Democracy, War, and Wealth

Evidence from Two Centuries of Inheritance Taxation[1]

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Abstract

In this paper we use an original data set to provide the first empirical analysis of the politics of inherited wealth taxation that covers a significant number of countries and a long time frame (1816-2000). Our goal is to understand why if inheritance taxes are often very old taxes, the implementation of inheritance tax rates significant enough to affect wealth inequality is a much more recent phenomenon. We hypothesize alternatively that significant taxation of inherited wealth depended on (1) the extension of the suffrage to the lower classes, and (2) political conditions created by mass mobilization for war. Using a generalized difference-in-differences framework for identification, we find little evidence for the suffrage hypothesis but very strong evidence for the mass mobilization hypothesis. Our study has implications for understanding the evolution of wealth inequality and the potential effect of democracy on redistribution. Our findings also inform scholarship on the current political context for estate and inheritance taxation. Understanding why majorities today may favor the repeal of such taxes requires understanding why past majorities favored implementing them in the first place.

1 Introduction

Bequest taxation is a controversial subject. Academic economists have often disagreed about the merits of taxing inherited wealth. Across a range of countries and time periods, attitudes of members of the general public have been no less divided. Many emphasize the potential usefulness of this form of taxation for raising revenue and simultaneously reducing inequality of opportunity for future generations. But others see bequest taxation as arbitrary because it depends on the timing of death, as unfairly interfering with the ability of parents to save for their children, and finally as having potentially severe efficiency costs.[1]

While the normative debates about bequest taxation are extensive, much less is known about the actual conditions that lead some governments in practice to levy significant taxes on inherited wealth while others refrain from doing so. This question is of increasing interest as a growing literature has suggested that progressive capital and income taxation has played an important role in the development of wealth accumulation during the course of the twentieth century.[2] So far, efforts to construct formal political economy models of inheritance taxation have been limited. Basic intuition nonetheless suggests that electoral democracy ought to be one of the most powerful conditions leading to the taxation of inherited wealth, and in particular to a form of bequest taxation where large estates are taxed at a significantly higher rate than small estates. In a society where most decedents leave either no estate or a small estate, the logic of electoral politics would seem to dictate that large estates should be taxed heavily. In this paper we use an original data set covering the taxation of inherited wealth in nineteen countries over two centuries to test this proposition. We find surprisingly little evidence that formal institutional changes associated with electoral democracy have been associated with increased taxation of large fortunes. What we find instead is very strong evidence that mass mobilization for war has been associated with increased taxation of large

[1] See Crémer and Pestieau (2003) for a survey of economic debates on optimal inheritance taxation. Beckert (2008) provides an excellent review of more long run debates over inheritance taxation law. See Batchelder (2008) for an overview of current debates related to the estate tax in the United States and Graetz and Shapiro (2005) and Bartels (2008) for the political context of this debate.

[2] See e.g. Piketty (2001), Piketty and Saez (2003), Piketty, Postel-Vinay, and Rosenthal (2006), and Roine and Waldenström (2009).

fortunes relative to small fortunes. Our empirical design allows us to suggest that this relationship may also be causal. This finding regarding war may reflect the fact that in societies where the masses are expected to fight, political pressures emerge for having the rich bear a disproportionate part of the financial burden of war.

While the extent of inheritance taxation is of direct interest, understanding its determinants is also important because of what we can learn about the political and economic conditions that lead countries to adopt progressive taxation more generally. More specifically, there are particular advantages in focusing on inheritance taxation when seeking an empirical strategy for identifying the effect of democracy and mass mobilization on progressive taxation. Unlike more recent forms of taxation, such as the income tax, inheritance taxes generally require less administrative capacity to collect. As long as heirs have an incentive to use the legal system to establish their right to property from an estate, then this allows tax authorities to use information collected by legal authorities to calculate taxes owed. It is for this reason that a former director of Great Britain's Inland Revenue observed "The estate duty is thus to a large extent a self-collecting tax and requires no elaborate machinery for enforcement."[3] The fact that an inheritance tax can be administered without a substantial expansion of bureaucratic capacity reduces the possibility that any empirical relationship we observe (or fail to observe) between electoral democracy and inheritance taxation might depend on the confounding factor of administrative capacity. For example, if nineteenth century suffrage extensions in some countries sometimes failed to trigger significant taxation of the rich through income taxation, this might be attributable to the fact that governments lacked the bureaucratic capacity to administer such a system. A similar finding with regard to inheritance taxation would be less subject to this objection. The fact that a less extensive administrative apparatus is necessary to administer an inheritance tax also increases confidence that any finding of a correlation between war mobilization and inheritance taxation is not biased by the possibility that levels of bureaucratic capacity are driving both of these outcomes. This certainly leaves other possible sources of endogeneity, and we will

[3] Johnston (1965 p.153).

discuss these in detail below. It nonetheless points to the usefulness of focusing empirical tests on inheritance taxation to learn about the conditions under which governments adopt progressive taxation.

To conduct our empirical tests we make use of an original data set that records marginal rates for bequest taxes in nineteen countries over the period between 1816 and 2000. It is generally known when a country first established an inheritance tax, but this simple fact often tells us very little about the extent to which governments actually taxed large fortunes at rates significant enough to influence wealth accumulation. In fact, we will show that top marginal rates of inheritance taxation were extremely low (i.e. $<5\%$) in many of our sample countries for long periods after their initial establishment. While information on changes in marginal inheritance tax rates for a country like the United States is easy to come by, for most other countries this is not the case, and it is not generally reported by finance ministries. We have compiled our data base of inheritance tax rates by consulting original legislation for each of the nineteen countries in our sample together with a range of other sources, all of which are listed in the appendix to this paper.

Why would we expect either electoral democracy or mass warfare to influence inheritance tax policies? One place to start when considering this question is the literature on the optimal taxation of capital, and in particular recent work which suggests that in the presence of political economy constraints, it is optimal to have progressive taxation of capital. Until recently, it was common to conclude that the optimal tax rate on capital should be zero, at least in the steady state. Chamley (1986) and Judd (1985) demonstrated this in a Ramsey framework with a representative agent where a benevolent policymaker disposes of an exogenously specified set of tax instruments, including a tax on capital. In a Mirrlees framework where the set of feasible taxes is not pre-determined and where individuals differ in an unobserved ability characteristic, the Atkinson-Stiglitz (1976) uniform taxation result implies a similar conclusion. The conclusion that the optimal rate of capital taxation in the steady state should be zero has, however, recently been called into question by Golosov, Kocherlakota, and Tsyvinsiki (2003). They demonstrate that in an economy where previous

assumptions about the evolution of skills over time and the set of feasible tax schedules are relaxed, then the optimal tax rate on capital may be positive.[4] More recently, Farhi and Werning (2008) demonstrate why it may be optimal to have positive and progressive taxation of capital when political economy constraints are present. In an infinite horizon model where citizens vote on the choice of capital tax in each period, progressive taxation of capital prevents the accumulation of excessive wealth inequality that would create political pressures for full scale expropriation of capital. In their paper citizens vote over taxation following the standard assumptions of a probabilistic voting model of the sort developed by Lindbeck and Weibull (1987). The conclusions of Farhi and Werning could also presumably carry over to a model of estate taxation.[5]

We can use the intuition of Farhi and Werning's model to suggest that there should be more progressive taxation of capital in a democracy where all citizens can vote as opposed to in a system where the suffrage is restricted or where policies are otherwise set by a narrow group. In the model of Farhi and Werning (2008), electoral democracy actually has two counteracting effects. First, the fact that capital taxes are chosen in a repeated fashion by a vote of all citizens creates a commitment problem involving the risk of ex post expropriation. Second, the existence of this commitment problem creates an incentive for candidates to propose policy platforms where there is progressive taxation of capital. Progressive taxation of capital then reduces wealth inequality, reducing incentives to engage in ex post expropriation, and therefore allowing capital accumulation to take place in equilibrium. How would this result change if it was instead the case that there was a restricted suffrage in which only the wealthy could vote? The risk of democratic ex post expropriation would be reduced, logically leaving less incentive to adopt progressive taxation of capital.[6] Since inheritance taxation is one form of capital taxation, we would expect to observe empirically that as countries democratize they ought to shift towards more progressive inheritance taxation.

[4] Saez (2002) considers how an optimal capital income tax rate might be positive in the transition to a steady state if there are initial wealth inequalities and an initial wealth levy is not feasible.

[5] See Cremer and Pestieau (2003) for a review of optimal inheritance taxation under alternative assumptions about bequest motivations. They do not consider optimal estate taxation under the political economy constraint identified by Farhi and Werning (2008).

[6] Though of course expropriation might still occur through extra-constitutional means.

The above prediction regarding suffrage extensions would parallel the conclusion of Acemoglu and Robinson (2000, 2006) regarding the effect of suffrage extensions on redistribution.[7] However, following more recent work by Acemoglu and Robinson (2008) if "de facto power" of those at the top of the wealth distribution outweighs the shift in "de jure" power, then we would not expect to observe that suffrage extensions produce shifts towards significantly more progressive policies in capital taxation. One of the main goals of this paper is to suggest which of these two dynamics may have been more frequently at play with respect to inheritance taxation over the last two centuries.

The prediction that universal suffrage and progressive inheritance taxation should go together seems, at first glance, to be strongly supported by the fact that they both emerged during the same general time period—the turn of the twentieth century. In fact, scholarly observers at the time explicitly stated that the development of progressive inheritance taxation was attributable above all to the spread of democratic ideas and democratic institutions.[8] For observers like Shultz (1926), progressive inheritance taxation characterized by high top marginal rates was particuarly likely to emerge in a democratic context where there was pressure from parties of the political left.

If early twentieth century observers commented on the possible association between democracy and inheritance taxation, some authors, such as Soward (1919) also commented on another more long-standing trend—innovations in inheritance taxation were driven by the exigencies of war. Since at least the time of Hintze (1906) and Schumpeter (1919), scholars have argued that during the course of European history, exogenous demands of war led governments to make administrative investments improving their ability to tax. Besley and Persson (2009) have recently clarified this issue by providing a model where the need to provide a public good (such as defense) influences government decisions to invest in building legal and administrative capacity.

What the literature on warfare and taxation has not considered is how mass mobilization for war might be associated with a shift in the burden of taxation between different social

[7] It would also fit with observations of Lindert (1994) and Boix (2003).

[8] On this point see in particular West (1908), Seligman (1913), and Soward (1919).

groups, and in particular with increased taxation of large fortunes relative to others. We will suggest two possible mechanisms through which this might occur. It will not be our goal to adjudicate between these two mechanisms in this paper, but we will provide suggestive evidence that the pattern of inheritance taxation observed in our sample is more consistent with the second.

One plausible reason why war participation might trigger increased taxation of capital is if greater uncertainty over a government's survival—implying a smaller discount factor—creates an incentive to engage in expedient actions, such as a one off levy on capital despite the loss of reputation this might entail. If this is the case, then we might expect to see sharp spikes in capital taxation during wartime, followed by a return to lower rates of taxation as soon as a war is over, or more generally as soon as a government's tenure is secure. This first possible war effect would not imply that taxation of capital during wartime should necessarily become more progressive. We will refer to this first mechanism whereby war might lead to increased taxation of capital as the *expediency effect*.

We also propose a second mechanism through which war participation might lead to increased taxation of capital, and in particular to increased progressivity of capital taxation. We will refer to this as the *mobilization effect*. When societies seek to mobilize the great mass of their citizens for war, then citizens may in turn demand that the wealthy bear a significant share of the financial burden for a war. These citizen demands are likely to be more pronounced when it is perceived that the wealthy are less likely to serve at the front (perhaps because wealthy individuals tend on average to be older and thus not subject to conscription). They are likely to be further strengthened when it is believed that those who have accumulated significant wealth have investments that stand to profit from prosecution of a war. We should expect the mobilization effect to be increasing in the fraction of a country's citizens that is engaged in the war effort. It will also logically be larger when governments use mass conscription as a means of recruitment, or any similar means to recruiting soldiers at a below market wage (such as a volunteer force where those who fail to enlist suffer a social stigma).

The mobilization effect is closely related to, but nonetheless distinct from, a long standing argument that when societies ask the great mass of their citizens to fight, these citizens are likely to demand an equalization of rights of political participation. This argument can be observed in writing as early as the Fifth Century B.C..[9] It has recently been formalized by Ticchi and Vindigni (2009) who suggest that a ruling elite during time of war may expand the suffrage as a means of committing to future redistributive policies. While their argument is a logical possibility, our statistical results suggest that, at least for the case of progressive taxation in the industrial countries, the extension of the suffrage was not a sufficient commitment to redistribute in this manner.

To analyze the relationship between war, wealth, and democracy we will employ two different empirical approaches. Our main reported results employ a generalized differences-in-differences framework. The top marginal rate of inheritance taxation is modeled as a function of several alternative democracy measures, a measure of war mobilization, country fixed effects which control for time constant unobserved country-level heterogeneity, time period effects which control for common shocks, and a couple of time-varying control variables, with standard errors that are adjusted to allow for within-country correlation. We also present results where individual linear time trends for each country are added to the specification. Our second approach is to estimate the effect of war mobilization and democracy on inheritance taxation by conditioning on the marginal tax rate in the previous period.[10] To estimate this we use a lagged dependent variable specification combined with dummy variables for common time effects. We have collected a data set with annual frequency from 1816 to 2000. However, since we do not know a priori how long it may take for democratization or war mobilization to influence policy choices, we focus our analysis on specifications employing annual data for the marginal tax rate but with observations spaced at five and ten year intervals.

These analyses yield two main results. First, our estimates do not suggest a positive

[9] An author known today as "the Old Oligarch" suggested with regard to Athenian government that it was just (*dikaios*) for the poor and the common people to be given rights of political participation and to be allowed to hold office because they rowed the ships of the Athenian navy which conferred power on the city. Similar observations have been made more recently by Titmuss (1958), Andreski (1968), Finer (1975), and Mann (1988).

[10] As suggested by Angrist and Pischke (2009).

relationship between democracy and the top rate of inheritance taxation. Our simplest measure of democracy, which directly captures the main mechanism suggested by the democratization hypothesis, is the extent of the suffrage, specifically an indicator variable for whether or not a country had universal male suffrage at a given time. Our estimates for this coefficient are inconsistently signed, small in magnitude, and statistically insignificant. While the 95-percent confidence intervals for these estimates are too wide for us to exclude the possibility of a substantively meaningful effect for universal suffrage, none of the results are consistent with a substantively and statistically significant positive relationship between democratization and the top marginal rate of inheritance taxation. This pattern of results is repeated for ordinal measures of the extent of the suffrage, measures based on the extent of political competition, and a measure of the presence of a secret ballot. The only partial exception to this pattern of results is mixed evidence of a positive relationship between the absence of a nondemocratic upper house with the power to veto legislation and a higher top inheritance tax rate.

Second, our estimates indicate a substantively and statistically significant positive relationship between war mobilization and the top rate of inheritance tax. Our estimates typically suggest that, all else equal, a country that mobilized for mass warfare for an entire five-year period increased its top inheritance tax rate by 17 to 23 percentage points compared to a country that did not mobilize for war. These results are evident across both our difference-in-differences and lagged dependent variable models with and without the inclusion of time-varying control variables and individual linear time trends for each country. We consider multiple measures of war mobilization, possible interactions between war mobilization and democracy and left partisanship, and several alternative econometric models.

Our statistical results show that there is a robust correlation between war mobilization and the progressive taxation of capital. This correlation remains robust when using alternative estimation strategies and different identifying assumptions, as described above. In the end, however, we must acknowledge that we do not provide a research design in which mass mobilization has been assigned at random. We can nonetheless offer several reasons why

our study is less likely to be subject to endogeneity concerns than would certain alternative designs. The first reason involves the benefits of focusing on inheritance taxation as described above. The second involves the nature of the two World Wars. From the standpoint of most participants (and arguably almost all participants in World War I), the timing of mobilization for these conflicts was exogenously imposed. This reduces the likelihood that states selected into the conflict because there was some temporary factor that increased their ability to generate revenue and thus led them to opt for war. What we cannot exclude is that over the longer term certain types of states, such as France, Germany, and the UK, selected into a potential war treatment group while other states, such as Sweden or Switzerland, opted out of large scale international conflict altogether. However, fixed but unobserved factors of this sort are controlled for in our analysis by the inclusion of country fixed effects.

In the remainder of this paper we will proceed as follows. In Section 2 we present the data set, discuss measurement issues, and illustrate key trends in marginal inheritance taxes by examining the data for Sweden and the United Kingdom in some detail. Section 3 then presents our econometric model. In Section 4 we present our core estimation results. Section 5 discusses alternative interpretations of the strong correlation that we observe between war mobilization and the taxation of inherited wealth. Section 6 concludes.

2 A New Data Set on Inheritance Taxation

To assess the comparative history of inheritance taxation over the last two centuries, we have constructed a new data set recording key features of inheritance taxation for nineteen countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Korea, the Netherlands, New Zealand, Norway, Sweden, Switzerland, the United Kingdom, and the United States.

The taxation of inherited wealth has taken three major forms over the last two hundred years.

First, some countries adopted stamp duties levied on the documents necessary to transfer the property of an estate. This was the most common form of inheritance taxation in the

19th century and generally involved very low rates or even fixed fees. The British probate duty, first established in 1694, is a good example of this phenomenon. Often, small estates were exempted from these taxes, and they also did not always apply to all types of wealth.

Second, countries adopted inheritance taxes for which the tax is on the beneficiary of the estate. In the early development of inheritance taxes, the rates for these taxes varied greatly depending on the identity of the beneficiary. In some countries, children were taxed at the lowest rates, if at all, while in others spouses were taxed the least. Variation based on the identity of the beneficiary could be dramatic. For example, the initial German federal inheritance tax enacted in 1906 exempted spouses and direct descendants but taxed non-relatives at a maximum rate of 25%. These taxes also included exemptions for small estates, and they often had progressive rates that depended on the size of the transfer.

Third, some countries implemented estate duties for which the tax is levied on the estate itself rather than the beneficiaries. These taxes also include exemptions and often progressive rates, but they do not typically vary by the identity of the beneficiary. Inheritance taxes are much more commonly found in civil law countries, whereas estate taxation has been more widespread in common law countries, but this is not a hard and fast rule. In some cases countries have also simultaneously maintained an estate tax and an inheritance tax. To further complicate matters, laws in some countries call what is in fact an estate tax an inheritance tax. In this paper, for simplicity we will refer to all forms of bequest taxation as inheritance taxes, and we will combine the taxes where necessary to determine the total amount of inheritance taxes at a given time.[11]

Our interest in inheritance taxes is based on the fact that they are a potentially important policy instrument for progressively funding the government and for influencing the distribution of wealth. If democratic governments wanted to redistribute wealth, or at the very least require the rich to pay a progressive proportion of the state's budget, inheritance taxes have long been recognized as an obvious policy instrument for achieving this goal. Given this

[11] Note that inheritance tax laws vary in many other features as well, and these features can have an impact on how much tax is actually paid. Rules for valuing estates can vary substantially across countries and time, affecting the impact of the laws on government revenue and inherited wealth.

goal, we focus on measuring the key feature of inheritance taxation that captures the burden of the tax on a country's wealthiest citizens—the top marginal rate for a direct descendant.[12] Obviously, the top rate captures the extent of the tax on the largest fortunes but importantly, given the existence of exemptions which means that the inheritance tax rate for the poor was zero, the top rate also provides a measure of the progressivity of inheritance taxation in each country. We focus on direct descendants because these were the most common beneficiaries, and it is the tax on the direct descendants that would have the biggest impact on government revenues and the distribution of wealth.[13] Ideally, in our analysis we would also be able to take account of the manner of which laws in some countries allow individuals to evade inheritance or estate taxes, such as by making untaxed gifts to children or by forming trusts, but at this stage we are unable to do so. The inclusion of country fixed effects in our statistical estimates will, however, control for constant features of a country's laws or legal system that facilitate such efforts.

Figure 1 presents our data for the top marginal tax rate for the nineteen countries in our sample over the period from 1816 (or the date of national independence) to 2000. The sources for these data vary, but we primarily rely on the legislation itself or other government sources. In most cases, we have been able to check our series with the secondary literature that focuses on inheritance taxation in a particular country. The data appendix to this paper describes our sources in detail. The graphs reveal several interesting patterns. First, from the beginning of the 19th century through the first decade of the 20th century, the taxation of direct descendants was rather limited. A number of countries had inheritance taxes but the rates are typically about 1% or less. Second, the 20th century was marked by tremendous variation over time and across countries. For example, Canada went from having no federal inheritance tax to a top marginal tax rate of over 50% to a repeal of the tax. In 2000, there were four countries—Australia, Canada, New Zealand, and Switzerland—without a national inheritance tax, but also six countries—France, Ireland, Japan, Korea, the U.K. and the

[12] More precisely, to make the data more comparable across countries, we focus on the top rate applied to a single descendant who inherits cash only.

[13] Importantly, the rates for direct descendants and spouses are often the same and, if not, tend to be quite similar.

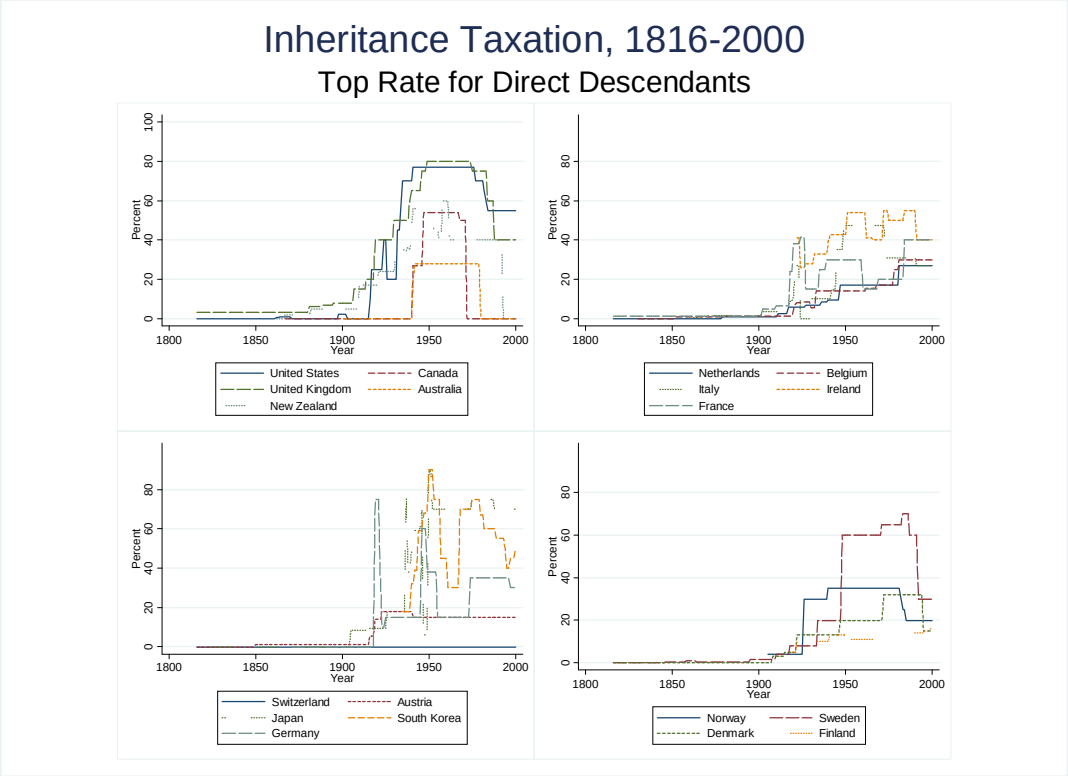

Figure 1: Inheritance Taxation, 1816-2000. This figure records the top marginal inheritance tax rate for direct descendants from 1816 (or independence) to 2000. See data appendix and text for full description of rate definitions and sources.

U.S.— with top marginal rates of 40% or higher.[14]

The figure also allows us to begin considering the potential effect of suffrage extensions on inheritance taxation. The striking answer provided by the data is that expansion of the franchise and democratic government are often in place for decades before inheritance taxes with high marginal rates are adopted. In other instances progressive inheritance taxation significantly precedes universal suffrage. France first established universal male suffrage in 1848, and the country was arguably a full democracy from 1870, yet it did not makes its long-standing inheritance tax progressive until 1902, and it did not adopt a top marginal rate exceeding 10% until 1918. Japan, in contrast, adopted a progressive inheritance tax in 1905

[14] See Duff (2005) for an analysis of the political context for inheritance tax abolition. It is also worth noting that both Austria and Sweden have abolished their inheritance taxes after 2000 when our sample period ends.

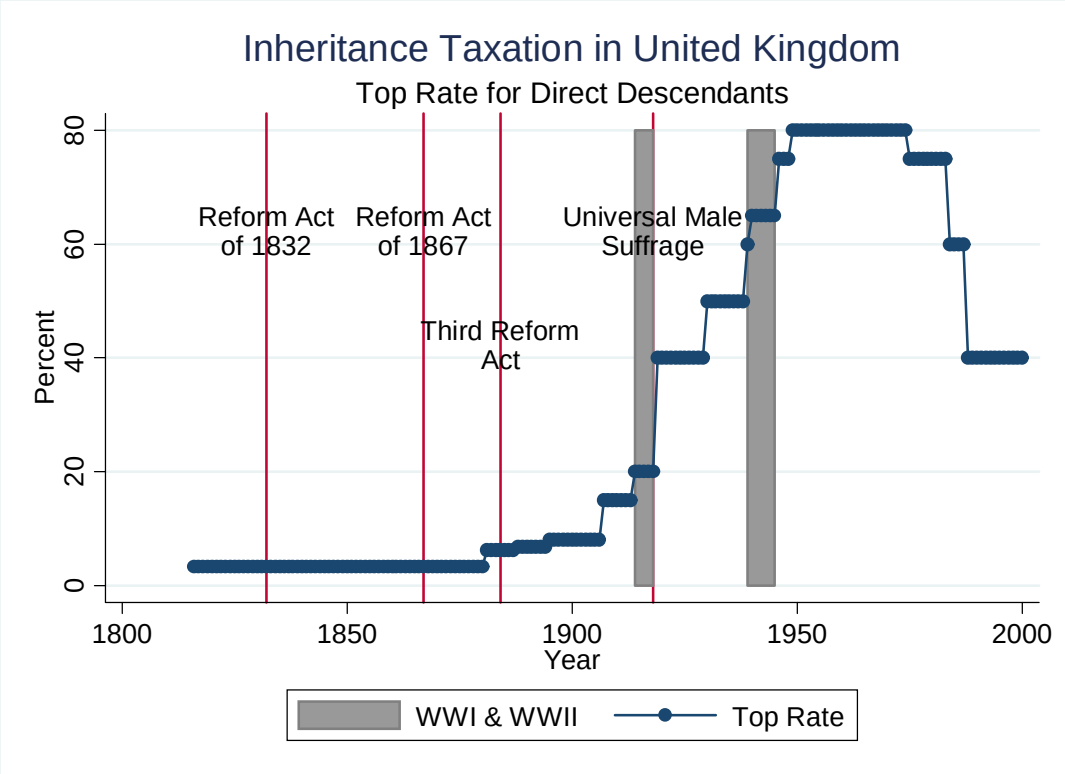

Figure 2: Inheritance Taxation in the United Kingdom. This figure plots the top marginal rate of inheritance taxation for direct descendants from 1816 to 2000 in the U.K. along with the major acts which expanded the franchise and the U.K.'s participation in world wars.

in conjunction with the Russo-Japanese war, some two decades prior to the establishment of universal suffrage in that country.

2.1 Inheritance Taxation in Sweden and the United Kingdom

Figures 2 and 3 plot the top rate of inheritance taxation for direct descendants in Sweden and the United Kingdom while highlighting dates for major extensions of the franchise and participation in World War I and II. The data in these two graphs suggest two important patterns of policymaking.

First, extensions of the franchise, even when they make most of the adult male population eligible to vote, do not result in high rates of inheritance taxation, certainly not in the short to medium term. In the case of the U.K., the Reform Act of 1832 reduced and

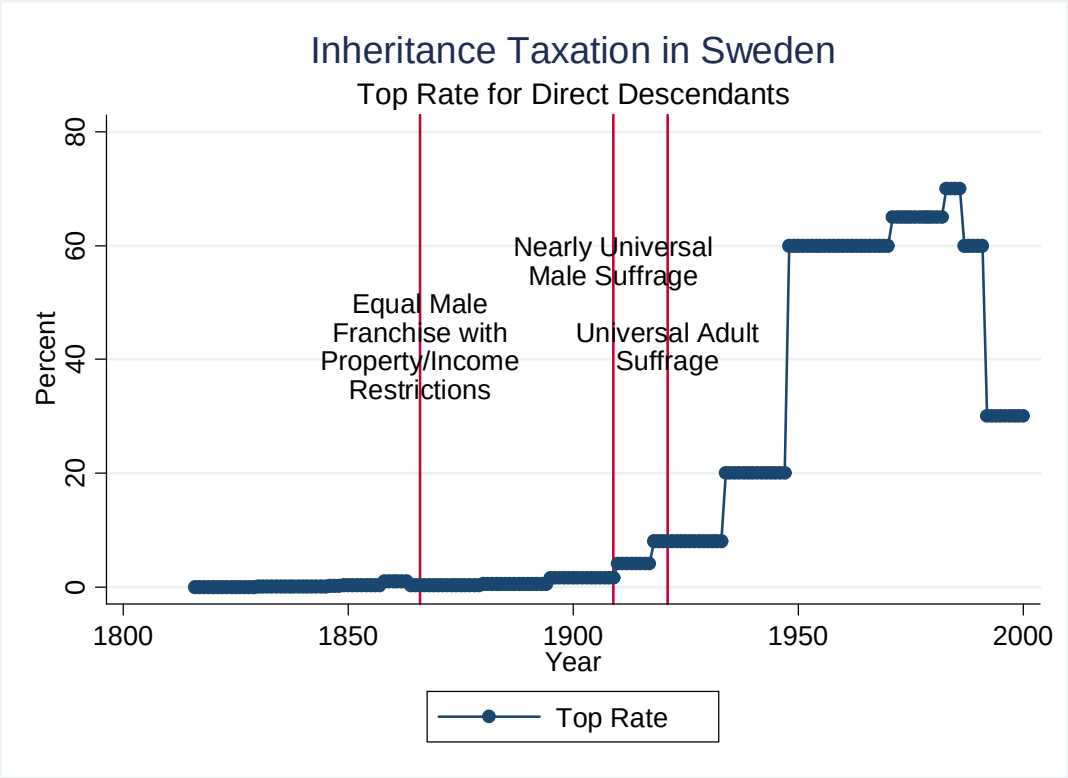

Figure 3: Inheritance Taxation in Sweden. This figure plots the top marginal rate of inheritance taxation for direct descendants from 1816 to 2000 in Sweden along with the dates of major expansions of the franchise.

standardized income and property qualifications leading to a small but important expansion of the franchise. The Reform Act of 1867 further reduced these requirements for England and Scotland and the Third Reform Act in 1884 introduced uniform franchise requirements in all of the United Kingdom and again reduced the income and property restrictions. At this point a majority of adult males, including the urban working class, were eligible to vote. By 1910, eligibility was at 88 percent of the adult male population and universal adult male suffrage was enacted in 1918.[15] Inheritance taxes in the U.K remained in the single digits until 1907 when the top rate was set at 15%, but it was not until 1919 that the U.K. adopted truly redistributive inheritance taxes with a top rate of 40%. While it may be tempting to point to the association between universal suffrage in 1918 and the setting of high rates in 1919, this is misleading because the move to universal suffrage in the U.K. was the end of a very long and gradual extension of the franchise. If the extension of the franchise was driving the setting of high inheritance tax rates in the U.K., we should have observed increases during many, if not all, of the significant expansions highlighted in Figure 2, but we do not. The evidence for Sweden is even more compelling. Sweden enacted equal but restricted suffrage in 1866 with relatively high economic qualifications.[16] The major expansion of the franchise for Sweden came with the 1907 electoral reform, which also established proportional representation. This abolished the property requirement and established nearly universal male suffrage. Figure 3 shows that the top rate of inheritance taxation was not raised substantially until the 1930s, and even then it was only set at 20%.

As a second pattern, a comparison across time for the U.K. and between the U.K. and Swedish cases suggests that mobilization for both world wars had a substantial impact on inheritance taxation. The U.K. did not adopt high inheritance tax rates (40%) until the end of World War I. In the U.K. once adopted, inheritance tax rates remained high and were raised somewhat during the interwar period. But it was only the onset of World War II that sent rates above 60, and eventually to 80 percent. The contrast with the Swedish case is stark. Sweden did not participate in World War I and there was no significant increase in

[15] Mackie and Rose (1991).
[16] Flora (1983).

inheritance tax until two decades later, and even this new rate was less than half the rate prevailing in the U.K. during the same period. Similarly, Sweden, which did not participate in World War II either, did not experience the same increases as the U.K. in the 1940s.

2.2 Interpreting the Evidence

The bulk of our analysis relies on using the top marginal rate of inheritance taxation. This choice was motivated by the need to make data collection feasible, by the fact that top rates can provide a useful measure of progressivity, and finally by the fact that it is inherently interesting to investigate the rate at which a society taxes its wealthiest citizens. Before proceeding further, however, we need to consider three questions about using this evidence to test propositions about the progressivity of inheritance taxation.

(1) Do people pay? - A first question is whether possibilities for engaging in fraud, in inter vivos transfers, or in exploiting legal loopholes have rendered the top rates of inheritance taxation essentially meaningless. Regarding the possibility of fraud, while this certainly exists, we have already emphasized how inheritance taxation inherently requires less administrative enforcement capacity than does an income tax. Regarding inter vivos transfers it is important to emphasize that most of the countries in our sample moved quickly to establish a gift tax on inter vivos transfers once they began to apply significant marginal rates of inheritance taxation. It is also known, at least for the United States, that even the majority of households that, because of their wealth, are likely to be subject to the estate tax do not avail themselves of opportunities for making significant inter vivos gifts of the form that could reduce their overall eventual tax liability (Poterba, 1998). In other words, the behavioral responses to estate taxes are weaker than some have assumed. Finally, regarding opportunities for exploiting what we have imprecisely called "legal loopholes," we have already emphasized that the top marginal rates we report do not take account of differences on how certain assets are valued or classified. A much more complete analysis of this issue would involve collection of evidence on actual revenues collected by type of estate, something that would be impractical for a nineteen-country sample. We have, however, collected data

on the total volume of inheritance taxes for five of our sample countries. This evidence can be used to demonstrate that historically, in each of the five countries sharp increases in top rates of inheritance taxation have been associated with sharp increases in total revenues collected from inheritance taxation.

(2) How many people paid and were these taxes actually progressive? - Even if tax avoidance possibilities are less present or less attractive than are sometimes imagined, there remains the issue of whether we are focusing on a symbolic tax paid by a very small number of individuals that may say very little about the overall progressivity of inheritance taxation in a country. To consider this possibility, we collected more complete evidence on inheritance tax schedules in six of our sample countries. We then used this to calculate the marginal tax rate faced by estates of different values, expressed as a ratio of estimated per capita GDP. The results of this exercise are presented in Table 1. A glance at the table also suggests a clear difference between participants and non-participants in each of the two world wars. If we focus on countries that mobilized heavily for World War I (the UK, US, and France) and on those that did not (Sweden, Japan, and the Netherlands) we see that marginal tax rates in 1925 remained similar across these two groups for estates up to the size of 100 times per capita GDP. The big difference between war participants and non-participants is observed for larger fortunes. Though we have data on the wealth distribution for only a few countries for a few years within our sample, the pattern appears to correspond to a situation where the top percentile of the wealth distribution was taxed more heavily in war participant countries while the bottom 99 percent of the distribution was taxed at broadly similar rates. Now consider the pattern of inheritance taxation in 1950, just a few years after the end of World War II. In three of the countries that mobilized significantly for World War II, there was a further dramatic increase in marginal tax rates on large fortunes with more modest increases in marginal rates applying to small fortunes. France was an exception to this pattern, something that may be attributable to its much different history of war time mobilization. Among the two countries that did not mobilize heavily for World War II,

Country	Estate Size	1850	1900	1925	1950	1975	2000
United Kingdom	1	0.0	0.0	**0.0**	**0.0**	0.0	0.0
	10	2.5	1.0	**2.0**	**1.0**	5.0	0.0
	100	4.1	3.0	**4.0**	**15**	43	40
	1000	3.4	4.5	**19**	**60**	70	40
	10,000	3.1	7.0	**29**	**80**	75	40
United States	1	0.0	0.0	**1.0**	**3.0**	7.0	22
	10	0.0	0.0	**1.0**	**11**	28	34
	100	0.0	0.8	**2.0**	**30**	37	55
	1000	0.0	1.5	**12**	**45**	73	55
	10,000	0.0	2.3	**30**	**77**	77	55
France	1	1.2	1.3	**4.8**	**15**	5	0
	10	1.2	1.3	**9.6**	**25**	20	0
	100	1.2	1.3	**18**	**30**	20	40
	1000	1.2	1.3	**34**	**30**	20	40
	10,000	1.2	1.3	**42**	**30**	20	40
Japan	1	0.0	1.5	1.0	**25**	10	15
	10	0.0	1.5	1.2	**30**	25	25
	100	0.0	2.0	2	**60**	55	50
	1000	0.0	4.5	5.5	**85**	75	70
	10,000	0.0	7.0	9.5	**90**	75	70
Sweden	1	0.1	0.5	0.6	2.0	10	10
	10	0.1	0.7	1.8	7.0	44	30
	100	0.2	1.3	3.4	40	58	30
	1000	0.3	1.5	8.0	52	65	30
	10,000	0.3	1.5	8.0	60	65	30
Netherlands	1	0.0	0.0	1.5	4.0	7.0	8.0
	10	0.0	1.0	3.0	7.0	13	23
	100	0.0	1.0	4.5	13	17	27
	1000	0.0	1.0	6.0	17	17	27
	10,000	0.0	1.0	6.0	17	17	27

Table 1: Marginal Tax Rates Applying to Estates of Different Sizes. Estate Sizes are measured as a multiple of per capita GDP. In cases where a country had not yet established an inheritance tax, the marginal rate is listed as 0.0. For Japan rates listed for 1900 are those enacted in 1905. Tax rates for periods immediately following mass mobilization for war are highlighted in bold.

marginal tax rates on large fortunes increased only slightly in the Netherlands. In Sweden they increased more substantially. Overall then, there seems to be a pattern whereby mass mobilization for war was associated with increases in taxation on the top percentile of the wealth distribution.

(3) Did governments without high inheritance tax rates simply choose to tax wealth in other ways? - There are different ways of taxing wealth, and we ought to consider the possibility that governments which chose to adopt relatively low rates of inheritance taxation compensated for this by using other mechanisms to tax large fortunes heavily. During the nineteenth century it was common for governments to levy direct taxes on visible and easily observable manifestations of wealth - such as a tax on property indexed according to a house's number of windows. But taxes of this form were not targeted at the largest fortunes in a society, and in no case were they equivalent to an inheritance tax of the sort that emerged after World War I. A more significant potential concern involves the net wealth taxes collected by many countries during the twentieth century, including those in Scandinavia in particular. It is certainly the case that by the 1970s these taxes were levied at rates that could have a significant effect on wealth accumulation, but it was also almost invariably true that in the first half of the twentieth century the marginal rates of these net wealth taxes were very low. Furthermore, when higher marginal rates of net wealth taxation were implemented, this shift was accompanied by the establishment of ceilings specifying that an individual or family would not pay more than a set percentage of their annual income in the form of net wealth taxes and income taxes. The existence of these ceilings reduced the discrepancy between countries with only an inheritance tax and income tax on the one hand and those that had an inheritance tax, an income tax, and a net wealth tax, on the other.

3 Econometric Model

In this section, we describe our econometric models for evaluating the effects of democratization and war mobilization on the taxation of inherited wealth. We focus our attention on our two main empirical strategies but also briefly describe several alternative approaches

that we adopt to evaluate the robustness of our results.

Each of our strategies requires a measure of democratization and war mobilization. To measure democracy, we focus our discussion on three variables. The first measure, *Universal Male Suffrage,* is set equal to one for years in which all adult males are eligible to vote in national elections and zero otherwise.[17] This variable focuses on the feature of democracy of most direct interest theoretically, the eligibility of poor voters to participate in elections. While suffrage is clearly central to most arguments about why democracy might affect the taxation of inherited wealth, other features of democratic government could also be influential. One possibility is that competitive elections with or without a full expansion of the franchise will lead to greater taxation of inherited wealth. Our second measure, *Boix-Rosato,* 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.[18] Our third measure, *No Upper*, is equal to one for the absence of an upper house with veto power for which representatives are either not directly elected, elected by a restricted constituency, appointed, or who sit by hereditary right. This allows us to evaluate the possibility that the presence of a non-democratic check on the policymaking process prevents increased taxation of inherited wealth.

Although we think these measures capture well the main institutional features of democratic political institutions, we consider a number of other possibilities and report results of these analyses in the appendix. For example, one potential limitation of the universal male suffrage measure is that it is insensitive to potentially important expansions of the franchise that fall short of universal suffrage. An alternative set of measures that we construct, *Electorate 25*, *Electorate 50*, and *Electorate 75*, are set respectively equal to one if 25%, 50%, 75% or more of adult males are eligible to vote and zero otherwise. This allows us to evaluate the

[17] The source for this variable is Caramani (2000) for the European cases and Mackie and Rose (1973) otherwise.

[18] This follows the definition used by Boix and Rosato (2001), which is a modification of the definition used by Przeworski et al. (2000) to a context where the suffrage may be restricted.

impact of expansions of the franchise that lead to less than universal suffrage.[19] Another possibility is that for poorer economic groups to pressure their representatives to tax the rich, ballots need to be confidential. The variable *Secret* is equal to one if the country uses a secret ballot for lower house elections and zero if not.[20] We also investigate whether it is simply the introduction of direct elections for the lower house that moves countries to tax inherited wealth at higher rates by constructing the variable *Direct Elections* equal to one if a country has direct elections for the lower house and zero if not.

To indicate whether or not a country engaged in mass warfare between 1816 to 2000, we constructed the dummy variable *War Mobilization* equal to 1 if in a particular year the country was engaged in an interstate war and at least 2 percent of the population was serving in the military.[21] This variable measures well the key characteristics necessary for conflict to have its hypothesized effect on taxing inherited wealth. There must be a war fought in which the citizens who fight in the conflict sacrifice not only their time and livelihood but also risk their lives. It must also be a conflict that involves a significant proportion of the population. This operationalization captures high mobilization years during the Franco-Prussian War, First World War, Second World War, and Korean War.[22]

Our first econometric model employs the following generalized differences-in-differences framework:

$$T_{it} = \alpha + \beta_1 D_{it-1} + \beta_2 W_{it-1} + \boldsymbol{\gamma}\mathbf{X}_{it-1} + \eta_i + \theta_t + \varepsilon_{it}$$

[19] The source for this data is Flora (1983) for the European cases and the *Statistical History of the American Electorate* for the US.

[20] The sources for this variable are Caramani (2000) and Mackie and Rose (1973).

[21] Our data for incidents of war comes from the 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).

[22] More precisely, our war mobilization variable is coded one for Austria in 1915, 1941-1945; Belgium in 1915-1918; for Australia in 1941-1945 (mobilization data is missing for Australia before 1920 and these years are not included in the analysis for this measure); for Canada in 1941-1945 (mobilization data is missing for Canada before 1920 and these years are not included in the analysis for this measure); for Finland in 1940-1944; for France in 1871, 1914-1920, 1940-1943; for Germany in 1871, 1915-1918, 1939-1945; for Italy in 1916-1918, 1935, 1940-43; for Japan in 1941-1945; for the Netherlands in 1951-1952; for New Zealand in 1941-1945 (mobilization data is missing for New Zealand before 1920 and these years are not included in the analysis for this measure); for South Korea in 1953, 1965, 1967-68, 1970; for the UK in 1915-1918, 1940-1945; and for the US in 1918, 1942-1945, 1951-1953.

where i indexes each country and t indexes the time period; T is the top inheritance tax rate for direct descendants discussed in the previous section; D is one of the several measures of the extent of democracy described above, W is our measure of participation in mass warfare; X_{it} is a vector of control variables and is excluded in some specifications[23]; α, β, and γ are parameters to be estimated; η_i are country fixed effects parameters also to be estimated; θ_t are period fixed effects parameters; and ϵ_{it} is the error term.[24] In some specifications, we also add individual linear time trends for each country to this model. We present the ordinary least squares estimates of this model and report country clustered standard errors to account for within-country correlations including serial autocorrelation in our data. The primary hypotheses evaluated in this paper are that increases in democracy (variously measured) cause the adoption of higher inheritance taxes on the largest fortunes ($\beta_1 > 0$) and that mass mobilization for warfare also increases inheritance taxation ($\beta_2 > 0$).

Our estimates measure the causal effect of democracy and mass mobilization for warfare on the taxation of inherited wealth under the usual assumptions of the generalized differences-in-differences framework. The country fixed effects control for all time constant unobserved factors influencing the top rate of inheritance taxation in a given country. The time period fixed effects control for common shocks and trends in inheritance taxation. Moreover, in some specifications we control for the time-varying factors of government partisanship and levels of development and include country-specific time trends. That said, it is, of course, possible for the assumptions of the model to be violated in a way that generates correlations

[23] Specifically, we add controls for partisan control of the government and GDP per capita. The idea that partisanship may influence the extent to which countries tax inherited wealth is a straightforward extension of the democratization argument. The claim is simply that it is only once left parties gain control of government that countries adopt significant taxes on inherited wealth. We include lagged values of the variable *Left Executive* equal to one if the head of government is from a socialist or social democratic party and zero otherwise in some of our specifications. Some caution should be made in interpreting the estimates in regressions including this variable because an observed correlation between partisanship and inheritance tax rates may simply indicate that some unobserved third factor leads countries to choose both left governments and significant taxation of inherited wealth. Moreover, left partisanship may be a consequences of both democracy and war which would make it an inappropriate control variable for regressions primarily designed to estimate the effect of these two variables. The inclusion of the variable real *GDP per capita* controls for the possibility that countries at different levels of development choose different levels of inheritance taxation. We evaluated several potential functional forms for this relationship including adding a squared term and taking the natural log but there was no evidence that these alternatives fit the data better. The source for the real GDP per capita measure is Angus Maddison, Historical Statistics of the World Economy, `http://www.ggdc.net/maddison/`.

[24] We omit one country and year due to the constant.

between the error term and our key independent variables that would bias our results.

For example, our estimates of $\beta_1$would be inconsistent if there are time-varying unobserved factors that influence inheritance taxation and are correlated with our democracy measures. That said, most of the plausible unobservables based on the existing literature would suggest a positive correlation between democracy and the error term—that is factors that would lead countries both to adopt democratic institutions and tax the rich at a higher rate. Such a correlation would suggest that our estimates, if inconsistent, are biased in a positive direction and as such we have, if anything, overestimated the effect of democracy on top inheritance tax rates. Unfortunately, it is probably not, however, plausible to treat our estimates solely as an upper bound of the effect of democracy on top inheritance tax rates. Specifically, there is the possibility of reverse causality in which a country under a nondemocratic form of government adopts higher taxes of inherited wealth in order to avoid having to democratize (see e.g. Acemoglu and Robinson 2006). Such a relationship would tend to bias our estimates in a negative direction, leading us to underestimate the positive effect of democracy on inheritance taxation.

In the case of our estimates of the effect of war mobilization on the top rate of inheritance taxation, β_2, we have the same general concerns. Specifically, it is possible that countries select into war participation in part because of their beliefs about their ability to finance the war by taxing the rich generally and inherited wealth in particular. This would bias our estimates in a positive direction and lead us to overestimate the effect of war on inheritance taxation. There are several reasons that we are skeptical about the importance of this potential selection issue. 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. Great powers such as Britain, France, and Germany remained active in international military competition whereas countries like Sweden and the Netherlands essentially opted out of international military competition from an early date. While these decisions may have depended on financing considerations, they are arguably fixed during the period of our study and are captured by the country fixed effects and country-specific time

trends. Moreover, 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. 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 expected the length of the conflict or the extent of mobilization necessary to fight the war.[25] [26]

Although, we have collected a data set with annual frequency from 1816 to 2000, we do not know a priori how long it may take for democratization or war mobilization to influence policy choices. Consequently, we focus our analysis on specifications with observations spaced at five and ten year intervals while reporting results for several annual specifications in the appendix. The dependent variable in the five and ten year interval analyses is from the first year of a given period. The independent variables are averages from the previous period. Given the infrequency of mass war mobilization, it is essential to measure the presence of war mobilization for the entire preceding period rather than simply the initial year of the preceding period. Moreover, for both democracy and war mobilization, we expect a more substantial effect the greater the number of years in the preceding period that were either democratic or mobilized for war.

Our second econometric model takes the following form:

$$T_{it} = \alpha + \rho T_{it-1} + \beta_1 D_{it-1} + \beta_2 W_{it-1} + \boldsymbol{\gamma}\mathbf{X}_{it-1} + \theta_t + \varepsilon_{it}$$

There are essentially two differences between this model and our initial approach. This specification adds the lagged dependent variable and deletes the country fixed effects. This model takes an alternative strategy to concerns about potential time-varying unobservables

[25] The often cited quote from Kaiser Wilhem to the departing troops in August 1914 is "You will be home before the leaves have fallen from the trees." Even U.S. entry into World War I does not seem likely to be a result of such a selection mechanism. Wilson won the 1916 election on a slogan of "he kept us out of war" and likely would have never entered the war if it were not for Germany's tactical decision to implement unrestricted submarine warfare.

[26] It is further worth noting that the sources of possible bias for the estimates of the parameters for democracy (war mobilization) could also bias our estimates for war mobilization (democracy). This, however, seems unlikely to be important in this setting as our estimates for democracy (war mobilization) are qualitatively the same when we exclude war mobilization (democracy).

which might bias our estimates of β_1 and β_2 by conditioning on the lagged value of the top rate of inheritance taxation. In this specification, we base our estimates on comparisons between democracies and non-democracies and mobilizers for war and non-mobilizers conditioning on a country's most recent tax policies, time period fixed effects to control for common shocks, and our other time-varying controls. As before, in some specifications we also add individual linear time trends for each country to this model. The country fixed effects are omitted here because OLS estimates are biased in models with a lagged dependent variable and fixed effects. We present the ordinary least squares estimates of this model and report panel corrected standard errors to account for country heterogeneity and cross-country correlations in our data.[27]

Generally, the same issues discussed for the first model are potential sources of bias for this second specification. The exception to this is that the inclusion of the lagged dependent variable controls for a number of potential time-varying unobservables that we might be concerned about, but, of course, dropping the fixed effects opens up a new set of concerns. Angrist and Pischke (2009) note that the different identifying assumptions in our two models can, under some simple assumptions about the sources of selection, be considered to bound our estimates of the positive treatment effects.

4 Estimation Results

Tables 2, 3, and 4 report the results for our main analyses. The first three columns in each table report the results of our fixed effects specifications for our five-year panels. Column (1) excludes our time varying control variables, column (2) includes them, and column (3) adds country-specific time trends. Columns (4)-(6) in each table report the results of our lagged dependent variable specifications also for our five-year panels. Column (4) excludes our time varying control variables, column (5) includes them, and column (6) adds country-

[27] The appendix reports results for specifications which include both a lagged dependent variable and country and time fixed effects. Although biased, the OLS estimator is consistent as the number of periods goes to infinity which, given our somewhat long time series, may justify consideration of the estimates for this specification. The main substantive findings discussed in the text hold for these alternative specifications.

specific time trends. Columns (7) and (8) report results for our ten-year interval panels for the fixed effects specification (with time-varying control variables and country-specific time trends) and the lagged dependent variable specification (also with time-varying control variables and country-specific time trends). Table 2 employs our *Universal Male Suffrage* measure of democracy, Table 3 uses the *Boix-Rosato* measure, and Table 4 includes the *No Upper* variable.

The estimates in Table 2 provide no evidence consistent with the idea that expansion of the franchise increased the taxation of inherited wealth. The estimated coefficient for *Universal Male Suffrage*$_{t-1}$ is positive in columns (1), (2), (6), and (8) but negative in columns (3), (4), (5), and (7). None of the positive estimates approach statistical significance at conventional levels and the magnitudes of the estimates are not particularly large. Importantly, for the five-year panels, the two specifications that include time varying controls yield estimates of less than one and relatively large standard errors (the fixed effects estimate is -0.634 with a standard error of 4.097 and the lagged dependent variable estimate is 0.620 with a standard error of 1.671). The results for the ten-year panels are qualitatively the same though the lagged dependent variable specification yields a slightly larger positive coefficient of 3.593 (with a standard error of 2.846). While the confidence intervals for these estimates are too wide for us to exclude the possibility of a substantively meaningful effect for *Universal Male Suffrage*$_{t-1}$, none of the results are consistent with a substantively and statistically significant positive effect of democratization on the top marginal rate of inheritance taxation.

Although we will discuss most of our robustness checks below, it is worth noting two measurement issues here. First, in unreported regressions, we obtained very similar results when using a dummy variable for countries with universal and equal male suffrage, that is excluding from the "democratic" years cases where there was universal suffrage but a plural voting system. As discussed in the previous section, we also evaluated the impact of expansions of the franchise that lead to less than universal suffrage by including the variables *Electorate 25*$_{t-1}$, *Electorate 50*$_{t-1}$, and *Electorate 75*$_{t-1}$ as our measure of the extent of suffrage. These results are reported in the Appendix in Table A-3 and also fail to provide

	5-year Data						10-year Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top Rate$_{t-1}$				0.866	0.868	0.656		0.359
				(0.040)	(0.037)	(0.063)		(0.128)
				0.000	0.000	0.000		0.005
War Mobilizaton$_{t-1}$	23.379	21.368	20.083	17.884	17.898	16.517	30.074	26.774
	(6.046)	(5.803)	(5.765)	(3.913)	(4.021)	(4.219)	(12.007)	(11.103)
	0.001	0.002	0.003	0.000	0.000	0.000	0.022	0.016
Universal Male Suffrage$_{t-1}$	4.212	7.313	-0.634	-2.921	-3.399	0.620	-0.189	3.593
	(7.202)	(6.704)	(4.097)	(1.553)	(1.564)	(1.671)	(5.264)	(2.846)
	0.566	0.290	0.879	0.060	0.030	0.711	0.972	0.207
Left Executive$_{t-1}$		0.558	4.271		3.391	4.577	5.750	4.703
		(5.544)	(3.638)		(1.615)	(1.677)	(6.070)	(3.094)
		0.921	0.256		0.036	0.006	0.356	0.128
GDP per capita$_{t-1}$		0.001	0.000		-0.000	0.001	0.001	0.001
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)
		0.532	0.722		0.496	0.072	0.588	0.094
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No
R-squared	0.713	0.723	0.842	0.878	0.876	0.892	0.848	0.840
Number of Observations	510	489	489	509	488	488	240	239

Table 2: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Universal Male Suffrage Measure of Democracy.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and the variable *Universal Male Suffrage* lagged one period. The specifications in columns 1-3 and 7 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6 and 8 include a lagged dependent variable and report panel corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-8 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

any evidence consistent with the hypothesized effect of democratization. The key result that can be inferred from these estimates is that there is no evidence that expanding the franchise increases the top rate of inheritance taxation in this data.

In contrast, the estimates in Table 2 are consistent with a substantively and statistically significant positive effect of war mobilization on the top rate of inheritance tax. Across all eight specifications reported, the coefficient estimate for the variable *War Mobilization*$_{t-1}$ is positive and statistically significant. In the fixed effects specifications for the 5-year panels, the coefficient estimates range between 20.083 and 23.379 with relatively small standard errors. This indicates that, all else equal, a country that mobilized for mass warfare for an entire five year period increased its top inheritance tax rate by 20 to 23 percentage points compared to a country that did not mobilize for war. This implies, of course, that a shorter conflict of one or two years would be associated with a 4 to 10 percentage point increase which while smaller is still substantively significant. The coefficient estimates for the five-year panels with a lagged dependent variable are between 16.517 and 17.898, again with relatively small standard errors. It is worth noting that the implied long-run effect of mobilization would be much larger in these estimates (between 48 and 134), but we do not focus on this quantity of interest because our approach is to use the lagged dependent variable to increase the plausibility that there are no important differences between the country time periods with and without war mobilization, allowing us to interpret our estimates of β_2 as measuring the causal effect. The estimates for the ten-year panels are qualitatively the same.[28]

[28] The coefficient estimates for our time-varying control variables merit some discussion. The results for partisanship as measured by *Left Executive*$_{t-1}$ are mixed. In the fixed effects specifications reported in columns (2), (3), and (7) of each table, the estimates are positive but they are imprecisely estimated with relatively large standard errors. In the lagged dependent variable specifications reported in columns (5), (6), and (8), however, the estimates are positive and statistically significant (though only marginally so in the ten-year panels). This finding is consistent with the idea that left governments representing relatively poorer constituents were more likely to implement higher taxes on inherited wealth. Overall, the mixed evidence is consistent with the qualitative pattern that we observe in closer analyses of the cases. Certainly, in some countries important increases and decreases seem to have followed a partisan logic, but there are many examples of right governments increasing the top rate of inheritance taxation and left governments decreasing or even eliminating the tax altogether. That said, we again caution against too much attention to this result, because left partisan control is not exogenously assigned to a country and is likely influenced by various time changing characteristics of a country, some observed and some not. Importantly, the empirical record suggests that wars themselves often make left partisan control more likely. As such, partisanship may be, in part, another mechanism by which wars influence inheritance taxation. The biggest concern about time-varying characteristics of countries, though, is the possibility of unobserved factors that may influence both partisan

	5-year Data						10-year Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top Rate$_{t-1}$				0.871	0.877	0.660		0.382
				(0.039)	(0.037)	(0.062)		(0.126)
				0.000	0.000	0.000		0.002
War Mobilizaton$_{t-1}$	23.860	23.278	20.126	16.869	16.539	16.479	29.808	27.549
	(6.183)	(6.309)	(5.948)	(4.002)	(4.172)	(4.308)	(12.463)	(11.359)
	0.001	0.002	0.003	0.000	0.000	0.000	0.028	0.015
Boix-Rosato$_{t-1}$	0.071	3.118	0.380	-1.424	-1.899	-0.462	-0.774	-0.106
	(7.856)	(6.191)	(2.870)	(1.287)	(1.207)	(1.225)	(3.711)	(2.062)
	0.993	0.621	0.896	0.272	0.115	0.706	0.837	0.959
Left Executive$_{t-1}$		0.497	4.193		3.213	4.691	5.835	5.118
		(5.581)	(3.758)		(1.615)	(1.708)	(6.328)	(3.235)
		0.930	0.279		0.036	0.006	0.369	0.114
GDP per capita$_{t-1}$		0.001	0.000		-0.000	0.001	0.001	0.001
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)
		0.599	0.738		0.941	0.083	0.600	0.164
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No
R-squared	0.711	0.719	0.842	0.877	0.875	0.892	0.848	0.839
Number of Observations	510	489	489	509	488	488	240	239

Table 3: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Boix-Rosato Measure of Democracy.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and the variable *Boix-Rosato* lagged one period. The specifications in columns 1-3 and 7 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6 and 8 include a lagged dependent variable and report panel corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-8 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

The results in Table 3, which employs the *Boix-Rosato* measure of democracy, follow those in Table 2 extremely closely. The coefficient estimates are negative (the wrong sign) and statistically insignificant at conventional levels in all but three specifications. In the fixed effects specifications that do have positive signs, the estimates are small in magnitude and again statistically insignificant. There is simply no evidence in these results consistent with the argument that democratization increases the top rate of inheritance taxation. The coefficient estimates for *War Mobilization*$_{t-1}$ closely mirror the estimates in Table 2, providing further evidence for the war mobilization effect. This pattern of null results for democracy measures and stable positive estimates for war mobilization is also evident in the appendix Tables A-1 and A-2, which employ the *Secret* and *Direct Elections* measures of democracy. All of these estimates follow quite closely those reported in Tables 2 and 3.

The one partial exception to the pattern of results that we have observed so far is for specifications which include the *No Upper* measure of democracy. These estimates are reported in Table 4. The first thing to note about the results is that the coefficient estimates for *War Mobilization*$_{t-1}$ are positive and statistically significant across all eight specifications. Moreover, the magnitudes of the estimates are, if anything, slightly larger than in Tables 2 and 3. Thus, the evidence in Table 4 remains strongly consistent with the war mobilization hypothesis. What differs in Table 4 from Tables 2 and 3 is that the coefficient estimates for the variable *No Upper* are positive across all specifications and statistically significant in four of the eight specifications. In our fixed effects specifications, the pattern of results suggests some doubt that there is a robust finding for *No Upper*. The estimates in columns (1) and (2) are large and statistically significant but once country-specific time trends are added in column (3), the estimate is substantially smaller and not statistically significant. This suggests that the presence of a nondemocratic upper house is not correlated with the top rate of inheritance taxation once we control for country-specific time trends. Interestingly, the pattern is exactly the opposite in the lagged dependent variable specifications. While the estimates in columns

control and inheritance taxation and thus generate bias in our estimates. The coefficient estimates for our other time-varying control variable *GDP per capita*$_{t-1}$ are not consistently signed and are not statistically significant. We tried a number of functional forms for this variable but none of them yielded significant results.

	5-year Data						10-year Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top Rate$_{t-1}$				0.866	0.872	0.644		0.343
				(0.039)	(0.037)	(0.064)		(0.128)
				0.000	0.000	0.000		0.007
War Mobilizaton$_{t-1}$	27.593	26.027	21.772	20.295	20.099	19.924	31.176	30.102
	(6.088)	(6.600)	(6.158)	(3.406)	(4.172)	(3.686)	(12.473)	(11.454)
	0.000	0.001	0.002	0.000	0.000	0.000	0.022	0.009
No Upper$_{t-1}$	14.383	16.155	5.696	1.205	0.904	4.813	5.104	9.204
	(6.047)	(7.145)	(6.021)	(1.040)	(1.049)	(1.489)	(5.628)	(2.919)
	0.029	0.036	0.357	0.247	0.389	0.001	0.376	0.002
Left Executive$_{t-1}$		0.552	4.147		2.977	4.690	5.687	5.031
		(5.602)	(5.602)		(1.578)	(1.671)	(6.425)	(3.162)
		0.930	0.304		0.059	0.005	0.388	0.112
GDP per capita$_{t-1}$		0.001	0.000		-0.000	0.001	0.001	0.001
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)
		0.647	0.753		0.756	0.160	0.580	0.072
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No
R-squared	0.731	0.742	0.843	0.879	0.877	0.896	0.849	0.846
Number of Observations	509	488	488	508	487	487	240	239

Table 4: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: No Upper Measure of Democracy.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and the variable *No Upper* lagged one period. The specifications in columns 1-3 and 7 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6 and 8 include a lagged dependent variable and report panel corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-8 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

(4) and (5) are quite small and statistically insignificant, the estimate in column (6), which includes country-specific time-trends, is 4.813 with a standard error of 1.489. The difference in estimates between the specifications with fixed effects and country-specific time-trends and a lagged dependent variable and country-specific time trends is repeated for the ten-year panel results in columns (7) and (8). Overall, Table 4 presents some mixed evidence consistent with a somewhat alternative form of the democratization argument in which democratic politics may lead to higher taxation of inherited wealth but only once key veto points such as a nondemocratic upper house are democratized.

The evidence in Tables 2-4 strongly suggests that war mobilization increases the top rate of inheritance taxation but casts substantial doubt on the importance of democratic institutions. We evaluated the robustness of these results in several ways.

First, as discussed above we consider several alternative measures of democracy including the presence of a secret ballot, the existence of direct elections, and partial extensions of the franchise. Results for these measures are reported in Appendix Tables A-1, A-2, and A-3. Across all specifications the coefficient estimates for our war mobilization measure are positive, statistically significant, and very close in magnitude to those reported in Tables 2-4. Moreover, the democracy measures themselves are not significantly correlated with the top rate of inheritance taxation.

Second, we also considered two alternative measures of war mobilization. The first alternative is a dummy variable set equal to 1 if in a particular year the country was engaged in an interstate war and at least 5 percent of the population was serving in the military. As such, this measure is the same as our $War\ Mobilization_{t-1}$ variable except that the threshold has been adjusted from two to five percent. Estimating analogous specifications to those reported in Tables 2-4 yields estimates for the mobilization coefficient that are larger than those reported in Tables 2-4. For the fixed effects specifications, the difference is substantial—about 25% larger. This pattern of results is consistent with the main argument of this paper that greater mobilization for war increases the top rate of inheritance taxation. The second alternative measure of mobilization that we defined was based simply on a qualitative coding

of significant participation in World War I and World War II. The main advantage of this variable is that it allows us to include Australia, Canada, and New Zealand for the years prior to 1920 for which their mobilization data is missing in the Correlates of War data set. These specifications also yielded positive and statistically significant estimates for the war mobilization coefficient.

Third, one might be concerned that the results were driven by policy choices under occupation—e.g. U.S. occupation of Japan after World War II—rather than the result of an independent country setting policy. We reestimated our specifications in Tables 2-4 dropping any five-year or ten-year period for which a country was occupied during any year of the period. The results of these estimates closely mirrored our findings reported in Tables 2-4 for both our democracy measures and war mobilization.

Fourth, we investigated two arguments related to the war mobilization hypothesis. Thus far, we have maintained the assumption that both democratic and non-democratic governments may be compelled to tax inherited wealth at a higher rate in order to mobilize the population for war, particularly to the extent that those tax policies help to ensure equal sacrifice in the war effort. This assumption is justified to the extent that nondemocratic leaders have an incentive to set policies which make protests and revolutions less likely and encourage effort during times of war. That said, it is certainly possible that the war mobilization effect would be larger in democratic states because these leaders have a greater incentive to respond to the policy preferences of their citizens. Table A-5 reports results in columns (1)-(4) which test this argument by interacting the *Universal Male Suffrage*$_{t-1}$ and *Boix-Rosato*$_{t-1}$ measures of democracy with *War Mobilization*$_{t-1}$. If the war mobilization effect was stronger in democracies, we would expect a positive coefficient on the interaction term. The estimates are mixed across measures and specifications. The only statistically significant results for the interaction term are in the wrong direction and even these are sensitive to specification choices. Another argument related to the war mobilization hypothesis is that left governments, who were more likely to support the taxation of capital in the first place, adapted their policies to the changes in preferences induced by war more significantly.

Table A-5 reports results in columns (5) and (6) which test this argument by interacting the *Left Executive*$_{t-1}$ with *War Mobilization*$_{t-1}$. If the war mobilization effect was stronger under left governments, we would expect a positive coefficient on the interaction term. Our estimates, however, are of mixed signs and not statistically significant. This is consistent with the idea that although the left certainly supported the taxation of inherited wealth more than the right, governments of both the left and the right felt compelled to raise these taxes as a consequence of a country's mobilization for war.

Fifth, our two econometric approaches make particular assumptions about the data generating process and each would produce biased estimates under the assumptions of the other model. Consequently, a model with fixed effects and a lagged dependent variable is of obvious interest. We do not consider this in our main specifications because OLS estimates are biased in models with a lagged dependent variable and fixed effects. Nonetheless, the OLS estimator is consistent as the number of periods goes to infinity, which given our somewhat long time series may justify consideration of the estimates for this specification. Appendix Table A-4 reports estimates for specifications including a lagged dependent variable and country fixed effects.[29] The main results reported in Tables 2-4 hold for these alternative specifications. *War Mobilization*$_{t-1}$ is positively and significantly correlated with the top rate measure of inheritance taxation. None of the coefficient estimates for *Universal Male Suffrage*$_{t-1}$ and *Boix-Rosato*$_{t-1}$ are statistically significant or large in magnitude. Interestingly, there is evidence that the variable *No Upper* is positively correlated with the *Top Rate* measure. In the five-year panels, the point estimate for this coefficient is roughly 4. This indicates that, all else equal, a country lacking a nondemocratic upper house with veto power set its top inheritance tax rate about 4 percentage points higher than a country which had such an institution throughout the entire five-year interval. Keeping in mind that this estimate is somewhat sensitive to specification choices, it is the one piece of evidence consistent with a small but important democracy effect on inheritance taxation.

[29] We also estimated several specifications with fixed effects and a lagged dependent variable using Arellano and Bond's (1991) GMM estimator and found qualitatively similar results. It is not clear that this estimator, however, is a good fit for our data given that we only have 19 cross-sectional units.

Finally, in the appendix, Table A-6 reports results reestimating our main specifications using the annual data set. The results here closely mirror those reported in Tables 2-4. We also estimated regressions with the annual data which included each independent variable lagged five times and found these results to be consistent with our overall findings.

All of these considerations help support the claim that we observe a strong positive correlation between our measures of war mobilization and the top rate of inheritance taxation, but we do not generally observe a positive correlation between our democracy variables and the top rate. The results are also consistent with war mobilization having a positive causal effect on the top rate of inheritance taxation under the identifying assumptions of our two sets of econometric models. As discussed in the previous section, there are good reason to think these assumptions hold. Most importantly, once we control for country fixed effects, period fixed effects, and country-specific time trends, our greatest remaining concern should be time-varying unobserved factors that would lead countries to enter wars and tax inherited wealth at particular times. But as we discussed previously, the timing of mass conflicts seems generally unpredictable—driven by factors such as assassination, geography, and military technology—and unanticipated by many of the combatants.

5 Interpreting the War Result: Expediency or Mobilization?

We have so far presented evidence of a robust correlation between war mobilization and top marginal rates of inheritance taxation. This finding persists when using a variety of different strategies to control for unobserved effects that may be biasing our conclusion. So far, however, we have not attempted to adjudicate between the two different war mechanisms proposed in the introduction to this paper. The first is the expediency effect. Mass warfare is a time of crisis in which leaders may find it expedient to increase taxation of accumulated capital even if this has future reputational costs. If our results are primarily explained by the expediency effect then they would not reflect an effect of mass mobilization per se. They would simply reflect the fact that mass mobilization for war often coincides with other conditions that shorten the time horizon of policymakers. The second mechanism we proposed

is the mobilization effect. Governments that seek to mobilize a broad cross-section of their populations for war can face political demands for having holders of capital bear an increased share of the financial burden for a war. These demands are likely to involve appeals to fairness. If the wealthy are less likely to be compelled to serve on the front lines of a conflict, then it will be argued that their capital ought to be conscripted in the same manner as is the labor of others. We consider this mobilization effect to be directly dependent on the fraction of a country's population that is mobilized. In a war where a small percentage of a country's population is mobilized those who fight may well feel that the rich should bear a greater share of the financial burden, but they will be too few in number to have a significant impact on national politics. In a war with more general mobilization the political impact of these demands is likely to be much more significant.

How can we distinguish empirically between the mobilization effect and the expediency effect? This is not an easy task. Take the example of the demands that emerged in many countries following World War I for a one-time levy on capital. These often involved fairness claims of the sort we have just referred to above, but these appeals to fairness may simply have served as a cloak for a policy that was above all fiscally expedient in a time of crisis. In an ideal world, we would be able to conduct the following experiment – compare the pattern of taxation in a country that fights a war of mass mobilization with that of a country that fights a war in which a more limited fraction of the population is mobilized but which is equally expensive and which places an equal strain on public finances. Unfortunately for our study (but fortunately for humanity) the twentieth century did not, in addition to World Wars I and II, produce a set of wars of the second category that could be used to allow for this sort of comparison. However, looking further back in history to the previous set of major European wars, we can gain some purchase on this question. For France and Great Britain we will compare patterns of taxation, mobilization, and war expenditure during the Napoleonic Wars and during World War I.

In nominal terms the British and French governments spent far more in World War I than they had in any previous war. It might therefore seem logical to observe that the advent of

heavy inheritance taxation would appear in the wake of this unprecedentedly expensive war. A closer look at the evidence reveals a much different picture. When compared to potential revenues, as proxied by national output, the Napoleonic wars appear to have been just as expensive as World War I, and they occurred, at least in the British case, with a government that entered the war under much more desperate fiscal circumstances. The crucial difference may have instead been that at any one time during this conflict, neither France nor Great Britain mobilized more than about a fourth of the number of troops that they would mobilize during World War I.

For Great Britain we can use the best available statistics to provide estimates of mobilization and expenditure during the Napoleonic Wars.[30] If we adopt the figures used by Colley (1994), then Great Britain at the peak of the Napoleonic Wars mobilized approximately 390,000 men between its army and navy out of a total population of roughly 18.8 million – a mobilization ratio of 2.1%.[31] Now contrast this with mobilization during World War I. At the peak of this latter conflict Great Britain mobilized 4.4 million men in its armed forces out of a population of 43.3 million – a mobilization ratio of 10.2%.[32] While the Napoleonic era statistics should be considered as estimates under uncertainty, it nonetheless seems clear from these figures that the British population was much more massively mobilized for World War I than was the case for the wars against Napoleon.

Estimates of British spending in the Napoleonic Wars and in World War I suggest a much different picture than is the case for mobilization levels. If we look at peak annual military spending relative to national output, we actually see a much more similar ratio during the two conflicts. For World War I we can make use of the total for military expenditures provided by government audit. Based on these figures, in the 1917-18 fiscal year Great Britain spent

[30] While the correlates of war data set provides estimates of mobilization for all countries beginning in 1815, it does not provide esitmates for the Napoleonic Wars.

[31] Armed forces include an army of 250,000 and a navy with 140,000 men. In addition, Great Britain had a significant number of militia forces to defend against a potential invasion. The population figure is based on data from the 1811 censuses of England and Scotland and the 1821 census of Ireland, all as reported in Mitchell (1975).

[32] These figures are from the Correlates of War data that we use for our main statistical analyses.

1,767,550,494 pounds sterling on its armed forces.[33] This was equivalent to 39% of GDP.[34] Now compare this with British military spending during the Napoleonic wars. In 1815, when military outlays were at their peak, according to the data in Mitchell (1988) the British navy spent 72 million pounds on its armed forces, or a sum equivalent to 22% of GDP.[35] This is significantly lower than peak World War I spending, but on this dimension the two periods of warfare appear far less different than when we consider mobilization levels. Overall then, if in nominal terms British military spending during World War I was almost fifteen times larger than spending in 1815, Britain's tremendous growth during the nineteenth century meant that this sum was being taken out of a rapidly expanding pie. When this factor is taken into account, it becomes harder to suggest that the scale of spending during World War I should have necessarily created incentives to engage in an expedient taxation of capital, if one is to simultaneously claim that Napoleonic War spending should not have had the same effect.

There is one final factor to consider for Britain which is initial fiscal conditions upon entering the Napoleonic Wars and World War I. If these initial conditions were the determining factor, we would have been more likely to observe an "expedient" move to tax capital during this earlier conflict. When Napoleon I seized power in France in 1799, British public debt already stood at a staggering level of 166% of GDP, a consequence of a series of war engagements that had seen this ratio grow relatively constantly after 1688.[36] By the end of the Napoleonic Wars British public debt had increased to an even more staggering level of 223% of GDP.[37] Under these conditions it is not surprising that British Prime Ministers like William Pitt increased rates on many existing taxes and created entirely new ones targeted at the wealthy. One of these policy changes was the imposition of higher death duties. Another change was William Pitt's establishment of an income tax. But the marginal rates on both

[33] Mallet and George (1929 p.392).

[34] Nominal GDP for the 1917-18 fiscal year is calculated using the series constructed by Officer (2009) that provides nominal GDP estimates for the 1917 and 1918 calendar years.

[35] Nominal GDP estimates from Officer (2009).

[36] The ratio is constructed using debt figures from Mitchell (1988 p.600) and the GDP estimate for 1801 reported by Officer (2009).

[37] Debt figure from Mitchell (1988 p.600). Ratio constructed using Officer's (2009) GDP estimate for 1811.

death duties and the income tax remained extremely low by modern standards. Moreover, Pitt was stymied in several of these initiatives, such as his effort in 1798 to have the coverage of the legacy duty extended to cover all transfers of real property (i.e. real estate), as opposed to applying exclusively to moveables.[38]

Now consider the British government's fiscal position upon entering World War I. In 1914 the ratio of public debt to GDP stood at the relatively low level of 25% of GDP. This was accounted for in part by the fact that Britain had experienced a period of sustained growth during the nineteenth century and without involvement in massive conflicts. By 1918 this figure had ballooned to 115% of GDP as Britain borrowed massively to finance war expenditures. So, during World War I Britain experienced a very large increase in its debt to GDP ratio, but even at the end of this conflict the British government's total indebtedness measured relative to national output stood at a significantly lower level than when the British state had entered the wars against Napoleon.

Turning now to France, while we do not benefit from expenditure or revenue data of the same quality as for Great Britain in the Napoleonic Wars, we can make a comparison between war mobilization during this period and during World War I. Here the received wisdom would seem to be that with the invention of the *levée en masse* in 1793, the French government initiated a new era – that of warfare fought by mass armies of citizens. Numerous authors have emphasized how this went hand in hand with both a new spirit of nationalism (perhaps necessary to motivate these armies) and the idea that in a republic all citizens might be required to serve.[39] But as Crépin (2009) emphasizes in her recent authoritative history of conscription in France, if we look at estimates for the actual numbers generated by this new policy, the invention of the *levée en masse* seems to have been much more of an evolution than a revolution. World War I, in contrast, was much more of a critical break in terms of mobilization levels. Under the *levée en masse*, which was a system involving short term requisition of labor, Crépin (2009) suggests that maximum mobilization reached

[38] On this point see the discussion in Soward (1919).

[39] For an excellent example of this interpretation see the discussion by Samuel Finer (1975) of "The Napoleonic Watershed."

800,000 men. This was a large number, but not massively larger than the total number of men under arms at the peak of conflicts under Louis XIV a century earlier. At the peak of war mobilization under Napoleon in 1812 the total armed forces reached nearly a million men. This was equivalent to 3.3% of the total French population. Now compare this with the peak mobilization of 5.3 million men during World War I, which was equivalent to 13.5% of the French population at the time.[40]

We have demonstrated that Great Britain engaged in the Napoleonic wars under fiscal conditions that we might have expected to trigger the use of an expedient tax on capital. But the British government did not significantly increase taxes on inherited wealth. This result is not exceptional. In an environment of recurrent warfare in Europe, for centuries states had fought wars under desperate financial circumstances, yet they did not seek to dramatically increase direct taxes on wealth.[41] The contrast between British and French tax behavior in the Napoleonic Wars and World War I may, therefore, reflect the operation of the mobilization effect as opposed to the expediency effect. We should note, however, that there was also another major difference between the political environment during the Napoleonic Wars and World War I – the latter conflict was fought in an environment of universal, or near universal, suffrage whereas the former conflict was fought under a regime of parliamentary government but with a much more restricted suffrage. But we should also remind readers of the results of our pooled analysis in which there is no evidence of a significant interaction effect between war mobilization and universal suffrage.

To repeat, we have not been able to present evidence in this paper that allows us to distinguish whether our observed correlation between mass warfare and the taxation of inherited wealth is attributable primarily to the expediency effect or to the mobilization effect. We do believe, however, that it is instructive to consider British and French participation in the Napoleonic Wars in order to demonstrate that expensive wars fought at a time of fiscal crisis are not necessarily accompanied by "expedient" actions such as increased taxation of

[40] World War I figures based on Correlates of War project data.

[41] They did of course often resort to other expedient measures, in particular indirect taxation of capital via debt defaults and inflation.

inherited wealth.

6 Conclusion

What factors prompt a society to begin significantly taxing inherited wealth? The evidence that we have collected for this paper suggests that democracy based on universal suffrage has not been a sufficient condition for this to occur. There is some mixed evidence in our data that institutional barriers provided by unelected upper chambers posed a significant obstacle to the implementation of steep inheritance taxes, but none of the other panoply of institutions commonly suggested to have limited working class influence seem to have mattered in this area of policy. The much more consistent result in our analysis is that warfare, and in particular mass warfare involving mobilization of a substantial segment of a country's population, has been a major force leading to heavy taxation of inherited wealth. We have presented some evidence that it was the political conditions created by mass warfare, and not simply the need for money, that explains this observed effect, but it has not been the primary goal of this paper to try to fully adjudicate between these two different interpretations. If our interpretation is correct, however, the question our finding raises is whether the shift we are now observing in many countries away from heavily taxing inherited wealth represents a return to a more common context in a democracy that is not seeking to mobilize the great mass of its citizens.

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A Appendix

A.1 Data for Inheritance Tax Rates

This section describes the data sources for the top rate of inheritance tax for direct descendants for the nineteen countries included in our sample from 1816 or the date of national independence to 2000.

Australia

The Australian government levied a federal estate tax from 1914 to 1979. Information on the Australian estate tax is mainly from the Australian Treasury's July 22, 2009 response to our inquiry in June 2009. The top rate schedules were cross checked with the online information at a website maintained by the Australian Attorney-General's department, http://www.comlaw.gov.au, and secondary sources such as Duff (2005).

Austria

All information was compiled directly from the applicable legislation. All legislation is available online via the Austrian National Library's ALEX webpage at http://alex.onb.ac.at/. For some historical information on the Austrian inheritance tax legislation, see Schanz (1901) and Dorazil (1975).

Belgium

Belgian data are taken from two primary sources. The first one, covering the period up to the 1990s, is called *Pasinomie*, a government publication that announces all changes in Belgian law. Publication of this series began in 1833, and its exact title has changed a few times. From 1833 to 1941, it was published under the name "Pasinomie, ou, Collection complète des lois, décrets, arrêtés et règlements généraux qui peuvent être invoqués en Belgique" (Bruxelles: Librairie de jurisprudence de H. Tarlier). For 1942 to 1944, the title is "Bulletin usuel des lois et arrêtés et Pasinomie reunis" (Bruxelles: Bruylant). From 1945 onwards, the title changed to "Pasinomie: Collection complète des lois, arrêtés et règlements généraux qui peuvent être invoqués en Belgique" (Bruxelles: Bruylant).

The second source used is a free government online database made available by the Ministry of Justice at http://www.ejustice.just.fgov.be/doc/rech_f.htm. It provides access to the *Moniteur Belge*, the official government gazette, for the last decade or so. All in all, about 250 laws were examined in an iterative process, and the actual frequency of legal changes is considerably higher than secondary accounts might lead one to believe.

Secondary sources that were consulted to cross-check the validity of our data series include de Wilde d'Estmael (2004) and van Gysel (2008), as well as a review in the 1912 *Pandectes Belges* (Picard et al. 1912), which lists numerous laws on inheritance taxation on pages 24 to 28 and as well as pertinent literature up to that point.

Canada

The Candian federal estate tax was in place from 1941 to 1971. A narrative history of the estate taxation in Canada can be found in Perry (1955, 1989), Carter (1973), and Duff

(2005). We used primary sources to extract the detailed schedules from pertinent Canadian statutes. Relevant legislations are included in *Statutes of Canada* in volumes containing statutes ratified in 1941, 1946, 1958, 1968, and 1971.

Denmark

For Denmark, all information was compiled directly from the applicable legislation, which can either be accessed online at https://www.retsinformation.dk/ and https://www.lovtidende.dk/ or in printed form in the *Dansk lovregister* (Copenhagen: G.E.C. Gad, 1929 and later). For detailed background on the historical development of Danish inheritance taxation, see the article by Munkholm Povlsen and Krog Thomsen (1982). In addition, Giuliani Fonrouge (1937) has some information on Danish inheritance taxation up to the early 20th century.

Finland

For Finland, all information was compiled directly from the applicable legislation, which is published under the title *Suomen Säädöskokoelma*. This government publication which announces all changes in Finnish law was published under the name *Suomen Asetuskokoelma* from 1917–1980, and the title changed to *Suomen Säädöskokoelma* from 1981 onwards. The publisher is Valtioneuvoston Kanslia, Helsinki, and the printer Valtioneuvoston Kirjapaino for 1917–1965, Valtion Painatuskeskus for 1966–1996, and Edita from 1996 onwards.

For recent background information, see the report by the Finnish Tax Administration (2009) at http://www.vero.fi/nc/doc/download.asp?id=2142;271836 and Rytöhonka (1996). For arguments presented for and against inheritance taxation in Finland, see Kohonen (2007) at http://www.vatt.fi/file/vatt_publication_pdf/k411.pdf.

France

The French case is one of the best documented ones. Several major monographs examine inheritance taxation in France, with the most comprehensive ones being Daumard (1973) for the 19th century and appendix J in Piketty (2001) for the 20th century. In addition, chapter 5.3 in Beckert (2008) provides ample background information on the major legislative changes. Other secondary sources consulted include Capgras & Domergue (1935), Coutot (1925), Dupeyron (1913), Faure (1922), Malaurie (2008), Perraud-Charmantier (1956), Say et al. (1894), and Schanz (1901).

Unfortunately, the secondary literature does not treat the myriad of changes in French inheritance tax law comprehensively, as a look at the actual legislation quickly makes clear. An effort was thus made to collect all relevant legislation affecting the taxation of inheritances. From 1948 onwards, the data series is based directly upon French legislation, as reprinted in the *Recueil Dalloz* (Paris: Dalloz), with the most recent information taken from the government website http://www.impots.gouv.fr.

Germany

An overview of the key German inheritance tax laws and changes up to 1996 can be found in Viskorf et al. (2001). Specific information on rates is taken from the government publication *Die deutsche Erbschaftsbesteuerung vor und nach dem Kriege* for the period from 1906

(introduction of a federal inheritance tax) to 1928, Model (1953) for the time from 1929 to 1953, Kisker (1964) for 1954 to 1963, and directly from the applicable laws (found in the *Bundesgesetzblatt* (BGBl)) for the subsequent period. The most recent changes are covered by a memo available online at http://www.rechtliches.de/info/_ErbStG.html (accessed: July 7, 2009). In addition, chapter 5.2 in Beckert (2008) provides a detailed narrative account of the changing inheritance tax legislation in Germany in the 20th century, while Schanz (1901) lays out the more than twenty different sub-national inheritance tax laws that were in effect in the 19th century.

Ireland

All information on Ireland was compiled directly from the relevant Irish legislation, which is available in its entirety online at http://www.acts.ie/ and, for the most recent years, http://www.irishstatutebook.ie/home.html. Irish legislation always mentions what is being modified, and thus we have a complete overview of the Irish inheritance tax laws going back to 1922.

Italy

All information on Italy was compiled directly from the relevant Italian legislation, which is partly available and searchable online via the website www.normeinrete.it (this covers the years from 1905 onwards, yet is incomplete even for this period). Nearly all of the legislation had to be copied from printed collections of laws, though, most notably the two series *Collezione celerifera delle leggi, decreti, istruzioni e circolari* for the time up to the 1920s and *Lex – Legislazione italiana: raccolta, cronologica con richiami alle leggi attinenti e ricchi indici semestrali ed annuali* from the 1920s onwards.

Our results were cross-checked with those referred to in the secondary literature (which, however, is generally less comprehensive than our work and moreover sometimes contradictory from one source to the next). The sources in Italian that we have consulted include Battiato (1974), Gallo-Orsi (1994), Garelli (1896), Grisolia Gesano (1958,1962), and Serrano (1974). We also cross-checked our info using two articles in German, namely Schanz (1901) and von Odkolek (1904).

Japan

Tax rates from 1997 onward are provided by the National Tax Agency. The agency website www.nta.go.jp provides statistical information on all taxes from 1949. The tax rate from 1953–2006 can be found in a book on personal tax relation law (Basic Taxation Law) edited by the National Tax Administration of Japan in 2006. The tax rate from 1905–1952 can be found in a 1954 publication by the Ministry of Finance (MoF) called "The Historical Recapitulation of the Internal Taxation's Tax Rate and Payment Period." It provides the rates and detailed summary of all relevant inheritance taxes up to 1954.

An analysis of historical tax changes can be found in the volumes of the "History of Taxation in Meiji/Taisho Era" and "History of Taxation in Showa Era," both edited by the MoF. The books provide accounts of tax changes and political and economic circumstances surrounding the introduction or modification of inheritance taxes. Another useful source is Hiromitsu Ishi (1989) *The Japanese Tax System* (Oxford: Oxford University Press).

Netherlands

Information on inheritance tax rates in the Netherlands is based upon the pertinent Dutch legislation, which has been published in the *Staatsblad* (van het Koningrijk der Nederlanden)} since 1813. Secondary sources consulted include Drukker (1957), Schanz (1901), Wattel (1881), and Zwemmer (2001).

New Zealand

For New Zealand, all information was compiled directly from the applicable legislation. Reprints of the legislation for 1908–1931 can be found in "The Public Acts of New Zealand (Reprint), 1908–1931" (Wellington: Butterworth, 1932–1933). From 1936 onwards they are contained in the publication "Statutory Regulations: Being the Regulations Issued under the Regulations Act, 1936, from 1st August, 1936, onwards" (Wellington: E. V. Paul, Govt. Printer) and partly online at Knowledge Basket New Zealand's http://legislation.knowledge-basket.co.nz/index.html. Copies of the earliest pieces of legislation were sent to us by the staff at the National Library of New Zealand. For detailed background information, see the article by McKay (1978) and the relevant passages in Goldsmith (2008).

Norway

Information on inheritance tax rates in Norway is based upon a July 21, 2009, reply by the Norwegian Royal Ministry of Finance to a request for this information sent out in June 2009. The information provided in turn mainly draws upon a 557–page report on the Norwegian inheritance tax ("Arveavgift") that was compiled by the Royal Ministry of Finance.

South Korea

The data for 1962–2009 were obtained directly from the Korean National Tax Agency. The data from 1950–1962 were collected from the "National Law Code Information Center," which makes information available online at http://www.law.go.kr. The initial rate and the information on the "Cho-Seun" inheritance tax that applied during the Japanese occupation of Korea can be found in "Cho-Seun Inheritance Tax Code" (1934) by Murayama Michio (who was the responsible officer of the Cho-Seun Administration). Note that we were unable to collect information on Korean taxation prior to the Japanese occupation.

Sweden

The official collection of Swedish statutes, *Svensk Författningssamling* (1825–), starts in 1825. Our data series was constructed by accessing original legislation, by using online sources to identify amendments and new statutes, and with the help of secondary sources. In particular, the entire list of amendments for 1941:416 \S 28 is taken from the Notisum online database at http://www2.notisum.com/rnp/sls/fakta/a9410416.htm. Secondary sources consulted include Eberstein (1956), Englund and Silfverberg (1997), and Ohlsson (2007).

Switzerland

Switzerland never had an inheritance tax at the federal level. To verify this information, we consulted the relevant passages in Schoenblum (1982, 2009) and Steinauer (2006) as well as the monographs by Boulenaz (1961) and Huber (1946), which provide information on the subnational level while mentioning the absence of a federal-level inheritance tax at the time of their publication.

United Kingdom

The British inheritance tax in the nineteenth century was enforced under several titles which were merged and unified as a single estate tax in 1894. The data prior to 1894 is from the primary sources containing relevant British statutes available in several volumes of *The Statutes of Great Britain*. For extracting the rates for legacy, probate, and stamp duties, we have cross-checked secondary sources such as Dowell (1965), West (1908), and Shultz (1926) with the original statues. We used the abridged statutes included in the appendix of *The Death Duties* (Green, 1936), to confirm the timeline for major changes in inheritance tax legislation in the nineteenth century.

Information on the period from 1894 to 1971 is taken from the 7th edition of "Green's Death Duties," which contains information on the rates of estate duty in appendix III. Information on subsequent changes is compiled directly from the *Acts of the UK Parliament*, which are available online at http://opsi.gov.uk/acts. The information contained in Lawday & Mann (1971) and the acts was cross-checked with the help of Chown (1975) and Barlow et al. (2008), among others. In addition, contextual information on key legislative changes was obtained from various newspaper reports in the Times of London.

United States

There is a comprehensive body of secondary literature on the American inheritance taxation. West (1908) contains a detailed review of federal inheritance taxation starting in 1797 up to the beginning of the twentieth century. In addition to the federal case, West (1908) includes a detailed summary of the inheritance taxation on the state level during the nineteenth century. The data for the early twentieth century is from Shultz (1926). Federal estate tax law was introduced in 1916 and amended multiple times during the twentieth century. Among the recent sources, we have used Beckert (2008), Luckey (2005), and Jacobson et al (2007) to report on the evolution of the federal estate tax rates.

A.2 Additional Results

	5-year Data						10-year Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top Rate$_{t-1}$				0.870	0.875	0.667		0.382
				(0.039)	(0.037)	(0.062)		(0.127)
				0.000	0.000	0.000		0.003
War Mobilizaton$_{t-1}$	24.673	23.237	20.915	19.811	19.730	18.982	30.130	27.653
	(5.827)	(6.319)	(5.830)	(3.259)	(3.350)	(3.608)	(12.205)	(11.142)
	0.000	0.002	0.002	0.000	0.000	0.000	0.024	0.013
Direct Elections$_{t-1}$	-3.861	-1.681	2.190	0.063	0.107	4.813	0.746	0.687
	(8.642)	(8.363)	(2.660)	(1.469)	(1.737)	(1.656)	(3.505)	(2.217)
	0.660	0.843	0.421	0.948	0.951	0.969	0.834	0.757
Left Executive$_{t-1}$		1.161	4.218		3.131	4.689	5.723	5.000
		(5.838)	(3.753)		(1.605)	(1.698)	(6.229)	(3.193)
		0.845	0.276		0.051	0.006	0.370	0.117
GDP per capita$_{t-1}$		0.001	0.000		-0.000	0.001	0.001	0.001
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)
		0.721	0.736		0.761	0.203	0.585	0.145
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No
R-squared	0.712	0.717	0.842	0.879	0.877	0.894	0.848	0.839
Number of Observations	509	488	488	508	487	487	240	239

Table A-1: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Direct Elections Measure of Democracy.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and the variable *Direct Elections* lagged one period. The specifications in columns 1-3 and 7 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6 and 8 include a lagged dependent variable and report panel corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-8 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

	5-year Data						10-year Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top Rate$_{t-1}$				0.870	0.875	0.667		0.381
				(0.039)	(0.037)	(0.062)		(0.127)
				0.000	0.000	0.000		0.003
War Mobilizaton$_{t-1}$	25.352	23.622	20.640	19.809	19.729	18.984	29.644	27.613
	(5.787)	(6.279)	(5.770)	(3.260)	(3.351)	(3.609)	(12.137)	(11.147)
	0.000	0.001	0.002	0.000	0.000	0.000	0.025	0.013
Secret Ballot$_{t-1}$	3.349	2.881	-1.958	0.091	0.092	0.280	-3.427	0.922
	(5.196)	(4.981)	(2.366)	(1.234)	(1.254)	(1.241)	(3.294)	(2.336)
	0.527	0.570	0.419	0.942	0.942	0.821	0.312	0.693
Left Executive$_{t-1}$		1.004	4.183		3.130	4.693	5.613	5.100
		(5.838)	(3.707)		(1.605)	(1.699)	(6.097)	(3.149)
		0.845	0.274		0.051	0.006	0.369	0.105
GDP per capita$_{t-1}$		0.001	0.000		-0.000	0.001	0.001	0.001
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)
		0.730	0.755		0.763	0.206	0.589	0.154
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No
R-squared	0.711	0.717	0.842	0.879	0.877	0.894	0.848	0.839
Number of Observations	509	488	488	508	487	487	240	239

Table A-2: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Secret Ballot Measure of Democracy.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and the variable *Secret Ballot* lagged one period. The specifications in columns 1-3 and 7 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6 and 8 include a lagged dependent variable and report panel corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-8 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

	5-year Data						10-year Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top Rate$_{t-1}$				0.872	0.876	0.658		0.380
				(0.038)	(0.037)	(0.062)		(0.126)
				0.000	0.000	0.000		0.002
War Mobilizaton$_{t-1}$	24.102	22.551	20.026	17.646	17.588	16.743	30.183	27.910
	(5.622)	(5.607)	(5.670)	(3.918)	(4.024)	(4.186)	(11.779)	(10.982)
	0.000	0.001	0.002	0.000	0.000	0.000	0.020	0.011
Electorate 25$_{t-1}$	-3.707	-1.675	-0.255	-1.424	-1.605	-1.671	-2.265	-3.677
	(7.740)	(8.222)	(3.405)	(2.819)	(2.685)	(2.449)	(7.094)	(5.197)
	0.638	0.841	0.941	0.613	0.550	0.495	0.753	0.479
Electorate 50$_{t-1}$	8.338	7.370	5.360	0.090	0.263	3.071	7.400	6.264
	(4.877)	(5.226)	(3.540)	(3.096)	(3.006)	(3.179)	(6.342)	(6.578)
	0.105	0.176	0.147	0.977	0.930	0.334	0.259	0.341
Electorate 75$_{t-1}$	-8.827	-6.628	-4.358	-0.247	-0.501	-1.558	-3.927	-2.436
	(6.247)	(5.598)	(3.776)	(1.859)	(2.004)	(2.139)	(4.285)	(3.632)
	0.175	0.252	0.264	0.895	0.803	0.466	0.372	0.502
Left Executive$_{t-1}$		1.119	4.270		3.022	4.614	5.771	5.097
		(5.762)	(3.772)		(1.613)	(1.698)	(6.203)	(3.194)
		0.848	0.272		0.061	0.007	0.364	0.111
GDP per capita$_{t-1}$		0.001	0.001		-0.000	0.001	0.001	0.001
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)
		0.678	0.710		0.686	0.074	0.571	0.133
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No
R-squared	0.714	0.719	0.843	0.877	0.874	0.893	0.849	0.840
Number of Observations	510	489	489	509	488	488	240	239

Table A-3: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Electorate Size Measure of Democracy.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and the variables *Electorate 25*, *Electorate 50*, and *Electorate 75* lagged one period. The specifications in columns 1-3 and 7 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6 and 8 include a lagged dependent variable and report panel corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-8 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

	5-year Data						10-year Data		
	Lag DV and Country Fixed Effects						Lag DV and Country FE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Top Rate$_{t-1}$	0.769	0.761	0.769	0.763	0.755	0.744	0.570	0.577	0.530
	(0.047)	(0.048)	(0.047)	(0.047)	(0.048)	(0.049)	(0.104)	(0.105)	(0.106)
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
War Mobilizaton$_{t-1}$	18.242	17.305	17.802	17.073	21.438	20.621	28.202	27.984	30.902
	(3.825)	(4.221)	(3.905)	(4.293)	(3.178)	(3.476)	(11.138)	(11.434)	(11.240)
	0.000	0.000	0.000	0.000	0.000	0.000	0.011	0.014	0.006
Universal Male Suffrage$_{t-1}$	-0.591	0.160					1.509		
	(1.818)	(1.790)					(3.217)		
	0.745	0.929					0.639		
Boix-Rosato$_{t-1}$			-1.212	-0.896				-1.192	
			(1.493)	(3.096)				(2.669)	
			0.548	0.977				0.655	
No Upper$_{t-1}$					4.048	4.606			9.695
					(1.465)	(1.462)			(2.868)
					0.006	0.002			0.001
Left Executive$_{t-1}$		4.147		4.302		3.944	4.887	5.317	4.551
		(1.649)		(1.687)		(1.653)	(3.012)	(3.155)	(3.044)
		0.012		0.011		0.017	0.105	0.092	0.135
GDP per capita$_{t-1}$		0.001		0.001		0.000	0.001	0.001	0.001
		(0.000)		(0.000)		(0.000)	(0.001)	(0.001)	(0.001)
		0.135		0.184		0.309	0.293	0.444	0.240
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.885	0.885	0.885	0.885	0.889	0.889	0.816	0.816	0.824
Number of Observations	509	488	509	488	508	487	239	239	239

Table A-4: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Lagged Dependent Variable and Fixed Effects Specificatoins.* The table reports the results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period and selected democracy measures also lagged one period. All specifications include a lagged dependent variable and period and country fixed effects. Each column reports the OLS estimate, panel corrected standard errors in parentheses, and p-values.

	5-year Data					
	Democracy and War Interacted				Partisanship and War Interacted	
	(1)	(2)	(3)	(4)	(5)	(6)
Top Rate$_{t-1}$		0.869		0.878		0.879
		(0.041)		(0.039)		(0.039)
		0.000		0.000		0.000
War Mobilizaton$_{t-1}$	48.930	10.406	21.204	27.058	22.525	18.682
	(7.946)	(6.876)	(14.505)	(5.762)	(6.344)	(4.457)
	0.000	0.130	0.161	0.000	0.002	0.000
Universal Male Suffrage$_{t-1}$	5.259	-3.226				
	(6.861)	(1.591)				
	0.453	0.043				
Universal Male Suffrage$_{t-1}$ * *War Mobilization*$_{t-1}$	-28.330	8.258				
	(9.059)	(6.786)				
	0.006	0.224				
Boix-Rosato$_{t-1}$			-0.231	-0.242		
			(8.700)	(1.218)		
			0.979	0.843		
Boix-Rosato$_{t-1}$ * *War Mobilization*$_{t-1}$			3.995	-15.605		
			(14.809)	(6.216)		
			0.790	0.012		
Left Executive$_{t-1}$					0.722	3.183
					(6.237)	(1.614)
					0.909	0.017
Left Executive$_{t-1}$ * *War Mobilization*$_{t-1}$					4.553	-5.318
					(9.464)	(8.052)
					0.636	0.509
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	No	Yes	No	Yes	No
R-squared	0.716	0.878	0.712	0.879	0.712	0.878
Number of Observations	510	509	510	509	510	509

Table A-5: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Interactions between War Mobilization and Democracy and Partisanship Measures.* Columns 1-4 report results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period, selected democracy measures lagged one period, and the interaction between the measures. Columns 5-6 report results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period, the variable *Left Executive* lagged one period, and the interaction between the measures. Specifications 1, 3, and 5 include country and period fixed effects and report robust standard errors clustered by country in parentheses and p-values. Specifications 2, 4, and 6 include a lagged dependent variable and period fixed effects and report panel corrected standard errors in parentheses and p-values.

	Annual Data					
	Country Fixed Effects			Lagged Dependent Variable		
	(1)	(2)	(3)	(4)	(5)	(6)
Top Rate$_{t-1}$				0.964	0.966	0.965
				(0.009)	(0.009)	(0.007)
				0.000	0.000	0.000
War Mobilizaton$_{t-1}$	7.166	8.620	9.071	2.649	2.489	2.666
	(2.941)	(3.561)	(3.398)	(0.686)	(0.698)	(0.620)
	0.025	0.026	0.016	0.000	0.000	0.000
Universal Male Suffrage$_{t-1}$	8.772			-0.585		
	(6.873)			(0.372)		
	0.218			0.116		
Boix-Rosato$_{t-1}$		5.820			-0.383	
		(6.211)			(0.397)	
		0.361			0.334	
No Upper$_{t-1}$			14.846			0.246
			(6.783)			(0.267)
			0.042			0.356
Left Executive$_{t-1}$	0.656	0.408	0.652	0.342	0.331	0.283
	(3.140)	(3.239)	(3.232)	(0.278)	(0.275)	(0.240)
	0.837	0.901	0.842	0.218	0.229	0.239
GDP per capita$_{t-1}$	0.001	0.001	0.001	-0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)	(0.000)
	0.548	0.567	0.590	0.611	0.998	0.744
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No
R-squared	0.720	0.716	0.734	0.962	0.962	0.962
Number of Observations	2,413	2,413	2,410	2,412	2,412	2,409

Table A-6: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Annual Data.* Columns 1-3 report results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period, selected democracy measures lagged one period, and control variables. Specifications 1-3 include country and period fixed effects and report robust standard errors clustered by country in parentheses and p-values. Specifications 4-6 include a lagged dependent variable and period fixed effects and report panel corrected standard errors in parentheses and p-values.