Democracy, War, and Wealth

Lessons 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 political economy 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 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 political conditions under which countries are likely to implement policies that significantly redistribute wealth and income.

1   Introduction

Like many public policies that have redistributive implications, estate 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. Within the United States these questions are certainly of current interest, given proposals to alter, reform, or eliminate bequest taxation.[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 evolution of wealth accumulation during the course of the twentieth century.[2] Basic intuition suggests that electoral democracy, characterized by universal suffrage, 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 in which large estates are taxed at a significantly higher rate than small estates. In a society where most decedents leave either no estate or a relatively small estate, the logic of electoral politics would seem to dictate that large estates will be taxed heavily.[3]

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[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 and 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. Kopczuk and Saez (2001), Piketty (2001), Piketty and Saez (2003), Piketty, Postel-Vinay, and Rosenthal (2006), and Roine and Waldenström (2009).

[3] This prediction regarding universal suffrage would parallel the conclusion of Acemoglu and Robinson (2000, 2006) and Boix (2003) regarding the effect of suffrage extensions on redistribution. 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 might not necessarily expect to observe that suffrage extensions produce shifts towards significantly more progressive policies in capital taxation. See Przeworski (2008) for an empirical examination of the circumstances under which franchise extensions occur.

At first glance, the prediction that universal suffrage and progressive inheritance taxation should go together seems strongly supported by the fact that they both emerged during the same general time period—the turn of the twentieth century. Scholarly observers at the time explicitly stated that the development of progressive inheritance taxation was attributable to the spread of democratic ideas and democratic institutions.[4] But if early twentieth century observers commented on the possible association between democracy and inheritance taxation, they also pointed to another empirical regularity—innovations in inheritance taxation were driven by the exigencies of war.[5] In this paper we present and test an argument that wars of mass mobilization are particularly likely to be characterized by progressive taxation of large fortunes. We argue that mass warfare played a greater role in the development of progressive inheritance taxation than did the advent of universal suffrage.

Why would wars of mass mobilization be associated with increased taxation of large fortunes via inheritance taxation? The most simple response to this question is to say that wars are expensive, and they need to be financed, but there are two main reasons why this answer is insufficient. First, simply referring to the need for finance does not tell us why taxes on large fortunes should be privileged, as opposed to drawing revenue from other sources, and in particular indirect taxes generally believed to be regressive in their incidence. Second, the simple "need for finance" explanation ignores the fact that European states had fought expensive wars for centuries prior to 1900 without ever resorting to anything resembling the progressive tax policies that emerged during the twentieth century.

In section 2 of this paper we develop an argument which suggests more precisely why wars of mass mobilization would be associated with increased taxation of top fortunes. Fighting a war in which a large segment of a country's population is mobilized requires societal consensus in favor of the war effort. This societal consensus will be easier to maintain if there is a sentiment that the burden of the war effort is fairly shared between different social groups. There are two specific reasons why a progressive tax on top fortunes (such as an inheritance

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[4]On this point see in particular West (1908), Seligman (1913), Soward (1919), and Shultz (1926), as well as the more recent discussion in Lindert (2004).

[5]See in particular Soward (1919).

tax) might be seen as part of equal burden sharing related to war. The first would be if wealthier individuals are less likely to fight, either because they have not enlisted or because they have avoided conscription through a deferment, an exemption, or simply because of age. In this case those who fight might demand that the wealthy bear a disproportionate share of the financial burden for a war in order to establish a greater equality of sacrifice.[6] The second possibility would be if wealth holders benefit financially from a war that increases demand for goods produced by companies in which they hold investments. This could further strengthen demands for having wealth holders bear a disproportionate share of the financial burden for a war. Taken together, this leads to a prediction that wars of mass mobilization will be associated with political pressures for increased taxation of top fortunes. Furthermore, to the extent that a war is financed by debt that is repaid with taxes levied after war's end, then political pressures for taxation of top fortunes will continue for some time.

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. Our sample includes the majority of the core industrial countries for which it is most commonly suggested that the extension of the suffrage led to greater redistribution. For our sample of countries, it is generally known when a country first established an inheritance tax, but this often tells us little about the extent to which governments actually taxed large fortunes heavily. 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 database 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.

We focus on inheritance taxation in this paper not only because it is an inherently inter-

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[6]The work of Margaret Levi (1997) has been particularly influential in emphasizing how compliance with a system of conscription would be easier to achieve if service was made universal. Age constitutes one reason why some individuals are exempted even under a system of universal service.

esting subject, but also because of the possibility that it affords us for testing propositions about the determinants of progressive taxation in an environment where our results are less likely to be biased by a failure to control for levels of administrative capacity. 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.[7] 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 democracy, war, and taxes might depend on the confounding factor of administrative capacity.

To analyze the relationship between democracy, war, and taxes 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 that control for time-constant unobserved country-level heterogeneity, time period effects that control for common shocks, and several time-varying control variables. 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. The identifying assumption in this approach is that the lagged value for the top marginal rate of inheritance taxation controls for any unobserved heterogeneity that might otherwise bias our estimates. To estimate this, we use a lagged dependent variable specification combined with dummy variables for common time effects.

These analyses yield two main results. First, our estimates do not suggest a positive 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 presence of universal male suffrage. Our estimates for the coefficient

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[7]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." See Johnston (1965 p.153).

on this variable are inconsistently signed, small in magnitude, and statistically insignificant. This pattern of results is repeated for ordinal measures of the extent of the suffrage, measures based of political competition, and a measure of the presence of a secret ballot.

In strong contrast to the suffrage results, our estimates indicate a substantively and statistically significant positive relationship between war mobilization and the top rate of inheritance tax. All else equal, a country that mobilized for mass warfare for an entire five-year period would be estimated to increase its top inheritance tax rate by 14 to 25 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, in addition to the further robustness test of including individual linear time trends for each country. We further consider multiple measures of war mobilization, possible interactions between war mobilization and democracy and left partisanship, and several alternative econometric models. Although our results clearly reflect the tight correlation between mass warfare and the establishment of high rates of inheritance taxation, our argument may also provide insight into the reasons why numerous countries have reduced or even eliminated taxes on large fortunes in recent decades. During a period in which the advanced industrial countries have shifted from a format of military force based on universal conscription to one characterized by small professional armies, war related arguments for heavy taxation of the rich have inevitably become less salient.

A final point worth emphasizing about our statistical results is that in addition to providing evidence about the evolution of progressive taxation during a critical historical period, they also provide a more general lesson about the conditions under which there will be broad political support for taxing those with high incomes or large fortunes at higher rates than other individuals. Such support is most likely to exist when there exists a clear argument that it is fair to tax the rich more heavily than others because this corrects for some pre-existing unfairness involving the way that incomes have been earned or the way in which some have been obliged to contribute disproportionately on other dimensions. During the course of the twentieth century, mass warfare has provided the primary context in which such

arguments have been successfully made, but it arguably does not have to be the only context in which this could occur.

In the remainder of this paper we will proceed as follows. Section 2 outlines our argument that mass mobilization for war leads to political pressures favorable to the progressive taxation of inherited wealth. In Section 3 we present the data set, discuss measurement issues, and illustrate key trends in marginal inheritance taxes by examining the data for the Netherlands and the United Kingdom in some detail. Section 4 then presents our econometric model. In Section 5 we discuss our core estimation results. Section 6 presents further evidence in favor of our argument about mass mobilization. Section 7 concludes.

2   War Sacrifice and the Taxation of Top Fortunes

In the introduction to this paper we briefly considered two alternative mechanisms that might be expected to lead to the progressive taxation of top fortunes. The first mechanism involving the extension of the suffrage is already familiar after the work of Acemoglu and Robinson (2000, 2006) and Boix (2003). The argument for why mass mobilization might lead to progressive taxation is not as well established, and so we devote the remainder of this section to this second possibility.

Consider the choice faced by a government seeking to raise an army. As one option it can pay a body of professional soldiers a sufficiently high wage that this pay outweighs the risks inherent in military service. As an alternative, a government can resort to some form of civic obligation in lieu of high pay. This obligation could be formal, such as in a system of conscription, or it could be informal, such as if those who fail to volunteer for a war suffer social sanctions or feelings of guilt. Since at least the time of Sidgwick (1883 p.545) it has been suggested that a government seeking to mobilize the great mass of its citizens for war will need to use obligation as a means of recruitment. The reason for this is that the deadweight costs of taxation involved in raising a mass army would be prohibitively high. Compliance with an obligation may be enforced by sanctions, but it is also now well established that individuals are more likely to comply if they believe that the burden for an

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obligation is fairly distributed. So, for example, it will be easier to ensure compliance with a system of conscription that is universal and which excludes possibilities such as paying for substitutes.[8] We suggest two prominent factors liable to create a perception that a burden of war sacrifice is unfairly shared even when there is universal conscription.

First, all modern forms of universal conscription exempt individuals above a certain age, and this raises the question of how older individuals might be compelled to participate in the war effort. One possibility, suggested by one of the founders of modern welfare economics, Arthur Pigou, is that because older men on average have accumulated much more wealth than younger men, then a tax on large fortunes would help re-equilibrate the burden of war sacrifice. It is worth quoting Pigou at length on this point.

> From the statistics of estates passing at death it can be deduced that practically all the material capital of the country is held by persons over twenty years of age; that persons over forty-five, who constitute about one-third of these persons, own about three-fourths of the whole; so that the representative man over forty-five holds about six times as much material capital as the representative man between twenty and forty-five. But young men, who excel older men in physical strength, have been forced to give their physical strength in the war, while older men have been exempted. The fact that old men excel young men so greatly in financial strength suggests that the balance might be partly adjusted, and something less unlike equality of sacrifice secured, by a special levy whose incidence would in the main fall upon persons exempted from military service. (Pigou 1918 p.145)

The most direct implication of Pigou's claim would seem to be that mass warfare will generate political pressures for a one time levy on wealth. However, to the extent that such a levy is judged infeasible, impractical, or otherwise undesirable, we can expect that Pigou's reasoning could be used to justify the taxation of wealth through alternative means, such as a progressive inheritance tax.

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[8]See in particular Levi (1997) on this point.

A second reason why perceptions of unequal sacrifice may emerge even under a system of universal conscription is if some individuals earn higher than usual profits during a war because they happen to have investments in firms involved in the production and distribution of materials necessary for the war effort. During the twentieth century perceptions regarding war profits and war profiteering have played a prominent part in political debates. One response to this phenomenon has been to propose new taxes on wealth justified largely on fairness grounds. As John Hicks observed in explaining the motivation for such schemes:

> The inequality of incomes is always one of the sore spots of modern society; when severe sacrifices have to be imposed on all classes, inequality of sacrifice may become a danger to national unity. New inequalities, which have not even custom and familiarity to recommend them, are less to be borne than old. The sense of unfairness is particularly aroused when the high incomes are earned, not by those who are in the centre of the war effort, but by those who are on the edge of it (Hicks, Hicks, and Rostas 1941 p.5).

From the above discussion we derive the prediction that when a government mobilizes the great mass of its citizens for war, pressures will emerge to tax top fortunes and high incomes, with inheritance taxes being one obvious policy instrument to do so. It is important to emphasize that our argument applies to wars of mass mobilization, not wars in general. For the reasons laid out by Sidgwick, war with a smaller army can be more easily fought by raising a professional army paid a sufficiently high wage. Under these circumstances, questions of fairness do not enter into the equation. In addition, even if a small scale war is fought by raising an army of conscripts, then there will be fewer people in practice who can make the argument that they have sacrificed on the field.

We expect that in a democratic context the mechanism through which mass warfare led to increased top rates of inheritance taxation would operate via a shift in the messages sent by parties and an alteration in opinion of the electorate. For parties previously supportive of progressive taxation with high top rates, arguments emphasizing the need for such a policy as a means of restoring "equality of sacrifice" should provide a potentially powerful message

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for increasing vote shares. The wartime context provides a way of supplementing standard "ability to pay" arguments for progressivity with an appeal to fairness. This appeal to fairness may ensure broader support. Parties previously opposed to heavy taxation of the rich would then face a choice of either maintaining their platforms or conceding some ground on this issue so as to maintain vote share. Following a war's conclusion, there is no reason to believe that the debate over progressive taxation should immediately shift back to where it stood prior to war's outbreak, as long as the issue of repaying war debt remained politically salient.

While it is perhaps easiest to suggest how our argument would apply in a democratic context, it could also apply to countries under autocratic rule. Autocrats pursuing a war still need to be concerned about issues of compliance with wartime conscription policies, and they also need to be concerned about broader social support for the war effort to the extent that civilians are engaged in necessary wartime production.[9] After a war, autocrats can be subject to demands by those who have fought. The mechanism through which such demands are made will involve street protests, rather than voting, seemingly implying higher costs of collective action. Yet there is no reason to believe that these higher barriers to collective action should be insurmountable. In saying this we are not suggesting that mass mobilization should have an identical effect on tax rates in democracies and autocracies (albeit through a different channel). We are simply suggesting that there is no reason to believe that the effect would operate exclusively in democracies.

Before proceeding further we should acknowledge the affinity between our argument and those made by other scholars who have emphasized the role of war in the development of progressive taxation and of war in leading to other social reforms. Important previous work has emphasized how participation in World War I led to political pressures for steeply progressive taxes in the United States (Brownlee, 2004 and Bank, Stark, and Thorndike, 2008) and in the United Kingdom (Daunton 2002), as well as a select number of additional

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[9]Even in the hypothetical "garrison state" described by Harold Lasswell, there would be a need to have "equalitarian adjustments in the distribution of income for the purpose of conserving the will to fight and to produce." The quote appears on page 461 of Lasswell (1941).

countries.[10] Our study is different first in that it conducts an empirical investigation across a broad set of countries, and second in that we lay out a precise theory suggesting why we should expect our effect to operate during wars of mass mobilization, as opposed to during more limited conflicts. Likewise, if existing work on war and taxation has often implied a one way mechanism in which war leads to higher taxes on the rich, we also suggest how transition towards a new format of military force may result in an eventual return to lower tax rates on the wealthy. This possibility that pressures for taxation of the rich might both wax and wane bears a similarity with the discussion of the effect of war participation on rights for African Americans as presented by Klinkner and Smith (2002). They emphasize how participation by African Americans in war efforts has resulted in claims for extension of new rights, but periods of peace have often given way to retrenchment in this regard.

A final question regarding our mechanism involves its persistence; if the underlying problem is one of achieving a new societal bargain during wartime, then why would this bargain not quickly unravel after war's end? We have already referred to two important reasons why this would not be the case. First, the question of who should pay for a war often extends for some time as the debate shifts to collection of revenues for settling war debts. Second, those who return from fighting a war may feel a new sense of entitlement, and this may influence their political behavior whether this be in the form of voting or street protest.[11] To these two important sources of persistence we can also add a third involving simple status quo bias. Status quo bias is not as relevant at war's outset because new revenue has to be found from some source (the reversion outcome being defeat), and the question is how the burden will be distributed between different social groups. After war's end the issue becomes how to arrive at a bargain over the tax burden that will avoid the reversion outcome of default. Yet

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[10]Important work that focuses on war and social policy rather than taxation includes Titmuss (1958) and Skocpol (1992). Skocpol (1992, Chapter 2) is particularly relevant because her account of U.S. Civil War pensions emphasizes the importance of service to the Union cause and the perceived deservingness of veteran beneficiaries.

[11]We might also want to consider whether a war leads to a permanent shift in redistribution because elites need to use redistributive policy to motivate the masses during wartime, and they use the extension of the suffrage as a commitment mechanism to ensure that this redistribution actually does take place after war's end (Ticchi and Vindigni, 2009). While plausible, the empirical results we present in this paper pose a challenge for this proposed mechanism - at least in terms of inheritance taxation, the extension of the suffrage does not appear to be a commitment to redistribute anything.

once the immediate issue of war finance is settled, high tax rates on the rich become a new status quo, and in the absence of a very penalizing reversion outcome associated with their maintenance, we can expect that they may endure for some time.

If the above discussion suggests why mass mobilization would lead to an effect of top tax rates that persists for some time, it can also be used to suggest what we would observe in terms of the decay of this effect. Decay of the mobilization effect will be hastened as the generation that fought a war ages and eventually becomes a smaller voting bloc. Even so, we can expect that the decay of the mobilization effect would vary from country to country, as those favorable to lowering top tax rates would still need to be able to gain control of the necessary veto points in a political system, and such opportunities will depend on both national institutional structures and exogenous events.

3   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.[12] In this paper, for simplicity we will refer to all forms of bequest taxation as inheritance taxes, and we will combine multiple bequest taxes where necessary to determine the total amount of inheritance taxes at a given time.[13] 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 inheriting an estate.[14] We prefer this to the alternative of simply asking whether there was an inheritance tax, because, as will be seen, countries often initially levied inheritance taxes but at extremely low rates. We

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[12]The dataset will be made publicly available, together with copies of all relevant national legislation upon completion of this project. The countries included in the sample are 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.

[13]In other words, by "inheritance taxes" we are referring both to taxes levied on the estates of the deceased, as well as taxes levied on those who inherit all of part of an estate. In addition to their marginal rate, bequest taxes have many features that can have an impact on how much tax is actually paid. In particular, rules for valuing estates can vary substantially across countries and time.

[14]More precisely, to make the data more comparable across countries, we focus on the top rate applied to a single descendant who receives an inheritance in cash.

also prefer this to the alternative of an indicator measuring whether there was a progressive scale of rates or not, because it was often the case that governments adopted the principle of progressivity at the same time that they maintained extremely low top rates. We focus on the top marginal rate of inheritance taxation for 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.

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 some interesting patterns.[15] First, from the beginning of the 19th century through the first decade of the 20th century, the taxation of direct descendants existed but rates were very low. 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 U.S.— with top marginal rates of 40% or higher.[16]

Although we will use this data to systematically test the suffrage and war mobilization hypotheses, it is useful as an exploratory analysis to focus on the contrast in top rates of inheritance tax between the UK and the Netherlands as highlighted in Figure 2. Despite being very different countries, in the first decades of the twentieth century the UK and the Netherlands shared a number of commonalities that one might expect to have led to similar developments with regard to inheritance taxation. During the course of the nineteenth century each country took successive steps to expand the suffrage, with universal male suffrage

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[15] For context, it is useful to note that the mean top rate for the entire sample (2,798 country years) is 17.1 with a standard deviation of 22.3.

[16] 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.
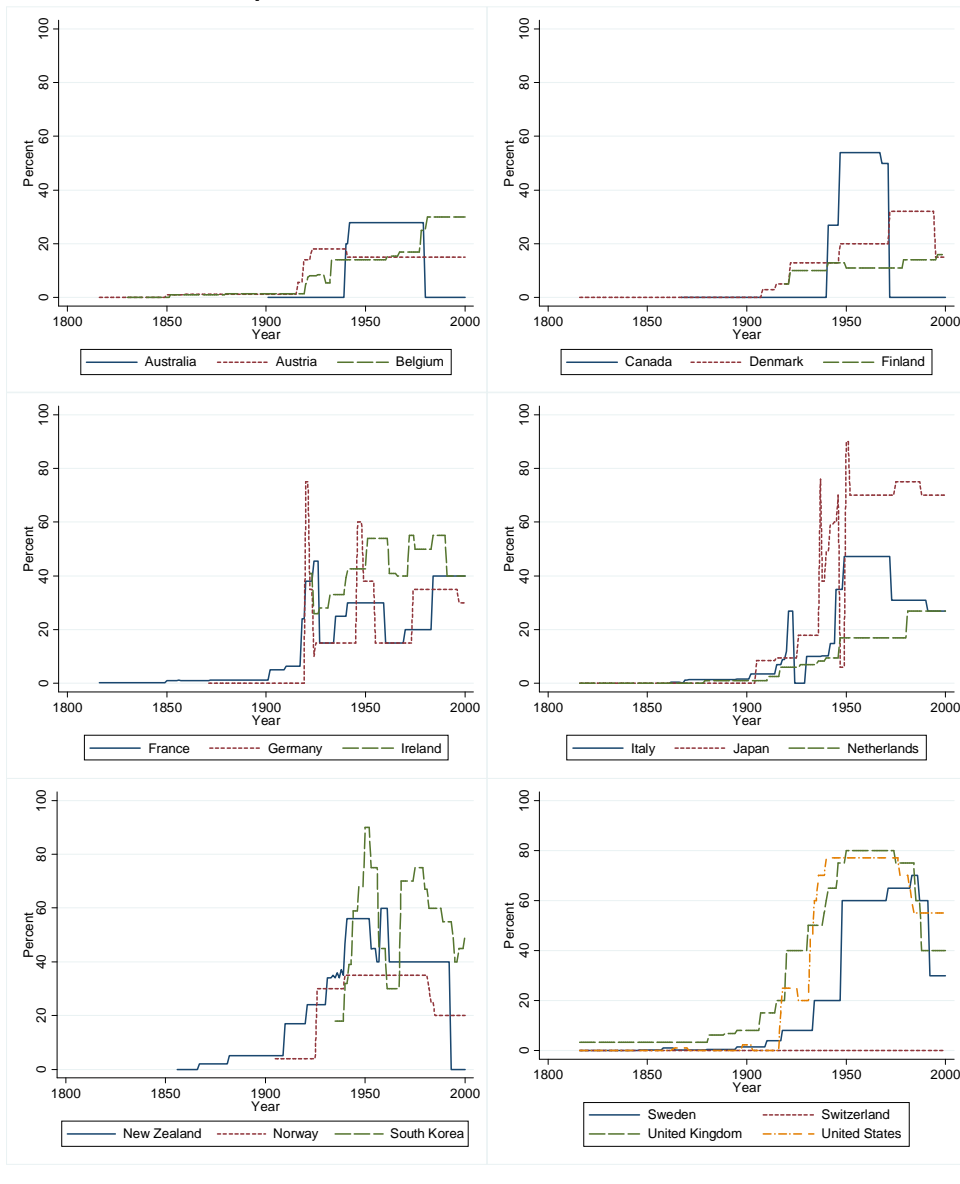
Figure 1: Inheritance Taxation, 1816-2000. This figure records the top marginal inheritance tax rate for direct descendants from 1816 (or independence) to 2000. See data appendix and text for full description of rate definitions and sources.

in both cases passed in 1918. Likewise, by the twentieth century each country had a political party mobilizing working class groups. In spite of these commonalities, the twentieth century evolution of top inheritance tax rates in these two countries has been marked by a very substantial divergence, followed by a recent convergence. In the UK, after several early modest increases, in the immediate wake of World War I the top rate of inheritance taxation was doubled from 20% to 40%, and then dramatically increased again during World War II, reaching a peak of 80%. After remaining at this level through the mid-1970s the top inheritance tax rate was reduced in steps and currently stands at 40%. Now contrast this with the evolution of the top rate in the Netherlands, a country that did not mobilize a large fraction of its population for either of the two world wars. In the Netherlands the top marginal rate of inheritance taxation long remained well below the top rate in the UK.

What interpretation should we give to the past divergence and recent convergence between top inheritance tax rates in the Netherlands and UK? A first observation is that suffrage extensions are not necessarily associated with increased redistribution via inheritance taxation. This is abundantly clear for the Netherlands, and even in the UK, though the establishment of universal suffrage coincided with an increase in the top rate of inheritance taxation, the case for the suffrage hypothesis is undermined by the fact that as early as 1886 over three quarters of the British adult male population could vote. If the case for the suffrage hypothesis seems weaker than is often suggested, the prima facie case for the mass mobilization hypothesis could hardly be more strong. In the UK the top marginal rate of inheritance taxation was doubled in 1919 in the immediate wake of an election where the opposition Labour party had adopted the "conscription of wealth" (via progressive taxation) as one of its manifesto commitments and where parliamentary statements by Conservative chancellors of the exchequer paid heed to concerns about the accumulation of "war wealth."[17] Similar logic continued to dominate discussions about inheritance taxation during World War II. Following the war, the UK retained a top marginal rate of inheritance taxation of 80% until 1975 and the current rate of 40% was established following the Conservative Chancellor

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[17]On this latter point see in particular Daunton (2002 p.78).
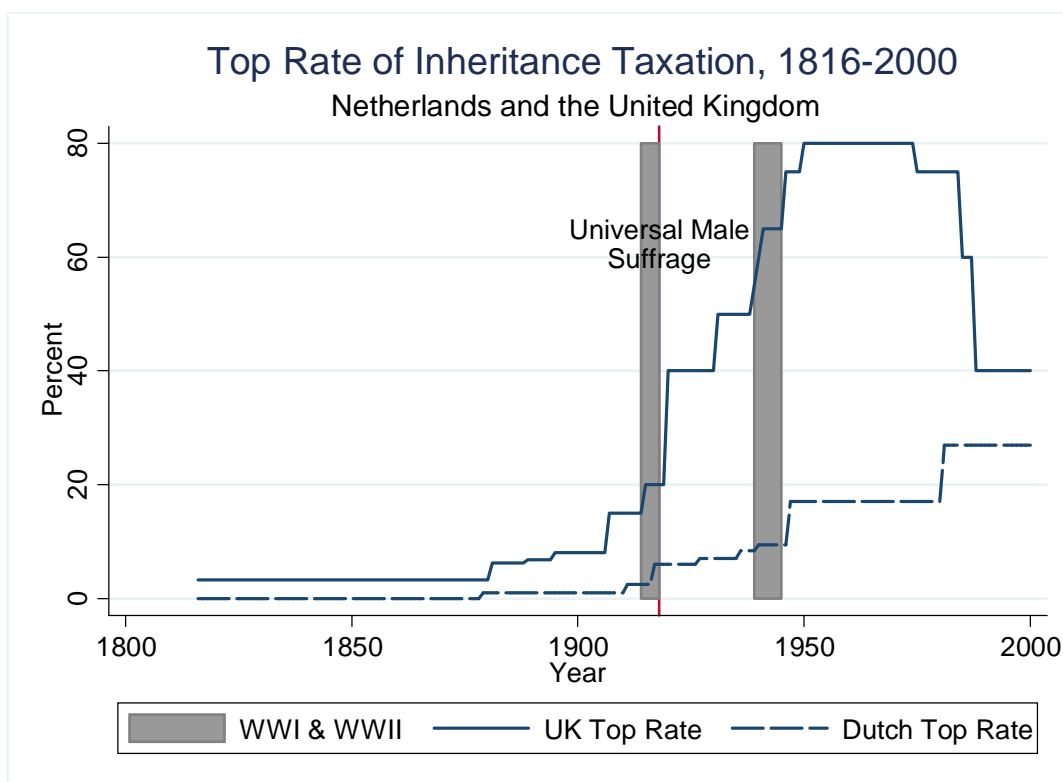
Figure 2: Inheritance Taxation in the Netherlands and the United Kingdom. This figure plots the top marginal rate of inheritance taxation for direct descendants from 1816 to 2000 in the Netherlands and the United Kingdom along with the date of universal suffrage in both countries and the U.K.'s participation in world wars.

Nigel Lawson's budget speech in 1988. Though the opposition Labour leader, Neil Kinnock, referred to the "immense injustice" created by these top rate reductions, in light of previous history, one wonders whether this argument would have had broader resonance if it had referred to an injustice involving war participation and war wealth, instead of the question of redistribution in a peace-time economy.[18]

Before proceeding further with our analysis, it is important to consider how useful top rates are likely to be for testing our hypotheses more generally. The choice of using top rates 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

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[18] For the text of Kinnock's speech the House of Commons debates 15 March 1988 vol 129 cc1017-37.

to investigate the rate at which a society taxes its wealthiest citizens. Nevertheless, the use of top rates for this sample of countries raises three questions for the analysis.

A first question is whether possibilities for engaging in fraud, in inter vivos transfers, or in exploiting legal loopholes render top statutory rates of inheritance taxation meaningless. Regarding the possibility of fraud, while it certainly exists, inheritance taxation inherently requires less administrative enforcement capacity than other modern taxes such as the income tax because the beneficiaries have an incentive to establish their property rights over bequests. 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). Finally, regarding opportunities for exploiting what we have imprecisely called "legal loopholes," the top marginal rates we report do not take account of differences in 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 several of our sample countries. Using this data one can show how significant increases in the top statutory rate of inheritance taxation have been associated with increases in revenues derived from inheritance taxes. As an example, when the UK's Chancellor of the Exchequer announced in April 1919 that effective January 1st 1920 the top rate of estate duty would be increased from 20% to 40%, he suggested that this and other increases in inheritance tax rates would produce 10 million pounds in additional revenue. As it turns out, statistics show that total revenues from estate duties in the following year increased by exactly this amount (Mallet and George, 1924).

A second set of questions focuses on whether these taxes were actually progressive. To consider this possibility, we collected evidence on complete inheritance tax schedules in six

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	**14**	**60**	70	40
	10,000	3.1	7.0	**28**	**80**	75	40
United States	1	0.0	0.0	**0.0**	**0.0**	0.0	0.0
	10	0.0	0.0	**0.0**	**0.0**	11	0.0
	100	0.0	0.8	**1.0**	**30**	35	55
	1000	0.0	1.5	**9**	**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	0.0	0.0	**0.0**	0.0	0.0
	10	0.0	1.5	1.2	**0.0**	0.0	0.0
	100	0.0	1.5	2	**0.0**	50	50
	1000	0.0	4.5	5.5	**0.0**	75	70
	10,000	0.0	7.0	9.5	**90**	75	70
Sweden	1	0.0	0.5	0.6	1.0	5.0	10
	10	0.1	0.7	1.8	11	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.

of our sample countries. We then used this data 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 quick glance at the table provides several important insights. First, inheritance tax rates were generally progressive, and top rates reflect the extent of progressivity. Second, as governments increased statutory rates of inheritance taxation during the course of the twentieth century, they generally increased rates on larger fortunes by greater amounts—an increase in progressivity. Third, statutory tax increases were not limited to symbolic increases of the top marginal rate.

A third question is whether there is a risk of sample bias given the countries and time period that we have chosen. As described in the introduction, our sample is concentrated on those countries for which it is most commonly suggested that the extension of the suffrage led to greater redistribution from rich to poor. Countries outside this sample, such as those in Latin America, are then often presented as deviations for which it needs to be explained why the advent of universal suffrage did not lead to substantial redistribution. But if we find that even in the core countries of Western Europe and North America, democracy did not result in greater redistribution (at least in the form of inheritance taxation), then there may be no deviation to be explained. Consequently, understanding inheritance taxation in this sample of countries is of considerable interest. With this said, we ought to still consider how the exclusion of certain cases might limit the generalizability of our findings. While a definitive conclusion on this question awaits the careful study of more countries there are good reasons to think our findings would hold for a larger sample. Take the case of Latin America. Its countries have on the whole not significantly mass mobilized for war[19] and, despite substantial episodes of democracy, have not heavily taxed inherited wealth.[20] We

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[19] Based on data from the Correlates of War project, in the period from 1815 to the present, the two cases for which mobilization for an interstate war was above the 2% of the population threshold that we use in our analysis are both in Paraguay during the War of the Triple Alliance in the 1860s and the Chaco War in the early 1930s.

[20] Schoenblum (1982) reports top marginal rates of inheritance taxes for a number of Central and South American countries at a time in which the move to lower inheritance tax rates elsewhere was just underway. Among the ten Latin American countries surveyed, the average rate was only 16%, and only two countries had marginal rates of inheritance taxation higher than 25%. This was the case with Chile (55%) and Ecuador (35%). See Kaldor (1963) for an earlier policy piece lamenting the fact that Latin American countries did little to tax top fortunes.

also think that it is unlikely that starting our analysis in 1816 biases our results. There were very limited expansions of the franchise before this date, making earlier periods of limited use for evaluating the democratization hypothesis. War mobilization is also less extensive prior to 1816 with even the most large scale wars having levels of mobilization of two or three percent if that.

4   Methods

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 two 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.[21] 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, *Competitive Elections,* is set equal to one if the legislature is elected in free multi-party elections, if the executive is directly or indirectly elected in popular elections and is responsible either directly to voters

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[21] As is the case with unitary states, for federal states, such as Germany, our variable takes account only of suffrage laws established at the national level and applying to the national legislature, provided that such laws exist. We also take account of available information involving restrictions on certain categories of men, such as male African Americans in the United States prior to 1965. In cases where a country established universal suffrage before becoming fully independent from another power, we use the date of the state's independence to code this variable. This is also the case with all other suffrage variables considered in this paper. Unless otherwise noted below we used either Caramani (2000 p.53) or Mackie and Rose (1974) to code this variable. Dates of establishment of universal suffrage for the countries in our sample are as follows: Australia 1901, Austria 1897, Belgium 1894, Canada 1921, Denmark 1918, Finland 1917, France 1848, Germany 1871, Ireland 1922, Italy 1913, Japan 1925, Korea 1948 (source: Croissant, 2002), Netherlands 1918, New Zealand 1879, Norway 1905, Sweden 1911, Switzerland 1848, United Kingdom 1918, United States of America 1965.

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.[22]

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 impact of expansions of the franchise that lead to less than universal suffrage.[23] Another possibility is that for poorer economic groups to pressure their representatives to tax the rich, ballots need to be confidential. The variable *Secret Ballot* is equal to one if the country uses a secret ballot for lower house elections and zero if not.[24] We also investigate whether it is 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.[25] Finally, we also consider the effect of having an unelected upper house by constructing the variable *No Upper* equal to

[22]This definition and data is from Boix and Rosato (2001). The definition is a modification of the definition used by Przeworski et al. (2000) to a context where the suffrage may be restricted. *Competitive Elections* is coded one for the following years: Australia 1901-2000; Austria 1920-1932, 1946-2000; Belgium 1894-2000; Canada 1867-2000; Denmark 1901-2000; Finland 1917-2000; France 1848-1851, 1870-1939, 1945-2000; Germany 1919-1932, 1946-2000; Ireland 1922-2000; Italy 1946-2000; Japan 1952-2000; Korea 1988-2000; Netherlands 1897-2000; New Zealand 1856-2000; Norway 1905-2000; Sweden 1911-2000; Switzerland 1848-2000; UK 1885-2000; United States 1816-2000.

[23]The source for this data is Flora (1983) for the European cases, the *Statistical History of the American Electorate* for the US, *New Zealand: A Handbook of Historical Statistics* for New Zealand, Griffin (1965) for Japan, Croissant (2002) for Korea, and Mackie and Rose (1974) for Australia. The dates for Canada are inferred from data on 1867 voter turnout.

[24]The sources for this variable are either Caramani (2000) or Mackie and Rose (1974) unless otherwise noted below. Dates for establishment of the secret ballot are as follows: Australia 1901, Austria 1907, Belgium 1878, Canada 1874, Denmark 1901, Finland 1917, France 1820, Germany 1871, Ireland 1922, Italy 1861, Japan 1900 (Hayashida 1967), Korea 1848 (Croissant 2002), Netherlands 1849, New Zealand 1871, Norway 1905, Sweden 1866, Swtzerland 1872, United Kingdom 1872, United States 1891 (Kentucky was the last state to adopt the secret ballot).

[25]This variable was coded using Caramani (2000 p.58) as the principal source and as otherwise noted below for the remaining countries. Australia 1901 (Mackie and Rose p.1), Austria 1907, Belgium 1847, Canada 1867 (Mackie and Rose p.65), Denmark 1849, Finland 1917, France 1831, Germany 1871, Ireland 1922 (Mackie and Rose p.181), Italy 1861, Japan 1889 (Mackie and Rose p.223), Korea 1948 (Croissant 2002), Netherlands 1848, New Zealand 1857 (Mackie and Rose p.289).

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.[26]

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 a pre-specified percent of the population was serving in the military. For our main estimates we set the cutoff at two percent of the total population, but we will also discuss results involving alternative cutoffs as well as other measures.[27] Our *War Mobilization* variable captures 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.[28]

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

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[26] More formally, this variable takes a value of 1 if any of the follow three conditions are satisfied and zero otherwise: (1) there is no upper house (2) there is an upper house that cannot veto legislation (3) there is an upper house in which members are directly elected through universal male suffrage. Our coding for this variable is based primarily on Marriot (1910, 1926) and on historical information contained on the websites of the respective upper chambers. Additional sources for specific countries are listed at the end of this footnote. The coding for this variable is as follows: Australia 1 for entire period, Austria 1 beginning in 1920, Belgium, 1 beginning in 1918, Canada 0 for all years, Denmark 1 from 1915, Finland 1 for all years, France 0 from 1815-1847 then 1 from 1848-1851 then 0 from 1852-1945 then 1 from 1946 onwards, Germany 0 for all years, Ireland 1 for all years, Italy 1 from 1948, Japan 1 from 1946, Korea 1 for all years, Netherlands 0 for all years, New Zealand 1 for all years, Norway 1 for all years, Sweden 1 from 1918, Switzerland 1 from 1848, United Kingdom 1 from 1911, United States 1 from 1913. Additional sources: Canada: Committees and Private Legislation Directorate, Senate of Canada (2001) "A Legislative and Historical Overview of the Senate of Canada", Denmark: Danish Parliament (2009) "The Parliamentary System of Denmark.", New Zealand: James Christie (1924) "The Legislative Council of New Zealand." *Journal of Comparative Legislation and International Law* pp.19-26. Italy: Gianfranco Pasquino (2009) "The Italian Senate." *The Journal of Legislative Studies*, 8:67-78.

[27] Our data for incidents of war comes from the Correlates of War Project, Militarized Interstate Dispute Data, Version 3.0 (2003). Our data on mobilization is from the Correlates of War Project, National Material Capabilities Data, Version 3.0 (2005). To count as an interstate war, the dispute had to be coded as a war and involve 1,000 or more deaths. We supplemented this data where it was missing and, in one case, where it was incorrect with additional sources.

[28] More precisely, our war mobilization variable is coded one for Austria in 1915-18, 1939-1945; Belgium in 1915-1918; for Australia in 1915-1918, 1941-1945; for Canada in 1915-1918, 1941-1945; for Finland in 1940-1944; for France in 1871, 1914-1920, 1940-1941; for Germany in 1871, 1915-1918, 1939-1945; for Italy in 1915-1918, 1935, 1940-43; for Japan in 1941-1945; for New Zealand in 1915-1918,1941-1945; 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.

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

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[29]; 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.[30]   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. In addition, in some specifications we control for the time-varying factors of government partisanship and levels of development and include country-specific time trends. With this said, it is, of course, possible for the assumptions of the model to be violated in a way that generates correlations between the error term and our key independent variables that would bias our results.

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[29] 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. The main source for the partisanship variable is Flora (1983). 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/.

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

For example, our estimates of β_1 would be inconsistent if there are time-varying unobserved factors that influence inheritance taxation and are correlated with democracy. 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 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 may have the same general concerns. 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 with our sample. First, many of the decisions by countries that lead them to be differentially exposed to mass warfare are long-term choices that remain fixed during the period of our study. In particular, it is implausible that the timing of war exposure for the key conflicts in our data, such as World War I and World War II, was determined by expectations about the ease of taxing inherited wealth. Skepticism about the importance of this potential source of bias is further bolstered by the fact that in critical cases, such as World War I, none of the initial participants correctly anticipated the length of the conflict or the extent of mobilization necessary to fight the war.[31]

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[31] The often cited quote from Kaiser Wilhelm to the departing troops in August 1914 is, "You will be home before the leaves have fallen from the trees."

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. It seems likely that the influence of these factors would not necessarily be immediate, making analyses based on annual frequencies problematic. Consequently, we focus our analysis on specifications with observations spaced at one, five, and ten year intervals with particular attention on the results over five year intervals. 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:

$$T_{it} = \alpha + \rho T_{it-1} + \beta_1 D_{it-1} + \beta_2 W_{it-1} + \boldsymbol{\gamma} \mathbf{X}_{it-1} + \theta_t + \varepsilon_{it} \tag{2}$$

There are 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 which might bias our estimates of β_1 and β_2. It conditions 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. 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 OLS estimates of this model and report panel-corrected standard errors to account for country heterogeneity and cross-country correlations in our data.[32]

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[32] 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.

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.

5   Estimation Results

Tables 2 and 3 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-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). Columns (9) and (10) report results for our annual 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 and Table 3 uses the *Competitive Elections* measure.

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), (3), (6), (7), (8), and (10) but negative in columns (4), (5), and (9). None of the positive estimates approach statistical significance at conventional levels and the magnitudes of the estimates are not particularly

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large. Importantly, for the five-year panels, the two specifications that include time varying controls and country-specific time trends yield estimates of less than one and relatively large standard errors (the fixed effects estimate is 0.934 with a standard error of 3.973 and the lagged dependent variable estimate is 0.751 with a standard error of 1.779). The results for the ten-year and annual panels are qualitatively the same. While the standard errors for these estimates are too large 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-4 and also fail to provide 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 ten 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 18.468 and 23.017 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 18 to 23 percentage points compared to a country that did not mobilize for war. The magnitude of this effect is somewhat

Table 2: War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Universal Male Suffrage Measure of Democracy.

| | 5-year Data | | | | | | 10-year Data | | Annual Data | |
| | Country Fixed Effects | | | | Lag DV | | Country FE | Lag DV | Country FE | Lag DV |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top Rate$_{t-1}$					0.871	0.653		0.367		0.931
					(0.038)	(0.062)		(0.123)		(0.012)
					0.000	0.000		0.003		0.000
War Mobilizaton$_{t-1}$	23.017	21.464	18.468	14.456	14.651	14.490	26.153	23.606	5.532	1.578
	(6.197)	(5.848)	(5.668)	(3.730)	(3.774)	(4.078)	(12.099)	(10.817)	(2.224)	(0.747)
	0.002	0.002	0.004	0.000	0.000	0.000	0.044	0.029	0.023	0.035
Universal Male Suffrage$_{t-1}$	3.505	6.024	0.934	-2.344	-2.638	0.751	0.959	3.060	-1.017	0.457
	(5.970)	(5.915)	(3.973)	(1.651)	(1.645)	(1.779)	(4.867)	(3.102)	(3.769)	(0.404)
	0.564	0.322	0.817	0.156	0.109	0.673	0.846	0.324	0.790	0.258
Left Executive$_{t-1}$		0.098	1.911		2.688	3.768	3.607	4.631	1.253	0.606
		(5.448)	(3.586)		(1.542)	(1.683)	(6.628)	(3.303)	(1.985)	(0.304)
		0.986	0.601		0.081	0.025	0.593	0.161	0.536	0.046
GDP per capita$_{t-1}$		0.001	0.001		-0.000	0.001	0.002	0.001	0.001	0.000
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
		0.676	0.335		0.587	0.066	0.321	0.209	0.349	0.405
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No	Yes	No
R-squared	0.711	0.721	0.836	0.877	0.874	0.892	0.844	0.840	0.831	0.964
Number of Observations	544	516	516	543	515	515	254	253	2,537	2,536

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, 7, and 9 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6, 8, and 10 include a lagged dependent variable and report panel-corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-10 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

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larger than the mean of the *Top Rate* variable (17) and about the same size as its standard deviation (22). 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 14.456 and 14.651, again with relatively small standard errors.[33] The estimates for the ten-year and annual panels are qualitatively the same.[34]

The results in Table 3, which employs the *Competitive Elections* measure of democracy, follow those in Table 2 extremely closely. The coefficient estimates for *Competitive Elections* have mixed signs and are statistically insignificant at conventional levels. 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.

The evidence in Tables 2 and 3 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.[35]

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[33] The implied long-run effects of these estimates are large and somewhat variable with arguably the most credible magnitude equal to 42 percentage points based on the specification with country-specific time trends. This would suggest an effect of 8 to 16 percentage points for a shorter conflict of one or two years. We note further that the magnitude of specifications without these trends but with country fixed effects—reported in Table A-6—have similar, though somewhat larger, long-run magnitudes as the Table 2 specification with country-specific time trends. We do not emphasize these long-run estimates because the strategy here is to use the lagged dependent variable to control for time-varying unobservables—except of course the influence of contemporaneous shocks—in estimating the parameters for the democratization and mass mobilization variables.

[34] 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), (7), and (9) of each table, the estimates are generally positive but they are imprecisely estimated with relatively large standard errors. In the lagged dependent variable specifications reported in columns (5), (6), (8), and (10) however, the estimates are positive and, in the 5-year and annual panels, statistically significant. 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. The coefficient estimates for our other time-varying control variable *GDP per capita$_{t-1}$* are generally positive but not statistically significant. We tried a number of functional forms for this variable but none of them yielded significant results.

[35] In addition to the tests discussed below, we conducted a number of standard sensitivity tests including dropping one country from the analysis at a time for our baseline specifications. Our coefficient estimates were quite stable across these different samples.

	5-year Data						10-year Data		Annual Data	
	Country Fixed Effects			Lag DV			Country FE	Lag DV	Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Top\ Rate_{t-1}$				0.871	0.877	0.657		0.385		0.931
				(0.039)	(0.037)	(0.062)		(0.122)		(0.012)
				0.000	0.000	0.000		0.002		0.000
$War\ Mobilizaton_{t-1}$	23.335	23.123	18.939	13.582	13.513	14.575	26.281	24.248	5.970	1.666
	(6.227)	(6.425)	(6.041)	(3.825)	(3.914)	(4.155)	(12.911)	(11.057)	(2.211)	(0.749)
	0.001	0.002	0.006	0.000	0.001	0.000	0.057	0.028	0.015	0.026
$Competitive\ Elections_{t-1}$	-0.050	3.055	1.158	-1.327	-1.612	-0.030	0.010	-0.027	2.159	0.356
	(6.107)	(5.182)	(3.460)	(1.316)	(1.257)	(1.231)	(4.094)	(2.321)	(3.171)	(0.485)
	0.994	0.563	0.742	0.313	0.200	0.980	0.998	0.991	0.505	0.463
$Left\ Executive_{t-1}$		-0.015	1.906		2.524	3.809	3.684	4.937	1.165	0.588
		(5.417)	(3.679)		(1.530)	(1.683)	(6.770)	(3.240)	(2.044)	(0.308)
		0.998	0.611		0.099	0.024	0.593	0.128	0.576	0.057
$GDP\ per\ capita_{t-1}$		0.001	0.001		-0.000	0.001	0.002	0.001	0.001	0.000
		(0.002)	(0.002)		(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
		0.703	0.384		0.954	0.068	0.349	0.247	0.394	0.391
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	No	Yes	No	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No	Yes	No
R-squared	0.709	0.718	0.836	0.877	0.874	0.892	0.844	0.839	0.832	0.964
Number of Observations	544	516	516	543	515	515	254	253	2,537	2,536

Table 3: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Competitive 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 *Competitive Elections* lagged one period. The specifications in columns 1-3, 7, and 9 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6, 8, and 10 include a lagged dependent variable and report panel-corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-10 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

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First, as discussed above we consider several alternative measures of democracy including the presence of a secret ballot, the existence of direct elections, partial extensions of the franchise, and the absence of nondemocratic upper house with the power to veto legislation. Results for these measures are reported in Appendix Tables A-2, A-3, A-4, and A-5. 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 and 3. Moreover, the democracy measures for *Direct Elections*, *Secret Ballot*, and partial extensions of the franchise are not significantly correlated with the top rate of inheritance taxation.

The one partial exception to the pattern of results that we have observed so far is for specifications that include the *No Upper* measure of democracy reported in Table A-5. It is still the case that the coefficient estimates for *War Mobilization*$_{t-1}$ are positive and statistically significant across all ten specifications. What differs is that the coefficient estimates for the variable *No Upper* are positive across all specifications and statistically significant in five of the ten specifications (albeit only at the 0.10 level in two of the specifications). The positive estimates are 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. However, this result is not robust to alternative specifications and as such does not substantially change the overall story that there seems little evidence in this data consistent with the democratization hypothesis.

Second, we also considered three 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 and 3 yields positive and statistically significant estimates for the mobilization coefficient. The second alternative we investigated set each country's value for war mobilization equal to the proportion of the population mobilized in war years and

equal to zero in all other years. Again, the results closely mirrored those reported in Tables 2 and 3. The third 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 does not rely on the Correlates of War mobilization data which may be measured with error but rather focuses simply on the dates of participation in these wars. 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 and 3 dropping any 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 and 3 for both our democracy measures and war mobilization.

Fourth, we investigated two arguments related to the war mobilization hypothesis. Thus far, we have assumed 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 that 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-7 reports results in columns (1)-(4) which test this argument by interacting the *Universal Male Suffrage*$_{t-1}$ and *Competitive Elections*$_{t-1}$ 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 result for the interaction term is in the wrong direction and is sensitive to specification choices.

Another argument related to the war mobilization hypothesis is that left governments,

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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-7 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-6 reports estimates for specifications including a lagged dependent variable and country fixed effects.[36] The main results reported in Tables 2 and 3 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 *Competitive Elections*$_{t-1}$ are statistically significant or large in magnitude.

Finally, in the appendix, Table A-8 reports further specifications using the annual data set. These specifications do not include country-specific time trends as in Tables 2 and 3 but the results are qualitatively the same. 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.[37]

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[36] 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.

[37] Another factor which might influence inheritances taxes but is not explicitly addressed in our main speci-

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 reasons 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.

6   Interpreting the War Result

We have presented evidence of a robust correlation between war mobilization and top marginal rates of inheritance taxation. We have further argued that this correlation is due to political conditions favoring setting higher taxes on the wealthy to establish greater equality of sacrifice in the war effort. For this interpretation of the war result to be compelling, it needs to be the case that the result for the top rate reflects overall changes in the progressivity of inheritance taxation—that is that war mobilization is associated with greater taxation of larger estates rather than just increased taxation of all estates. Further, our argument requires that other forms of taxes and spending did not counterbalance the progressive effect of inheritance

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fications is policy diffusion in which the setting of rates in one country or set of countries influences the policy choices in others. Many of the most plausible diffusion processes are controlled for in the analysis by the inclusion of period fixed effects. For example, if states respond to average tax rates in the sample or to the maximum rate chosen by any country in the sample, our period fixed effects capture this common shock. But alternative policy diffusion processes may be based on some subset of countries influencing a given country more than others. The most obvious candidate for this is based on the influence of neighboring countries. Qualitatively, we seem to observe this effect in specific cases. For example, most of the inheritance tax legislation adopted by Finland after independence can be traced directly to Swedish law. We looked for more systematic evidence for neighborhood influence by adding a spatial lag—defined by contiguity—to our main specifications. We found little systematic evidence of a neighborhood effect, and the inclusion of the spatial lag did not affect our estimates for mobilization and democratization.

taxation. Finally, we need to explicitly consider the most obvious alternative interpretation of the war result which is that top rates of inheritance taxation were raised simply because the wars were expensive affairs that needed to be financed. In this section we focus on each of these issues in turn.[38]

6.1 Evidence of Progressivity from Complete Inheritance Tax Schedules

We can explore the first question by using the data in Table 1 that reports the marginal tax rate on the last unit of wealth for estates of different sizes. Ideally, we would be able to report the rates prevailing for estates at specific points in the wealth distribution for each country, but the sort of information necessary to construct such statistics is only available for an even more limited number of cases. As a feasible alternative, we can consider estates at different multiples of GDP per capita.

Using the above evidence, now consider changes in tax rates immediately following periods of mass mobilization for war. This shows clearly that the war effect observed in the previous section applies for large fortunes more generally. For the case of World War I it is clear that participant countries increased rates very substantially for fortunes equivalent to 1000 times per capita GDP or more (roughly $45 million in the United States today). However, for fortunes equivalent to 100 times per capita GDP or less, the war effect is much less apparent. In the US and UK fortunes of this magnitude were not taxed at higher rates after World War I. In France, smaller fortunes were more heavily taxed after the war, but the magnitude of this increase was small compared to the magnitude of the tax increase on the largest fortunes. Now consider the case of participation in World War II. For the United Kingdom, the United States, and Japan war participation was accompanied by increases in rates of inheritance taxation and in the progressivity of inheritance tax schedules. France was an exception to this pattern.

It is also worth noting how the Table 1 evidence can provide further insight into our results

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[38] For qualitative evidence that highlights how the rhetoric of equal sacrifice informed debates about progressive taxation, see Scheve and Stasavage's (2010, pp. 549-552) discussion of Canadian and UK policymaking during World War I.

regarding suffrage extensions. In our econometric analysis we found essentially no evidence that the extension of the suffrage was associated with an increase in the top marginal rate of inheritance taxation. Perhaps this result is attributable to the fact that new democracies actually increased inheritance tax rates at other levels. The Swedish and Dutch examples strongly suggest that this was not the case. Irrespective of the level of fortune considered, prior to the end of the Second World War both of these countries maintained low marginal tax rates. Nor is there evidence in any other country of a significant post-suffrage extension increase in tax rates during peacetime.

6.2   Compensating Taxes and Transfers

Our argument that mass mobilization for war creates political conditions favoring more progressive inheritance taxation suggests that the overall tax and transfer system should be more redistributive in order to ensure equal sacrifice in the war effort. Although producing a full and comprehensive evaluation of this larger claim is beyond the scope of the current study, it is, nonetheless, important to consider how likely it is that the overall pattern of taxation and spending is consistent with our results for inheritance taxation.

We begin by focusing on the progressivity of a country's tax system. A first question we might ask is whether the war mobilization effect is also apparent when we look at top marginal rates of income taxation. We have focused on inheritance taxation in this paper because doing so helps reduce the likelihood that bureaucratic capacity is a confounding factor in our analysis. But over time in all of our sample countries the income tax has become much more important than inheritance taxation as an aggregate revenue source. As a result, it is important to see whether we draw similar conclusions regarding war mobilization (and universal suffrage) when looking at income tax rates. Appendix Table A-9 reports top marginal income tax rates for the same set of six countries considered in Table 1. Among these six countries the UK, US, and France crossed the 2.0% threshold for mass mobilization in World War I whereas the other three countries did not. Among these six countries the UK, US, France, and Japan crossed the 2.0% threshold during World War II whereas

Sweden and the Netherlands did not. If we compare the difference in rates between 1900 and 1920 we observe that the countries that mobilized for the war increased their top tax rates very substantially whereas the three non-mobilizers implemented only moderate increases. Turning next to World War II the evidence for the effect of war mobilization is not as stark as in the case of World War I, but it is still present. It should also be noted here that among the two countries that did not mobilize for war, the increase in the top income tax rate in the Netherlands was actually a decision made by the Nazi occupying forces.[39] That said, Sweden's adoption of relatively high marginal income tax rates in the mid and late twentieth century suggests that war mobilization was not the only path to progressive taxation for modern states.

The information in Table A-9 can also be used to draw inferences about the effect of the extension of the suffrage. The evidence for the universal suffrage hypothesis in this table is weak. Sweden, the Netherlands and France all had universal suffrage for a number of decades before top rates of income taxation reached levels above 40%. In the UK the final achievement of universal suffrage in 1918 did coincide with a very substantial increase in the top marginal rate of income taxation, but we need to remember that a substantial majority of adult males had the vote for several decades before this date. Finally, neither Japan, which adopted universal suffrage in 1925, nor the United States, which adopted universal suffrage for whites before the period considered here and for all groups in 1965, provides a particularly compelling case for the suffrage hypothesis. Using annual data for the 1900-30 period in a slightly larger set of countries that also included Canada and Spain, we did identify a statistically significant effect of universal suffrage on the top rate of income taxation, but the magnitude of this effect was small (an approximately seven percentage point increase) relative to the very large effects that we identify for World War I mobilization.[40]

Overall then there is little evidence that our main conclusions regarding democracy, war, and taxes are biased by focusing on inheritance taxation to the exclusion of income taxation.

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[39] The Dutch government in exile in London advocated maintaining a much lower top rate (see Vording and Ydema, 2009).

[40] Scheve and Stasavage (2010).

But we have still said little about the overall burden of taxation on households, which would include direct taxes on income and inheritance, indirect taxes on consumption goods, and taxes on corporate profits for owners of capital. It may have been the case that income and inheritance taxes became more progressive as a result of war, but increases in indirect taxes (commonly thought to be regressive in their incidence) meant that the overall burden of taxation did not become more progressive. Dealing with this question in full is extremely complicated, because it depends on knowledge of consumption and ownership patterns of households with different levels of income. Fortunately, there is one existing study that attempts this exercise for the United Kingdom over the first half of the twentieth century. The results of the study by Shirras and Rostas (1943) are reported in Table 4. The figures for each cell in the table represent the total burden of taxation as a percentage of gross income for a family of five, and they include income taxation, death duties (with cost spread over a lifetime), indirect taxes (assuming moderate consumption alcohol, tobacco, sugar, and tea), and finally the burden from taxation of business profits.

The results of the Shirras and Rostas study are very revealing. At the beginning of the twentieth century the total burden of taxation in the UK was essentially identical across different income groups. At the outset of World War I, in spite of the fact that a large majority of adult males had enjoyed the right to vote for some time, the schedule for the total burden of taxation was only mildly progressive. As can be seen, by 1918 this situation had changed very dramatically. For those with incomes less than £1000 tax rates had doubled, but for those at the top of the income scale tax rates had increased by more than a factor of five, so upwards of half of gross income would now be paid in one form of taxes or another. This situation remained relatively unchanged until fiscal 1941, the first year of World War II in which the government significantly increased direct tax rates. While households at all income levels saw their tax burden increase quite substantially, those with high incomes were by far the hardest hit.

These results suggest that at least for the United Kingdom, the inferences we have drawn about progressivity by looking at top rates of inheritance taxation are not biased by the

Income		Total taxation: percentage of income						
£	1903	1913	1918	1923	1925	1930	1937	1941
100	5.6	5.4	9.9	14.1	11.9	11.0	10.4	19.1
150	4.5	4.4	9.0	13.5	11.6	10.9	9.5	16.7
200	4.8	4.0	7.9	11.8	10.2	9.6	8.4	14.8
500	5.3	4.4	10.2	8.0	6.2	4.5	5.6	18.4
1000	6.1	5.2	16.9	14.1	11.0	9.7	11.8	32.2
2000	5.7	4.9	24.0	17.9	15.2	15.7	18.0	40.5
5000	5.5	6.7	36.6	28.5	23.2	26.3	29.2	56.1
10000	5.0	8.0	42.5	37.1	31.2	35.8	39.1	68.3
20000	4.9	8.3	47.6	42.3	37.5	43.5	47.9	80.7
50000	4.8	8.4	50.6	48.0	44.4	51.4	56.7	90.7

Table 4: The Total Burden of Taxation in the UK as reported by Shirras and Rostas (1943 p.59). The estimate includes the burden from all forms of taxation, both direct and indirect. Direct taxes include income tax and death duties. Indirect taxes include all customs and excise duties. The estimated burden from taxes on business profits is also included. All estimates are for the case of a married taxpayer with three children under the age of 16. Data are for fiscal years.

failure to consider the burden of indirect taxation. Is there any reason to believe that the UK would be unrepresentative in this regard? We know from our investigation of inheritance and income tax rates that the UK was certainly not alone among war participants in increasing the progressivity of direct taxation. The main question then would be whether other war participants increased indirect taxation more substantially than the UK, which would likely imply that their overall burden of tax was allocated in a less progressive fashion. There is no reason to believe that the UK was dramatically different in this regard, even if it is true that there were variations between countries in the extent to which governments raised money primarily through direct versus indirect taxation.[41]

Though our evidence suggests that wartime increases in top rates of inheritance taxation were indicative of a broader shift towards tax progressivity, there does remain a further important question. The shift towards tax progressivity appears to have come above all in the form of taxes on those at the top of the income and wealth distributions. However, it raises the question whether increased taxes on the rich were primarily of symbolic importance because the number of individuals concerned was so small, or whether it was instead the case that these increased taxes on the rich made it possible to tax everyone else significantly less than would otherwise have been the case. If the first of the above two interpretations held our results would still be important; because of the need to satisfy fairness demands, the rich in a number of industrial countries were taxed at rates that previously seemed unimaginable, and we know from the studies cited in the introduction that this had a notable effect on overall wealth inequality. But we can go further than this to suggest that higher taxation on the rich actually did make it possible to alleviate the extent to which taxes on other social groups would also need to increase. Take the case of Great Britain in the wake of the First World War.[42] In a country with 24 million tax units, 3 million of these were liable for the reduced and standard rates of income tax (15% and 30%). Within this group roughly 79,000 tax units (or 0.3% of total tax units) were also liable for the super-tax which had

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[41] See the detailed study by Seligman (1924) for the period before and after World War I.

[42] The tax and revenue figures we use are are from the 1920-1921 fiscal years, drawing on Mallet and George (1929) and Mitchell (1988). The estimation of the total number of tax units is from Atkinson (2007).

a maximum rate of 30% (to be added to the standard rate of income tax). Now imagine the hypothetical case in which the supertax was abolished, death duties were also abolished (another tax hitting only those at the top), and the government compensated for this revenue loss by increasing excise taxes on common consumption goods such as alcohol, cigarettes, tea, and sugar. In order to compensate for the £55 million pounds in lost super-tax revenue, and the £41 million pounds in lost revenue from death duties, the government would have been obliged to increase receipts from excise taxation by almost fifty percent (these stood at £200 million in that same year). This is very clear evidence that the taxes paid by even a small number of wealthy individuals could actually have a significant impact on the taxes it was necessary to charge on the broader British population.

In addition to other taxes, theoretically patterns of government spending could in some countries redistribute to the poor in the absence of increased tax progressivity and undermine our interpretation of the war result. While this question merits further investigation, it should be emphasized that prior to World War II levels of social spending (health, pensions, and welfare) were very low across all of our sample countries. According to the most comprehensive data set able, which was collected by Peter Lindert (2004) and which overlaps significantly with our own sample, as late as 1930 the average combined level of health, pensions, and welfare spending was only 1.34 percent of GDP, with a maximum of 3.4%. Therefore the tax system was the principal means through which any redistribution was occurring in our sample countries, and so at least for this period, our results are not biased by the failure to take account of spending.[43]

Even if one considers later years in the 20th century when social spending becomes a more significant factor in overall redistribution, there are few reasons to think that the consideration of social spending would undermine our main conclusion that mass mobilization for war pushes policy in a more redistributive direction in order to preserve equal sacrifice in the war effort. It is certainly the case that some countries which did not mobilize for war

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[43]Note that the small size of transfers during this period also suggests that if there was an effect of democracy on social spending, this effect was very small in magnitude, an increase in total social spending on the order of half of one percent of GDP.

eventually adopted more redistributive spending policies than some war mobilizers. This, however, simply suggests that war mobilization is not the only factor driving redistributive policymaking. The relevant question for our study is whether those differences in spending between countries that mobilized for war and those that did not would have been even larger in the absence of war mobilization. The importance of war in accounting for important redistributive spending policies such as the National Health Service in the UK suggests that this is the case and bolsters our interpretation of the war effect on progressive taxation.[44]

6.3 Alternative Interpretations of the War Result

The most obvious alternative interpretation of the war effect is that mass mobilized wars are expensive affairs that need to be financed and that states respond to this need by taxing inherited wealth and high incomes. As suggested in the introduction, it seems hard to sustain this claim given that European states had fought expensive wars for centuries and often found themselves in desperate fiscal straits, yet they did not respond by levying significant taxes on top fortunes. The first and second world wars did indeed involve unprecedentedly large expenses for combatant states, but this feature of these wars should not be overemphasized. If we take the case of Great Britain we observe that its peak annual level of spending during World War I was 39% of GDP.[45] Peak spending during the Napoleonic wars amounted to 22% of GDP, a significantly, but not dramatically smaller figure.[46] Moreover, if we look at the fiscal position of the British government as it entered these two periods of conflict we see that it was dramatically worse during the Napoleonic Wars. In 1914 Great Britain had a low level of public debt that amounted to only 25% of GDP, leaving substantial room for further borrowing without increased taxation. In strong contrast, when Napoleon seized power in France, British public debt already stood at 166% of GDP as a result of more than a century

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[44]See e.g. Titmuss (1958).

[45]Spending figures from Mallett and George (1929 p.392). 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.

[46]Spending data from Mitchell (1988). Nominal GDP estimates from Officer (2009).

5-year Data

	Country Fixed Effects		Lagged Dependent Variable	
	(1)	(2)	(3)	(4)
Top Rate$_{t-1}$			0.830	0.834
			(0.025)	(0.026)
			0.000	0.000
War Mobilizaton$_{t-1}$	17.247	18.207	15.144	14.602
	(4.255)	(5.008)	(1.619)	(1.498)
	0.001	0.002	0.000	0.000
Military Expenditures$_{t-1}$	0.202	0.227	0.065	0.066
	(0.064)	(0.068)	(0.017)	(0.017)
	0.006	0.004	0.000	0.000
Universal Male Suffrage$_{t-1}$	6.695		-2.119	
	(7.616)		(0.904)	
	0.391		0.019	
Competitive Elections$_{t-1}$		8.962		-0.577
		(6.065)		(1.070)
		0.157		0.590
Left Executive$_{t-1}$	1.810	1.722	3.410	3.306
	(5.241)	(5.348)	(2.191)	(2.185)
	0.734	0.751	0.120	0.130
GDP per capita$_{t-1}$	-0.001	-0.001	-0.001	-0.000
	(0.002)	(0.002)	(0.000)	(0.000)
	0.664	0.659	0.012	0.042
Period Fixed Effects	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	No	No
R-squared	0.754	0.758	0.877	0.876
Number of Observations	452	452	451	451

Table 5: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Conditioning on Military Expenditures.* The table reports results of pooled-cross-sectional OLS regressions of the variable *Top Rate* on the variable *War Mobilization* lagged one period, the variable *Military Expenditures* lagged one period, selected democracy measures lagged one period, and control variables. The specifications in columns 1-2 include country and period fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 3-4 include a lagged dependent variable and period fixed effects and report panel-corrected standard errors in parentheses and p-values.

of borrowing to fund participation in conflicts.[47]   As a result, if simple fiscal necessity was the main force prompting the British government to raise the top rate of inheritance taxation, we would have expected this development to occur considerably earlier.   The main explanation for why it was only during World War I that the British government significantly raised inheritance taxes may therefore lie elsewhere, and in particular with the fact that a much greater percentage of the British population was mobilized during this later conflict.[48]

The comparison between Great Britain during World War I and during the Napoleonic Wars is obviously not ideal, because many other features also differed between these two periods, in particular the extension of the suffrage.   One further way to consider the fiscal necessity argument is to augment our regression specifications from equations 1 and 2 with a variable representing total military spending.   After this modification we can observe whether the β_2 coefficient on our war mobilization variable remains of similar magnitude and significance. If the effect was primarily due to the need for revenue, we would expect it to be substantially attenuated once we control for military spending.

Our measure of military spending, *Military Expenditures*, is equal to total military expenditures in a given country and year.[49]   The results of this analysis are reported in Table 5. The coefficient estimates for the variable *Military Expenditures* are positive and statistically significant, as would be expected if spending needs put upward pressure on states to tax inherited wealth. However, there is no evidence that the inclusion of the spending variable significantly attenuates the impact of mass mobilized wars on inheritance taxation.   This pattern of estimates is consistent with our argument that the chief mechanism driving the war effect is that mass mobilized wars create political conditions conducive to the progressive

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

[48]According to the Correlates of War data, at the peak of World War I Britain mobilized approximately 4.4 million men, or 10.2% of the total British population. If we adopt the figure used by Colley (1994), then Great Britain at the peak of the Napoleonic Wars mobilized approximately 390,000 men between its army and navy, or 2.1% of the total British population at the time.

[49]The source for the variable is the Correlates of War National Material Capabilities Data and the original measure is in current British Pounds (billons) for 1816-1913 and current US dollars (billions) for after 1914. We convert all measures to US dollars in real terms with 1982-84 as the base year using data from http://www.measuringworth.com. Our estimates add this variable lagged one period to our main estimating equations.

taxation of wealth in order to ensure equal sacrifice in the war effort but is not consistent with the alternative mechanism that war-generated revenue needs alone account for the war effect.

7   Conclusion

What factors prompt a society to significantly tax inherited wealth? The evidence that we have collected suggests that democracy based on universal suffrage has not been a sufficient condition for this to occur. This result has important implications for the extensive literature on the political economy of redistribution, taxation, and political regimes. The idea that democracy generally, and expansion of the franchise specifically, constitutes a credible commitment to redistribute plays a central role in much work in this field. Our study suggests that in at least one important policy domain—the taxation of inherited wealth—the absence of a relationship between democratization and redistribution may be more general. This raises a number of questions for future research. If the result is specific to certain policy domains and democratization systematically leads to policies favoring the poor in some issue areas but not others, identifying compelling accounts for when democratic institutions are influential is an important research agenda. If, in contrast, more systematic data collection and analysis throws doubt on the importance of democratization across many or most redistributive policy instruments, more attention should be focused on identifying the alternative mechanisms by which democracy fails to result in greater redistribution from rich to poor.

The much more consistent result in our analysis is that mass mobilization for war has been a major force leading to heavy taxation of inherited wealth. Trends in inheritance taxation have closely followed shifts in the format of military force. As the industrial countries adopted militaries based on universal conscription and they fought major wars against each other, this generated pressures for an analogous conscription of wealth based on fairness grounds. As the industrial countries have shifted away from fighting large wars with mass armies the argument for a conscription of wealth has no longer had such salience. This may provide one important reason (although certainly not the only reason) why so many governments have

lowered taxes on top fortunes over the last few decades.

Finally, while we have made a specific claim about mass warfare, our results also have more general implications for progressive taxation, including during periods of peace. In modern societies there is a strong sense that individuals ought to be treated equally. Yet progressive taxation involves treating individuals unequally by obliging some individuals to pay a higher tax rate than others. A main lesson of our work is that support for progressive taxation is greatest when its advocates can make a convincing case that it is necessary to tax some individuals more heavily in order to compensate for some prior source of unfairness. In the absence of such an appeal, arguments that the rich should pay more simply because they have a greater ability to pay may fall on deaf ears.

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A Appendix

A.1 Data for Inheritance Tax Rates

We have constructed a new data set recording key features of inheritance taxation for nineteen countries from 1816 or the date of national independence to 2000.[50] 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 refer to all forms of bequest taxation as inheritance taxes, and in constructing this data set, we combine the taxes where necessary to determine the total amount of inheritance taxes at a given time. The remainder of this section describes the data sources for our primary measure of the extent of inheritance taxation—the top rate of inheritance tax for direct descendants inheriting cash.

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).

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[50]The dataset will be made publicly available, together with copies of all relevant national legislation and more general descriptions of the key features of each country's inheritance taxation laws upon completion of this project.

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 (2009).

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   Data for Income Tax Rates

France

For purposes of measuring the top marginal tax rate in France Piketty (2001 ch.4) provides full schedules showing marginal income tax rates for France for the years 1915 to 1998. He also reports a series for the top marginal tax rate that takes into account surcharges (*majorations*), including those levied only on certain types of households, such as those without children (p.325, 566). His goal is to consider the marginal tax rate faced by the household in the most unfavorable position. Our goal is slightly different in that we seek exclusively to measure the marginal tax rate faced by the richest households. In addition, we also face some uncertainty whether any surcharges of the sort reported by Piketty for France have been taken into account in the other country series that we use. In order to maximize the likelihood of inter-country comparability, we constructed a top rate series for France based exclusively on the top marginal rates (*barèmes d'imposition*) reported in Piketty (2001 Tables 4-1 to 4-5).

Japan

Moriguchi and Saez (2007 Table A0) report statutory top marginal tax rates for Japan for all years 1886-2005.

Netherlands

For the top rate Salverda and Atkinson (2007 p.455) report effective top share tax rates for the period following the establishment of the modern Dutch income tax 1914-1999. We use the series for the effective tax rate on the top 0.05% income group. For the period prior to 1914 we rely on Vording and Ydema (2009).

Sweden

Roine and Waldenström (2008) report top share tax rates for the years 1903-2004 including both the state (national) income tax and the communal (local) income tax. We use their series for the highest marginal tax rate.

United Kingdom

For the top rate during the period between 1900 and 1919 we refer to the standard rate of income tax as reported in Mitchell (1988) and to super tax rates as reported by Mallett and George (1929 p.399). For the period between 1920 and 2002 we use data on the top marginal tax rate on wage income provided by Anthony Atkinson and Andrew Leigh.

United States

We use the top marginal tax rate as reported in Senate Committee on Finance (2001). The rates presented are statutory top marginal tax rates, and these include any surtax.

Variable	Observations	Mean	Standard Deviation
Top Rate	2,798	17.141	22.276
War Mobilization	2,798	0.038	0.191
Universal Male Suffrage	2,798	0.643	0.479
Competitive Elections	2,798	0.670	0.470
No Upper	2,795	0.496	0.500
Direct Elections	2,795	0.860	0.347
Secret Ballot	2,795	0.784	0.412
Electorate 25	2,798	0.785	0.411
Electorate 50	2,798	0.727	0.446
Electorate 75	2,798	0.708	0.455
Left Executive	2,798	0.167	0.368
GDP per capita	2,555	6,682	5,603

Table A-1: Descriptive Statistics, 1816-2000, Annual Data.

A.3   Additional Results

	5-year Data						10-year Data		Annual Data	
	Country Fixed Effects				Lag DV		Country FE	Lag DV	Country FE	Lag DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top Rate$_{t-1}$				0.871	0.876	0.663		0.385		0.932
				(0.039)	(0.037)	(0.061)		(0.123)		(0.010)
				0.000	0.000	0.000		0.002		0.000
War Mobilizaton$_{t-1}$	24.307	23.191	19.158	16.196	16.317	16.743	26.275	24.290	5.556	1.587
	(6.013)	(6.407)	(5.737)	(3.368)	(3.366)	(3.608)	(12.293)	(10.826)	(2.244)	(0.671)
	0.001	0.002	0.004	0.000	0.000	0.000	0.047	0.025	0.023	0.018
Direct Elections$_{t-1}$	-2.694	-0.988	1.899	0.147	0.186	0.226	-0.050	0.694	-0.006	0.049
	(8.562)	(8.461)	(2.875)	(1.355)	(1.583)	(1.487)	(3.853)	(2.208)	(2.399)	(0.550)
	0.757	0.908	0.517	0.914	0.906	0.879	0.990	0.753	0.998	0.930
Left Executive$_{t-1}$		0.544	1.926		2.592	3.872	3.687	4.816	1.236	0.615
		(5.725)	(3.696)		(1.547)	(1.698)	(6.808)	(3.318)	(2.040)	(0.269)
		0.925	0.609		0.094	0.023	0.595	0.147	0.552	0.022
GDP per capita$_{t-1}$		0.001	0.001		-0.000	0.001	0.002	0.001	0.001	0.000
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
		0.805	0.345		0.781	0.172	0.318	0.242	0.358	0.413
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	No	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No	Yes	No
R-squared	0.709	0.716	0.835	0.878	0.875	0.893	0.844	0.839	0.830	0.963
Number of Observations	543	515	515	542	514	514	254	253	2,534	2,533

Table A-2: *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, 7, and 9 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6, 8, and 10 include a lagged dependent variable and report panel-corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-10 include control variables for lagged partisan control of government and lagged GDP per capita. All specifications include period fixed effects.

Table A-3: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Secret Ballot Measure of Democracy.*

| | 5-year Data | | | | | | 10-year Data | | Annual Data | |
| | Country Fixed Effects | | | Lag DV | | | Country FE | Lag DV | Country FE | LagDV |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top Rate$_{t-1}$				0.871	0.876	0.663		0.384		0.932
				(0.039)	(0.037)	(0.061)		(0.123)		(0.010)
				0.000	0.000	0.000		0.002		0.000
War Mobilizaton$_{t-1}$	24.581	23.383	19.054	16.194	16.314	16.738	26.070	24.243	5.529	1.593
	(6.025)	(6.504)	(5.672)	(3.369)	(3.367)	(3.610)	(12.249)	(10.831)	(2.237)	(0.671)
	0.001	0.002	0.003	0.000	0.000	0.000	0.047	0.025	0.024	0.018
Secret Ballot$_{t-1}$	4.151	4.014	-1.099	0.145	0.164	0.633	-2.460	1.465	-2.018	0.278
	(5.396)	(5.512)	(2.544)	(1.294)	(1.257)	(1.197)	(3.416)	(2.068)	(1.902)	(0.423)
	0.452	0.476	0.671	0.911	0.896	0.597	0.481	0.479	0.303	0.510
Left Executive$_{t-1}$		0.320	1.980		2.591	3.888	3.721	4.903	1.252	0.614
		(5.655)	(3.676)		(1.547)	(1.698)	(6.721)	(3.265)	(2.031)	(0.263)
		0.956	0.597		0.094	0.022	0.587	0.133	0.545	0.020
GDP per capita$_{t-1}$		0.001	0.001		-0.000	0.001	0.002	0.001	0.001	0.000
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
		0.821	0.364		0.784	0.181	0.320	0.261	0.362	0.444
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	No	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No	Yes	No
R-squared	0.710	0.717	0.835	0.878	0.875	0.893	0.844	0.840	0.831	0.963
Number of Observations	543	515	515	542	514	514	254	253	2,534	2,533

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, 7, and 9 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6, 8, and 10 include a lagged dependent variable and report panel-corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-10 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 | | Annual Data | |
| | Country Fixed Effects | | | Lag DV | | | Country FE | Lag DV | Country FE | LagDV |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top Rate$_{t-1}$				0.872	0.876	0.655		0.381		0.932
				(0.039)	(0.037)	(0.061)		(0.122)		(0.012)
				0.000	0.000	0.000		0.002		0.000
War Mobilizaton$_{t-1}$	23.643	22.429	18.567	14.252	14.376	14.648	26.425	24.399	5.546	1.579
	(5.844)	(5.825)	(5.531)	(3.730)	(3.766)	(4.047)	(11.796)	(10.743)	(2.180)	(0.747)
	0.001	0.001	0.004	0.000	0.000	0.000	0.038	0.023	0.020	0.034
Electorate 25$_{t-1}$	-5.970	-4.137	-1.267	-0.763	-0.926	-1.746	-3.545	-4.131	-2.348	-0.116
	(5.634)	(6.213)	(2.695)	(2.585)	(2.484)	(2.247)	(5.128)	(3.956)	(1.859)	(0.839)
	0.303	0.514	0.644	0.768	0.709	0.437	0.498	0.296	0.223	0.890
Electorate 50$_{t-1}$	7.960	7.083	4.902	-0.181	-0.038	2.690	6.467	5.338	4.288	0.567
	(4.039)	(4.335)	(3.263)	(3.190)	(3.133)	(3.008)	(4.990)	(4.943)	(2.839)	(0.806)
	0.064	0.120	0.150	0.955	0.990	0.371	0.211	0.280	0.148	0.482
Electorate 75$_{t-1}$	-6.139	-4.032	-2.696	-0.313	-0.495	-0.949	-2.482	-1.262	-2.547	-0.189
	(5.609)	(4.852)	(3.704)	(2.445)	(2.518)	(2.440)	(4.079)	(3.920)	(2.937)	(0.614)
	0.288	0.417	0.476	0.898	0.844	0.697	0.550	0.747	0.397	0.758
Left Executive$_{t-1}$		0.517	1.973		2.450	3.785	3.711	4.847	1.232	0.618
		(5.516)	(3.670)		(1.540)	(1.679)	(6.740)	(3.289)	(2.036)	(0.305)
		0.926	0.597		0.112	0.024	0.589	0.140	0.553	0.043
GDP per capita$_{t-1}$		0.001	0.001		-0.000	0.001	0.002	0.001	0.001	0.000
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
		0.768	0.337		0.751	0.065	0.329	0.221	0.342	0.396
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No	Yes	No
R-squared	0.712	0.718	0.837	0.877	0.873	0.892	0.845	0.840	0.832	0.964
Number of Observations	544	516	516	543	515	515	254	253	2,537	2,536

Table A-4: *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, 7, and 9 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6, 8, and 10 include a lagged dependent variable and report panel-corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-10 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 | | Annual Data | |
| | Country Fixed Effects | | | Lag DV | | | Country FE | Lag DV | Country FE | Lag DV |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Top\ Rate_{t-1}$				0.868	0.873	0.646		0.363		0.926
				(0.039)	(0.037)	(0.063)		(0.123)		(0.010)
				0.000	0.000	0.000		0.003		0.000
$War\ Mobilization_{t-1}$	25.968	24.928	19.827	16.451	16.489	17.470	27.149	25.755	5.802	1.718
	(6.324)	(6.741)	(6.039)	(3.377)	(3.397)	(3.662)	(12.475)	(11.023)	(2.156)	(0.672)
	0.001	0.002	0.004	0.000	0.000	0.000	0.043	0.019	0.015	0.011
$No\ Upper_{t-1}$	12.220	14.198	5.087	0.882	0.601	3.728	4.545	6.150	2.365	1.062
	(6.095)	(7.077)	(6.913)	(1.079)	(1.063)	(1.259)	(6.383)	(2.445)	(5.405)	(0.381)
	0.060	0.060	0.471	0.414	0.571	0.003	0.486	0.012	0.667	0.005
$Left\ Executive_{t-1}$		-1.043	1.674		2.452	3.646	3.413	4.479	1.159	0.539
		(5.306)	(3.784)		(1.522)	(1.692)	(6.856)	(3.266)	(2.091)	(0.263)
		0.846	0.664		0.107	0.031	0.625	0.170	0.586	0.041
$GDP\ per\ capita_{t-1}$		0.001	0.001		-0.000	0.001	0.002	0.001	0.001	0.000
		(0.002)	(0.001)		(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
		0.771	0.352		0.770	0.197	0.315	0.239	0.357	0.388
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific Time Trends	No	No	Yes	No	No	No	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	No	No	No	Yes	No	Yes	No
R-squared	0.726	0.738	0.837	0.879	0.876	0.894	0.845	0.843	0.831	0.964
Number of Observations	543	515	515	542	514	514	254	253	2,534	2,533

Table A-5: *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, 7, and 9 include country fixed effects and report robust standard errors clustered by country in parentheses and p-values. The specifications in columns 4-6, 8, and 10 include a lagged dependent variable and report panel-corrected standard errors in parentheses and p-values. Specifications in columns 2, 3, and 5-10 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		Annual Data	
	Lag DV and Country Fixed Effects				Lag DV and Country FE		Lag DV and Country FE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Top\ Rate_{t-1}$	0.772	0.762	0.772	0.763	0.581	0.586	0.932	0.933
	(0.046)	(0.047)	(0.046)	(0.047)	(0.100)	(0.100)	(0.012)	(0.012)
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
$War\ Mobilization_{t-1}$	15.698	15.005	15.338	14.899	24.281	23.911	1.581	1.666
	(3.714)	(4.005)	(3.803)	(4.080)	(10.922)	(11.246)	(0.746)	(0.749)
	0.000	0.000	0.000	0.000	0.026	0.033	0.034	0.026
$Universal\ Male\ Suffrage_{t-1}$	-0.409	0.250			0.999		0.449	
	(1.973)	(1.955)			(3.444)		(0.407)	
	0.836	0.898			0.772		0.270	
$Competitive\ Elections_{t-1}$			-0.925	-0.437		-1.283		
			(1.476)	(1.501)		(2.948)		
			0.531	0.771		0.663		
$Left\ Executive_{t-1}$		3.571		3.651	5.415	5.802	0.600	0.583
		(1.656)		(1.668)	(3.172)	(3.116)	(0.304)	(0.308)
		0.031		0.029	0.088	0.063	0.048	0.058
$GDP\ per\ capita_{t-1}$		0.001		0.000	0.000	0.000	0.000	0.000
		(0.000)		(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
		0.172		0.189	0.544	0.658	0.425	0.412
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.885	0.884	0.885	0.884	0.816	0.816	0.964	0.964
Number of Observations	543	515	543	515	253	253	2,536	2,536

Table A-6: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Lagged Dependent Variable and Fixed Effects Specifications.* 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$_{t-1}$		0.871	0.879	0.879	0.879	0.879
		(0.040)	(0.039)	(0.039)	(0.039)	(0.039)
		0.000	0.000	0.000	0.000	0.000
War Mobilizaton$_{t-1}$	36.248	4.120	21.493	23.951	21.570	15.302
	(14.932)	(9.026)	(13.904)	(5.292)	(6.244)	(3.985)
	0.026	0.648	0.140	0.000	0.003	0.000
Universal Male Suffrage$_{t-1}$	4.214	-2.879				
	(5.745)	(1.710)				
	0.473	0.092				
Universal Male Suffrage$_{t-1}$ * War Mobilization$_{t-1}$	-14.946	11.618				
	(14.636)	(9.301)				
	0.321	0.212				
Competitive elections$_{t-1}$			-0.233	-0.158		
			(6.778)	(1.195)		
			0.973	0.895		
Competitive Elections$_{t-1}$ * War Mobilization$_{t-1}$			2.788	-15.973		
			(14.681)	(5.623)		
			0.852	0.005		
Left Executive$_{t-1}$					-0.477	2.610
					(5.901)	(1.530)
					0.937	0.088
Left Executive$_{t-1}$ * War Mobilization$_{t-1}$					8.808	-5.656
					(8.986)	(8.628)
					0.340	0.512
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	No	Yes	No	Yes	No
R-squared	0.712	0.878	0.710	0.879	0.710	0.877
Number of Observations	544	543	544	543	544	543

Table A-7: *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)
$Top\ Rate_{t-1}$			0.966	0.967
			(0.009)	(0.009)
			0.000	0.000
$War\ Mobilizaton_{t-1}$	7.427	8.978	1.520	1.431
	(2.695)	(3.157)	(0.717)	(0.736)
	0.013	0.011	0.034	0.052
$Universal\ Male\ Suffrage_{t-1}$	7.303		-0.423	
	(6.030)		(0.340)	
	0.242		0.213	
$Competitive\ Elections_{t-1}$		6.366		-0.167
		(5.524)		(0.408)
		0.264		0.682
$Left\ Executive_{t-1}$	0.280	-0.065	0.382	0.363
	(3.270)	(3.287)	(0.278)	(0.276)
	0.933	0.984	0.213	0.189
$GDP\ per\ capita_{t-1}$	0.001	0.001	-0.000	-0.000
	(0.002)	(0.002)	(0.000)	(0.000)
	0.666	0.633	0.619	0.794
Period Fixed Effects	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	No	No
R-squared	0.721	0.720	0.963	0.963
Number of Observations	2,537	2,537	2,536	2,536

Table A-8: *War Mobilization, Democracy, and Inheritance Taxation, 1816-2000: Annual Data.* Columns 1-2 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. These specifications include country and period fixed effects and report robust standard errors clustered by country in parentheses and p-values. Columns 3-4 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 control variables. These specifications also include a lagged dependent variable and period fixed effects and report panel-corrected standard errors in parentheses and p-values.

	1900	1910	1920	1930	1940	1950	1960	1970	1980	1990	2000
UK	3.3	8.3	60	63.8	90	97.5	88.8	91.3	60	40	40
USA	0	0	73	25	81.1	91	91	71.8	70	28	39.6
France	0	0	50	33	40	60	60	60	60	57	54
Japan	5.5	20.4	36	36	65	55	70	75	75	50	37
Sweden	0	12	13	18.5	45.3	53.8	59.6	66.7	85	65	56.5
Netherlands	0	3.2	12.9	11	9.4	61.7	54.7	53.3	53	42.6	45.5

Table A-9: The Top Marginal Rate of Income Taxation Across Six Countries. See Data appendix for sources.

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