003**
	(0.002)	(0.002)	(0.001)	(0.001)
Legislator fixed effects	No	Yes	No	Yes
Country × year fixed effects	Yes	Yes	Yes	Yes
R^2	0.01	0.02	0.01	0.02
Observations	4,045,632	4,045,632	4,045,632	4,045,632

B.4. Additional tables and figures

Table B.3: Top five politicians by number of meetings each year

The table reports the top five politicians by the total number of meetings each year.

Year	1	2	3	4	5
2000	Donald Payne	Trent Lott	Tom Lantos	Norman D Dicks	Bob Graham
2001	Davis Tom	Trent Lott	Dana Rohrabacher	Henry Hyde	John Mccain
2002	Chuck Hagel	Trent Lott	Tom Lantos	Doug Bereuter	Barbara Lee
2003	Mike Simpson	Chuck Hagel	Doug Bereuter	Lincoln Diazbalart	Robert Wexler
2004	Roy Blunt	Ed Whitfield	Robert Wexler	Tom Lantos	Jim Kolbe
2005	Charles E Schumer	Robert Wexler	Betty Mccollum	Tom Lantos	Chuck Hagel
2006	Ed Whitfield	Dan Burton	Robert Wexler	John Mccain	Roy Blunt
2007	Gus M Bilirakis	Robert Wexler	Tom Lantos	Mich Mcconnell	John S Tanner
2008	Howard L Berman	Robert Wexler	John S Tanner	Donald Payne	Bob Filner
2009	Melissa Bean	Michael E Mcmahon	John F Kerry	Robert Wexler	John S Tanner
2010	Howard L Berman	Melissa Bean	Alcee Hastings	Steve Cohen	Lincoln Diazbalart
2011	Daniel K Inouye	Mark Steven Kirk	Mich Mcconnell	Chris Murphy	Roy Blunt
2012	Tom Marino	Jeanne Shaheen	Steve Cohen	Christopher Coons	James M Inhofe
2013	Chris Murphy	Jim Risch	Jeanne Shaheen	Tim Kaine	Karen Bass
2014	Michael R Turner	Tim Kaine	Chris Murphy	Gerald E Connolly	Jim Risch
2015	Tim Kaine	Gregory W Meeks	Mich Mcconnell	Benjamin Cardin	John Boehner
2016	Michael R Turner	Darrell Issa	Gerald E Connolly	Steve Cohen	Christopher Coons
2017	Chris Murphy	Tim Kaine	Cory Booker	Bob Corker	Gerald E Connolly
2018	Michael T Mccaul	Joe Wilson	Jim Risch	Todd C Young	Benjamin Cardin

Table B.4: Legislator characteristics and meetings intensity, Poisson estimation

This table relates meetings between foreign country representatives and US legislators to individual characteristics. The unit of analysis is a country-legislator-year triad. The dependent variable is the natural logarithm of the number of meetings in a year with U.S legislators, *Log (Number of meetings)*. In column 1, we include the following legislator characteristics: whether the legislator is a member of the House of Representatives (*House member*), vote share in the elections (*Vote share*), an indicator capturing party affiliation (*Democrat*), member of the party that is in control of the senate (*Majority*) and rank within the party (*Seniority*). In column 2, we include the following ideological characteristics: Distance from median (ideology), and measures of legislator ideology, *DW-NOMINATE 1* and *DW-NOMINATE 2*. In column 3, we include the characteristics that might affect influence: lawmaking effectiveness of the legislator (*Legislative Effectiveness Score*), whether she is a senate or house committee and sub-committee chair (*Committee (Sub-committee) chair*), a member of rules, ways and means, and appropriations committee (*Power committee membership*). We also capture whether the legislator is a member (ranking member or chair) of, either a senate or house committee, the following committees: (i) the rules, ways and means, and appropriations, (ii) foreign affairs, (iii) Security & Intelligence, (iv) Armed Services, and (v) Energy & Commerce. All specifications include *Country*$\times$*Year* fixed effects and specification 5 includes legislator fixed effects to control for time-invariant legislator characteristics. We use Poisson to estimate the coefficients. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Log (meetings)				
Characteristics	Legislator	Ideology	Importance	All	All
	(1)	(2)	(3)	(4)	(5)
House member	-0.055			0.004	-0.240**
	(0.041)			(0.047)	(0.099)
Vote share (Log)	0.195***			0.198***	0.221***
	(0.041)			(0.048)	(0.082)
Democrat	0.009				
	(0.026)				
Majority	-0.046**			-0.141***	-0.034
	(0.019)			(0.037)	(0.067)
Seniority	0.005*			0.007**	0.015
	(0.003)			(0.003)	(0.009)
Distance from median (ideology)		-0.017		-0.232***	-0.057
		(0.045)		(0.076)	(0.187)
DW-NOMINATE 1		-0.033**		-0.018	0.563
		(0.014)		(0.014)	(0.655)
DW-NOMINATE 2		-0.004		0.006	0.481*
		(0.012)		(0.014)	(0.265)
Legislative Effectiveness Score			0.004	0.004	0.010*
			(0.006)	(0.006)	(0.005)
Committee chair			0.001	-0.012	-0.014
			(0.051)	(0.047)	(0.038)
Sub-committee chair			-0.027*	0.005	-0.029
			(0.015)	(0.024)	(0.026)
Power committee membership			0.083**	0.066**	0.017
			(0.036)	(0.033)	(0.029)
Foreign affairs			0.201***	0.221***	0.204***
			(0.036)	(0.043)	(0.052)
Foreign affairs (chair)			0.035	0.023	0.093
			(0.081)	(0.079)	(0.106)
Foreign affairs (Ranking member)			0.031	-0.090	-0.081
			(0.074)	(0.076)	(0.122)
Security & Intelligence			0.070***	0.074***	-0.006
			(0.027)	(0.025)	(0.031)
Security & Intelligence (chair)			0.083	0.104	0.061
			(0.103)	(0.101)	(0.112)
Security & Intelligence (Ranking member)			-0.046	-0.092	-0.090
			(0.065)	(0.063)	(0.075)

Dependent variable:	Log (meetings)				
Characteristics	Legislator	Ideology	Importance	All	All
	(1)	(2)	(3)	(4)	(5)
Armed services			-0.011	0.006	0.025
			(0.031)	(0.032)	(0.056)
Armed services (chair)			-0.158*	-0.198**	0.003
			(0.093)	(0.090)	(0.097)
Armed services (Ranking member)			0.222***	0.153**	0.063
			(0.065)	(0.075)	(0.091)
Energy & Commerce			-0.030	-0.019	0.001
			(0.034)	(0.034)	(0.067)
Energy & Commerce (chair)			-0.267*	-0.314**	-0.298**
			(0.142)	(0.148)	(0.139)
Energy & Commerce (Ranking member)			-0.303**	-0.378***	-0.358*
			(0.130)	(0.134)	(0.187)
Legislator fixed effects	No	No	No	No	Yes
Country $\times$ year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	36,555	36,555	36,555	36,555	36,525

Table B.5: Meetings with legislators and aid received by foreign countries, year-month analyses

This table presents panel regressions estimating the relationship between meetings and aid received by foreign countries from the US. The unit of analysis is legislator-state-foreign country-year month. The dependent variable in panel A is, $\mathbb{1}_{Aid>0}$, an indicator for receiving a foreign aid from the US while in panel B the dependent variable is, *Log (1+Aid amount)*, natural logarithm of one plus the aid amount received by a foreign country from the US. The independent variable of interest is *Log (1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and U.S. legislators from the respective state. In columns 2, 4, and 6, we include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *State* $\times$ *year-month* fixed effects to control for local economic confounds and general state policies. Specifications 3 and 4 additionally include *Country* fixed effects to control for time-invariant country characteristics while specifications 5 and 6 further include *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country-level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Extensive margin					
Dependent variable:	$\mathbb{1}_{Aid>0}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.663***	0.663***	0.659***	0.658***	0.659***	0.658***
	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)
Controls	No	Yes	No	Yes	No	Yes
State × year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.65	0.65	0.66	0.66	0.66	0.66
Observations	1,841,862	1,841,862	1,841,862	1,841,862	1,841,862	1,841,862

	Panel B: Intensive margin					
Dependent variable:	Log (1+Aid amount)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	9.785***	9.781***	9.726***	9.724***	9.726***	9.724***
	(1.055)	(1.048)	(1.038)	(1.037)	(1.036)	(1.035)
Controls	No	Yes	No	Yes	No	Yes
State × year-month	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.64	0.64	0.65	0.65	0.65	0.65
Observations	1,841,862	1,841,862	1,841,862	1,841,862	1,841,862	1,841,862

Table B.6: Meetings with legislators and local subsidies to foreign firms, district-level analyses

This table presents panel regressions estimating the relationship between meetings and congressional-district level subsidies granted to foreign firms. The unit of analysis is legislator-district-foreign country-year. The dependent variable in panel A is, $\mathbb{1}_{Subsidy>0}$, an indicator equal to one if a foreign firm whose representatives meet with a US legislator, receives a subsidy from the legislator's district while in panel B the dependent variable is, *Log (1+Subsidy amount)*, natural logarithm of one plus the amount of subsidy received by a foreign firm from that district. The independent variable of interest is *Log (1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators from the respective district. In columns 2, 4, and 6, we include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *district* × *year* fixed effects to control for local economic confounds and general district policies. Specifications 3 and 4 additionally include *Country* fixed effects to control for time-invariant country characteristics while specifications 5 and 6 further include *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country-level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Extensive margin					
Dependent variable	$\mathbb{1}_{Subsidy>0}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.004*	0.004*	0.004*	0.004*	0.004*	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls	No	Yes	No	Yes	No	Yes
District × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.05	0.05	0.05	0.05	0.05	0.05
Observations	213,542	213,542	213,542	213,542	213,542	213,542

	Panel B: Intensive margin					
Dependent variable	Log (1+Subsidy amount)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.048*	0.048*	0.050*	0.050*	0.050*	0.050*
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Controls	No	Yes	No	Yes	No	Yes
District × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.05	0.05	0.05	0.05	0.05	0.05
Observations	213,542	213,542	213,542	213,542	213,542	213,542

Table B.7: Meetings with legislators and government contracts to foreign firms

This table presents panel regressions estimating the relationship between meetings with US legislators and new government contract allocated to foreign firms in the state. The unit of analysis is legislator-state-foreign country-year month. The dependent variable in panel A is, $\mathbb{1}_{Contract>0}$, an indicator equal to one if a foreign firm, whose representatives meet with a US legislator, receives a new government contract from the legislator's state while in panel B the dependent variable is, *Log (1+Contract amount)*, natural logarithm of one plus the contract amount received by the foreign firm to perform the contract in that given state. The independent variable of interest is *Log (1+Meetings$_t$)*, natural logarithm of one plus the number of meetings between representatives of a foreign country and US legislators from the respective state. In columns 2, 4, and 6, we include the following country characteristics as control variables: GDP per capita (*Gross Domestic Product*), total population (*Population*), total value of imports (*Imports*), total value of exports (*Exports*), total number of unrest events at source country (*Total unrest (source)*), total number of unrest events at target country (*Total unrest (target)*), and extent to which electoral democracy is achieved (*Electoral democracy index*). All regressions include *State* × *year-month* fixed effects to control for local economic confounds and general state policies. Specifications 3 and 4 additionally include *Country* fixed effects to control for time-invariant country characteristics while specifications 5 and 6 further include *Legislator* fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Extensive margin					
Dependent variable	$\mathbb{1}_{Contract>0}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.009**	0.009**	0.009**	0.009**	0.009**	0.009**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Controls	No	Yes	No	Yes	No	Yes
State × year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.01	0.02	0.02	0.02	0.02	0.02
Observations	3,325,149	3,325,149	3,325,149	3,325,149	3,325,149	3,325,149

	Panel B: Intensive margin					
Dependent variable	Log (1+Contract amount)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Meetings$_t$)	0.088*	0.088*	0.089*	0.089*	0.090*	0.090*
	(0.048)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)
Controls	No	Yes	No	Yes	No	Yes
State × year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Yes	Yes	Yes
Legislator fixed effects	No	No	No	No	Yes	Yes
R^2	0.01	0.01	0.02	0.02	0.02	0.02
Observations	3,325,149	3,325,149	3,325,149	3,325,149	3,325,149	3,325,149

Table B.8: Foreign aid received by foreign countries from the US around legislator deaths

This table examines changes in foreign aid received by the foreign country in one year around legislator deaths in a difference-in-differences setting. The unit of analysis is state-foreign country-year. The dependent variable in column 1 is, $\mathbb{1}_{Aid>0}$, an indicator equal to one if the country received aid from the US while in column 2 the dependent variable is, *Log (1+Aid amount)*, natural logarithm of one plus the amount of aid received by the foreign country. *Lost connection* is an indicator variable taking the value of one if the foreign country connected to a US representative or a senator in a state, respectively, loses the connection due to the legislator's death. All regressions include *State* $\times$ *year* fixed effects to control for local economic confounds such as general state policies and *Country* fixed effects to control for time-invariant country characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are double-clustered at the country-event and state levels and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	$\mathbb{1}_{Aid>0}$	Log (1+ Aid amount)
	(1)	(2)
Lost connection $\times$ After	-0.108***	-0.907***
	(0.007)	(0.131)
Controls	No	No
State $\times$ year fixed effects	Yes	Yes
Country fixed effects	Yes	Yes
R^2	0.76	0.91
Observations	689	683

Table B.9: Local government subsidies to foreign firms around legislator deaths

This table examines changes in state-level subsidies granted to foreign firms, whose country representatives met with a deceased US legislator, in one year around the death in a difference-in-differences setting. The unit of analysis is state-foreign country-year. The dependent variable in column 1 is, $\mathbb{1}_{Subsidy>0}$, an indicator equal to one if the foreign firms, whose country representatives met with the deceased US legislator, receives a subsidy from the legislator's state while in column 2 the dependent variable is, *Log (1+Subsidy amount)*, natural logarithm of one plus the amount of subsidy received by a foreign firm from the legislator's state. *Lost connection* is an indicator variable taking the value of one if the firm whose country representatives are connected to a deceased US representative or a senator in a congressional district or state, respectively, where the subsidy is granted and this connection is lost due to the legislator's death. All regressions include *State* $\times$ *year* fixed effects to control for local economic confounds such as general state policies and *Country* fixed effects to control for time-invariant country characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are dobule-clustered at the country-event and state levels and are robust to heteroscedasticity. They are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	$\mathbb{1}_{Subsidy>0}$	Log (1+ Subsidy amount)
	(1)	(2)
Lost connection $\times$ After	-0.062**	-0.671*
	(0.027)	(0.322)
Controls	No	No
State $\times$ year fixed effects	Yes	Yes
Country fixed effects	Yes	Yes
R^2	0.47	0.44
Observations	689	689

Table B.10: Local subsidies to foreign firms around effective legislator deaths, district-level analyses

This table examines changes in congressional-district level subsidies to foreign firms, whose country representatives met with a deceased US legislator, in one year around deaths in a difference-in-differences setting. The unit of analysis is district-foreign country-year. The dependent variable in column 1 is, $\mathbb{1}_{Subsidy>0}$, an indicator equal to one if the foreign firms, whose country representatives met with the deceased US legislator, received a subsidy from the legislator's district while in column 2 the dependent variable is, *Log (1+Subsidy amount)*, natural logarithm of one plus the amount of subsidy received by a foreign firm from the legislator's district. *Lost connection* is an indicator variable taking the value of one if the firm whose country representatives are connected to a deceased US representative in a congressional district, where the subsidy is granted and this connection is lost due to the legislator's death. All regressions include *district* × *year* fixed effects to control for local economic confounds such as general district policies and *Country* fixed effects to control for time-invariant country characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are dobule-clustered at the country-event and district levels and are robust to heteroscedasticity. They are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	$\mathbb{1}_{Subsidy>0}$	Log (1+ Subsidy amount)
	(1)	(2)
Lost connection × After	-0.049**	-0.786**
	(0.016)	(0.266)
Controls	No	No
District × year fixed effects	Yes	Yes
Country fixed effects	Yes	Yes
R^2	0.52	0.52
Observations	167	167

Table B.11: New government contracts to foreign firms around legislator deaths

This table examines changes in new government contract awards to foreign firms, whose country representatives met with a deceased US legislator, in one year around deaths in a difference-in-differences setting. The unit of analysis is state-foreign country-year. The dependent variable in column 1 is, $\mathbb{1}_{Contract>0}$, an indicator equal to one if the foreign firms, whose country representatives met with the deceased US legislator, received a new government contract from the legislator's state while in column 2 the dependent variable is, *Log (1+Contract amount)*, natural logarithm of one plus the contract value of the government contract received by the foreign firm from the legislator's state. *Lost connection* is an indicator variable taking the value of one if the firm whose country representatives are connected to the deceased US representative or senator in a congressional district or state, respectively, where the government contract is administered and this connection is lost due to the legislator's death. All regressions include *State* × *year* fixed effects to control for local economic confounds and general state policies and *Country* fixed effects to control for time-invariant country characteristics. We use ordinary least squares (OLS) in estimations. Standard errors are dobule-clustered at the country-event and state levels and are robust to heteroscedasticity. They are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	$\mathbb{1}_{Contract>0}$	Log (1+ Contract amount)
	(1)	(2)
Lost connection × After	-0.067***	-0.534**
	(0.020)	(0.221)
Controls	No	No
State × year fixed effects	Yes	Yes
Country fixed effects	Yes	Yes
R^2	0.64	0.69
Observations	689	686

Table B.12: Importance of foreign country population in congressional district for meetings intensity

This table relates meetings with US legislators to individual legislator characteristics. The unit of analysis is a country-legislator-year triad. The dependent variable is the natural logarithm of the number of meetings in a year with US legislators, *Log (meetings)*. In column 1, we include the following legislator characteristics: natural logarithm of vote share in the elections (*Vote share*), member of the party that is in control of the senate (*Majority*), rank within the party (*Seniority*) and the number of lobbyists engaged by the foreign agent who previously worked with the legislators (*Employment connection*). We also include the following ideological characteristics: measures of legislator's political ideology, *DW-NOMINATE 1* and *DW-NOMINATE 2*, and distance from the median ideology in the congress. Lastly, we consider characteristics that are important for influence, i.e., lawmaking effectiveness of the legislator (*Legislative Effectiveness Score*), and whether she is a senate or house committee and sub-committee chair (*Committee (Sub-committee) chair*). We also capture whether the legislator is a member, ranking member, or chair of, either a senate or house committee, the following committees: (i) the rules, ways and means, and appropriations , (ii) foreign affairs, (iii) Security & Intelligence, (iv) Armed Services, and (v) Energy & Commerce. In columns 2 and 3, we consider *Fraction of country's population*, defined as the fraction of the electoral base that were born in the foreign country with whom the representative meets with. All specifications include *Country* $\times$ *Year* fixed effects while specifications 2 and 3 additionally include legislator fixed effects to control for time-invariant legislator characteristics. We use ordinary least squares (OLS) regressions to estimate the coefficients. Standard errors are clustered at the country level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Log (meetings)		
	(1)	(2)	(3)
Fraction of country's population		0.023***	0.022***
		(0.005)	(0.006)
Vote share (Log)	0.280***	0.279***	0.263***
	(0.073)	(0.073)	(0.070)
Majority	-0.096	-0.095	-0.110
	(0.068)	(0.069)	(0.188)
Seniority	-0.003	-0.003	-0.014
	(0.003)	(0.003)	(0.028)
Distance from median (ideology)	-0.148	-0.147	-0.214
	(0.103)	(0.103)	(0.435)
DW-NOMINATE 1	-0.050**	-0.047**	1.820
	(0.022)	(0.022)	(3.323)
DW-NOMINATE 2	-0.022*	-0.022**	0.000
	(0.011)	(0.011)	(.)
Legislative Effectiveness Score	0.012	0.013	0.020**
	(0.009)	(0.009)	(0.009)
Committee chair	0.064	0.055	-0.006
	(0.062)	(0.057)	(0.062)
Sub-committee chair	0.065**	0.067**	0.041
	(0.028)	(0.028)	(0.032)
Power committee membership	0.093***	0.093***	0.058
	(0.032)	(0.033)	(0.041)
Foreign affairs	0.140***	0.140***	0.120*
	(0.052)	(0.052)	(0.062)
Foreign affairs (chair)	-0.052	-0.049	-0.140
	(0.088)	(0.087)	(0.105)
Foreign affairs (Ranking member)	0.271***	0.271***	0.058
	(0.077)	(0.077)	(0.121)
Security & Intelligence	0.037	0.034	0.018
	(0.044)	(0.045)	(0.052)

Dependent variable:	Log (meetings)		
	(1)	(2)	(3)
Security & Intelligence (chair)	-0.210**	-0.218**	-0.065
	(0.097)	(0.100)	(0.087)
Security & Intelligence (Ranking member)	0.073	0.082	-0.093
	(0.106)	(0.107)	(0.118)
Armed services	-0.001	-0.005	0.008
	(0.032)	(0.032)	(0.059)
Armed services (chair)	-0.052	-0.046	0.142
	(0.091)	(0.088)	(0.121)
Armed services (Ranking member)	0.448***	0.451***	0.466***
	(0.161)	(0.161)	(0.171)
Energy & Commerce	0.032	0.032	-0.017
	(0.050)	(0.049)	(0.063)
Energy & Commerce (chair)	-0.405***	-0.397***	-0.416***
	(0.123)	(0.121)	(0.148)
Energy & Commerce (Ranking member)	-0.447**	-0.458**	-0.708***
	(0.206)	(0.208)	(0.240)
Legislator fixed effects	No	No	Yes
Country × year fixed effects	Yes	Yes	Yes
R^2	0.31	0.31	0.43
Observations	8,558	8,558	8,465

APPENDIX C

Supplementary Material for Chapter 3

In this section, we give a detailed account of the methodology described in Section 3.4. We explain the construction of our dataset to allow for replication and explain our discount rate assumptions. We then describe the adjustments we make to reflect life expectancy differences, early/late retirement choices, and benefit adjustments for those who receive survivor benefits, or do not receive benefits at all. Finally, we provide a lengthy discussion of the steps followed to assign simulated Social Security wealth to the top and bottom of the marketable wealth distribution.

C.1. Data Appendix

C.1.1. Survey of Consumer Finances variables

We use the triennial Survey of Consumer Finances for two main purposes: (i) measuring marketable wealth shares, and (ii) estimating aggregate Social Security wealth, and determining the share of Social Security wealth going to the wealthy. The SCF is well suited to measure marketable wealth shares as it covers most asset classes and provides detailed information on households' liabilities. We measure marketable wealth using the net worth variable: the sum of assets net of liabilities. One caveat is that the SCF does not survey extremely wealthy households. To fill this gap, we follow Saez and Zucman (2016) by adding data from the Forbes 400 list to the richest 0.01%.

Raw SCF To study Social Security in the SCF, we collect several variables from the raw SCF data which are listed below. We report the variable name for the second person in the household (typically the spouse) in parentheses.

- `X5306` (`X5311`): Social Security benefit amount. Note that these are reported at different frequencies.
- `X5307` (`X5312`): Social Security benefit frequency. The variable values and their corresponding frequencies are as follows: 4) monthly, 5) quarterly, 6) annually, 12) every two months, -7) other, 0) no benefits.
- `X5304` (`X5309`): Social Security benefit type. This variable takes three values, which represent three benefit categories: 1) retirement, 2) disability, and 3) survivor.
- `X5305` (`X5310`): Number of years receiving Social Security benefits.
- `X19`: Age of second person.
- `X103`: Gender of second person.

From these we create a series of variables. First, we create a payment frequency variable, given by

$$\texttt{pay\_freq} = \begin{cases} 12 & \text{if } \texttt{X5307}\ (\texttt{X5312}) = 4 \\ 4 & \text{if } \texttt{X5307}\ (\texttt{X5312}) = 5 \\ 1 & \text{if } \texttt{X5307}\ (\texttt{X5312}) = 6 \\ 2 & \text{if } \texttt{X5307}\ (\texttt{X5312}) = 12 \\ 0 & \text{otherwise} \end{cases}$$

which allows us to calculate annual benefits, given by

$$\texttt{ssinc} = \begin{cases} \texttt{X5306} * \texttt{pay\_freq} & \text{if Head of Household} \\ \texttt{X5311} * \texttt{pay\_freq} & \text{if Second Person in Household.} \end{cases}$$

We further subdivide this income by benefit type, with retirement income given by

$$\texttt{ssinc\_ret} = \begin{cases} \texttt{ssinc} & \text{if } \texttt{X5304}\ (\texttt{X5309}) = 1 \\ \texttt{ssinc} & \text{if } \texttt{X5304}\ (\texttt{X5309}) = 2\ \&\ \texttt{age}\ (\texttt{X19}) \geq 62 \end{cases}$$

and observed survivor benefits given by

$$\texttt{ssinc\_ben} = \texttt{ssinc} \quad \text{if } \texttt{X5304}\ (\texttt{X5309}) = 3.$$

Note that the second condition for retirement benefits assigns disability benefits going to people of retirement age as retirement benefits, consistent with the SSA. Finally, we calculate the age at retirement, which is given by

$$\texttt{ret\_age} = \begin{cases} \texttt{age} - \texttt{X5305} & \text{if Head of Household} \\ \texttt{X19} - \texttt{X5310} & \text{if Second Person in Household} \end{cases}$$

and is used to calculate full retirement age benefits in Section C.2.2.

Cleaned SCF Extract All wealth variables come from the cleaned SCF extract data. In particular, we use the `networth` variable to calculate the wealth distribution in each survey. This variable includes all assets less debt given in the SCF. We add to this the wealth held by the Forbes 400 as listed in the replication code of Saez and Zucman (2016). The SCF does not survey people beyond a certain wealth threshold, so people in the Forbes 400 are excluded from the sample. To fill this gap, we add aggregate Forbes 400 to the aggregate wealth of the Top 0.01%.

We also calculate a liquid wealth variable which is used to construct Appendix Figure C.6, Panel A. The component pieces of this variable are as follows:

- `liq`: liquid accounts, which is the sum of all checking, savings, and money market accounts, call accounts at brokerages, and prepaid cards.
- `cds`: certificates of deposit.
- `nmmf`: directly held mutual funds.
- `stocks`: wealth held in stocks.
- `bond`: wealth held in bonds of any type excluding savings bonds.
- `retqliq`: quasi-liquid retirement accounts, which are the sum of IRAs, thrift-type accounts, current pensions, and future pensions.
- `savbnd`: savings bonds.
- `homeeq`: home equity, which is the value of the home less the outstanding mortgage principal.

From these, liquid wealth is given by

$$\texttt{liquid\_wealth} = \texttt{liq} + \texttt{cds} + \texttt{nmmf} + \texttt{stocks} + \\ \texttt{bond} + \texttt{retqliq} + \texttt{savbnd} + \texttt{homeeq}.$$

Finally, it is important to note that the Raw SCF values are in nominal terms (e.g. the 1995 Raw SCF is in 1995 dollars) while the Cleaned SCF Extract are in the dollars of the most recent survey year (e.g. 2019 dollars at the time of this writing). The SCF adjusts the Cleaned SCF Extract using the Consumer Price Index for all urban consumers (CPI-U-RS) from the Bureau of Labor Statistics. To make the two datasets consistent, we adjust the Cleaned SCF Extract to nominal dollars.

C.2. Assumptions and adjustments

C.2.1. Market implied vs. SSA yield curve assumptions

Appendix Figure C.3 shows the differences in the yield curve assumptions implied from Treasury notes and the assumptions used by the SSA to compute the present value of Social Security obligations. The SSA discount rates are based on historical business cycles rather than market-implied rates, which is erroneous given the persistence of the current low interest rate environment.[70] An additional piece of evidence of the issues with the SSA's approach comes from the Federal Reserve, which reported in December, 2019 FOMC meeting projections that median long-run nominal rates are expected to be around 2.4-2.8%, with an upper bound of 3.3%, significantly below the 5+% suggested by the SSA.

[70] Summers, Lawrence, "U.S. Economic Prospects: Secular Stagnation, Hysteresis, and the Zero Lower Bound," *Business Economics*, 2014, *49* (2).

C.2.2. Full retirement benefits

To validate the simulation methodology, we compare benefits in the simulated and SCF data. In reality, individuals can choose to retire early or delay retirement, meaning we must adjust their benefits in the data to compare them with benefits implied by the simulation. Beneficiaries retiring before the full retirement age receive reduced benefits, while beneficiaries retiring after the full retirement age receive increased benefits. Therefore, we define individual i's *full retirement benefit* as

$$\text{Full Retirement Benefit}_i = \frac{\text{Benefit}_i}{\text{Adjustment}}$$

where the adjustment term depends on the number of years that the beneficiary retires early or late.

For beneficiaries retiring early, the discount is 5/9% for each month before the full retirement age, up to 36 months, and 5/12% for each additional month. For beneficiaries retiring late, the amount of the credit depends of the beneficiary's birth year and can be found here. Further, the full retirement age is different for each cohort and can be found here. From these data, we create the `full_retirement_age` variable allowing us to determine the number of years of early or late retirement as

$$\texttt{ret\_discount\_years} = \texttt{full\_retirement\_age} - \texttt{ret\_age}.$$

This variable allows us to compute the appropriate benefit adjustment.

Here is an example to help clarify the procedure: Take a 62 year retiring in 2019. This person was born in 1957, meaning that the full retirement age for her cohort is 66 years and 6 months old. For this person, we have $\text{Adjustment} = (1 - \frac{5}{9} \cdot 36 - \frac{5}{12} \cdot 18)$, meaning that the full retirement benefit is given by

$$\text{Full Retirement Benefit}_i = \frac{\text{Benefit}_i}{(1 - \frac{5}{9} \cdot 36 - \frac{5}{12} \cdot 18)}.$$

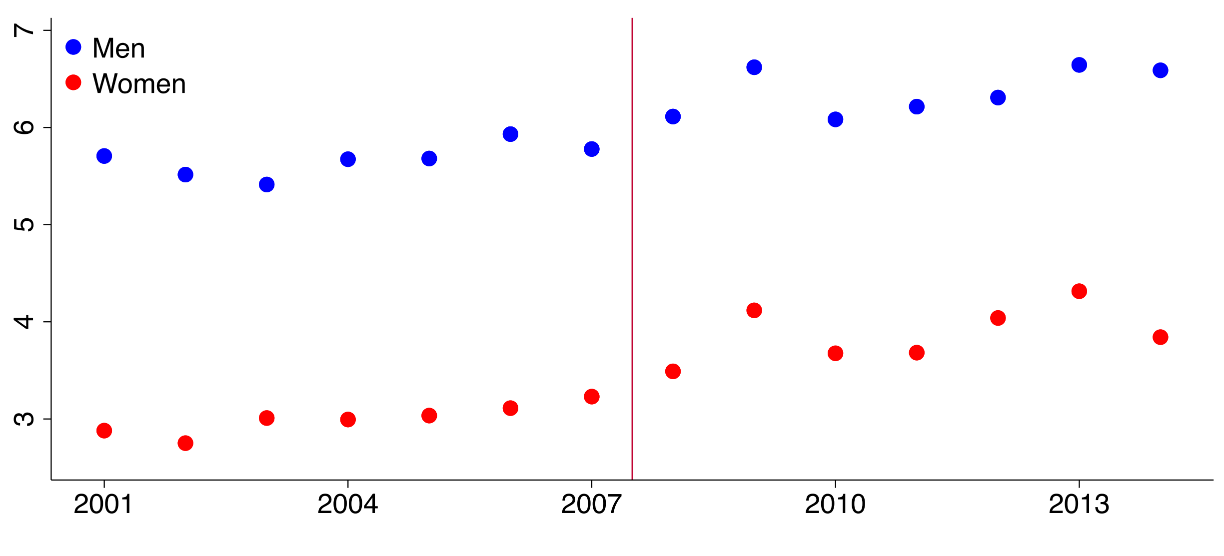
In this case, the observed benefit is adjusted upward to account for the early retirement discount. Conversely, if the individual retires late, her observed benefit will be greater than the calculated full retirement benefit.

C.2.3. Adjusting life expectancy by income

We adjust for differential life expectancy across income centiles using data from Chetty, Friedman, Leth-Peterson, Nielsen, and Olsen (2014) as reported by the Health Inequality Project (HIP). These data provide life expectancy at age 40 for each lifetime income centile from 2001 to 2014. Since our sample starts in 1989 and goes until 2014, we apply the 2001 data for all years between 1989–2001 and the 2014 data for 2014–2019. Assigning the 2001 values to previous years seems to be a reasonable assumption, as the life expectancy differential between high and low income individuals is flat from 2001–2007, then expands after the 2008 Financial Crisis, as shown in Figure C.1.

Figure C.1: Life expectancy differential, 2001–2014

This figure plots the difference in life expectancy for people in the top half and bottom half of the lifetime earnings distribution. The differences for men and women are plotted separately. The vertical line in the middle of the graph denotes the period before and after 2007.



Using these data, we compute the number of years fewer (more) that a retired SCF respondent will live given their lifetime income centile. We then adjust the respondents age to reflect the shorter (longer) longevity implied by the data. To do this, the compute the *life expectancy spread* for each lifetime income centile in the HIP data, which is given by

$$\text{life expectancy spread}_{centile,t} = \frac{\text{life expectancy}_{centile,t}}{\frac{1}{100}\sum_{centile=1}^{100} \text{life expectancy}_{centile,t}}.$$

We then take these life expectancy spreads and merge them with our primary mortality dataset coming from the Human Mortality Database (HMD). We then calculate the number of years fewer (more) people in the lower (higher) centiles of the income distribution live based on the unconditional life expectancy (i.e. at age 0). We define this as the *year difference* which is given by

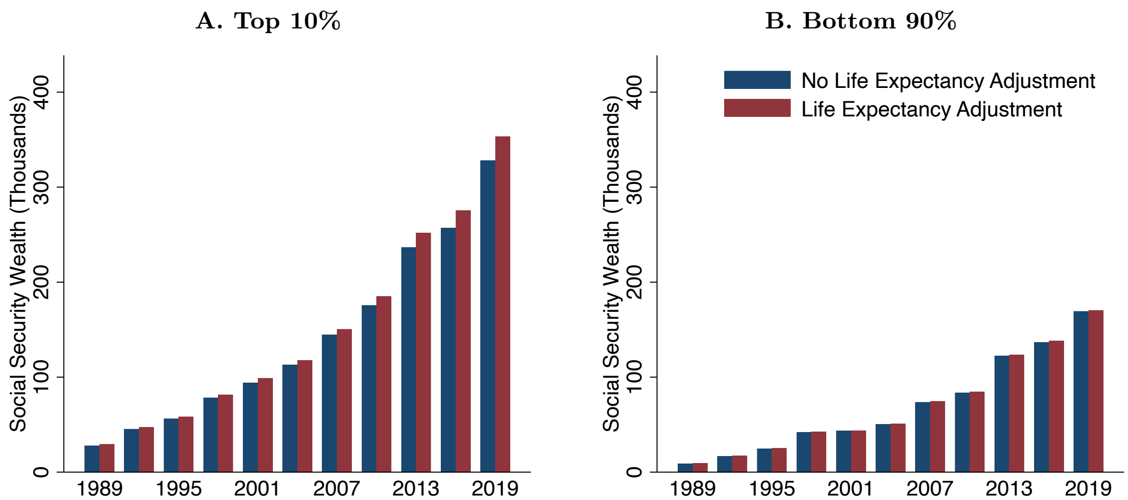
$$\text{year difference}_{centile,t} = (\text{life expectancy spread}_{centile,t} - 1) \times \text{unconditional life expectancy}_t.$$

Note, that this will be negative for people in the bottom half of the lifetime income distribution and positive for people in the top half. From this, we calculate the *effective mortality age* for each SCF respondent, which is given by

$$\text{effective mortality age}_{i,centile,t} = \text{current age}_i - \text{year difference}_{centile,t}.$$

Figure C.2: Adjusting for differential in life expectancy

This figure shows per capita Social Security wealth for each person in the SCF, applying population weights, for people in the top 10% (Panel A) and bottom 90% (Panel B) of the non-Social Security wealth distribution. Life expectancy adjusted values incorporate differential life expectancy across income centiles using data from the Health Inequality Project (HIP), as outlined in Appendix C.2.3.



We then assign survival probabilities to that individual based on their effective mortality age.

Completing the life expectancy adjustment requires a valid proxy for lifetime income. Unfortunately, the SCF does not provide income histories. However, we can extrapolate based on the Social Security retirement benefits centile. Since Social Security benefits are a monotonically increasing function of lifetime income, this proxy allows us to preserve the order of individuals within the lifetime income distribution, which we then apply to the life expectancy adjustment.

An example is illustrative on this procedure: the life expectancy for men in 2019 in the HMD data is 76 years, and in that year, a person in the 1st lifetime income centile lives approximately 9 years less than the average person. Therefore, a 40 year old man in the 1st lifetime income centile has an effective mortality age of 49 years old, and he would be assigned the survival probabilities of a 49 year old man in 2019. We apply this life expectancy correction both to retired workers and to those still in the workforce, whose earnings histories we simulate.

Figure C.2 shows how a life expectancy adjustment impacts Social Security wealth across the Social Security benefits distribution among current retirees. When differences in mortality rates are accounted for, per capital Social Security wealth that accrues to the bottom decile falls by nearly 25%, and per capita Social Security wealth falls for all but the top three deciles. We modify our estimates of cohort Social Security wealth to reflect these differences.

However, this adjustment does not have a large impact on top wealth shares. This is due to an increase in the benefit-weighted average life expectancy of beneficiaries in the bottom 90%. Specifically, those in upper deciles of the marketable wealth distribution live for longer (more years of benefits) than those in lower deciles. Within the bottom 90%, the effect of this adjustment is to decrease benefit-years for individuals with lower benefits, and increase benefit-years for individuals with higher benefits.

As such, adjusting for the relationship between income level and mortality rates increases Social Security wealth for both the top and bottom of the overall wealth distribution. Though the increase in aggregate Social Security wealth goes disproportionately to the wealthy, it remains, nonetheless, much more equally distributed than marketable wealth.[71]

C.2.4. Capitalizing implied survivor benefits

Widows can receive a share of the Social Security benefits of their deceased spouses. We account for this when capitalizing benefits by computing how likely it is that a respondent's spouse is alive given that the respondent is deceased, under the assumption that the survival probabilities of the couple are uncorrelated. In particular, widows can receive the maximum of their benefit and their deceased spouse's benefit. The implied present value of survivor benefits is therefore given by

$$\text{Implied Survivor Benefits}_{i,t} = \max\left\{\text{Spouse Benefits}_{i,t} - \text{Benefits}_{i,t}, 0\right\} \times \sum_{s=0}^{\infty} \frac{\prod_{k=t}^{s-1} m_{i,t+k}(1 - m_{i,t+k}^{spouse})}{1 + r_{t,t+s}}$$

where m represents the survival probability and r the real discount rate.

C.2.5. Proportion of people with no benefits

The vast majority of retirees receive some form of Social Security benefits. However, a fraction of retirees have insufficient work history to receive benefits. When aggregating Social Security benefits, we must take this into account. This requires a reasonable estimation of the proportion of people in each cohort that do not receive benefits.

We estimate this using Deaton-Paxson regressions for each gender, which is a constrained regression of the following form

$$\log(Pr(\text{No Retirement Benefits}))_{t,a,b} = \gamma_t + \eta_a + \delta_b + \varepsilon_{t,a,b} \tag{C.2.1}$$

[71] It is worth noting that this exercise illustrates the issue with a singular focus on top shares as a measure of wealth inequality. Differences in life expectancy disproportionately impact those at the bottom of the wealth distribution, but standard measures of wealth concentration focus on the share of aggregate wealth accruing to those at the top, thus missing out on such dynamics.

subject to

$$\sum_{1989}^{2016} \gamma_t = 0 \tag{C.2.2}$$

$$\sum_{1989}^{2016} \gamma_t (t - 2002.5) = 0 \tag{C.2.3}$$

$$\eta_{72} = 0. \tag{C.2.4}$$

where a represents each age, t each survey year, and b each birth year.[72][73] The coefficients of interest are the birth year fixed effects, where this empirical set-up allows us to adjust for survey specific sampling error and age specific effects. The fitted values by birth year are shown in Appendix Figure C.4, where the average number of zero Social Security income respondents is shown to be 10% for men and 20% for women. In the simulation, these estimates are used to determining average Social Security wealth.

C.3. Assignment and aggregation

After generating the age-year-gender averages from the simulated panel, we merge the simulated data with the SCF. The steps we use to assign Social Security wealth to the top 10% and top 1% of the marketable wealth distribution are as follows:

1. Determine how Social Security wealth is distributed across the marketable wealth distribution among retirees aged 65–75.
   (a) Find the share of full retirement age Social Security wealth accruing to each wealth centile in each survey year.
   (b) Define the share going to each wealth centile w as $\alpha_{w,t} = \frac{SSW_{w,t}}{\sum_{w=1}^{100} SSW_{w,t}}$. Then define $\phi_t(x) = \sum_{w=x}^{100} \alpha_{w,t}$ as the cumulative share of benefits going to people above centile x, where the subscript t denotes different survey years. For 2019, this function is shown in Panel B of Figure 3.5.
2. Determine proportion of the population of each cohort in the top 10% and top 1% of wealth distribution, which we denote by $k^g_{c,t}$, where the subscript c denotes different cohorts, and the superscript g denotes different populations (i.e. in this case either the top 10% or top 1%).
   (a) This means that the top 10% share of population is given by $k^{\text{Top 10\%}}_{c,t} \equiv \frac{N^{\text{Top 10\%}}_{c,t}}{N^{\text{Full}}_{c,t}}$ and the top 1% share by $k^{\text{Top 1\%}}_{c,t} \equiv \frac{N^{\text{Top 1\%}}_{c,t}}{N^{\text{Full}}_{c,t}}$, where $N^g_{c,t}$ is the total size of cohort c in survey t in population g. Mathematically, this is given by $N^g_{c,t} \equiv \sum_i \text{wgt}_i \mathbb{1}_i(c,t,g)$ where wgt_i is the weight in the SCF for observation i and $\mathbb{1}$ is equal to 1 if observation i is in year t, of cohort c, and in population g.

[72] Note that respondents are grouped into three-year age and birth year cohorts in this estimation.

[73] Deaton, Angus S. and Christina Paxson, "Saving, Growth, and Aging in Taiwan," *Studies in the Economics of Aging*, National Bureau of Economic Research, 1994, pp. 331–362.

(b) For example, for respondents aged 40 in 2019, 4.8% are in the top 10% and 1.2% are in the top 1% of the aggregate 2019 wealth distribution.

3. Assign average Social Security wealth by cohort-year-gender from the simulated panel to each person in the SCF. This is denoted by $\overline{\text{SSW}}_{c,t,s}$ where the subscript s denotes each sex.

4. For all cohorts less than 66 years of age, calculate average Social Security wealth from the simulation for the top 1%, the rest of the top 10% and bottom 90%. For people in the top 1%, this is given by

$$\overline{\text{SSW}}_{c,t}^{\text{Top 1\%}} = \frac{\phi_t(k_{c,t}^{\text{Top 1\%}})}{N_{c,t}^{\text{Top 1\%}}} \cdot \text{Cohort Social Security Wealth}_{c,t},$$

for people in the rest of the top 10% by

$$\overline{\text{SSW}}_{c,t}^{\text{Rest of Top 10\%}} = \frac{(\phi_t(k_{c,t}^{\text{Top 10\%}}) - \phi_t(k_{c,t}^{\text{Top 1\%}}))}{(N_{c,t}^{\text{Top 10\%}} - N_{c,t}^{\text{Top 1\%}})} \times \text{Cohort Social Security Wealth}_{c,t},$$

and for people in the bottom 90% by

$$\overline{\text{SSW}}_{c,t}^{\text{Bottom 90\%}} = \frac{1 - \phi_t(k_{c,t}^{\text{Top 10\%}})}{N_{c,t}^{\text{Bottom 90\%}}} \times \text{Cohort Social Security Wealth}_{c,t},$$

where Cohort Social Security Wealth$_{c,t} \equiv \sum_s \left(N_{c,t,s}^{\text{Full}} \times \overline{\text{SSW}}_{c,t,s} \right)$, and $\phi_t(x)$ is the function from Step 1.

5. For respondents less than 62 years of age, nothing else needs to be done. We calculate their aggregate Social Security wealth, which is given by the sum of the SCF weights multiplied by the assigned averages from (4).

6. For respondents aged 62–69, the simulation meets the data, meaning that we have respondents in the data with observed Social Security benefits, as well as respondents not receiving benefits that will receive them in the future. For respondents currently receiving benefits, we calculate the present value of those benefits to determine their Social Security wealth. For respondents not currently receiving benefits, we fill in their benefits using either the average benefits from (4) or a backfilling methodology.

   (a) For respondents aged 62–65, we fill in the average benefits calculated in (4) for all non-recipients.

   (b) For respondents aged 66–69, we fill in the average observed retirement-adjusted Social Security wealth in the data for recipients aged 70–73 from the succeeding survey adjusted for inflation. This, of course, only works for 1989–2016, so for 2019, we fill in the average observed retirement-adjusted Social Security wealth in the data for recipients aged 70–73 from the 2019 survey.

   (c) However, we must adjust these filled benefits downward, as these respondents have a higher probability of being a non-recipient. This is because we assume that 10% of males and 20% of females will not receive retirement benefits (this is verified in the data). For people ineligible for benefits (i.e. less than

62 years old), no additional adjustment must be made. But for people above 62, we must adjust. An example will clarify why. Assume that 50% of men will claim benefits at age 62. This means that 50% of male beneficiaries receive no benefits in that year, and 10% of those men will never receive benefits, meaning that the proportion of people never receiving benefits in that subsample is 20%. In this case, the average benefit will be given by $\left(\frac{0.8}{0.9}\right)\overline{\text{SSW}}^g_{a,t}$ to account for the increased probability of never receiving benefits in the subpopulation. Formally, this adjustment is given by

$$\text{adj}^g_{a,t,s} = \frac{\sum \mathbb{1}\{\text{No Benefits}\} - .1(1 + \mathbb{1}\{\text{Female}\})}{\sum \mathbb{1}\{\text{No Benefits}\}(1 - .1(1 + \mathbb{1}\{\text{Female}\}))}$$

where $\mathbb{1}\{x\}$ is an indicator variable equal to 1 when conditions x are met. This adjustment is calculated for each year-age-sex-population combination.

7. For all cohorts older than 70, we aggregate all values from the data. Nothing needs to be filled in for these observations, as there is no benefit from claiming after 70. In reality, some people may claim later, but we assume that these individuals will not receive benefits for the remainder of their lives.

C.4. Market beta of aggregate labor income

Consider the following exogenous system of stochastic processes

$$dy_t = -\kappa y_t dt + \begin{bmatrix} \sigma_l \\ -\sigma_s \end{bmatrix}^T dz_t, \tag{C.4.1}$$

$$ds_t = \left(\mu - \frac{\sigma^2}{2} + \phi y_t\right) dt + \begin{bmatrix} 0 \\ \sigma \end{bmatrix}^T dz_t, \tag{C.4.2}$$

$$l_{1,t} = y_t + s_t - \delta t, \tag{C.4.3}$$

$$d\pi_t = -r\pi_t dt - \begin{bmatrix} 0 \\ \lambda \end{bmatrix}^T \pi_t dz_t, \tag{C.4.4}$$

where y_t is log output, $s_t \equiv \log S_t$ is log stock price, $l_{1,t} \equiv \log L_{1,t}$ is log wage, π_t is the state-price density, $\lambda \equiv \frac{\mu - r}{\sigma}$, and $z_t = \begin{bmatrix} z_{1,t} & z_{2,t} \end{bmatrix}^T$ is a standard Brownian motion. Note that, for now, we allow the $\sigma \neq \sigma_s$, which is different than in Equation (3.5.2) and gives us a more general solution.

We want to find the beta at time t on a "wage strip", which is a security that pays out $L_{1,t+n}$ at $t+n$ and is denoted by

$$\beta^{L_1,n}_t = \frac{\text{Cov}_t\left(r^m_t dt, r^{L_1,n}_t dt\right)}{\text{Var}_t\left[r^m_t dt\right]}.$$

In this economy, the instantaneous return on the market r_t^m is defined by

$$r_t^m dt = \frac{dS_t}{S_t} = ds_t + \frac{1}{2}(ds_t)^2 = (\mu + \phi y_t)\, dt + \begin{bmatrix} 0 \\ \sigma \end{bmatrix}^T dz_t,$$

and the instantaneous return on the wage strip $r_t^{L_1,n}$ by

$$r_t^{L_1,n} dt = \frac{dP_t^{L_1,n}}{P_t^{L_1,n}},$$

where $P_t^{L_1,n}$ is the price of the wage strip. By no-arbitrage, the price of the wage strip is given by

$$P_t^{L_1,n} = \mathbb{E}_t \left[\frac{\pi_{t+n}}{\pi_t} L_{1,t+n} \right] = \mathbb{E}_t \left[\exp\left\{ \tilde{\pi}_{t+n} - \tilde{\pi}_t + l_{1,t+n} \right\} \right], \tag{C.4.5}$$

where $\tilde{\pi}_t \equiv \log \pi_t$. The process $\tilde{\pi}_t$ is given by

$$d\tilde{\pi}_t = \frac{d\pi_t}{\pi_t} - \frac{1}{2}\left(\frac{d\pi_t}{\pi_t}\right)^2 = \left(-r - \frac{1}{2}\lambda^2\right) dt - \begin{bmatrix} 0 \\ \lambda \end{bmatrix}^T dz_t$$

$$\Rightarrow \tilde{\pi}_t = \left(-r - \frac{1}{2}\lambda^2\right) t - \begin{bmatrix} 0 \\ \lambda \end{bmatrix}^T z_t$$

which comes from a straightforward application of Ito's lemma.

To solve Equation (C.4.5), we are left with finding $l_{1,t+n}$, which is equivalent to solving for y_t and s_t. Using Ito's lemma, we find that

$$y_t = e^{-\kappa t} \left(y_0 + \begin{bmatrix} \sigma_l \\ -\sigma_s \end{bmatrix}^T \int_0^t e^{\kappa s} dz_s \right).$$

Now, to find s_t, we introduce a new variable $\tilde{s}_t$ defined as

$$\tilde{s}_t = s_t + \frac{\phi}{\kappa} y_t,$$

which is given by

$$d\tilde{s}_t = ds_t + \frac{\phi}{\kappa} dy_t = \left(\mu - \frac{\sigma^2}{2}\right) dt + \begin{bmatrix} \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s \end{bmatrix}^T dz_t$$

$$\Rightarrow \tilde{s}_t = \left(\mu - \frac{\sigma^2}{2}\right) t + \begin{bmatrix} \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s \end{bmatrix}^T z_t$$

Using this expression, we solve for s_t, yielding

$$s_t = \tilde{s}_t - \frac{\phi}{\kappa} y_t = \left(\mu - \frac{\sigma^2}{2}\right) t + \begin{bmatrix} \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s \end{bmatrix}^T z_t - \frac{\phi}{\kappa} e^{-\kappa t} \left(y_0 + \begin{bmatrix} \sigma_l \\ -\sigma_s \end{bmatrix}^T \int_0^t e^{\kappa s} dz_s \right)$$

which implies that $l_{1,t}$ equals

$$l_{1,t} = y_t + s_t - \delta t = \left(\mu - \frac{\sigma^2}{2} - \delta\right) t + \begin{bmatrix} \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s \end{bmatrix}^T z_t + \left(1 - \frac{\phi}{\kappa}\right) y_t.$$

Plugging everything back into the exponential expression of Equation (C.4.5), we obtain

$$\begin{aligned}
\tilde{\pi}_{t+n} - \tilde{\pi}_t + l_{1,t+n} &= \left(-r - \frac{1}{2}\lambda^2\right)(t+n) - \begin{bmatrix} 0 \\ \lambda \end{bmatrix}^T z_{t+n} - \left(-r - \frac{1}{2}\lambda^2\right) t + \begin{bmatrix} 0 \\ \lambda \end{bmatrix}^T z_t \\
&+ \left(\mu - \frac{\sigma^2}{2} - \delta\right)(t+n) + \begin{bmatrix} \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s \end{bmatrix}^T z_{t+n} + \left(1 - \frac{\phi}{\kappa}\right) y_{t+n} \\
&= \left(-r - \frac{1}{2}\lambda^2\right) n + \left(\mu - \frac{\sigma^2}{2} - \delta\right)(t+n) + \begin{bmatrix} 0 \\ \lambda \end{bmatrix}^T z_t + \\
&\begin{bmatrix} \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s - \lambda \end{bmatrix}^T z_{t+n} + \left(1 - \frac{\phi}{\kappa}\right) y_{t+n}
\end{aligned}$$

Note that all components inside the exponent in Equation (C.4.5) are normal variables. Hence, we can rewrite the equation as

$$P_t^{L_1,n} = \exp\left\{ \mathbb{E}_t\left[\tilde{\pi}_{t+n} - \tilde{\pi}_t + l_{1,t+n}\right] + \frac{1}{2}\mathrm{Var}_t\left[\tilde{\pi}_{t+n} - \tilde{\pi}_t + l_{1,t+n}\right] \right\}, \tag{C.4.6}$$

which leaves us with finding the two components in the exponent. Also note how we can express y_{t+n} via y_t:

$$\begin{aligned}
y_{t+n} &= e^{-\kappa(t+n)} \left(y_0 + \begin{bmatrix} \sigma_l \\ -\sigma_s \end{bmatrix}^T \int_0^{t+n} e^{\kappa s} dz_s \right) \\
&= e^{-\kappa n} \left(y_t + \begin{bmatrix} \sigma_l \\ -\sigma_s \end{bmatrix}^T \int_t^{t+n} e^{\kappa(s-t)} dz_s \right)
\end{aligned}$$

The first expression, $\mathbb{E}_t\left[\tilde{\pi}_{t+n}-\tilde{\pi}_t+l_{1,t+n}\right]$, is given by

$$\mathbb{E}_t\left[\tilde{\pi}_{t+n}-\tilde{\pi}_t+l_{1,t+n}\right]=\left(\mu-\frac{\sigma^2}{2}-\delta\right)t-\left(\frac{1}{2}\left(\lambda-\sigma\right)^2+\delta\right)n+$$

$$\left(1-\frac{\phi}{\kappa}\right)e^{-\kappa n}y_t+\begin{bmatrix}\frac{\phi}{\kappa}\sigma_l\\ \sigma-\frac{\phi}{\kappa}\sigma_s\end{bmatrix}^T z_t$$

and the second expression, $\mathrm{Var}_t\left[\tilde{\pi}_{t+n}-\tilde{\pi}_t+l_{1,t+n}\right]$, by

$$\begin{aligned}\mathrm{Var}_t\left[\tilde{\pi}_{t+n}-\tilde{\pi}_t+l_{1,t+n}\right]=&\left(\left(\frac{\phi}{\kappa}\sigma_l\right)^2+\left(\sigma-\frac{\phi}{\kappa}\sigma_s-\lambda\right)^2\right)n\\&+\left(1-\frac{\phi}{\kappa}\right)^2\left(\sigma_l^2+\sigma_s^2\right)\frac{1}{2\kappa}\left(1-e^{-2\kappa n}\right)\\&+2\left(1-\frac{\phi}{\kappa}\right)\left(\frac{\phi}{\kappa}\sigma_l^2+\frac{\phi}{\kappa}\sigma_s^2-\sigma\sigma_s+\lambda\sigma_s\right)\frac{1}{\kappa}\left(1-e^{-\kappa n}\right).\end{aligned}$$

From this, we obtain the solution for $P_t^{L_1,n}$,

$$P_t^{L_1,n}=\exp\left\{at+b+cy_t+d^Tz_t\right\},\tag{C.4.7}$$

where

$$\begin{aligned}a&\equiv\mu-\frac{\sigma^2}{2}-\delta\\b(n)&\equiv-\left(\delta-\frac{1}{2}\frac{\phi^2}{\kappa^2}\left(\sigma_l^2+\sigma_s^2\right)+\frac{\phi}{\kappa}\sigma_s\left(\sigma-\lambda\right)\right)n+\left(1-\frac{\phi}{\kappa}\right)^2\left(\sigma_l^2+\sigma_s^2\right)\frac{1}{4\kappa}\left(1-e^{-2\kappa n}\right)\\&\quad+\left(1-\frac{\phi}{\kappa}\right)\left(\frac{\phi}{\kappa}\left(\sigma_l^2+\sigma_s^2\right)-\sigma_s\left(\sigma-\lambda\right)\right)\frac{1}{\kappa}\left(1-e^{-\kappa n}\right)\\c(n)&\equiv\left(1-\frac{\phi}{\kappa}\right)e^{-\kappa n}\\d&=\begin{bmatrix}\frac{\phi}{\kappa}\sigma_l\\ \sigma-\frac{\phi}{\kappa}\sigma_s\end{bmatrix}.\end{aligned}$$

From Equation (C.4.7), we can find the return on the wage strip by differentiating its price. To do that, we can rewrite its price as

$$P_t^{L_1,n}=\exp\left\{P_t^{L_1,n}\right\},$$

where

$$P_t^{L_1,n}=at+b(n)+c(n)y_t+d^Tz_t.$$

By Ito's lemma we have (note that $dn = -dt$)

$$dP_t^{L_1,n} = \left(a - b'(n) - c'(n)y_t - \kappa c(n)y_t\right)dt + \left(c(n)\begin{bmatrix}\sigma_l \\ -\sigma_s\end{bmatrix} + d\right)^T dz_t, \tag{C.4.8}$$

where

$$\begin{aligned} b'(n) &= \frac{1}{2}\left(\sigma_l^2 + \sigma_s^2\right)\left(\frac{\phi}{\kappa} + c\right)^2 - \sigma_s\left(\sigma - \lambda\right)\left(\frac{\phi}{\kappa} + c\right) - \delta \\ c'(n) &= -\kappa\left(1 - \frac{\phi}{\kappa}\right)e^{-\kappa n} = -\kappa c(n). \end{aligned}$$

Then, the return on the wage strip equals

$$\begin{aligned} r_t^{L_1,n}dt = \frac{dP_t^{L_1,n}}{P_t^{L_1,n}} &= dP_t^{L_1,n} + \frac{1}{2}\left(dP_t^{L_1,n}\right)^2 \\ &= \left(a - b'(n) + \frac{1}{2}\left(c\sigma_l + \frac{\phi}{\kappa}\sigma_l\right)^2 + \frac{1}{2}\left(\sigma - \frac{\phi}{\kappa}\sigma_s - c\sigma_s\right)^2\right)dt + \begin{bmatrix} c\sigma_l + \frac{\phi}{\kappa}\sigma_l \\ \sigma - \frac{\phi}{\kappa}\sigma_s - c\sigma_s \end{bmatrix}^T dz_t \end{aligned}$$

meaning that the expected return is

$$\mathbb{E}_t\left[r_t^{L_1,n}\right] = \mu - (\mu - r)\frac{\sigma_s}{\sigma}\left(\frac{\phi}{\kappa} + c\right).$$

This gives the beta on the wage strip as

$$\beta^{L_1,n} = \frac{\mathrm{Cov}_t\left(r_t^m dt, r_t^{L_1,n}dt\right)}{\mathrm{Var}_t\left[r_t^m dt\right]} = 1 - \frac{\sigma_s}{\sigma}\left(\frac{\phi}{\kappa} + c\right)$$

Further, we can test if the CAPM holds in this economy. To do this, we assess if $\mathbb{E}_t\left[r_t^{L_1,n} - r\right] = \beta^{L_1,n}\mathbb{E}_t\left[r_t^m - r\right]$ holds. The RHS of the expression is given by

$$\beta^{L_1,n}\mathbb{E}_t\left[r_t^m - r\right] = \left(1 - \frac{\sigma_s}{\sigma}\left(\frac{\phi}{\kappa} + c\right)\right)\left(\mu - r + \phi y_t\right)$$

and the LHS by

$$\mathbb{E}_t\left[r_t^{L_1,n} - r\right] = \left(1 - \frac{\sigma_s}{\sigma}\left(\frac{\phi}{\kappa} + c\right)\right)(\mu - r).$$

Therefore, the CAPM only holds when y_t is zero in this economy.

Finally, note that if we assume no contemporaneous correlation between the labor and stock market ($\sigma_s = \sigma$), the results reduce to

$$\beta_t^{L_1,n} = \left(1 - \frac{\phi}{\kappa}\right)\left(1 - e^{-\kappa n}\right)$$
$$\mathbb{E}_t\left[r_t^{L_1,n}\right] = \left(1 - \frac{\phi}{\kappa}\right)\left(1 - e^{-\kappa n}\right)(\mu - r) + r$$

while the discount rate remains unchanged as it does not depend on σ_s. So, when $n \to \infty$, the beta converges to $1 - \frac{\phi}{\kappa} = 1 - \frac{0.08}{0.16} = 0.5$.

C.5. Additional figures

Figure C.3: Market Implied and Social Security Administration Yield Curve Estimates

This figure presents the differences between the yield curves implied by treasury markets and those used in SSA reports. The market series is extended by extrapolating the 29-to-30 year forward rate into the future, as described in Section 3.3.

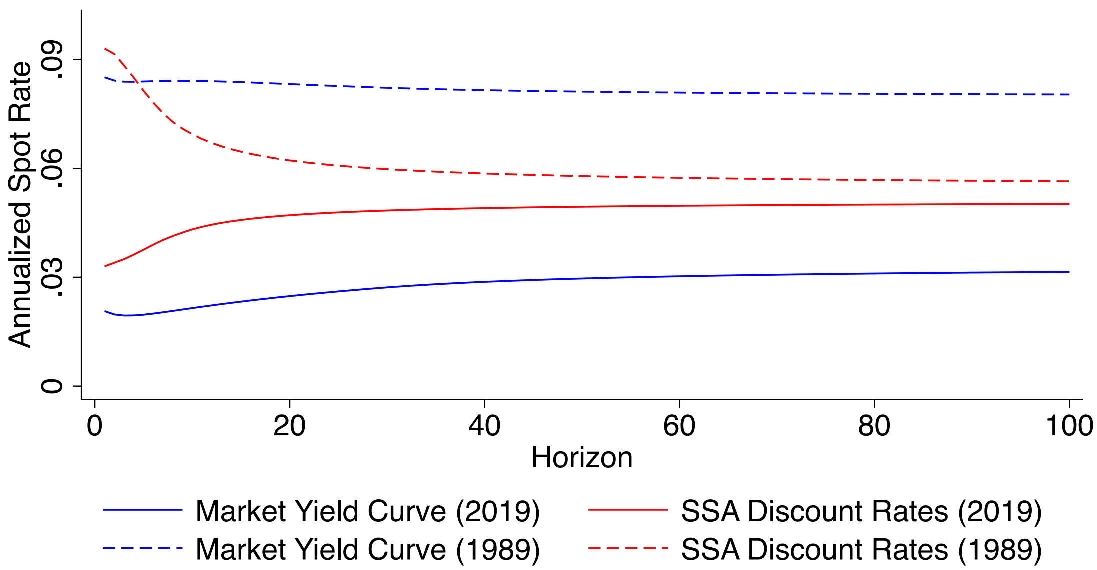


Figure C.4: Zero-Social Security Income Estimates: Deaton-Paxson Regressions

This figure shows the results for the Deaton-Paxson regressions outlined in Appendix C.2.5. The solid lines represent the estimated proportion of male and female respondents not receiving benefits after adjusting for survey-year and age specific fixed effects in a constrained. The dashed lines represent the mean proportion not receiving benefits for the 1929-1953 birth cohorts.

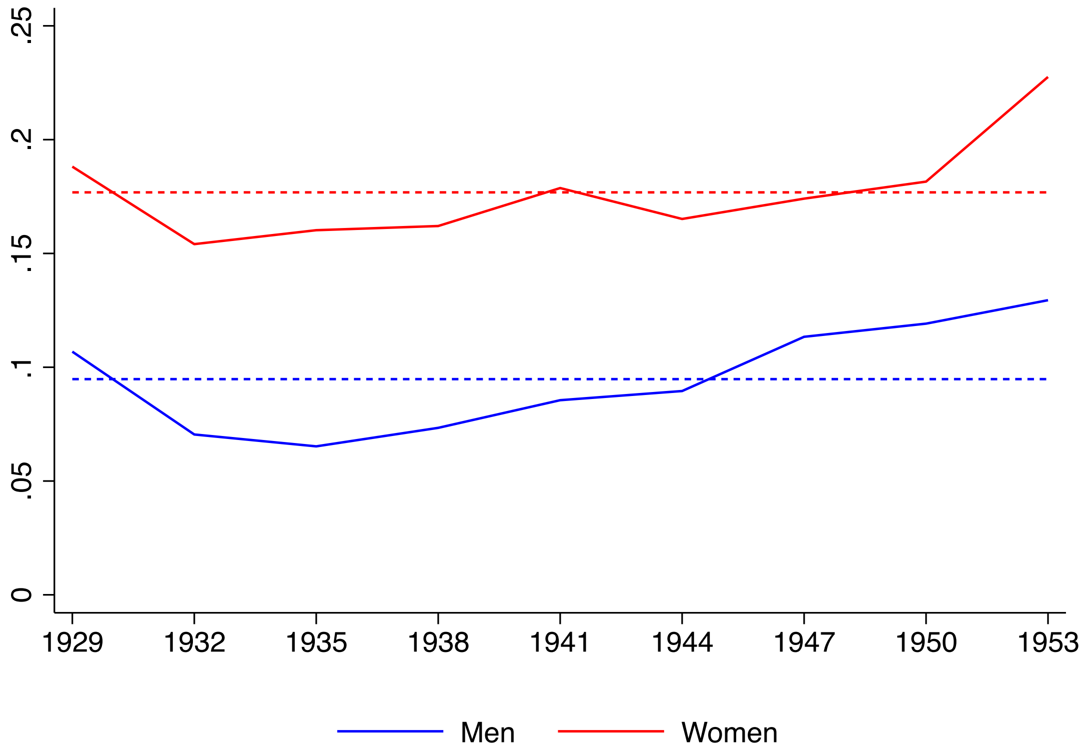
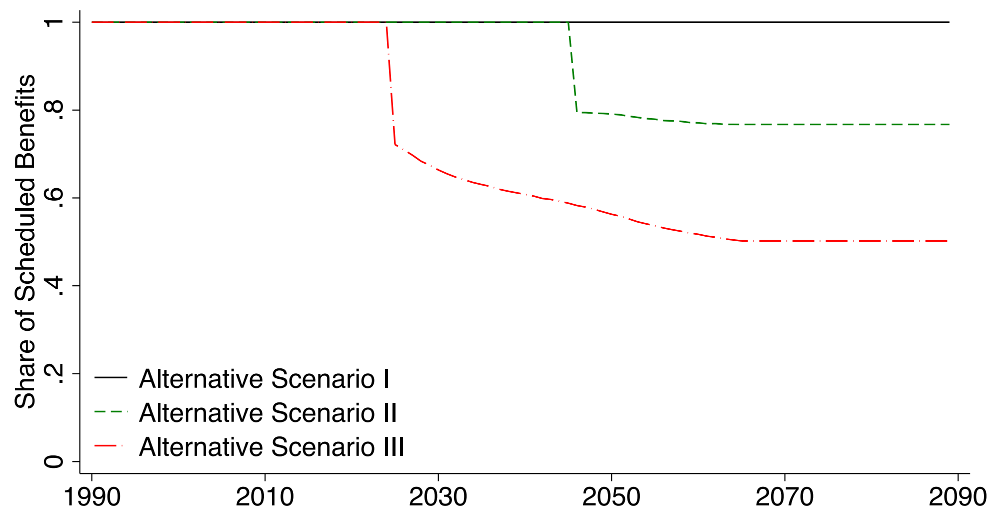


Figure C.5: Funding Gap: Payable Benefits under 1989 and 2019 SSA projections

This figure shows the proportion of payable benefits under the SSA's different funding gap assumptions. Benefits cuts for horizons greater than 75 years are assumed to be the same as the 75th year benefits cuts.

A. 1989



B. 2019

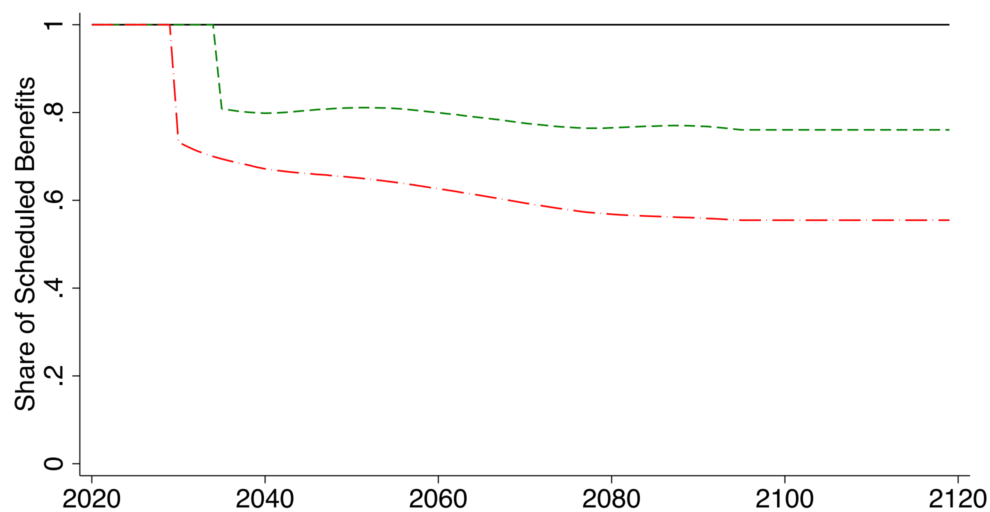


Figure C.6: Accessible and Social Security Wealth over the Lifecycle

Panel A shows the weighted proportion of SCF respondents with more than \$10,000, \$50,000, and \$100,000 of accessible wealth by three-year age group in the 2019 SCF. The measure of accessible wealth we employ sums all wealth from liquid savings, stocks, bonds, mutual funds, quasi-liquid retirement accounts, and home equity and subtracts the total value of all non-mortgage debt. Panel B shows the cumulative share of Social Security wealth by age.

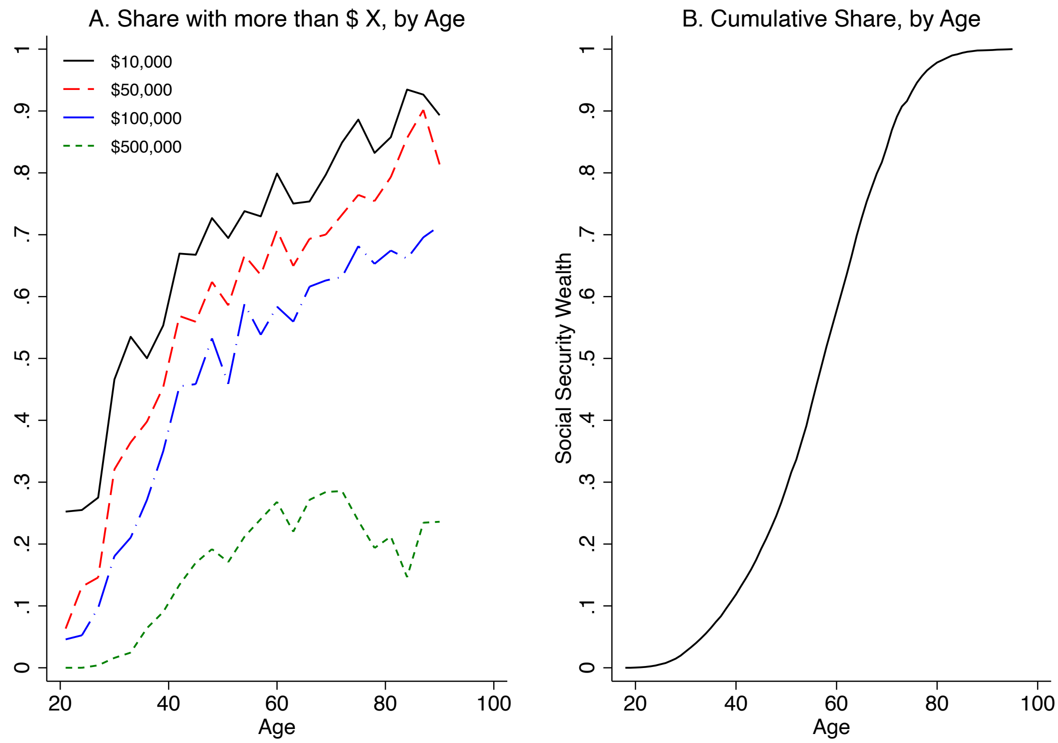


Table C.1: Calibration of labor income process

Parameter estimates for Sections 3.2.1 and 3.4 come from Specifications (5) and (6) in Guvenen et al. (2021). Parameters can be found in Table IV and Table D.3 of the 2021 working paper version.

Parameter	Section 3.2.1 Calibration	Section 3.4 Calibration
ρ	0.991	0.959
p_z	17.6%	40.7%
$\mu_{\eta,1}$	-0.524	-0.085
$\sigma_{\eta,1}$	0.113	0.364
$\sigma_{\eta,2}$	0.046	0.069
$\sigma_{z_1,0}$	0.450	0.714
λ	0.016	0.0001
p_ε	4.4%	13.0%
$\mu_{\varepsilon,1}$	0.134	0.271
$\sigma_{\varepsilon,1}$	0.762	0.285
$\sigma_{\varepsilon,2}$	0.055	0.037
σ_α	0.472	0.300
$\sigma_\beta \cdot 10$	N/A	0.196
$\text{corr}_{\alpha\beta}$	N/A	0.768
$a_\nu \cdot 1$	-2.495	-3.353
$b_\nu \cdot t$	-1.037	-0.859
$c_\nu \cdot z_t$	-5.051	-5.034
$d_\nu \cdot t \cdot z_t$	-1.087	-2.895
$a_{z_1} \cdot 1$	0.176	0.407

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