

Origins of Wealth Equality

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Abstract

In advanced economies, the distribution of wealth became much more equal during the twentieth century. We investigate the origins of this process using newly available data on wealth inequality and wealth equalizing policies. We ask first whether government policy mattered, with the alternative being that wealth inequality depended only on underlying economic trends or exogenous shocks to capital. Difference in differences estimates, supplemented by country time trends, suggest that capital income taxation and inheritance taxation substantially reduced top one percent wealth shares. Compulsory schooling had a similar effect after an expected time lag. Consistent with economic theory, we also find that pretax returns on capital income and their variance had large effects on wealth inequality. We ask next what political conditions produced these policy choices, finding that expansion of the suffrage alone is an insufficient explanation. We argue that the adoption of wealth equalizing policies depended on newly enfranchised voters reaching a consensus on progressive taxation and universal education and that this consensus emerged when religious conflict was limited and when countries participated in wars of mass mobilization.

1 Introduction

Wealth inequality is a subject of tremendous interest, and one that has received increased attention since the publication of Thomas Piketty's *Capital in the Twenty-First Century*. Piketty's work has sparked considerable debate. Even so, careful empirical studies of the determinants of wealth inequality are still relatively few and far between, particularly at the cross country level where the political factors driving wealth inequality can be expected to operate.¹ There are propositions about the effect of capital income and inheritance taxes on wealth inequality, but these have not been systematically tested. This is important because theory suggests that behavioral responses to increases in top tax rates might offset their impact on wealth inequality. In fact, a number of economists have suggested that heavier taxes on the rich do not reduce wealth inequality.² Others suggest that shocks to pre-tax capital income have driven wealth inequality, as policy has played a secondary role.³ Uncertainty also exists about the forces that allow those who aren't already rich to begin accumulating wealth, thus reducing inequality. This could occur as a result of underlying economic trends, perhaps determined by technology, or it could instead depend on government policies, and in particular education provision. In this paper we will use newly available data to examine the effect of education provision and taxation on wealth inequality. We will then investigate the political origins of these policies.

The data we will use on wealth inequality have recently been compiled by Roine and Waldenstrom (2015) drawing on their own work on Sweden and on individual country series produced by other authors using estate or inheritance tax records. We will combine this with data on income and inheritance tax rates collected for Scheve and Stasavage (2016) as well as data on the introduction of compulsory education. The most extensive coverage in the Roine-Waldenstrom wealth inequality data is for the fraction of a society's total wealth held by the top one percent of individuals or households, and so we will restrict our analysis to

¹See the extensive review provided by Roine and Waldenstrom (2015).

²See Becker and Tomes (1979) for a prominent example. Clark (2014) suggests that taxes on the rich have had little effect on social mobility.

³This would be one reading of the claims in Piketty (2014).

this measure. Any use of this data for cross-country empirical analysis should come with a clear caution; wealth can be defined and valued in different countries in different ways, and in a number of cases Roine and Waldenstrom (2015) were obliged to use data from more than one source to provide a longer time series. As we will discuss below, our estimation strategy can control for some, but certainly not all of the variation induced by these factors. In the end, however, confidence in our results is increased by the fact that we find clear correlations between wealth inequality and both the policies and the pretax returns on capital that economic theory predicts should matter.

Our estimation strategy for the determinants of wealth inequality is based on a generalized difference in differences design where each unit of observation is a country during a five year period and all specifications include country fixed effects and time period fixed effects. We also include country time trends in all specifications as a further control for unobserved factors that might bias our estimates. We use lagged values of our key policy variables and a lagged dependent variable in the specification. This is appropriate because changes in tax or education policy will only impact wealth inequality gradually. Since economic models suggest that the level of wealth inequality will depend on market rates of return on capital, we include a proxy for this by using the yield on ten year government bonds net of inflation. Recent work by Benhabib, Bisin, and Zhu (2011) suggests further that wealth inequality will also be increasing in capital income risk, so we also include the within period standard deviation of this measure. Since the return on capital and capital income risk might be considered to be “post-treatment” in any investigation of the effect of tax and education policies, we also repeat our estimates while excluding these two variables. Finally, we also estimated all of our wealth inequality specifications while including the real rate of per capita economic growth to test the argument made by Piketty (2014) regarding $r-g$. The variable for income growth was never statistically significant, and so we excluded it from our main analyses.

Our findings on taxation suggest that higher taxes on wealth have had a clear effect on reducing wealth inequality, contrary to theoretical models, such as that of Becker and Tomes (1979), in which behavioral responses by households will offset this effect. We use the

top marginal income tax rate as a proxy for taxes on capital income and the top marginal inheritance rate as a proxy for wealth transfer taxes. The estimated long run effect of a one standard deviation increase in the top marginal income tax rate (28 percentage points) is to reduce the top one percent wealth share by seven percentage points, or roughly half a standard deviation. The estimated long run effect of a one standard deviation increase in the rate of inheritance taxation (21 percentage points) is to reduce the top one percent wealth share by five percentage points.

Our findings on education suggest that the introduction of compulsory mass education was associated with a substantial equalization of wealth over the long run. We use a variable that takes a value of one when a country has adopted compulsory education. This is based on the assumption that this change was associated with increased state support for education and curriculum improvements. Clearly, introducing compulsory education in year t would not impact wealth inequality for some time. The cohort that experienced the change would need to complete its education, enter the labor market, and then reach the age at which it would accumulate wealth. In economic models of wealth accumulation and transition, it is sometimes assumed that a generation lasts thirty years. Based on this rule of thumb, in our regression specifications we include our compulsory education dummy variable lagged by six five-year time periods. In our preferred specification we find that the long run effect of the introduction of compulsory education is to reduce the top one percent wealth share by eleven percentage points. Importantly, we see no evidence of a negative correlation between compulsory education and wealth inequality when using short lag lengths of only one, two, or three periods for our education variable. This increases confidence that our estimates using the sixth lag actually reflect the impact of education and not other changes that covaried with education.

If taxes on wealth and compulsory mass education helped reduce wealth inequality, then the next question is whether these two policies had a political origin. The first explanation that comes to mind when asking this question involves the extension of the suffrage (Acemoglu Robinson 2006, 2000; Boix 2003). The idea that democracy pushes top tax rates

upwards was also suggested by early twentieth century scholars including Seligman (1911) and Shultz (1926). When it comes to mass education, the work of Peter Lindert (2004) has been prominent in arguing that the expansion of the suffrage led to increased state provision of mass education. We investigate the determinants of tax and education policy in regression specifications including country and period fixed effects as well as country time trends. Across a range of different measures for suffrage extensions, competitive elections, or related institutional change we fail to find any evidence that democratization drove top tax rates, a result we also report in Scheve and Stasavage (2016). Nor did we find any evidence that suffrage extensions or related changes led to the adoption of compulsory education. We also found no evidence for a reduced form relationship between wealth inequality and democratization. It seems unlikely that our null result for the suffrage is produced by a form of reverse causality whereby governments facing pressures to democratize choose to redistribute instead. For taxes in particular across our sample substantial taxation of the rich never preceded democratization.

Our null result for suffrage extensions does not mean that democratization was irrelevant for wealth inequality. There is sufficient case study evidence to show that the expansion of the suffrage was accompanied by increased debate about income and inheritance taxation as well as education provision.⁴ We then explore two features of the political environment, beyond suffrage, that influenced wealth equalizing policy. The first involved internal conflict over religion, which helped shape education policy. The second involved external involvement in wars of mass mobilization which influenced opinions about tax policy.

The first reason that democratization may fail to produce wealth equalizing policies is if voters are divided over issues other than the distribution of wealth. In an early contribution, Bendix and Rokkan (1962) argued that the adoption of compulsory education was delayed when there were disagreements over educational content, and in particular whether education would be religious or secular, and if religious then which particular creed. Our empirical analysis follows the strategy that these two authors proposed. We use the presence of an

⁴See Scheve and Stasavage (2016, chapter six) for debates about the income tax.

official state religion to proxy for a state of the world in which the church-state cleavage for political conflict is not salient. We also distinguish between countries with a Protestant state religion and those with a Catholic state religion. This is based on the idea that even when there is a state religion, in Catholic countries there may still be tension about the appropriate balance between secular and state authority. Protestant churches never claimed the temporal authority that Catholic churches once did. Results of cross-sectional OLS estimates strongly support the prediction regarding religious conflict, though we should acknowledge that these are tests based on a small sample without a robust identification strategy. A country having any sort of state religion is estimated to adopt compulsory mass education 29 years earlier than one without. A country with a Protestant state religion is estimated to adopt compulsory education 38 years before one that lacks a state religion or which has Catholicism as its state religion. These results also fit with what we know from case study evidence.

The second reason democratization may not produce wealth equalizing policy is if newly enfranchised voters think it is fair that the rich are taxed no more heavily than everyone else. In forthcoming work (Scheve and Stasavage 2016) we suggest that support for taxing the rich will be greatest when it is believed that those with wealth have gotten to where they are because of unequal treatment by the state. Over the last two centuries the most prominent instance during which resentment of this type developed was during mass mobilization for war. The fact that labor could be conscripted while capital could sit home created new fairness based arguments for taxing wealth. In this paper we will provide macro evidence that mass mobilization led to higher rates of taxation for the sample of countries for which we have established a relationship between tax rates and wealth inequality. We will also show that the impact of mass mobilization for war on tax policy was greatest in democracies.

What light do our results shed on the movement towards greater wealth equality over the course of the twentieth century and about future developments? The twentieth century shift towards lower wealth inequality was not simply the result of some underlying economic trend, nor was it only the result of shocks to pretax returns on capital. Across our eleven countries government policy appears to provide a significant part of the explanation for why wealth

inequality began to decline during the early decades of the twentieth century. This period coincided with a steep increase in top marginal tax rates in many countries. It also coincided with the entry in the labor market of new cohorts who were better educated than their predecessors. In an environment today where mass warfare appears to be a thing of the past, taxes on the rich are likely to play less of a role in restraining wealth inequality than was the case a century ago. Rather than taxing the wealth of the top one percent, politically feasible wealth equalizing policies in the future may instead depend more on education provision that helps those who don't start off in the top one percent.

In the remainder of this paper in Section two we will first review theories of wealth inequality, starting with what economic theory has to say about the factors that drive disparities in wealth and then continuing by considering how politics may drive these factors. This will be followed in Section three by our wealth inequality estimates where we will examine the impact of wealth taxation and compulsory education. Following this we will turn to asking which factors drove wealth taxation and education. In Section four we consider the impact of suffrage expansions and related institutional change. In Section five we then consider how internal religious conflict and external conflict in an era of mass mobilization helped shape political conflict over wealth equalizing policy.

2 Theories of Wealth Inequality

Theoretical work by economists has pointed to a number of different determinants of wealth inequality. Broadly, any factor that influences rates of return on capital or which influences the ability to earn, save, and pass on wealth to future generations can influence equilibrium levels of wealth inequality. We will focus on two such factors dependent on government policy. The first of these is taxation of capital income and taxation of inheritance. The second is public provision of education. We will then consider the potential political determinants of policy in these two areas

2.1 Economic Determinants of Wealth Inequality

At first glance, the idea that taxation of capital income and of inheritance will help equalize wealth seems sufficiently straightforward that we might not even need to discuss it. These taxes, particularly if they are progressive, should reduce the wealth held by the top one percent of wealth holders. However, the conclusion turns out to not be as obvious as it sounds. In an early contribution Becker and Tomes (1979) showed how, under certain assumptions, households faced with increased inheritance taxes would alter their savings and consumption behavior, offsetting the wealth equalizing effect of these taxes. It is also possible that changes in tax policy will affect certain economy wide parameters, again offsetting the wealth equalizing effect (see Jones 2015 for discussion). Most of the models used to consider this issue assume an economy in which rates of return on capital are certain. Benhabib, Bisin, and Zhu (2011) show that once one assumes, more realistically, that rates of return on capital are stochastic, then taxes on capital incomes and inheritance clearly reduce wealth inequality, even when one considers any offsetting effects due to household behavior or other endogenous changes in the economy. The intuition for why capital income risk matters is that it magnifies wealth inequality. In a world where returns on investments are stochastic, some wealth holders turn out *ex post* to have made good investments whereas others turn out to have made bad ones. In this environment, taxation (and especially progressive taxation) of capital incomes and inheritance will reduce the average rate of return on capital while also dampening the effect of this stochastic component. The idea that progressive taxation of capital income and inheritance influences wealth inequality has also of course been emphasized by Thomas Piketty (2014) in his work and in work with Emmanuel Saez (Piketty and Saez, 2013).

The second idea we will pursue is that state provision of mass education will reduce wealth inequality by increasing the wealth of those who start off in the bottom ninety-nine percent. This could be because membership in the top one percent and the bottom ninety-nine remains static, but those in the bottom nine have new opportunities to accumulate wealth. In their early work on the British wealth distribution Atkinson and Harrison (1978)

demonstrated that part of the reduction in wealth inequality during the twentieth century was attributable to the expansion of “popular wealth”, and Roine and Waldenstrom (2015) have recently also emphasized this idea. Mass education can also produce social mobility if membership in the top one percent becomes less static from one generation to the next.⁵ There are several ways in which the effect of public education provision on wealth inequality could be modeled. In Galor and Zeira (1993) individuals incur a fixed cost, and there will be greater take up for education if the government subsidizes this cost. In Saint Paul and Verdier (1993) individuals may either pay for private education or receive a level of public education that is free of charge. Though both of these papers consider income, rather than wealth inequality, it is not hard to see how public provision of education might also have an effect on wealth inequality over time. If individuals with higher labor income are able to save more, then public education provision that generates high labor income for more people will eventually result in reduced wealth inequality. It is also possible that there could be a more direct route to wealth equality, if public provision of education increases the stock of people who have the entrepreneurial talents necessary to earn a higher rate of return on a capital investment. The key word in both of these scenarios is “eventually.” It will take some time before increased provision of education for children today results in more talented individuals entering the labor market, beginning to earn incomes high enough that they can save, and then accumulating wealth.

2.2 Political Origins of Wealth Equalizing Policies

If we turn to the political determinants of wealth equalizing policies, the most obvious one to consider is expansion of the suffrage. During the nineteenth and early twentieth centuries

⁵In his discussion of $r-g$ and why economic growth (in particular growth that requires new skills from each generation), should reduce wealth inequality, Thomas Piketty (2014 p.85) suggests “growth can thus increase social mobility for individuals whose parents did not belong to the elite of the previous generation.” In their critique of Piketty’s $r-g$ framework Acemoglu and Robinson (2015, 2014) also emphasize the role of social mobility. They consider a world of “workers” and “capitalists” where some people earn only capital income and have a high savings rate and others earn only labor income and have a low savings rate, a setting that can generate wealth inequality. In this setting, any mechanism that results in a worker transitioning to becoming a capitalist or vice versa will lower wealth inequality. Finally, Benhabib, Bisin, and Zhu (2015, 2011) also consider the effect of social mobility in a more general setting in which mobility means lower correlation in earnings processes for capital and labor income from one generation to the next.

European and other countries progressively reduced income and education requirements for the suffrage, and as they did so new political parties emerged seeking to mobilize working class voters. It is plausible that this movement had a major impact on tax and education policy. In a world where capital income is concentrated within one part of the population, with high inequality within this group itself, then we might expect majorities to choose high top marginal rates on capital income and inheritance, and this should reduce wealth inequality. The same can be said of publicly provided mass education. In the model of Saint-Paul and Verdier (1993) relatively poorer people logically prefer increased spending on public education. Therefore if voting rights are expanded to include more people with low incomes then public spending on education will increase and wealth inequality will (eventually) decrease. This prediction about education fits well with some, but not all, existing discussions of the subject.⁶

Even if the expansion of the suffrage is the first place to look when thinking about political origins of wealth equality, histories of party competition in Europe also suggest two reasons why an expanding suffrage may not suffice to produce wealth equalizing policies. The first reason is that there may be internal political conflict over issues other than economic distribution. The second reason is that attitudes may depend on beliefs about whether a given policy is fair.

Consider first the case of internal conflict over other policies. For Lipset and Rokkan (1967) European political parties often developed along the “worker-owner” cleavage, but other cleavages still mattered. They identify three other salient dimensions of politics: attitudes on the relationship between church and state, on relations between center and periphery, and on land vs. industry. Existing political economy work has shown how the presence of additional dimensions of policy may result in less redistribution by the state than

⁶Mariscal and Sokoloff (2000) attribute early differences in school enrollment rates between North America and Latin America to the fact that near universal (white) manhood suffrage was attained at a much earlier date in countries like the United States. Kaestle (1983) makes the same argument for the United States in contrast with Europe, and Lindert (2004) has also been influential in making this same claim. However, looking at a broader set of educational expansion episodes, Aghion, Persson, and Rouzet (2012) fail to find any evidence that democratization had this sort of a causal effect.

would otherwise have been the case.⁷ In nineteenth and early twentieth century European societies (and in their offshoots) the adoption of state funded mass education depended on first solving the problem of whether it should be secular or religious, and if it was to be religious then which religion. In countries with active conflict over this question, then there was a potential for the provision of mass education to be delayed. Bendix and Rokkan (1962) suggested that the religious question in mass education was solved the earliest in countries with a Protestant state religion, such as those in Scandinavia. In those cases there was little question about what religion education would be associated with and there was also little conflict between church and state over authority. This paved the way for the introduction of publicly supported mass education at an early date. The introduction of this policy was delayed in countries, such as the Netherlands, where there were multiple religious groups, or where there was a strong conflict between secular and state authority, as in France. In our empirical tests to follow we will examine how internal conflict over religion influenced the timing of mass education adoption and therefore patterns in wealth inequality.

Consider next beliefs about whether a policy is fair. Classic observers of conflict, such as R.H. Tawney (1938) and T.H. Marshall (1938), emphasized that mobilization depends not only on economic position but also on the degree of resentment that those nearer the bottom feel with respect to those at the top. In modern models of redistributive politics this same idea is often investigated by considering how individuals form beliefs about the role of luck vs. effort in economic success, and how this influences their preferences for redistribution.⁸ In work elsewhere (Scheve and Stasavage, 2016) we have argued that the resentment of which Tawney and Marshall spoke will be strongest when there is a sense not just that the wealthy are lucky, but that they have achieved their positions because of unequal state policies. We have then shown that over the last two centuries the most prominent instance in which this has occurred has involved external conflict. Mobilization for World War I produced a new argument on the left that if labor was being conscripted, then capital should be conscripted

⁷See Roemer (1998) for an important example.

⁸Piketty (1995), Alesina and Angeletos (2005), Bénabou and Tirole (2006), Durante, Putterman, and van de Weele (2014)

as well. Access to this argument goes a long way towards explaining how parties of the Left, such as the UK Labour Party, suddenly found that they could command more support. In our empirical tests to follow we will provide evidence of this effect of external conflict on tax policy.

2.3 A First Look At the Data

Can the wealth equalizing mechanisms outlined above help account for long run trends in wealth inequality? Several important research projects have recently produced new data or compiled data that can be useful for answering this question. Roine and Waldenstrom (2015) review the progress made in measuring historical wealth inequality and compile data for ten countries. We use this data along with wealth inequality measures for Ireland produced by Turner (2010).⁹ The measures of wealth inequality in this research are based on top wealth shares and we focus our attention on the proportion of wealth owned by the top one percent. As Roine and Waldenstrom emphasize, there are important differences across countries in the methodologies used for constructing top wealth shares. Perhaps most importantly, the data from some countries is primarily based on wealth tax data and the wealth holding unit is the household whereas for other countries the data is primarily from estate tax information and the wealth holding unit is the individual. These and other differences mean that comparisons across countries need to be made with great caution and that our primary emphasis will be to describe changes over time within countries. Finally, we will focus most of our analysis on changes in wealth inequality during the twentieth century. For some but not all of the countries in our sample, there are a useful number of wealth measures for the nineteenth century but even for those countries, the data are quite sparse.

Figure 1 presents the variable *Top 1% Wealth Share* for the eleven countries in our study since the beginning of the twentieth century. The striking feature of this data is that wealth concentration in almost all countries was high in the beginning of the twentieth century and

⁹Roine and Waldenstrom (2015) present wealth inequality measures for Australia, Denmark, Finland, France, The Netherlands, Norway, Sweden, Switzerland, the United Kingdom, and the United States. See their detailed discussion for original sources and methodology. It merits noting that their compilation does not include a new wealth inequality series for the United States compiled by Saez and Zucman (2014).

declined substantially until the 1970s and 1980s. If one were asked to describe the main feature of Figure 1 it would be of a common secular trend towards greater wealth equality. It is possible that there was some general process, perhaps strictly economic, or perhaps economic choices driven by politics. Look closer though and we can see that initial levels of inequality varied between countries as did the timing and pace of any decline in inequality. In what follows we will use this variation to identify whether tax and education policies reduced inequality and what the political determinants of these policies were. We should also note that it is unsurprising that we see little increased wealth inequality in recent years despite recent increases in top incomes shares. This is because wealth inequality is a more persistent process than income inequality.

3 Investigating Wealth Equalizing Policies

Our next step is to investigate empirically whether tax and education policies have influenced wealth inequality, as measured by *Top 1% Wealth Share*, over the course of the twentieth century.

3.1 Data and Econometric Model

Our baseline economic model of wealth inequality requires measures of the return on capital and the riskiness of these returns, which we will proxy by their variance. We constructed the variable r equal to the nominal yield on government debt minus the inflation rate.¹⁰ The yield and price data is from the GFDatabase version 2.0.¹¹ We linearly imputed missing data for this series. To measure the variance of capital returns, we calculated the standard deviation of the variable r over five-year periods constructing the variable $var(r)$. Our theoretical framework emphasizes the importance of tax and education policy in determining the long

¹⁰This follows the approach used by Acemoglu and Robinson (2015, 2014). There is no doubt that this is a very imperfect proxy for the true return on capital. So, for example, our measure would be a poor proxy in the current period of extremely low government bond yields. However, using it we will recover plausible estimates of the effect of r on wealth inequality.

¹¹Downloaded from <https://www.globalfinancialdata.com/Databases/GFDatabase.html>, accessed July 2015.

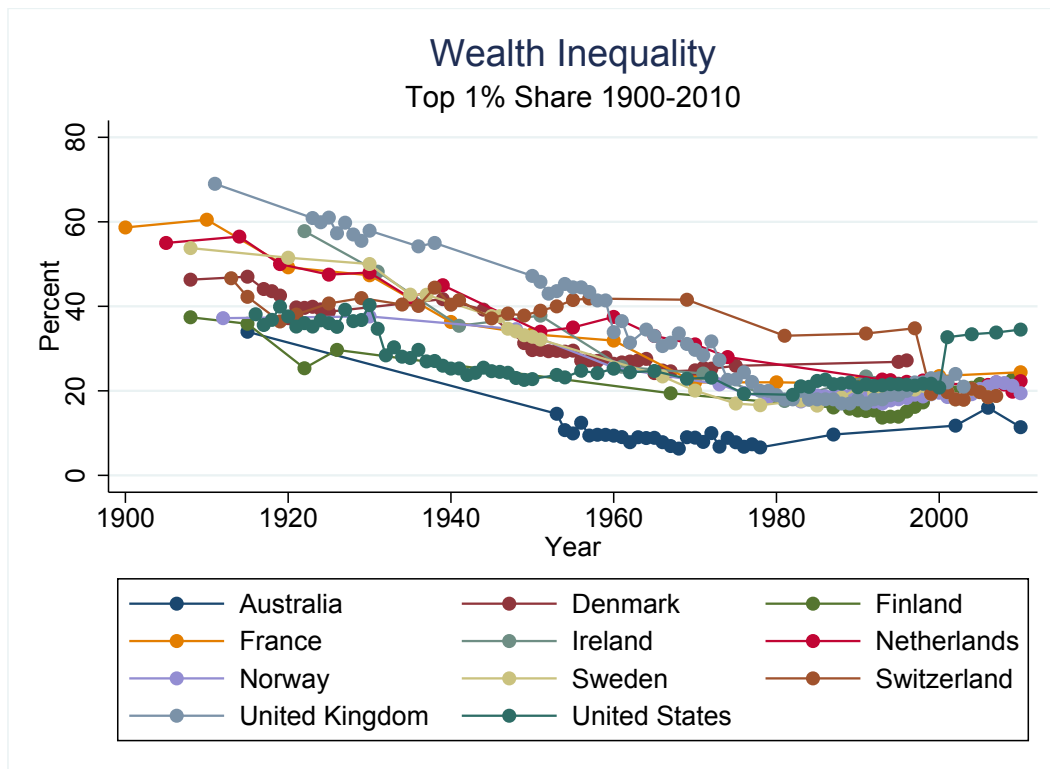


Figure 1: *Wealth Inequality, Top 1% Share, 1900-2010.*

run development of wealth inequality. Although countries employ a variety of taxes on capital, inheritance and income taxation are the most common and important policy instruments. Even countries that employ wealth taxes often do so with rules that limit the combined wealth and income taxes that a given taxpayer may owe. We use the top statutory rates of inheritance and income taxation from Plagge, Scheve, and Stasavage (2011) and Genovese, Scheve, and Stasavage (2015) to construct the variables *Top Inheritance Tax Rate* and *Top Income Tax Rate*. In some of our analyses, we are interested in the linear combination of these rates and add the rates together to construct the variable *Sum of Top Tax Rates*. Finally, our theoretical discussion emphasized how state provision of mass education can influence the wealth distribution. The variable *Compulsory Education* is equal to one if a country has passed compulsory schooling laws for primary school aged children and zero if it has not.¹² We use the presence of compulsory schooling rather than school enrollments in our estimation because enrollments are influenced by both government decisions and household choices regarding education. This better isolates the effect of government policy.¹³

The econometric model of wealth inequality that we estimate is:

$$W_{it} = \alpha + \rho W_{it-1} + \beta_1 r_{it} + \beta_2 var(r)_{it} + \beta_3 P_{it-1} + \eta_i + \theta_t + \gamma_i T_i + \varepsilon_{it} \quad (1)$$

where i indexes each country and t indexes the five-year time period; W is the wealth inequality measure *Top 1% Wealth Share* with missing values imputed by linear interpolation; r is the return on capital; $var(r)$ is the within period within country standard deviation in

¹²The source for this data is Bandiera et. al (2015) unless indicated otherwise, and Bandiera et al used Flora (1983) as their core source. Australia is coded 1 throughout its independence in 1901. The actual date for adoption is 1880 with the Public Instruction Act of 1880. Denmark is coded 1 from 1814. Finland is coded 1 throughout its independence with an adoption date of 1866. France is coded 1 from 1882. Ireland is coded 1 throughout independence, adopting compulsory laws in 1892. Netherlands is coded 1 from 1900. Norway is coded 1 from 1827. Sweden is coded 1 from 1842. Switzerland is coded 1 from 1860. Switzerland is coded based on the discussion in Flora (1983) and indicates when a majority of cantons required compulsory education. United Kingdom is coded 1 from 1880. The United States coding is determined by when a majority of states had passed compulsory education laws and is coded 1 from 1891.

¹³As should be the case, in a generalized difference in differences setting we observe that our compulsory schooling variable is a very strong predictor of primary school enrollments based on enrollments data compiled from Mitchell's *International Historical Statistics*. At the micro level compulsory schooling laws have been used as an instrument for school attendance (Acemoglu and Angrist, 2000). However, at the country level the exclusion restriction for this same strategy would be much less plausible. Therefore one can think of our direct inclusion of our compulsory schooling variable in the regression as providing a reduced form relationship.

r ; P is one of or a combination of our policy variables; T is a time trend; α , ρ , and β are parameters to be estimated; η_i are country fixed effects parameters also to be estimated; θ_t are period fixed effects parameters; γ_i are country-specific time trends, and ϵ_{it} is the error term.

The inclusion of a lagged dependent variable in this model is necessary because top wealth shares can be expected to adjust only gradually in response to the policy changes we investigate. The country fixed effects allow us to focus on within country variation over time. This is especially desirable in this setting because of the differences in measurement and methodology in the construction of the *Top 1% Wealth Share*. The fixed effects also control for time constant factors that may be correlated with both our policy measures and wealth inequality. While it is the case that the presence of a lagged dependent variable and country fixed effects can generate bias, that bias is decreasing in the length of the time series and we have long time series, making this problem a minimal concern.¹⁴ The period fixed effects are included to control for common shocks, such as changes in the global economy, technology, and political events. The country-specific time trends are necessary because wealth inequality trended down during the twentieth century and any trending variable will be appear to be correlated with it. This problem is exacerbated by the fact that we have linearly interpolated the *Top 1% Wealth Share* variable. Although our data are annual, we do not expect our policy variables to have an immediate impact on wealth inequality and do not have strong expectations about the exact time it will take for these policies to have an effect. Consequently, we conduct our analysis using five-year averages. Even with these five-year periods, it is clear that compulsory education should operate with a substantial lag on wealth inequality. For this policy variable, a lag of six periods is used with the idea that during this thirty year period, citizens would have received higher levels of primary education, started to earn higher salaries, and have had sufficient time to accumulate some personal wealth. We present the ordinary least squares estimates of this model and report robust standard errors.

Our analysis yields informative estimates of the partial correlations between our tax and

¹⁴See the Monte Carlo results in Judson and Owen (1999).

education policy measures and the top one percent wealth share. The estimates have a causal interpretation under the usual assumptions of a generalized difference in differences design. Both reverse causality and time-varying unobservables correlated with our policy measures and wealth inequality are concerns. We cannot eliminate the possibility that countries decide to raise taxes or implement compulsory schooling laws in response to high wealth inequality. However, Scheve and Stasavage (2016) show that wealth inequality does not “Granger cause” high inheritance taxes or high income taxes, so this is probably not a source of bias in our estimates. Also, if there was a bias of this sort it would actually be a positive bias that would lead us to underestimate the effect of taxation on top wealth shares. Another example of an omitted variable that would bias our policy estimates is if countries implemented other policies that induced wealth equality at the same time as the tax and education policies that we measure. Although this possibility is one that must be kept in mind in interpreting our estimates, it is not clear what these alternative policies would be and in the case of compulsory education why they would have the same extended lag structure in their effect on wealth inequality.

3.2 Estimation Results

Table 1 reports our main estimates. Column 1 reports estimates of our baseline economic model of wealth inequality. The coefficient estimate for r is 0.094 with a standard error of 0.051 and p-value of 0.068. Substantively, this implies that a standard deviation change in r has a long-run impact on the *Top 1% Wealth Share* of 10.9 percentage points.¹⁵ The coefficient estimate for $var(r)$ is also positive, 0.097, with a standard error of 0.039 and p-value of 0.013. The implied long run effect of a standard deviation change of this measure on the *Top 1% Wealth Share* is 9.0 percentage points. These estimates are both consistent with the key features of the Benhabib, Bisin, and Zhu (2011) framework. It should be emphasized that although capital return and its variance are taken as given, they are themselves in part determined by government actions. It is particularly evident in the data that inflation is a

¹⁵All long-run estimates discussed in the paper are calculated by dividing the coefficient of interest by one minus the coefficient on the lag dependent variable.

major driver of variation in the variables r and $var(r)$.

Column 2 reports results for a specification adding *Top Inheritance Tax Rate* to the baseline model. The coefficient estimate for *Top Inheritance Tax Rate* is -0.035 with a standard error of 0.015. A standard deviation increase in the top rate of inheritance taxation is associated with a long run decline in the *Top 1% Wealth Share* of 5.3 percentage points. This is a large and substantively important policy effect on wealth inequality. Column 3 reports an equivalent specification with *Top Income Tax Rate* added to the baseline model. The coefficient estimate implies that a standard deviation increase in the top rate of income taxation leads to a decline in the *Top 1% Wealth Share* of 7.0 percentage points which is again a substantial effect. Column 4 reports our estimate for *Compulsory Education*. The coefficient is negative, large in magnitude, and relatively precisely estimated. It suggests that a transition to compulsory schooling laws ultimately decreases the *Top 1% Wealth Share* by 19 percentage points.

The results that we have discussed so far do not control for both sets of policies. Column 5 does this and uses the variable *Sum of Top Tax Rates* to measure the overall magnitude of taxes levied on high incomes and wealth.¹⁶ The coefficient estimate on *Sum of Top Tax Rates* is negative and relatively precisely estimated. Although the magnitude of the coefficient appears smaller than in our specifications with inheritance and income top rates entered separately, because the standard deviation of the sum is larger, the actual implied long run effect of a standard deviation change is quite similar to that reported without controlling for compulsory education (7.2 percentage points). The coefficient estimate for *Compulsory Education* is somewhat smaller with a relatively larger standard error but is still significant at the 0.10 level. Column 6 reports estimates that do not control for r and $var(r)$ because each of these variables could be affected by the top rates of inheritance and income taxation. This could cause post-treatment bias in our estimates for the policy variables. However,

¹⁶Simply taking the sum of the two top rates is somewhat ad hoc but has a theoretical foundation. An income tax applies on capital income each year, but income earned each year will be a small portion of total wealth. An inheritance or estate tax applies only once per lifetime, but it applies to all wealth. In any given year then if the marginal rates of these two taxes were identical, their expected incidence might be of similar magnitude.

the coefficients reported in column 6 are almost identical to those reported in column 5. In column 6 the estimated long run effect of introducing compulsory education is a decrease in the top one percent wealth share by about 11 percentage points. The magnitude of this effect when we also control for taxation is more plausible than the 19 percentage point decrease implied in column 4.

We examined the robustness of our estimates in a number of ways. First, the top wealth share data are available for some countries in the nineteenth century though there is a great deal of missing data. We reestimate all the specifications reported in Table 1 for the period from 1800 to 2010 (see Appendix Table A-1). The results are remarkably stable for all the variables of interest. The one difference of note is that the coefficient estimates for *Compulsory Education* are somewhat smaller in magnitude but more precisely estimated. The greater precision is intuitive because there is more variation in the lagged values of compulsory education with the added years, and the magnitude of the estimate is still quite substantial. Second, it is possible that wealth inequality is systematically related to levels of income and that by omitting this time varying factor, our estimates are biased. We re-estimated the models in Table 1 adding a control for real GDP per capita (see Appendix Table A-2). Although our measure of real GDP per capita is positively correlated with *Top 1% Wealth Share*, its inclusion does not significantly change any of our coefficient estimates. Third, Piketty (2014) has argued that in addition to capital returns, economic growth influences the concentration of wealth. Appendix Table A-2 reports results adding this measure to our main models. The coefficient estimate on per capita growth is negative as predicted but is not statistically significant and has no impact on our key results.¹⁷

Taken together our results provide some of the first evidence about the long run determinants of wealth inequality. Importantly, they suggest that the variance of returns to capital, in addition to mean returns, is central for understanding the conditions under which wealth inequality will be high. Our findings also clearly suggest a role for policy choices made by governments. Tax and education policies appear to have played a significant role in the

¹⁷Including a variable equal to $r-g$ yields estimates very similar to just using the capital return measure. Taken together, there is not evidence that adding growth to these specifications improves the fit of our models.

OLS Estimates						
5-Year Averages of <i>Top 1% Wealth Share</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Top 1% Wealth Share</i> _{t-1}	0.878 (0.052)	0.855 (0.051)	0.862 (0.048)	0.891 (0.052)	0.868 (0.049)	0.864 (0.048)
<i>r</i> _t	0.000	0.000	0.000	0.000	0.000	0.000
<i>var(r)</i> _t	0.094 (0.051)	0.084 (0.049)	0.089 (0.050)	0.070 (0.047)	0.066 (0.046)	
	0.068	0.087	0.077	0.140	0.156	
	0.097 (0.039)	0.087 (0.038)	0.076 (0.040)	0.097 (0.038)	0.082 (0.039)	
<i>Top Inheritance Tax Rate</i> _{t-1}		0.013 -0.035 (0.015)	0.058	0.012	0.036	
<i>Top Income Tax Rate</i> _{t-1}			-0.036 (0.016)			
			0.022			
<i>Sum of Top Tax Rates</i> _{t-1}					-0.020 (0.009)	-0.021 (0.009)
					0.023	0.015
<i>Compulsory Education</i> _{t-6}				-2.108 (0.863)	-1.545 (0.836)	-1.522 (0.847)
				0.016	0.067	0.074
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
S.E.R.	1.897	1.865	1.858	1.869	1.837	1.842
Observations	221	220	220	214	213	213

Table 1: The Policy Correlates of Wealth Inequality, 1900-2010. The table reports the results of OLS regressions of the variable *Top 1% Wealth Share* on its one period lag, *r*, *var(r)*, and various policy measures. All specifications include country fixed effects, period fixed effects, country-specific time trends, and report the coefficient estimates, robust standard errors in parentheses, and p-values.

substantial decline in wealth inequality over the first eight decades of the twentieth century.

4 Policy Impact of Suffrage Expansions

The previous section provided evidence that tax and education policy may cause changes in wealth inequality. If so this raises the question of why countries chose these policies and why they chose them at different times. In this section, we evaluate the role of democracy and conclude that there is very little evidence that changes to political institutions alone drove major innovations in tax and education policy.

To evaluate the democracy hypothesis, we constructed the variable *Suffrage %* equal to the percent of adult males eligible to vote in national elections.¹⁸ Although this is our primary measure for examining the role of democracy, we also employed other measures of democratic institutions, such as competitive elections, universal male suffrage, and the existence of an upper chamber that is not directly elected with veto powers over legislation.¹⁹

We employ two main empirical strategies for estimating the effect of suffrage extensions on inheritance and income taxation and compulsory education. Our first model employs the

¹⁸The source of this variable is Flora (1983) for the European cases, the *Statistical History of the American Electorate* for the US, and Mackie and Rose (1974) for Australia.

¹⁹*Competitive Elections*, is set equal to one if the legislature is elected in free multi-party elections, if the executive is directly or indirectly elected in popular elections and is responsible either directly to voters or to a legislature elected according to the first condition, and finally if at least 50 percent of adult males have the right to vote. This definition and data is from Boix, Miller, and Rosato (2013). The definition is a modification of the definition used by Przeworski et al. (2000) to a context where the suffrage may be restricted. *Competitive Elections* is coded one for the following years: Australia 1901-2010; Denmark 1901-2010; Finland 1917-2010; France 1848-1851, 1870-1939, 1946-2010; Ireland 1922-2010; Netherlands 1897-2010; Norway 1905-2010; Sweden 1911-2010; Switzerland 1848-2010; UK 1885-2010; United States 1800-2010. The variable *Universal Male Suffrage* is set equal to one if a country has reached universal male suffrage and zero otherwise. The sources for this variable are the same as for *Suffrage %*. *Universal Male Suffrage* is coded one for the following years: Australia 1902-2010; Denmark 1918-2010; Finland 1917-2010; France 1848-2010; Ireland 1922-2010; Netherlands 1918-2010; Norway 1905-2010; Sweden 1911-2010; Switzerland 1848-2010; UK 1918-2010; United States 1965-2010. The variable *No Upper* is equal to one if any of the follow three conditions are satisfied and zero otherwise: (1) there is no upper house (2) there is an upper house that cannot veto legislation (3) there is an upper house in which members are directly elected through universal male suffrage. Our coding for this variable is based primarily on Marriot (1910, 1926) and on historical information contained on the websites of the respective upper chambers. For Denmark, we also consulted Danish Parliament (2009) "The Parliamentary System of Denmark." The coding for this variable is as follows: Australia 1 for entire period, Denmark 1 from 1915, Finland 1 for all years, France 0 from 1815-1847 then 1 from 1848-1851 then 0 from 1852-1945 then 1 from 1946 onwards, Ireland 1 for all years, Netherlands 0 for all years, Norway 1 for all years, Sweden 1 from 1918, Switzerland 1 from 1848, United Kingdom 1 from 1911, United States 1 from 1913.

following generalized difference-in-differences framework:

$$P_{it} = \alpha + \beta_1 S_{it-1} + \eta_i + \theta_t + \gamma_i T_i + \varepsilon_{it} \quad (2)$$

where i indexes each country and t indexes the time period; P is one of the three policy variables; S is the extent of the suffrage or one of our other measures of democracy; T is a time trend; α and β are parameters to be estimated; η_i are country fixed effects parameters also to be estimated; θ_t are period fixed effects parameters; γ_i are country-specific time trends, and ε_{it} is the error term. We present the ordinary least squares estimates of this model and report robust standard errors clustered by country to account for within-country correlations, including serial autocorrelation, in our data.

As in our analysis of wealth inequality, our estimates measure the causal effect of suffrage expansion on these policies under the assumptions of the generalized difference in differences framework. For example, our estimates of β_1 would be inconsistent if there are time-varying unobserved factors that influence policy outcomes and are correlated with suffrage expansions. That said, most of the plausible unobservables based on the existing literature would suggest a positive correlation between suffrage expansion and the error term—that is factors that would lead countries both to expand the franchise and to increase taxes and adopt compulsory education. Such a correlation would suggest that our estimates, if inconsistent, are biased in a positive direction meaning we actually overestimate the effect of democracy on the policy variables. It is not, however, plausible to treat our estimates solely as an upper bound of the effect of suffrage expansion on these outcomes. Specifically, there is the possibility of reverse causality in which a country under a nondemocratic form of government adopts higher taxes or compulsory education to avoid having to democratize (see e.g. Acemoglu and Robinson 2006). Such a relationship would bias our estimates in a negative direction, leading us to underestimate the positive effect of democracy on wealth equalizing policies. There is little evidence that this form of bias applies to our tax results. Countries generally did not adopt high marginal rates of income and inheritance taxation until well after the achievement of universal male suffrage, and none did so prior to expanding the suffrage. This bias could

conceivably apply to our null results for suffrage expansions and the adoption of compulsory education because this is a policy change that in a number of cases happened well before the achievement of universal suffrage. Yet there is little indication in existing historical work that education reforms of this sort were taken in order to postpone the need to expand the suffrage.

Although we have annual data from 1800 (or independence) to 2010, we do not know a priori how long it may take for democratization to influence policy choice and we again focus on analyzing five-year averages. In most of our specifications, we consider data from 1800 forward because the key adoptions for compulsory education are in the nineteenth century. We report some analyses for tax policy employing twentieth century data only to match our wealth inequality analyses in the previous section.

Our second econometric model takes the following form:

$$P_{it} = \alpha + \rho P_{it-1} + \beta_1 S_{it-1} + \eta_i + \theta_t + \gamma_i T_i + \varepsilon_{it} \quad (3)$$

This model adds a lagged dependent variable to our initial approach.²⁰ This addition controls for lagged but not contemporaneous time-varying unobservables. Otherwise, the same issues discussed for the first model are potential sources of bias for this second specification.

Table 2 reports the results of our main evaluation of the impact of suffrage extension on our policy variables. Across all specifications the coefficient on *Suffrage %* is small, inconsistently signed, and statistically insignificant. There is no evidence of a relationship between an expanding franchise and tax and education policy outcomes.

Although by the early twentieth century all countries in our sample had compulsory education, one might wonder if the relationship between suffrage expansion and tax policy might be more evident if we focused on the twentieth century alone. Table A-4 in the Appendix reports estimates repeating our specifications in Table 2 but limiting our analysis

²⁰This model includes both a lagged dependent variable and country fixed effects which can generate bias in short time series. Our time series are fairly long and so this is not a significant issue in this application. Our estimates for the coefficient on *Suffrage %*_{*t*-1} in specifications with a lagged dependent variable are qualitatively similar dropping the country fixed effects, both with and without country-specific trends.

OLS Estimates					
5-Year Averages					
<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>
(1)	(2)	(3)	(4)	(5)	(6)
<i>Top Inheritance Tax Rate_{t-1}</i>			0.891 (0.041)		
			0.000	0.786 (0.057)	
<i>Top Income Tax Rate_{t-1}</i>				0.000	
<i>Compulsory Education_{t-1}</i>					0.786 (0.075)
					0.000
<i>Suffrage %_{t-1}</i>	0.031 (0.063)	-0.040 (0.087)	0.001 (0.003)	-0.016 (0.021)	0.001 (0.001)
	0.637	0.659	0.813	0.446	0.370
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes
S.E.R.	9.268	9.833	0.193	5.033	0.113
Observations	365	366	366	361	366

Table 2: Suffrage, Tax, and Education Policymaking, 1800-2010. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate*, *Top Income Tax Rate*, and *Compulsory Education* on *Suffrage %* in columns (1-3) and on each dependent variable's lag and *Suffrage %* in columns (4-6). All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-3)), and p-values.

to the twentieth century. These results are quite similar and do not indicate a relationship between suffrage expansion and income and inheritance tax rates. Another potential concern is that our wealth data are limited to eleven countries, and it might be difficult to detect a relationship in a sample of this size. We have data for the policy variables and democracy measures for an additional seven European and neo-European cases.²¹ Table A-5 reports estimates adding these countries to the sample. The addition of these cases has no impact on the results. Another possible concern is whether the presence of country-specific trends may be adding too many parameters given the amount data that we have. Table A-6 in the Appendix reports quite similar results dropping the country-specific time trends. It is worth further noting that dropping the period fixed effects and the country-specific time trends does change our estimates. As reported in Table A-7, the coefficient on *Suffrage %* is positive and statistically significant in these specifications. This result does not change our view of the absence of a causal relationship between the expansion of the suffrage and these policy outcomes. First, dropping the period fixed effects means that we are failing to control for common shocks to these policy variables. Consequently, any factor such as technology, state capacity, norms, or public policy fashion that are correlated with suffrage expansion could account for the relationship. It is not a convincing specification for discerning the effect of an expanding franchise. Second, the substantive size of the effect is quite small across all the specifications.

One explanation for why increasing suffrage may not lead to higher inheritance or income taxes or to compulsory education is that other institutional features must be in place for democracy to influence these policy outcomes. In particular, it may be necessary for elections to be competitive. We evaluate this possibility by repeating the regressions in Table 2 while substituting the variable *Competitive Elections* for our suffrage measures. This variable measures whether countries with at least 50% male suffrage have multi-party elections for their legislature (see footnote for full coding rules). Table A-8 in the Appendix reports our estimates. There is no evidence of a relationship between competitive elections and the tax

²¹The additional cases are Austria, Belgium, Canada, Germany, Italy, New Zealand, and Spain.

policy outcomes. The results for compulsory education are mixed. In our first econometric model without a lag dependent variable, the estimate is positive but not statistically significant while in our second model with a lag dependent variable, the estimate is positive and statistically significant. This result is consistent with the intuition that countries adopted more universal education policy in response to the demands of a growing electorate. However, given the sensitivity of this estimate to our modeling assumptions, it is not possible to conclude with much confidence that democracy had a substantial impact on the adoption of compulsory education.²²

Our analysis in this section suggests that expanding the franchise alone does not account for key tax and policy innovations that influenced wealth inequality in the twentieth century. It remains a possibility, however, that suffrage extensions still had an effect on wealth inequality but not through these policies. We evaluate this possibility by returning to our wealth inequality regressions but adding our suffrage measure while dropping the policy variables. Table 3 reports these estimates. We examine specifications with and without controls for r and $var(r)$ because each of these variables, although a theoretically important determinant of wealth inequality, could be affected by democratic policymaking and induce post-treatment bias in our estimates. Across all of the specifications, our coefficient estimate for *Suffrage %* has the “wrong” sign (positive) and is not statistically significant. Table A-9 in the Appendix reports similar results for our *Competitive Elections* measure of democracy. There is no evidence that democracy alone induced policies that lead to systematic changes in top 1% wealth shares.

²²We also employed two other measures of democracy in our analysis. The first was *Universal Male Suffrage*. This variable was not significantly correlated with any of the policy measures. The second was *No Upper*. This variable was not significantly correlated with compulsory education but the results for the tax policy variables were mixed. In the specifications without a lag dependent variable, the coefficient estimate for *No Upper* for inheritance tax was positive but not statistically significant while the estimate for income tax was fairly large in magnitude (12.9) and marginally significant (p-value of 0.073). In the specifications with a lag dependent variable, the estimate for the inheritance tax was positive and marginally significant (p-value of 0.062) with similar results for income tax (p-value of 0.118). This evidence, while far from convincing, is consistent with the presence of non-democratic veto points being a contributing factor to determining whether countries adopt higher inheritance and income taxes. Finally, we also evaluated whether left governments, which became possible as the franchise was expanded, were more likely to adopt higher tax rates and compulsory education. Our estimates suggest little evidence consistent with this related argument for our sample of countries with wealth inequality data. See Scheve and Stasavage (2016) for a fuller discussion of the role of partisanship in income and inheritance tax policymaking.

OLS Estimates				
5-Year Averages of <i>Top 1% Wealth Shares</i>				
	(1)	(2)	(3)	(4)
<i>Top 1% Wealth Share</i> _{t-1}	0.876 (0.054)	0.894 (0.055)	0.872 (0.053)	0.893 (0.054)
<i>r</i> _t	0.000 (0.052)	0.000 (0.050)	0.000	0.000
<i>var(r)</i> _t	0.071 (0.039)	0.193 (0.038)		
<i>Suffrage %</i> _{t-1}	0.013 (0.016)	0.033	0.004 (0.015)	
<i>Suffrage %</i> _{t-6}	0.689	0.008 (0.017)	0.778	0.008 (0.017)
Country Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes
S.E.R.	1.902	1.890	1.909	1.891
Observations	221	212	222	212

Table 3: Suffrage and Wealth Inequality, 1900-2010. The table reports the results of OLS regressions of the variable *Top 1% Wealth Share* on its one period lag, *r*, *var(r)*, and *Suffrage %*. All specifications include country fixed effects, period fixed effects, country-specific time trends, and report the coefficient estimates, robust standard errors in parentheses, and p-values.

5 War, Religion, and Wealth Equalizing Policy

Our results on democracy suggest that the expansion of the suffrage by itself did not serve as a sufficient condition for wealth equalizing policy change. A primary reason for this may be that even newly enfranchised voters still disagree over whether governments should tax wealth and provide mass education. In this section we will show how external conflict between states and internal religious conflict between different social groups helped determine when and where wealth equalizing policy changes were implemented. In countries where there was less conflict about the appropriate roles of church and state, compulsory education was adopted considerably earlier. In countries that mass mobilized for war greater efforts were made to increase taxes on the wealthy relative to the rest of the population.

5.1 Religious Conflict and Compulsory Schooling

We will now examine the cross-sectional correlation between the year a country first implemented compulsory education and whether it had a state religion. In this context, the presence of a state religion proxies for the absence of significant religious conflict that could delay compulsory education. We will also employ a measure that captures whether a country has a Protestant state religion. This reflects the fact, suggested by Bendix and Rokkan (1962), that in countries where Catholicism was the state religion there may have been consensus over the choice of religion but conflict over the roles that state and secular authorities should have.

Our dependent variable is *Initial Year of Compulsory Education* and is set equal to the year a country first adopted compulsory education. We measure whether a country has a state religion in two ways. The variable *State Religion* is equal to one for a country with an official or de facto state religion in 1900 and zero otherwise. The codings were made by Barrett (1982) and Barrett, Kurian, and Johnson (2001) and were compiled by Barro and McCleary (2005). The classification is based on the assessment whether the state favors a given religion or constrains other religions. In most cases, the country explicitly has an official state religion in the constitution, but Barrett also codes countries that significantly subsidize

a particular religion or require its teachings in public schools as having a state religion. The variable *Protestant State Religion* eliminates Catholic countries from the list of state religions. This alternative measure is useful because having a state religion for Protestant countries is arguably a better indication of consensus about the role of religion in the provision of education. Because of the Catholic Church's institutional history and separate center of power in Rome, there was still conflict between the state and Church about how involved the Church would be in the educational system in countries coded as having a Catholic state religion. In our sample of countries with wealth data, Denmark, Finland, Ireland, Norway, Sweden, and the United Kingdom are coded as having a state religion with Ireland being dropped for the Protestant state religion measure.²³

Table 4 reports the results of cross-sectional OLS regressions of *Initial Year of Compulsory Education* on our two measures of state religion. For each measure, we report results for our sample of countries with wealth inequality data and for an extended sample that includes seven additional European and Neo-European cases. Across all four specifications the coefficient estimate is negative, large in magnitude, and fairly precisely estimated. The most informative of the estimates is in column 3 because this is for our wealth sample and employs the *Protestant State Religion* measure that we think best captures the relative absence of religious conflict about state provision of religion. The estimated coefficient in this specification is -38.4 indicating that countries with a Protestant state religion adopted compulsory education 38 years earlier than other countries. This illustrates the idea that overcoming policy conflict can be a significant barrier to adopting wealth equalizing policies.²⁴

We should note that external state conflict may also have played a role in the development of compulsory education in several of our sample countries, though not through the fairness based mechanism that we propose for the link between warfare and top tax rates.

²³Our analysis also presents results for a somewhat larger set of European and Neo-European countries for which we do not have wealth inequality data. Among these cases, we add Italy and Spain to the set of countries with state religions though both of these are dropped for the set of Protestant state religions.

²⁴The importance of overcoming policy conflict is also suggested by noting that the introduction of a national income tax was enacted earlier in countries with state religions, especially Protestant state religions. We do not focus on this result in this paper because the introduction of an income tax alone does not have a significant effect on wealth inequality most likely due to the fact that the initial rates were uniformly low across countries.

OLS Estimates				
<i>Initial Year of Compulsory Education</i>				
	(1)	(2)	(3)	(4)
<i>State Religion</i>	-29.100 (14.295)	-24.625 (10.863)		
	0.072	0.038		
<i>Protestant State Religion</i>			-38.367 (13.316)	-34.277 (12.385)
			0.018	0.014
Constant	1,882.600 (6.601)	1,881.500 (5.142)	1,884.167 (5.723)	1,880.077 (4.510)
	0.000	0.000	0.000	0.000
S.E.R.	25.00	21.74	20.87	19.38
Observations	11	18	11	18

Table 4: Religious Conflict and Initial Year of Compulsory Education. The table reports the results of OLS regressions of the variable *Initial Year of Compulsory Education* on *State Religion* or *Protestant State Religion*. Columns 1 and 3 report the results for the eleven countries in our wealth inequality sample while columns 2 and 4 report the results for our expanded sample. The table reports the coefficient estimates, robust standard errors in parentheses, and p-values.

In an environment of military rivalry it may simply have been desirable to have an educated population that would serve as better soldiers. This was certainly true in France after the defeat of 1870, and this may help explain why church-state conflict in that country did not delay the adoption of compulsory education by even more. This link between military rivalry and state educational investments has recently been explored extensively by Aghion, Persson, and Rouzet (2012).

5.2 Mass Warfare and Taxation of the Rich

Scheve and Stasavage (2016, 2012, 2010) evaluate the effect of mass mobilization for war on top rates of inheritance and income taxation in a sample of twenty countries and find a significant and robust positive effect. In this section we examine whether there is evidence that mass mobilization for war led to differential tax policies in the sample of eleven countries for which we have long run wealth inequality data.

To indicate whether or not a country engaged in mass warfare, we constructed the dummy variable *Mass Warfare* equal to 1 if in a particular year the country was engaged in an interstate war and at least two percent percent of the population was serving in the military.²⁵ This operationalization captures high mobilization years during the Franco-Prussian War, First World War, Second World War, and Korean War.²⁶

We use the same estimation strategy here as in our evaluation of the impact of suffrage extensions on tax and education policy. For the effect of mass warfare, we have the same general concerns about possible violations of the difference-in-differences assumptions. It is possible that countries select into war participation in part because of their beliefs about their ability to finance the war by taxing inheritances and income. This would bias our

²⁵Our data for incidents of war comes from the Correlates of War Project, Militarized Interstate Dispute Data, Version 3.0 (2003). Our data on mobilization is from the Correlates of War Project, National Material Capabilities Data, Version 3.0 (2005). To count as an interstate war, the dispute had to be coded as a war and involve 1,000 or more deaths. We supplemented this data where it was missing and, in one case, where it was incorrect with additional sources. However, the Correlates of War data starts in 1816 and we did not code any values before this year.

²⁶ More precisely, for our sample of countries with wealth inequality data, our war mobilization variable is coded one for Australia in 1915-1918, 1941-1945; for Finland in 1940-1944; for France in 1871, 1914-1920, 1940-1941; for the UK in 1915-1918, 1940-1945; and for the US in 1918, 1942-1945, 1951-1953.

	OLS Estimates			
	5-Year Averages			
	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>
	(1)	(2)	(3)	(4)
<i>Top Inheritance Tax Rate</i> _{t-1}			0.874 (0.044)	
<i>Top Income Tax Rate</i> _{t-1}			0.000	0.760 (0.057)
<i>Mass Warfare</i> _{t-1}	20.190 (6.909)	24.761 (5.202)	6.883 (4.216)	15.578 (5.305)
	0.015	0.001	0.104	0.004
Country Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes
S.E.R.	9.180	9.600	5.118	6.078
Observations	342	342	342	342

Table 5: Mass Warfare and Tax Policymaking, 1816-2010. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate* and *Top Income Tax Rate* on *Mass Warfare* in columns (1-2) and on each dependent variable's lag and *Mass Warfare* in columns (3-4). All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-2)), and p-values.

estimates in a positive direction and lead us to overestimate the effect of war on tax policy outcomes. There are several reasons that we are skeptical about the importance of this potential selection issue with our sample. First, many of the decisions by countries that lead them to be differentially exposed to mass warfare are long-term choices that remain fixed during the period of our study. In particular, it is implausible that the timing of war exposure for the key conflicts in our data, such as World War I and World War II, was determined by expectations about the ease of taxing inherited wealth and income. Skepticism about the importance of this potential source of bias is further bolstered by the fact that in critical cases, such as World War I, none of the initial participants correctly anticipated the length of the conflict or the extent of mobilization necessary to fight the war.

Table 5 reports our main estimates. Across the tax specifications, the coefficient estimate for *Mass Warfare* is positive and substantively and statistically significant, though the inheritance tax specification with a lag dependent variable has a relatively high p-value of 0.104. The estimates in columns 1 and 2 indicate that experiencing a five year mass mobilization war is, all else equal, associated with a 20 percentage point increase in the top rate of the inheritance tax and a nearly 25 percentage point increase in the top income tax rate. The typical length of a mass mobilization war in our data is closer to four years but the effect for this somewhat shorter conflict is nonetheless large. The implied long-run estimates in the specifications in columns 4 and 5 with a lag dependent variable are somewhat larger in magnitude. There is substantial evidence in our data that mobilization for mass warfare drove inheritance and income tax rates higher in our wealth inequality sample. Also, Henrekson and Waldenstrom (2015) and Scheve and Stasavage (2016) discuss evidence of ways in which citizen mobilization and the impact of the world wars on the economy in non-participant countries sometimes set political conditions conducive to raising the top rates of income and inheritance taxation. This suggests that our regressions may actually underestimate the effect of war mobilization and that the world wars also help to account for tax increases in non-participant or non-mobilized countries.

These results are robust to the same additional tests that we discussed for the relationship

between suffrage expansion and policy outcomes. Table A-10 in the Appendix reports the results for the twentieth century only. For results on a larger sample of countries and with alternative econometric specifications, see Scheve and Stasavage (2012, 2016). Employing an extended sample, dropping the country-specific time trends, or dropping both the period fixed effects and country-specific time trends does not change the pattern of results that we observe in Table 5.

In interpreting this result, we have emphasized that mass mobilization for war generates consensus about taxing the rich. Implicit in this argument is the recognition that democracy still matters for the adoption of wealth equalizing policies in at least two ways. First, the fairness arguments that are activated in the context of mass mobilization are more likely to achieve consensus about taxing the rich in democratic settings where norms about treating citizens as equals are more widely held. Second, consensus on taxation matters more for policy outcomes in democracies. While non-democratic regimes may also respond to the war mobilization with higher taxation of the rich to ensure greater effort in the war, democratic regimes need to respond more directly to tax policy preferences. These considerations suggest that the effect of mass mobilization for war should be greater in democracies.

We can evaluate this prediction in two ways. First, it is crucial to note that in the results for *Mass Warfare* reported in Table 5, all the countries that mass mobilized for war are democracies. For our wealth inequality sample, the effect of mass warfare is only observed in democratic countries. Second, we can employ our extended sample of additional European and Neo-European countries and reestimate the regressions in Table 5 for democracies and non-democracies, as measured by our *Competitive Elections* variable, separately.

Table 6 reports our results. For the dependent variable *Top Inheritance Tax Rate*, the estimates in columns 1, 2, 5, and 6 indicate that *Mass Warfare* increase top rates of inheritance taxes in democracies but not non-democracies. For the dependent variable *Top Income Tax Rate*, the estimates in the specifications without a lag dependent variable in columns 3 and 4 indicate only about a 1 percentage point difference in the effect of war mobilization between democracies and non-democracies though the difference is in the expected direction.

OLS Estimates							
5-Year Averages				5-Year Averages			
<i>Inheritance Tax</i>		<i>Income Tax</i>		<i>Inheritance Tax</i>		<i>Income Tax</i>	
<i>Dem</i>	<i>Non-Dem</i>	<i>Dem</i>	<i>Non-Dem</i>	<i>Dem</i>	<i>Non-Dem</i>	<i>Dem</i>	<i>Non-Dem</i>
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Top Inheritance Tax Rate_{t-1}</i>				0.826 (0.051) 0.000	0.847 (0.115) 0.000		
<i>Top Income Tax Rate_{t-1}</i>						0.660 (0.053) 0.000	0.591 (0.153) 0.000
<i>Mass Warfare_{t-1}</i>	21.385 (5.808) 0.002	-3.382 (9.833) 0.739	23.643 (4.982) 0.000	22.727 (2.488) 0.000	10.492 (4.058) 0.010	-1.621 (2.607) 0.536	11.830 (6.296) 0.063
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E.R.	10.75	0.835	10.23	2.758	6.537	0.455	1.943
Observations	381	135	386	164	381	135	164

Table 6: Mass Warfare and Tax Policymaking, 1816-2010, Extended Sample by Democracy. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate* and *Top Income Tax Rate* on *Mass Warfare* in columns (1-4) and on each dependent variable's lag and *Mass Warfare* in columns (5-8) by democracy as measured by the variable *Competitive Elections*. All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-4)), and p-values.

A more substantial difference is reported for the specifications with a lag dependent variable in columns 7 and 8. Overall, the evidence is consistent with our expectation that war mobilization generates greater consensus for taxing income and wealth and that this effect is generally larger in democracies.²⁷

6 Conclusion

We have provided one of the first examinations of the determinants of wealth inequality in a cross country setting over a lengthy period of time. Factors suggested by economic theory, and in particular the pre tax return on capital and capital income risk turn out to have a substantial impact on the wealth distribution. In addition, government policies including taxation of capital income, taxation of inheritance, and provision of education also have a significant effect on wealth inequality. The expansion of the suffrage alone did not produce wealth equalizing policies, but it did undoubtedly have an effect once other conditions were also met. First, there had to be a strong fairness based argument for high top rates of income and inheritance taxation to create a consensus for such policies. This was provided by mass mobilization for war. Second, internal conflict over other dimensions of policy, and in particular, religion needed to be solved so that state provision of compulsory education could proceed.

²⁷For additional evidence and discussion on this point, see Scheve and Stasavage (2016).

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A Appendix Tables

OLS Estimates						
5-Year Averages of <i>Top 1% Wealth Share</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Top 1% Wealth Share</i> _{t-1}	0.941 (0.027)	0.931 (0.025)	0.930 (0.025)	0.952 (0.027)	0.939 (0.025)	0.940 (0.025)
<i>r</i> _t	0.000	0.000	0.000	0.000	0.000	0.000
<i>var(r)</i> _t	0.069 (0.039)	0.062 (0.038)	0.071 (0.039)	0.045 (0.038)	0.045 (0.037)	
	0.077	0.103	0.070	0.241	0.236	
	0.062 (0.029)	0.058 (0.028)	0.049 (0.030)	0.061 (0.029)	0.053 (0.030)	
<i>Top Inheritance Tax Rate</i> _{t-1}		0.032 -0.033 (0.014)	0.106	0.040	0.078	
<i>Top Income Tax Rate</i> _{t-1}			-0.034 (0.015)			
			0.022			
<i>Sum of Top Tax Rates</i> _{t-1}					-0.019 (0.008)	-0.019 (0.008)
					0.028	0.022
<i>Compulsory Education</i> _{t-6}				-1.212 (0.373)	-0.882 (0.355)	-0.969 (0.348)
				0.001	0.014	0.006
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
S.E.R.	1.665	1.641	1.635	1.651	1.624	1.619
Observations	310	307	307	298	295	297

Table A-1: The Policy Correlates of Wealth Inequality, 1800-2010. The table reports the results of OLS regressions of the variable *Top 1% Wealth Share* on its one period lag, *r*, *var(r)*, and various policy measures. All specifications include country fixed effects, period fixed effects, country-specific time trends, and report the coefficient estimates, robust standard errors in parentheses, and p-values.

OLS Estimates					
5-Year Averages of <i>Top 1% Wealth Share</i>					
	(1)	(2)	(3)	(4)	(5)
<i>Top 1% Wealth Share</i> _{t-1}	0.853 (0.046)	0.828 (0.046)	0.836 (0.044)	0.865 (0.045)	0.842 (0.044)
<i>r</i> _t	0.000 0.092 (0.054)	0.000 0.081 (0.051)	0.000 0.086 (0.052)	0.000 0.069 (0.050)	0.000 0.064 (0.048)
<i>var(r)</i> _t	0.088 0.098 (0.039)	0.114 0.088 (0.039)	0.099 0.077 (0.040)	0.166 0.098 (0.039)	0.183 0.083 (0.039)
<i>Real GDP Per Capita</i> _t	0.014 0.000 (0.000)	0.025 0.000 (0.000)	0.057 0.000 (0.000)	0.013 0.000 (0.000)	0.037 0.000 (0.000)
<i>Top Inheritance Tax Rate</i> _{t-1}		0.089 -0.034 (0.015)		0.074	0.060
<i>Top Income Tax Rate</i> _{t-1}			-0.034 (0.015)		
<i>Sum of Top Tax Rates</i> _{t-1}			0.021		-0.019 (0.008)
<i>Compulsory Education</i> _{t-6}				-1.978 (0.867)	-1.366 (0.833)
				0.024	0.103
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes
S.E.R.	1.869	1.835	1.830	1.840	1.807
Observations	221	220	220	214	213

Table A-2: The Policy Correlates of Wealth Inequality, 1900-2010, Controlling for *Real GDP Per Capita*. The table reports the results of OLS regressions of the variable *Top 1% Wealth Share* on its one period lag, *r*, *var(r)*, *Real GDP Per Capita*, and various policy measures. All specifications include country fixed effects, period fixed effects, country-specific time trends, and report the coefficient estimates, robust standard errors in parentheses, and p-values.

OLS Estimates					
5-Year Averages of <i>Top 1% Wealth Share</i>					
	(1)	(2)	(3)	(4)	(5)
<i>Top 1% Wealth Share</i> _{t-1}	0.878 (0.052)	0.854 (0.051)	0.862 (0.048)	0.891 (0.052)	0.868 (0.049)
<i>r</i> _t	0.000 (0.052)	0.000 (0.051)	0.000 (0.050)	0.000 (0.048)	0.000 (0.047)
<i>var(r)</i> _t	0.075 (0.039)	0.107 (0.039)	0.080 (0.040)	0.151 (0.039)	0.172 (0.039)
<i>Growth</i> _t	0.014 (4.425)	0.027 (4.034)	0.061 (4.165)	0.014 (3.970)	0.040 (3.687)
<i>Top Inheritance Tax Rate</i> _{t-1}		0.753 -0.036 (0.015)	0.569	0.846	0.796 0.747
<i>Top Income Tax Rate</i> _{t-1}			-0.036 (0.016)		
<i>Sum of Top Tax Rates</i> _{t-1}			0.023		-0.020 (0.009)
<i>Compulsory Education</i> _{t-6}				-2.112 (0.862)	-1.549 (0.836)
				0.015	0.066
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes
S.E.R.	1.902	1.869	1.863	1.875	1.843
Observations	221	220	220	214	213

Table A-3: The Policy Correlates of Wealth Inequality, 1900-2010, Controlling for *Growth*. The table reports the results of OLS regressions of the variable *Top 1% Wealth Share* on its one period lag, *r*, *var(r)*, *Growth*, and various policy measures. All specifications include country fixed effects, period fixed effects, country-specific time trends, and report the coefficient estimates, robust standard errors in parentheses, and p-values.

	OLS Estimates			
	5-Year Averages			
	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>
	(1)	(2)	(3)	(4)
<i>Top Inheritance Tax Rate</i> _{t-1}			0.901 (0.049)	
<i>Top Income Tax Rate</i> _{t-1}			0.000	0.723 (0.062)
<i>Suffrage %</i> _{t-1}	-0.159 (0.292)	-0.114 (0.292)	0.039 (0.067)	0.028 (0.077)
	0.598	0.706	0.556	0.715
Country Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes
S.E.R.	10.98	10.92	6.163	7.438
Observations	232	232	230	230

Table A-4: Suffrage and Tax Policymaking, 1900-2010. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate* and *Top Income Tax Rate* on *Suffrage %* in columns (1-2) and on each dependent variable's lag and *Suffrage %* in columns (3-4). All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-2)), and p-values.

OLS Estimates					
5-Year Averages					
<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>
(1)	(2)	(3)	(4)	(5)	(6)
<i>Top Inheritance Tax Rate_{t-1}</i>			0.838 (0.042)		
			0.000	0.728 (0.048)	
<i>Top Income Tax Rate_{t-1}</i>				0.000	
<i>Compulsory Education_{t-1}</i>					0.774 (0.059)
					0.000
<i>Suffrage %_{t-1}</i>	0.010 (0.041)	-0.073 (0.063)	0.000 (0.002)	-0.030 (0.019)	0.000 (0.001)
	0.813	0.263	0.878	0.123	0.472
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes
S.E.R.	9.538	9.978	0.189	5.748	0.112
Observations	555	596	597	551	597

Table A-5: Suffrage and Tax and Education Policymaking, 1800-2010, Expanded Sample. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate*, *Top Income Tax Rate*, and *Compulsory Education on Suffrage %* in columns (1-3) and on each dependent variable's lag and *Suffrage %* in columns (4-6). All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-3)), and p-values.

OLS Estimates					
5-Year Averages					
<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>
(1)	(2)	(3)	(4)	(5)	(6)
<i>Top Inheritance Tax Rate_{t-1}</i>			0.939 (0.018)		
			0.000	0.900 (0.024)	
<i>Top Income Tax Rate_{t-1}</i>				0.000	
<i>Compulsory Education_{t-1}</i>					0.901 (0.040)
					0.000
<i>Suffrage %_{t-1}</i>	0.309 (0.091)	0.472 (0.085)	0.008 (0.002)	0.021 (0.008)	0.053 (0.017)
	0.007	0.000	0.000	0.008	0.002
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	No	No	No	No	No
Country-specific Time Trends	No	No	No	No	No
S.E.R.	16.21	20.43	0.286	5.384	7.913
Observations	365	366	366	361	363

Table A-7: Suffrage and Tax and Education Policymaking, 1800-2010, No Period Fixed Effects and No Country-Specific Trends. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate*, *Top Income Tax Rate*, and *Compulsory Education* on *Suffrage %* in columns (1-3) and on each dependent variable's lag and *Suffrage %* in columns (4-6). All specifications include country fixed effects. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-3)), and p-values.

OLS Estimates					
5-Year Averages					
<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Compulsory Education</i>
(1)	(2)	(3)	(4)	(5)	(6)
<i>Top Inheritance Tax Rate_{t-1}</i>			0.890 (0.041) 0.000		
<i>Top Income Tax Rate_{t-1}</i>				0.787 (0.057) 0.000	
<i>Compulsory Education_{t-1}</i>					0.765 (0.077) 0.000
<i>Competitive Elections_{t-1}</i>	3.132 (4.268) 0.480	-1.112 (6.361) 0.865	0.261 (0.185) 0.190	-1.063 (1.252) 0.396	0.133 (0.060) 0.027
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes	Yes
S.E.R.	9.270	9.804	0.187	6.138	0.110
Observations	362	363	363	363	363

Table A-8: Competitive Elections and Tax and Education Policymaking, 1800-2010. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate*, *Top Income Tax Rate*, and *Compulsory Education on Competitive Elections* in columns (1-3) and on each dependent variable's lag and *Competitive Elections* in columns (4-6). All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-3)), and p-values.

OLS Estimates				
5-Year Averages of <i>Top 1% Wealth Share</i>				
	(1)	(2)	(3)	(4)
<i>Top 1% Wealth Share</i> _{t-1}	0.874 (0.053)	0.895 (0.059)	0.870 (0.052)	0.892 (0.058)
<i>r</i> _t	0.000 (0.051)	0.000 (0.053)	0.000	0.000
<i>var(r)</i> _t	0.083 (0.039)	0.260 (0.041)		
<i>Competitive Elections</i> _{t-1}	0.019 (0.941)	0.060	0.666 (0.833)	
<i>Competitive Elections</i> _{t-6}	0.838	0.500 (0.688)	0.425	0.590 (0.676)
Country Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes
S.E.R.	1.900	1.920	1.908	1.918
Observations	220	203	220	203

Table A-9: Democracy and Wealth Inequality, 1900-2010, *Competitive Elections* Measure of Democracy. The table reports the results of OLS regressions of the variable *Top 1% Wealth Share* on its one period lag, *r*, *var(r)*, and *Competitive Elections*. All specifications include country fixed effects, period fixed effects, country-specific time trends, and report the coefficient estimates, robust standard errors in parentheses, and p-values.

	OLS Estimates			
	5-Year Averages			
	<i>Inheritance Tax</i>	<i>Income Tax</i>	<i>Inheritance Tax</i>	<i>Income Tax</i>
	(1)	(2)	(3)	(4)
<i>Top Inheritance Tax Rate</i> _{<i>t</i>-1}			0.882 (0.049)	
<i>Top Income Tax Rate</i> _{<i>t</i>-1}			0.000	0.698 (0.061)
<i>Mass Warfare</i> _{<i>t</i>-1}	18.293 (7.421)	22.272 (5.339)	6.262 (4.376)	15.042 (5.513)
	0.033	0.002	0.154	0.007
Country Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Country-specific Time Trends	Yes	Yes	Yes	Yes
S.E.R.	10.80	10.49	6.114	7.165
Observations	230	230	230	230

Table A-10: Mass Warfare and Tax Policymaking, 1900-2010. The table reports the results of OLS regressions of the dependent variables *Top Inheritance Tax Rate* and *Top Income Tax Rate* on *Mass Warfare* in columns (1-2) and on each dependent variable's lag and *Mass Warfare* in columns (3-4). All specifications include country fixed effects, period fixed effects, and country-specific time trends. The table reports the coefficient estimates, robust standard errors in parentheses (clustered on country in columns (1-2)), and p-values.