War, International Finance, and Fiscal Capacity in the Long Run*

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Abstract

This manuscript revisits the relationship between war and state making in modern times by focusing on two prominent types of war finance: taxes and external finance. Financing war with tax money enhances the capacity to assess wealth and monitor compliance, namely fiscal capacity. Tax-financed war facilitates the adoption of power-sharing institutions, which transform taxation into a nonzero-sum game, carrying on the effect of war in the long-run. Financing war with external debt does not contribute to long-term fiscal capacity in the presence of debt relief or when old war bonds are exchanged for nontax revenue as part of the default settlement. The empirical evidence draws from war around the world as early as 1816. Results suggest that the advent of a genuinely global capital market in the early nineteenth century undermined the association between war, state making, and political reform.

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1 Introduction

War, although devastating, offers a matchless opportunity to transform the state. The magnitude of resources a country must amass to finance the means of war offers rulers the incentives to invest in state making while reducing domestic resistance to taxation. War clears the path to fiscal centralization,[1] the professionalization of the tax administration,[2] and the adoption of new taxes—from excises[3] to progressive income taxes.[4] Fiscal innovations are often accompanied by complementary organizations, including treasuries and central banks,[5] and improved budgeting technologies.[6] Far from disappearing, the financial innovations that make war possible are expected to exert lasting effects on the extractive capacity of the state;[7] that is, states make war as much as war makes states.[8]

The argument above, known as the *bellicist theory of state formation*, draws heavily from the history of state building in Europe, from the fifteenth to the eighteenth century.[9] But the evidence is mixed outside the European continent. Why did war make states in Europe but did not in the so-called *periphery* (i.e., Asia, Africa, and Latin America)? Modern states outside Europe were created only in the nineteenth century, coinciding with or following the first globalization of international finance.[10] Readily available external finance, this manuscript shows, weakened the incentives to expand taxation and develop domestic credit institutions, breaking the connection between war- and state-making.

Others have revisited the bellicist hypothesis by focusing on initial conditions: urbanization and regime type[11] and initial state capacity and social composition.[12] Insightful as

[1] Dincecco 2011.
[2] Ardant 1975.
[3] Brewer 1988.
[4] Scheve and Stasavage 2010.
[5] O'Brien 2001.
[6] Dincecco 2011.
[7] Besley and Persson 2011; Brewer 1988; Dincecco and Prado 2012.
[8] Tilly 1990.
[9] Dincecco 2011; Ertman 1997; Hintze 1975.
[10] Neal 1990; Taylor 2006.
[11] Karaman and Pamuk 2013.
[12] Kurtz 2013; Soifer 2015.

they are, these studies omit the fundamental change that increased liquidity in international credit markets in the nineteenth century represented for state formation. That is the focus of this study. Centeno and Thies, the two closest contributions, rightfully argue that international loans limited state capacity in Latin America;[13] however, the theoretical mechanism by which rulers preferred external finance over taxation is left unspecified. If taxation is as beneficial in the long run as previous works suggest, why would rulers not prefer to finance war with tax money? Second, the mechanism of transmission that connects warfare (or lack thereof) in the nineteenth century to the present day is absent. Third, the Ricardian equivalence states that loans act as deferred taxes. The implication for state building is that financing war one way or another should not make a difference in the long run; however, the empirical evidence in Centeno and Thies suggests that loan-financed war in Latin American did not build capacity (i.e., did not "make states"). An explanation for the reason that the Ricardian equivalence does not always hold is missing. Fourth, the empirics suffer from the limitations of purely observational work.

I fill these vacuums, first, by advancing a political economy of war finance: Exerting a fiscal effort to finance war with tax revenue expands the future share of private income that the ruler can tax. However, taxation requires political concessions in order to overcome taxpayers' resistance to higher rates, which explains the ruler's reservations to finance war with taxes, let alone if alternative sources of funding like external loans are available. A simple decision-theoretic model suggests that the ruler's preference for foreign loans relative to taxes depends on a combination of domestic and international conditions: First, shorter time horizons strengthen the ruler's preference for external finance because gains in fiscal capacity are realized only in the long run. Shorter time horizons are consistent with political instability and scenarios in which political survival depends on winning war, hence the ability to mobilize resources in a timely manner. Second, low levels of initial state capacity make external finance more appealing because the latter secures funds that exceed the short-run

[13]Centeno 2002; Thies 2005.

possibilities of taxation. Third, low levels of initial power-sharing institutions strengthen preference for external finance because the alternative—taxation—involves higher political concessions. Fourth, cycles of high liquidity in the international credit market favor external finance. High liquidity implies more favorable loan terms even for countries with dubious fundamentals, thus increasing the present value of external finance. Fifth, an extended practice of external debt relief and the exchange of old bonds for nontax revenue and state monopolies limit the future cost of default, strengthening the preference for external loans over taxes. This battery of domestic and international conditions suggest that the scope conditions to finance war with taxation, even if only partially, are rather narrow, hence the opportunities to build long-run fiscal capacity.

I address the other three gaps in the literature as follows: First, I advance (and show empirically) a mechanism of transmission of the effect of war finance that builds on its political economy. Financing war with taxes facilitates political compromise between the ruler and taxpayers. In turn, these power-sharing institutions transform taxation into a nonzero-sum game, carrying on the effect of warfare into long-term fiscal capacity. Second, I show that the Ricardian equivalence does not necessarily hold in the presence of external debt relief or if defaulters can exchange war debt for nontax revenue and state monopolies. When that is the case, war does not necessarily make states. Third, I address threats to inference in previous work by exploiting unannounced systemic shocks in international capital markets known as *sudden stops* of credit. These global crises created time windows in which, for exogenous reasons, warring states could not rely on external loans to finance the means of war. To cope with endogeneity in war participation, I concentrate on a subsample of wars that were initiated while credit still flowed but suddenly dried up, thus disconnecting the decision to go to war or the type of war to fight from availability of external finance.

The empirical analysis draws on a sample of 100+ countries as early as 1816. Holding a host of initial economic and political characteristics constant, results show that war is conducive to stronger states in the short and long run if it is waged in the absence of

external finance, that is, when incentives to tax were strong. On the contrary, making war while having access to international capital markets tends to be inconsequential—even detrimental—for state building. Consistent with the original work of Charles Tilly—often oversimplified—results confirm that state building is not merely a function of war making but also access to domestic capital, namely taxes or domestic loans.[14]

The last section of the empirical analysis offers evidence of the transmission mechanism: Tax-financed war in the long ninteenth century strengthened executive constraints in the short and long run, facilitating transmission of the fiscal effort of war into future tax capacity. The conclusion section compares state building in the periphery with that of European countries in early modern times.

2 A Political Economy of War Finance

Modern warfare is funded by a combination of loans and taxes.[15] Resorting to one or the other is as much a matter of possibility (has the state enough capacity to tax and access to credit markets?) as of political opportunity (who benefits from borrowing and taxing?). In this section I present a decision-theoretic model that incorporates both types of considerations. I model an incumbent's decision to finance the cost of war, W, with taxes T or loans L, which, for the sake of exposition, are assumed mutually exclusive.[16] I also assume that war, which begins and ends in Period 1, is exogenous and that the incumbent seeks to maximize war funds and minimize political concessions. This stylized set up seeks to identify how the incumbent's incentives to resort to external finance vary as a function of domestic political calculus and the liquidity of international credit markets. The result of this exercise informs the empirical design. Other forms of war finance are discussed at the

[14]Tilly 1990.

[15]Poast 2015; Sprague 1917.

[16]War may be funded by additional means, including expropriation, money printing, and forced labor. The more alternatives to taxation are available to the ruler to assume the costs of war, the more uncertain it is that war activates political and institutional mechanisms that are conducive to long-term state capacity. Refer to the final paragraph of this Section and the Mechanism section for further details.

end of this section.

Taxation is politically delicate because it involves some form of extraction from elites, the masses, or both. Rulers can rarely impose new taxes on elites without their consent, consultation, and negotiation.[17] In return for newer taxes, elites may demand veto powers over spending decisions. Consistently, tax increases to finance the means of war yielded major advances in parliamentary representation in early modern Europe.[18] Taxing the masses may not be easier, especially when the tax increase is accompanied by a military draft. In such circumstances political concessions may be required to prevent tax revolts from below.[19] One way or another, power-sharing institutions "were the *price and outcome* of bargaining with different members of the subject population for the wherewithal of state activity, especially the means of war."[20] Throughout, c_t denotes the ruler's political concessions from financing war with taxes.

Financing war with taxes requires some capacity to assess wealth and monitor compliance, namely fiscal capacity. Tax revenue is bounded above by the present stock of fiscal capacity $\kappa \in [0, 1]$. Tax capacity can be expanded via institutional investment at the cost of forgone public consumption.[21] Here I take a different route, one in which tax collection improves over time as a result of *know-how* accumulation by the tax administration: e.g., tax collectors learn over time common avoidance schemes.[22] Know-how expands future fiscal capacity by $\eta < 1$ units, which captures the ratchet effect of taxation in a reduced form. Future payoffs are discounted at a rate $\delta \leq 1$. Together, the present discounted value of financing war with taxes is

$$\kappa T - W - c_t + \delta\big[(\kappa + \eta)T - c_t\big] \tag{1}$$

[17] Hintze 1975; Levi 1988; Tilly 1990.

[18] Bates and Lien 1985; Ferejohn and Rosenbluth 2016; Stasavage 2016. For a competing view see Downing 1993.

[19] Hintze 1975.

[20] Tilly 1990, p.64, italics added.

[21] Besley and Persson 2011; Queralt 2015. Investment-based models of capacity building like these emphasize types of inter-temporal dilemmas that deviate attention from the key political trade-off at stake: Assuming political costs today vs. A lottery of debt service and default tomorrow. A know-how accumulation model is sufficient to articulate this dilemma.

[22] Brewer 1988.

Expression 1 captures in a stylized fashion the intrinsic dilemma of fiscal capacity building. It expands permanently the volume of resources that can be mobilized for national defense, $\kappa + \eta$, but it does so by increasing executive constraints, c_t. It is not obvious that the ruler will always prefer to finance war with taxation particularly if other options are available.[23]

The political consequences associated with taxation—power-sharing institutions—are crucial to understanding persistence of fiscal effects of war mobilization, $\kappa + \eta$. Limited government is conducive to "quasi-voluntary compliance" by taxpayers because political accountability grants credibility to the promised returns for taxes.[24] Besley and Persson formalize the opportunities of sustained cooperation in tax policy created by power-sharing institutions.[25] In "common-interest states"—where government revenue is used in the common interest (e.g., national defense)—taxation becomes a positive-sum self-sustaining game: The ruler secures a constant stream of funds to produce the public good while taxpayers are protected from arbitrary expropriation.[26] Power-sharing institutions, in sum, carry on the fiscal effect of warfare in the long run.

Financing war with *domestic* loans is expected to carry political concessions similar than taxation;[27] however, domestic borrowing requires levels of capital accumulation that cannot be taken for granted, especially not in the developing world.[28] When domestic credit is tight, states finance externally.

Compared to taxation and domestic finance, external finance carries lower political costs, $c_l < c_t$. First, rulers do not need to concede political rights or representation to international lenders—a good margin suffices. Second, loans have low public visibility, which preempts

[23]Expression 1 resonates with the leader's dilemma of investing in "good institutions" that Acemoglu 2003 and Besley and Persson 2011 have formally addressed. Although high fiscal capacity might be a better societal outcome (better national defense together with executive constraints), political incentives to prevent future extraction via taxation by the opposition might work against it.

[24]Levi 1988.

[25]Besley and Persson 2011.

[26]Fujihira 2000 points out the role of two power-sharing institutions—parliaments and political parties—in facilitating sustained levels of taxation. These institutions aggregate competing tax preferences of capital and labor facilitating compromise and sustained cooperation after war. Consistently, Lee and Paine 2018 show that, in the presence of executive constraints, a history of warfare translates into long-run fiscal capacity.

[27]North and Weingast 1989.

[28]della Paolera and Taylor 2013.

social contestation during wartime.[29] Third, external finance decreases domestic accountability: Not having to depend on their citizens for tax revenue, rulers have a freer hand in enacting war policy.[30]

Loans operate as deferred taxes—the Ricardian equivalence states.[31] Debt is repaid in Period 2 out of tax revenue, which carries political concessions c_t. Repayment, however, is often uncertain; default happens with probability d, and carries a sanction, $\beta \geq 0$. This parameter reflects the stringency of a future default settlement. Together, the ruler's present discounted value of financing war externally is

$$L - W - c_l + \delta\big[(1-d)\big(\kappa T - (1+i)L - c_t\big) - d\beta\big] \tag{2}$$

where $(1+i)L$ denotes repayment of the standing principal plus interest. The latter is set in the international capital market, where loans are floated. Countries with a history of default are expected to pay a premium p for their lack of reputation.[32] Let $i = r + p$, where r is the interest rate of a risk-free sovereign bond (e.g., the British Consol) and $p = (1+r)d/(1-d)$, which is strictly increasing in the probability of default.[33]

When does the ruler prefer to finance war with external loans? When tax revenue is lower than the cost of war, $\kappa T < W$; or when $\kappa T \geq W$ provided that

$$L \geq \frac{\kappa T - \Delta c + \delta\big[\eta T + d(\kappa T - c_t + \beta)\big]}{1 - \delta(1 + r^*)} \tag{3}$$

[29]Flores-Macías and Kreps 2017; Fujihira 2000.

[30]**?**. Given the time period under consideration, we can conceive a fourth advantage: External funds minimize the political cost of rearmament for conservative governments—the vast majority in the long-nineteenth century. Facing an external threat, conservative governments can levy higher tax rates on their core supporters—the economic elite—to expand military capacity at the risk of causing revolt within their own coalition; alternatively, they can seek international allies or concede to demands of the adversary as a substitute for internal strengthening (Narizny, 2003). External loans allow conservative governments to finance rearmament while saving political costs derived from raising taxes on their core constituency.

[31]Barro 1979.

[32]Tomz 2007.

[33]The premium is derived by setting the international investors' profit when lending is risk free, $W(1+r) - W$, equal to the international investors' profit when the probability of default is nonzero, $d * 0 + (1-d) * (1+r+p)W - W$, and solving for p.

where $\Delta c = (c_t - c_l)$. We can draw several lessons from Expression 3. First, the preference for external finance strengthens as the costs of political concessions Δc grow relative to the discounted value of fiscal capacity expansion, $\kappa + \delta\eta$. This result illuminates why common-interest states characterized by sustained taxation and political accountability are harder to reach when initial fiscal capacity κ is low.[34] Interestingly, the preference for loans over taxes decreases as c_l approaches c_t, that is, as Δc shrinks. This gap narrows when some form of power-sharing institutions are already in place in Period 1. Empirically, these results recommend controlling for initial fiscal capacity and political conditions whenever possible.

Second, Expression 3 suggests that the preference for external finance depends on the cycle of the international capital market. To see this point, let's assume that r is set in equilibrium in the international market in which capital supply is defined by the inverse linear function $r_s = \alpha_s + \phi_s q_s$, where q_s denotes the global supply of capital, and $\phi_s > 0$. Without loss of generality, global loan demand is given by the inverse function $r_d = 1 - \phi_d q_d$, $\phi_d > 0$. The international market clears at $r^* = 1 - \phi_d(1 - \alpha_s)/(\phi_d + \phi_s)$, the price of the risk-free asset (e.g., British Consols). When international capital markets experience a positive shock—for instance, following a capital surplus in a major economy—the international credit supply shifts to $r_s = \alpha'_s + \phi_s q$, $\alpha'_s < \alpha_s$, hence $(r')^* < r^*$, $p((r')^*) < p(r^*)$, and $i((r')^*, p((r')^*)) < i(r^*, p(r^*))$. That is, in expansive cycles, more and cheaper credit is available even for countries with a history of default.[35] And viceversa, international capital market contractions—for instance, following a global financial crisis—make credit tighter and more expensive across the board, weakening preference for external finance. For a large enough negative shock, $\alpha'_s > 1$, international lending ceases, strengthening incentives to finance war with taxes. The empirical design builds from this result to identify periods in which incentives to tax are strongest.

Third, preference for external finance is stronger in the presence of short time horizons,

[34] Besley and Persson 2011.

[35] Ballard-Rosa, Mosley, and Wellhausen 2019 show that even today borrowers with weak and undemocratic institutions access international finance in favorable terms when the global financial cycle is good. In other words, in good credit cycles, investors are risk-tolerant and less sensitive to political risk.

or low δ. These may reflect individual time preferences, general political instability, or the importance of winning war for political survival. Either way

> [b]orrowing provides the current leader with resources today, while repayment typically has to be made by a future government. From the national perspective, loans are not free resources, but unless the leader is fortunate enough to have a long tenure, they are from the leader's perspective. (**?**, p.527)

Fourth, the effect of the probability of default d depends on magnitude of the default sanction: For $\beta > c_t - \kappa T$, preference for external finance weakens with the probability of default.[36] This is driven by aversion to sanction β associated with default. When $\beta < c_t - \kappa T$, the disutility of defaulting is smaller than the disutility of taxing; hence, external finance will be preferred the most by those with a high probability of default, potentially creating a lending-and-default cycle. To prevent this scenario, international investors can increase the severity of default sanctions β. However, the opposite often happened in the first globalization of credit: Despite recurrent default,[37] haircuts were frequent and substantial, reaching 40 and 50% of war debt.[38] When debt was unforgiven, default settlements included reductions in interest rates and extensions of maturities, which weakened further the incentives to enhance fiscal capacity after default. Importantly, some default settlements did not even involve a transfer of tax money for war debt. Instead, defaulters would exchange old bonds for nontax revenue (e.g. land) and state monopolies;[39] The exchange of these assets for old bonds, first, explains why lending to serial defaulters took place in the first globalization of finance (more in Section 3.3); second, and specific to state building, it allowed defaulters to regain access to international credit markets without enhancing tax capacity, precluding the Ricardian equivalence.[40]

[36] This inequality follows from the FOC of d for the right-hand side of (3).

[37] Reinhart and Rogoff 2009.

[38] Lindert and Morton 1989; Jorgensen and Sachs 1988.

[39] For instance, as part of the 1890 default settlement of Peru's standing war debt, state railroads, land, and mining were placed in the hands of British bondholders (Vizcarra, 2009). International investors could also take control over customs or other forms of revenue collection (e.g. tobacco monopoly) and redirect resources towards debt service. This practice, however, did not necessarily enhance the capacity to tax of the borrower. See Gardner 2017, Maurer and Arroyo Abad 2017, and Reinhart and Trebesch 2015 for the effects of foreign financial administration in Liberia, Greece and eight Latin American economies, respectively.

[40] Appendix A models the use of equity to finance warfare as well as collateral. This extension shows

Empirical Implications

The previous discussion suggests that having access to external credit is consequential to understanding the conditions under which war makes states *precisely because* taxes and loans may not exert the same transformative effects on fiscal capacity. The bellicist hypothesis assumes the Ricardian equivalence, namely, that loans operate as deferred taxes. Accordingly, rulers assume responsibility for war debt, $(1 - d) = 1$, and enhance taxation to service debt after military conflict, thus contributing to state making. However, generalized debt relief and the exchange of war debt for equity may unravel that causal chain, thus the equivalence of taxes and loans for the purpose of state building.[41] Based on the uncertain effects of loan-financed war on fiscal capacity and the positive impact of financing war with taxes, I expect that the more war is financed with taxes relative to loans, the stronger fiscal capacity should be after military conflict, holding everything else constant.

Finally, war may be (partially) financed with tools other than taxes and loans: Rulers might rely on nontax revenue (e.g., royalties from state monopolies),[42] expand the money supply,[43] exert financial repression,[44] expropriate, or sell offices.[45] Choosing taxation rather than any of these routes is, again, a matter of capacity and political calculus. Importantly, in terms of state building, any alternative path to taxation is expected to exert effects similar to external finance. That is, these revenue alternatives do not require building a tax apparatus capable of assessing wealth and securing a stable stream of revenue—namely enhancing fiscal capacity—nor activate a key mechanism of transmission of the ratchet effect of war: namely strengthening power-sharing institutions. In sum, this discussion implies that the

that, in anticipation of debt-equity swaps, the cost of borrowing decreases and the incentives to float loans strengthen, thus expanding the states of the world in which war is financed externally.

[41] Probably no scholar would defend a strict version of the Ricardian equivalence, but there is a general understanding that loans and taxes operate in roughly similar ways. The argument made in this article is that the conditions under which the Ricardian equivalent hold may be narrower than previously thought, and this helps us to understand why externally-financed war may not translate into state-making.

[42] Refer to Appendix A.

[43] Sprague 1917; Fujihira 2000.

[44] Menaldo 2016.

[45] Hoffman 1994.

scope conditions of the bellicist hypothesis are somewhat narrow. Easier access to foreign credit following the globalization of capital in the nineteenth century made those conditions narrower.

3 Design

To investigate the lasting effect of war finance on fiscal capacity, one could rely on war-specific finance data. That is, what was the proportion of taxes relative to external loans that country i mobilized to finance war j, and how did that shaped i's long-term fiscal capacity? This design is unfeasible and inadequate. First, cross-national conflict-specific data regarding the manner in which war is financed are unavailable in any systematic way. Second, even if such data existed, that design would raise concerns of endogeneity because access to international capital markets is not randomly assigned.

Alternatively, I propose comparing the relative effect of war waged when countries have and lack access to the international capital markets for exogenous reasons (more below). The logic of this test is based on the political economy of war finance elaborated in the previous section. Access to external finance structures incentives to tax. When rulers cannot borrow externally, the incentives to raise taxes to finance the means of war should be strongest. By contrast, having access to external loans should weaken the incentives to strengthen the tax apparatus because loans allow the ruler to finance war while eluding the political costs of taxation. Next I specify the time period, unit of analysis, and nature of credit shocks.

3.1 Time Period

To test for legacies, I estimate the effect of war taking place between 1816 and 1913 on various proxies for fiscal capacity circa 2000. This strategy mimics Dincecco and Prado, who find that countries that fought more wars and suffered the largest number of casualties

between 1816 and 1913 had higher ratios of direct taxes to GDP by 1995.[46] The lower cut-off, 1816, is deliberately chosen to maximize the number of cases in the sample. Most countries in the periphery were created only in the nineteenth century. The upper cut-off, 1913, serves two purposes: First, it guarantees that fiscal efforts are driven by military need. The boom in welfare spending following WWI makes it harder to isolate the effect of war on fiscal capacity because the newly created social programs also pushed for higher taxation. Second, the financial costs of WWI and WWII are unprecedented. Most participants were countries with high fiscal capacity to begin with. Including total war in the analysis would exacerbate problems of selection.

Importantly, whereas Dincecco and Prado emphasize the lasting effect of war making, I focus on war *finance*. I use finer proxies of long-term fiscal capacity while showing evidence of short-term effects of war finance, its transmission, and transmission mechanisms. In addition, I address endogeneity in war participation as well as credit access.

3.2 Unit of Analysis

Most wars from 1816 to 1913 were interstate, involving European powers as well as internationally unrecognized states. Wars were fought against colonial powers and also between neighboring countries, also in Africa, Latin America, and Southeast Asia.[47] In an effort to move beyond the experience of war making in the developed world, I work with Wimmer and Min war data, which includes all military disputes exceeding 1,000 casualties and involving internationally recognized and unrecognized states around the world since 1800.[48]

With the use of internationally unrecognized states in the analysis, I assume that these political entities exerted a fiscal effort in financing war comparable to recognized states. This is the case in, for instance, the wars of independence in Latin America,[49] the African wars

[46] Dincecco and Prado 2012.
[47] Butcher and Griffiths 2015.
[48] Wimmer and Min 2009.
[49] Centeno 2002.

before and after the arrival of the Europeans[50] or interstate wars over succession disputes in Southeast Asia.[51] One caveat is the extent to which internationally unrecognized state could issue loans in the international markets. Plenty of evidence exists for Latin American countries, but less so for African and Asian countries. Suppose these units were fully excluded from international markets. Then, this would make war even more consequential. That is, excluded from international markets, unrecognized polities would have strong incentives to finance war with taxes. Econometrically, this would work against the research hypothesis, by which we should not observe state building when international markets are operative.

Importantly, results do not hinge on the inclusion on internationally unrecognized units. Table 5, in which only states recognized by the international system by the time they go to war are considered, and Table 7, in which Wimmer and Min data are replaced by *Correlates of War* (COW) data, which includes only internationally recognized states, yield the same results.

Wimmer and Min data stand out in three additional ways: First, wars are mapped onto current state boundaries, making possible tracking which state should inherit the legacy of war making, as well as investigating the effect of fighting war within national territories or elsewhere.[52] Second, Wimmer and Min distinguish civil from secessionist war. As part of the robustness tests, the latter type (defined as fights against the political center with the aim of establishing an independent state) is considered. After all, secessionist war may contribute to revenue maximization in a fashion similar to interstate war.[53] Third, in Wimmer and Min nonproxy wars waged by colonial subjects against third territories are attributed to the colonial subject and not to the metropolis, thus maximizing the match between war makers and fiscal outcomes.

Altogether, I consider 147 armed conflicts between 1816 and 1913, of which 114 are inter-

[50]Reid 2012 and Gardner 2012, respectively.

[51]Butcher and Griffiths 2015.

[52]Refer to Appendix B for country splits and merges.

[53]Non-secessionist civil wars are used as a control only because their contribution to state building has yet to be established.

state wars and 33 are secessionist.[54] War participants and locations are evenly distributed across space, with the exception of Oceania, which remained peaceful. Eight percent of the inter-state war-years between 1816 and 1913 involved African countries, 23% Asian, 48% European, 20% Latin American, and 1% North American.[55] Little military conflict occurred in the European territory (consistent with the characterization of the Hundred Year's Peace), while in other regions, most prominently Asia and Latin America, war was pervasive (and often involved European powers).[56]

For every war, I establish whether it was waged while having access to international lending. A natural way to proceed is to focus on default periods; however, this measure—or interest spreads or gold standard adoption—is endogenous.[57] To gain leverage on identification, I exploit shocks in the international lending markets throughout the long nineteenth century. As it will become clear, these credit crunches, also known as "sudden stops" of credit,[58] dried up capital flows at once on a global scale. Key for the identification strategy, sudden stops precluded countries from external borrowing irrespective of their (un)observed characteristics. In other words,

> [b]anking crises in global financial centers (and the credit crunches that accompany them) produce a 'sudden stop' of lending to countries at the periphery [...]. Essentially, capital flows from the 'north' dry up in a manner *unrelated* to the underlying economic fundamentals in emerging markets. (Reinhart and Rogoff 2009 2009:74, italics added).

The empirical section exploits sudden stops as a form of exogenous variation in access to external credit, which in turn structures the incentives to invest in fiscal capacity for countries at war. Notice that these shocks are captured by the loan supply shifter α_s introduced in Section 2. Contractions of global credit, $\alpha_s < \alpha_s'$, increase equilibrium interest rate and

[54] Appendix B lists all wars included in the analysis.

[55] Figure A-2 shows that the location of warfare is also evenly distributed across regions.

[56] Kirshner 2007 offers a financial explanation of the Hundred Year's Peace: Because of potential disastrous macroeconomic consequences of war between Great powers, financial communities in the European core were reluctant to support war between them. Except for the Franco-Prussian War, war in the nineteenth century took place outside European soil and pitted great powers against smaller powers, or smaller powers against each other.

[57] Refer to Appendix J for analysis with default episodes. Results hold.

[58] Calvo 1988.

weaken incentives to finance war externally. When international lending suddenly ceases, $\alpha'_s > 1$, incentives to finance war with tax money are strongest. Next, I elaborate on the nature and timing of these shocks.

3.3 The Globalization of Capital

The nineteenth century witnessed the first globalization of financial markets, resulting from excess savings generated by the industrial revolution in Western Europe.[59] Relative to world GDP, the volume of cross-border loans was three times larger in 1880s than a hundred years later.[60] Old and newly created states financed externally at very favorable conditions. Countries in the periphery paid a premium for their lack of reputation;[61] however this was historically low: First, drawing on annual interest rates from 1870 to 1914, Bordon and Rockoff show that developing nations paid between 2 and 3% premium relative to British Consols;[62] for reference, the United States paid a 1% premium. By the eve of WWI, the gap between British Consols and sovereign debt from the periphery had closed by half.[63] Second, spreads between developed and developing nations were between two to four times smaller in 1877-1913 than in 1994-2000.[64] That is, capital was significantly cheaper for developing economies at the turn of the nineteenth century than at the turn of the twentieth century. Third, rates paid by new states in the nineteenth century were significantly smaller than those paid by European powers in premodern times, when they lacked strong economic fundamentals. The average nominal yield in the 15th–17th centuries in Castile, France and the UK were 8.75, 7.25, and 7.78, respectively.[65] In stark contrast, no Latin American economy paid nominal interests above 6% by the turn of the nineteenth century.[66] Given

[59]Neal 1990; Taylor 2006.
[60]Eichengreen 1991, p.150.
[61]Tomz 2007.
[62]Bordo and Rockoff 1996.
[63]Results in Lindert and Morton 1989 are consistent: Between 1850 and 1914, the largest Latin American economies barely paid a 2% premium relative to the European core despite their radically different fundamentals.
[64]Mauro, Sussman, and Yafeh 2002.
[65]Calculations based on Stasavage 2011.
[66]Marichal 1989, Appendix A and B.

the relative low price of money in the nineteenth century, it is not surprising that peripheral countries financed war externally.[67]

A detailed account of the reasons that made money relatively inexpensive in the nineteenth century far exceeds the limits of this piece. Briefly put, the literature points to four nonmutually exclusive factors: First, at the margin, the higher premium charged on developing nations worked as a *pull factor*, while low interest rates in the UK also *pushed capital out*.[68] Second, the "lending frenzy" was sustained on strong information asymmetries, speculative operations, and blatant fraud.[69] Investors would inform their decisions based on the economic press published in London; however, this press did not establish full-time correspondents overseas until the late in the game.[70] Third, over time investors received more and more guarantees from foreign governments—probably in response to a context of frequent default. Loans increasingly incorporated clauses by which governments committed entire revenue sources to the payment of foreign debt. Fourth, default settlements increasingly involved the exchange of war debt for land, state monopolies, and other assets, which also minimized risk for investors. Indeed, Appendix A shows that the anticipated exchange of old bonds for equity decreases the interest rate of countries with a history of default.

Most of the international credit was channeled through the London Stock Exchange (LSE). Its leadership was consolidated throughout the nineteenth century, when it became the world's leading capital exporter, far exceeding the combined capital exports of its nearest competitors—France and Germany. Table 1 reports the best approximation of the market shares in lending throughout this period. At its peak, the British share of total global foreign investment was almost 80%. This contrasts with the US share of global assets of 25%

[67]Centeno 2002, Flandreau and Flores 2012, and Marichal 1989 list over 25 wars that were explicitly financed with war loans by peripheral countries in Asia, Eurasia, Latin America, and Southern Europe in the long nineteenth century.

[68]See Bordo and Rockoff 1996, p.396 for a review of this literature.

[69]Taylor 2006.

[70]Jones 1979 shows that *The Times*, a key source of financial information in London, did not have a permanent correspondent in Argentina, a key market, until the 1890s. Instead, they relied on local sources that often had conflict of interest.

Table 1: **External Capital Stock by Country in the Long Nineteenth Century**

	1825	**1855**	**1870**	**1890**	**1914**
Great Britain	0.5	0.7	4.9	12.1	19.5
France	0.1	-	2.5	5.2	8.6
Germany	-	-	-	4.8	6.7
Netherlands	0.3	0.2	0.3	1.1.	1.2
United States	0.0	0.0	0.0	0.5	2.5
Canada	-	-	-	0.1	0.2
All	0.9	0.9	7.7	23.8	38.7
UK/all	0.56	0.78	0.64	0.51	0.50
World GDP	-	-	111	128	221

Values represent gross foreign assets in current USD billion. Source: Table 2.1 in Obstfeld and Taylor 2004.

in 2000 and even with the US maximum share of 50% circa 1960. Consistently, at that time the British were known as "the bankers of the world."[71]

The LSE was not immune to crisis. Table 2 enumerates the onset of all banking panics and stock crashes experienced by Great Britain in the long nineteenth century as listed in Reinhart and Rogoff.[72] Given Great Britain's central position in the international lending market, crashes in London rapidly spread to Paris, Frankfurt, and New York. Contagion took different routes, including arbitrage in commodities and securities and movement of money in various forms (specie, bank deposits, bill of exchange), cooperation among monetary authorities, and pure psychology.[73] One way or another, financial crashes in London dried up international lending on a global scale.[74]

Importantly for exogeneity purposes, the causes of the British financial collapses in the nineteenth century are domestic. This is certainly the case for the major crises of 1825, 1847, 1857, and 1866 but less true for the 1890 panic, in which a large financial imbalance in Argentina halted British lending.[75] More importantly, British panics did not respond

[71]Obstfeld and Taylor 2004.
[72]Reinhart and Rogoff 2009.
[73]Kindleberger and Aliber 2005, p.126.
[74]Bordo 2006.
[75]For the domestic origins of the 1825, 1847, 1866, and 1890 crises, see Neal 1998, Dornbusch and Frenkel

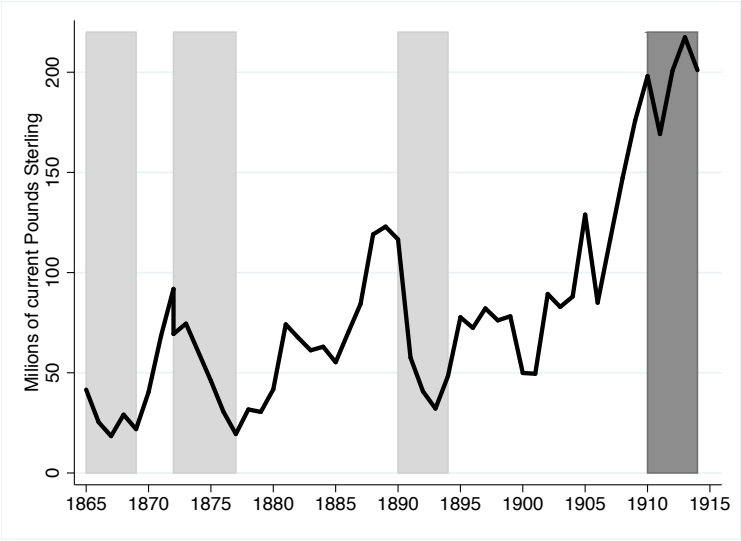
Table 2: **Banking Crises and Stock Market Crashes in London, 1816-1913**

Banking Crises		**Stock Market Crises**
1825	1849	1865
1837	1849	1866
1838	1850	1867
1839	1857	1910
1840	1866	1911
1847	1873	1912
1848	1890	1913

Source: Reinhart and Rogoff (2009). 1873 banking panic added. Results robust to its exclusion (see Appendix I).

to defaults by borrowers, which would cast doubt on the exogeneity of these shocks. Most of the countries that defaulted in the nineteenth century were in the periphery. Although the defaulted quantities were significant relative to their home economies from a global prospective, they were a "sideshow" for the British economy.[76] All things considered, the periods of sudden stops can be safely treated as exogenous to every country except Great Britain and, arguably, 1890 Argentina.

Figure 1: **British Capital Exports from 1865 to 1914.** In light gray: Banking panics of 1865, 1873, and 1890. In dark gray: The stock crisis of 1907. Source: **?**.



1982, Mahate 1994, and Kindleberger and Aliber 2005, respectively.

[76]Eichengreen 1991, p.151.

For the purposes of illustration, Figure 1 shows the evolution of British capital exports since earliest date while indicating the years of banking panics and stock crises as dated by Reinhart and Rogoff.[77] Figure 1 reflects the boom-and-bust cycles preceding and following a banking crisis as exemplified by the financial crises of 1873 and 1890. Prior to each bust, lending was ferocious. Once the debt bubble burst, international capital flows temporarily dried up across the board. Precisely, during periods of sudden stop, I expect rulers to have strong incentives to finance military campaigns by means other than external borrowing, namely taxes.[78]

To assess the unanticipated nature of sudden stops, Table 3 shows the frequency and duration of war during periods in which international loans flow and dry up.[79] If sudden stops are predictable, more war should occur in periods in which credit flows; yet 52% of war-years coincide with periods in which the international lending market is down.[80] In addition, Figure A-7 in Appendix W shows no increase in war right before the onset of credit crunches, consistent with the unanticipated nature of sudden stops.

Table 3: **Frequency and Duration of War as a Function of Exogenous Credit Access.** Refer to Appendix W for a Visual Illustration.

	Interstate War		**Interstate and Secessionist War**	
	Credit Flows	Credit Stops†	Credit Flows	Credit Stops
Frequency	47.74%	52.25%	50.89%	49.11%
Duration in years	2.32	2.25	2.23	2.29
	(1.87)	(1.51)	(1.73)	(1.58)
War-Year-Country	465		615	
Countries	107		107	

†Credit Stops refers to periods of sudden stop. Standard deviation in parentheses.

Lastly, consider the decision to *end* wars. A weak state that finances war with external

[77]Reinhart and Rogoff 2009.

[78]Based on Figure 1, banking crises might be more damaging than stock market crises (e.g., 1907). Appendix I shows results excluding stock market crises. Results hold.

[79]Table A-7 lists a complete tally of war and capital access at the country level.

[80]The distribution of war-years having and lacking access to foreign capital within region is also fairly distributed despite the sample size drop: 39% of war-years in Africa were fought lacking access to capital, 48% in Asia, 54% in Europe, 59% in Latin America, and 40% in North-America.

credit may be more prone to surrender during sudden stops. If that is the case, weak states would end up with a higher proportion of war-years when credit flows and lower proportion of war-years during sudden stops. This would bias the estimation results toward finding a negative effect of war for years when credit flows. If this pattern was systematic, on average, we should observe shorter wars during sudden-stop periods, precisely because many wars in this period involved a Great Power against a developing country. However, Table 3 suggests that the duration of war in and outside sudden stops is balanced: 2.24 years in periods of sudden stop compared to 2.31 years when credit flows. When secessionist wars are also considered (Columns 3 and 4), duration is virtually balanced.

If war is judged by its frequency and duration, Table 3 suggests a comparison of apples to apples when tackling war waged during periods in which international lending flows and war waged in episodes of sudden stop of credit. A final caveat involves the source of the variation of warfare and capital access. Above I showed that warfare took place all around the world, with the exception of Oceania. Capital imports from Britain were also a pervasive phenomenon, involving sovereign countries and colonial dominions, British and non-British. From 1865-9, government bodies and enterprises in 65 nations sought funds in Great Britain; by 1914, this number had risen to 120.[81] Africa received 12% of all government loans (in £000 value) for the period from 1865 to 1914; Asia 20%, Europe 18%, Latin America 18%, Oceania 15%, and North America 15%.[82] Thirteen percent of these loans were intended for military ends (the rest being invested in public works and transportation). All things considered, the nineteenth century offers an ideal setting in terms of geographic distribution of warfare and capital imports to study the effect of war making *and* external finance on state making.

[81] **?**, p.6.
[82] Computed from **?**.

3.4 Specification

Sudden stops in the nineteenth century lasted, on average, four years.[83] Accordingly, I establish four-year windows following the onset of each sudden stop and assume that within these windows countries had no access to external loans.[84] For each of these periods of time, I count the number of years that country i is at war. To fully test the theoretical expectation, I also compute the number of years that a country is at war while credit flows in the international market.[85] Then, I regress various proxies of fiscal capacity circa 2000 on the number of years at war in the long nineteenth century having and lacking access to external finance as exogenized by sudden stops:[86]

$$\begin{aligned}\text{Long-Run Fiscal Capacity}_i = \quad & \alpha + \beta_1(\#\text{years at war between 1816-1913} \mid \text{no access to external loans}) \\ & + \beta_2(\#\text{years at war between 1816-1913} \mid \text{access to external loans}) \\ & + \boldsymbol{X}_i\delta + \gamma + \rho + \epsilon_i\end{aligned} \tag{4}$$

I consider three proxies for long-run fiscal capacity: First, *Personal Income Tax* (PIT). Implementing a PIT requires a sophisticated bureaucratic apparatus capable of assessing a highly atomized tax base, enforcing compliance and sanctioning defectors. In light of its administrative challenges, this tax is considered to be the endpoint of fiscal capacity building.[87] Accordingly, it sets a clear benchmark to establish how far each country has gone in building tax capacity. In the empirical analysis, I work with average PIT to GDP ratios between 1995 and 2005 to minimize the effect of anomalous observations.[88]

Because PIT might capture both capacity and willingness to tax, a second outcome variable is considered, one that emphasizes the infrastructural component of fiscal capacity:

[83] Catao 2006.

[84] Refer to Appendix I for windows of longer duration. Longer windows can be interpreted as a placebo test. Accordingly, results hold but turn weaker as windows expand.

[85] A given war might be fought entirely while credit flows, while credit dries up, or across periods. In the latter case, I split war-years proportionally across periods. Refer to Appendix C for the distribution of both counts per country.

[86] The analysis is cross-sectional because for most countries time-varying tax data does not exist for the nineteenth century.

[87] Besley and Persson 2011; Tilly 1990.

[88] Appendix B lists all data sources. PIT data availability caps the sample size to 107 countries.

the *Size of the Tax Administration* circa 2005, measured as the number of staff employed by the tax administration per thousand capita. Finally, I also proxy long-term fiscal capacity with *Value-Added Taxes* (VAT), now standard also in the developing world.[89]

Following the discussion at the beginning of this Section, I expect war making to strengthen the ruler's incentives to invest in fiscal capacity whenever the country cannot finance externally, contributing to long-term fiscal capacity, $\beta_1 > 0$.[90] By contrast, in light of the commitment problems in war-debt repayment, I expect a null (if not negative) effect of war making when countries wage war having access to external credit, $\beta_2 \leqslant 0$. A negative sign for β_2 would suggest that the fiscal disequilibrium associated with excess borrowing combined, potentially, with the exchange of state monopolies for default settlements, may fully reverse the effect of war on state making.[91]

Three clarifications are in order: First, the expectation $\beta_2 \leqslant 0$ works against the Ricardian equivalence, which assumes no commitment problem in debt repayment. Based on that logic, borrowing and taxes are equivalent in the long run, implying $\beta_1 \approx \beta_2 > 0$, everything else constant. Second, in the absence of external credit, rulers might resort to printing money, domestic loans, or financial repression to finance the means of war. If any, these alternatives introduce a downward bias on β_1 because they weaken the incentives to enhance taxation in times when external credit dries up.[92] Third, crucial for the quasi-experimental setting, sudden stops are predictable only *ex post*, as I discussed in the immediately preceding section; but suppose that some rulers had inside information and banked external loans in anticipation of sudden stops. Then one should expect no investment in fiscal capacity when financial markets are down. If any, anticipation creates an attenuation bias on β_1.

As part of Expression 4, all models below include a battery of region fixed effects, γ, that account for continent-specific characteristics in the frequency of war, access to credit,

[89]VAT models are reported in Appendix M. Results are equivalent for the three outcome variables.

[90]The baseline category is *fighting no war in the nineteenth century*. Forty-eight percent of the sampled states fought no interstate or secessionist war in the long nineteenth century.

[91]Refer to Appendix D for models in which β_1 and β_2 are estimated separately. Results hold.

[92]Appendix K considers two of these alternatives: domestic credit and money printing.

and statehood timing;[93] and a battery of *Colonial Origins* indicators, ρ, because I expect the colonies' opportunities to go to war, the tax structure that they build up, and the terms of external credit to be conditioned by the metropolis.[94] Relatedly, Appendix N reruns the analysis after dropping former British colonies (and military allies) from the sample, given their privileged relationship with the financial capital of the world. Results hold.

In addition, all models include a vector of potential confounders, $\boldsymbol{X}$, affecting fiscal capacity today as well as war participation, credit access, or both, back in the nineteenth century. First, I consider a measure of initial wealth because wealthier countries are more likely to go to war and have stronger fiscal capacity in the first place.[95] In the absence of systematic GDP data for the early nineteenth century, I include a measure of *Population Density as of 1820*, which is argued to be the best proxy of a country's wealth in the early nineteenth century[96] Second, I also include two geographic characteristics that could affect both sides of the equation. The first one, *Sea Access*, is the percentage of the land surface area of each country that is within 100km of the nearest ice-free coast. I expect sea access to correlate with trade activity (thus access to international lending) and monetization, a precondition for modern taxation.[97] By the same token, I expect territories with sea access to be militarily valuable, thus increasing their likelihood of experiencing war. The second geographic control is the percentage of territory that is *Desert*. I expect deserts to inhibit industrial growth and preempt monetization, but desert territory might also work as a natural barrier to foreign invasion, thus reducing the frequency of war. Finally, I control for an important source of nontax revenue that could also shape the incentives to go to war (or suffer attack): being an *Oil Producer*. Arguably, this variable gains relevance for the later years of the period under consideration. Two additional geographic conditions, *Terrain Ruggedness* and *Land Area*, are evaluated in Appendix L, where they are interpreted as

[93] Appendix F reports models without fixed effects. Results hold.
[94] Accominotti, Flandreau, and Rezzik 2011.
[95] Gennaioli and Voth 2015.
[96] Appendix B lists the source of *Population Density* and all other variables.
[97] Tilly 1990.

geographic determinants of *initial* political conditions.[98]

4 Addressing Endogeneity in External Credit Access

In Table 4, I use the periods of sudden stop to identify periods in which rulers of warring states have stronger incentives to enhance their fiscal foundations. Great Britain—the banker of the world—is excluded from every model to maximize exogeneity.[99]

To establish a benchmark, Column 1 tests for the *unconditional* version of the bellicist hypothesis; that is, does long-term fiscal capacity increase with the number of years at war in the long nineteenth century? Or more generally, does war make states? With a 90% confidence interval, Personal Income Tax (PIT) today increases in the number of years at war in the long ninteenth century, holding everything constant. This result confirms those in Dincecco and Prado.[100]

Column 1 should be compared to Column 2, in which I distinguish the effect of war fought without access to external credit, β_1, from war fought with access to international lending markets, β_2. Consistent with the political economy of war finance, $\hat{\beta}_1$ is positive and significantly different from zero. A one standard deviation increase in the number of years at war while international lending stops increases PIT today by 49% with respect to the sample mean. By contrast, a one standard deviation increase in the number of years at war when credit flowed decreases average PIT today by 31.4%. This result suggests that debt-financed war might create fiscal imbalances that are too hard to fix. These should be strongest among states that hand over state monopoly revenues to lenders in order to regain market access after defaulting.[101]

The opposite signs of $\hat{\beta}_1$ and $\hat{\beta}_2$, plotted in Figure 2, suggest that the effect of war estimated in Column 1, the unconditional hypothesis, is the average of two radically different

[98] Scott 2009.

[99] Appendix N shows results when British colonies are dropped, British military allies are dropped, and all wars in which the British are involved are dropped. Results hold.

[100] Dincecco and Prado 2012.

[101] Refer to Appendix D for models that estimate β_1 and β_2 separately.

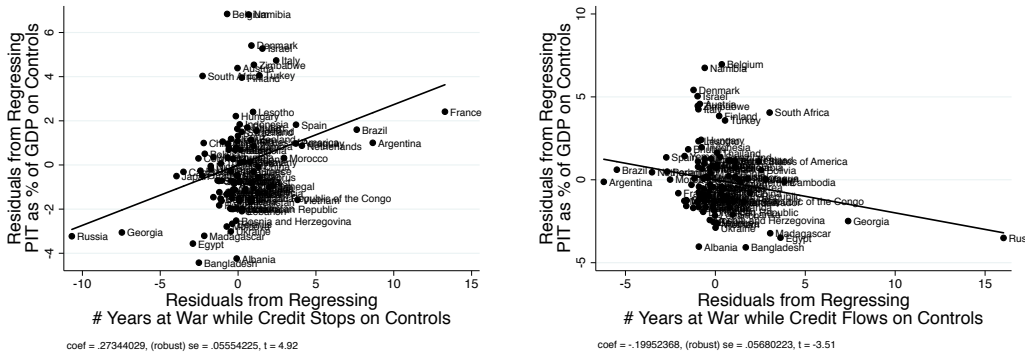
Table 4: **Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long Nineteenth Century.** See Appendix H for Wild-Woostrap cluster standard errors at the region level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# Years at War in 1816-1913	0.052*									
	(0.028)									
# Years at War while Credit Stops in 1816-1913		0.273***	0.251***	0.271***	0.303***	0.273***	0.275***	0.269***	0.234***	0.261***
		(0.056)	(0.055)	(0.073)	(0.081)	(0.060)	(0.056)	(0.055)	(0.056)	(0.053)
# Years at War while Credit Flows in 1816-1913		-0.200***	-0.252***	-0.199***	-0.206***	-0.201***	-0.200***	-0.198***	-0.181***	-0.214***
		(0.057)	(0.069)	(0.057)	(0.068)	(0.052)	(0.057)	(0.058)	(0.060)	(0.056)
Population Density in 1820	1.623	1.238	0.788	1.233	2.314	1.243	1.247	1.220	1.251	0.799
	(1.365)	(1.318)	(1.396)	(1.335)	(1.485)	(1.336)	(1.332)	(1.572)	(1.318)	(1.246)
Oil Producer	0.098	0.127	0.130	0.129	0.156	0.125	0.108	0.218	-0.001	-0.006
	(0.474)	(0.468)	(0.464)	(0.469)	(0.679)	(0.466)	(0.474)	(0.498)	(0.478)	(0.459)
Sea Access	0.027***	0.028***	0.029***	0.028***	0.028***	0.028***	0.028***	0.026***	0.029***	0.029***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Desert Territory	0.004	0.013	0.015	0.013	0.028	0.013	0.014	0.006	0.013	0.011
	(0.045)	(0.045)	(0.045)	(0.045)	(0.067)	(0.045)	(0.046)	(0.048)	(0.046)	(0.045)
Great Power			2.712**							
			(1.166)							
War Location 1816-1913				0.002						
				(0.041)						
War Casualties 1816-1913					-0.481					
					(0.880)					
War Duration 1816-1913						0.008				
						(0.124)				
# Years in Default 1816-1913							0.008			
							(0.010)			
Ethnic Fractionalization								-0.306		
								(1.254)		
# Years at Civil War 1816-1913									0.088**	
									(0.035)	
WWI Participant										1.261**
										(0.533)
Intercept	1.250	1.331	1.279	1.336	1.347	1.327	1.295	1.591	1.275	0.739
	(0.862)	(0.829)	(0.811)	(0.842)	(1.131)	(0.843)	(0.843)	(1.263)	(0.825)	(0.810)
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	106	106	106	106	87	106	106	105	106	106
R-squared	0.551	0.587	0.610	0.587	0.554	0.587	0.588	0.585	0.593	0.609

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

worlds. Indeed, this result advances understanding of the conditions under which war makes states. More important than war itself is the way that it is financed. The remaining columns in this and subsequent tables establish how robust this result is to endogeneity bias, sample selection, and measurement decisions, while making sure not to control for endogenous covariates to war making (e.g., Per Capita GDP).[102]

Figure 2: **Partial Correlations of Personal Income Tax and Exogenous War Finance.** Estimates are drawn from Column 2 in Table 4. Appendix E shows that Russia, Georgia, and France do not bias the estimates.



(a) War while Credit Stops (b) War while Credit Flows

The first potential confounder, being a *Great Power* in the nineteenth century, is examined in Column 3. This control accounts for the idiosyncratic paths of state and war making in the Great Britain, France, Germany, Italy, Austria-Hungary, and Russia.[103] These countries were major military and economic powers in the nineteenth century and could drive results. The coefficient of this indicator variable is positive and statistically significant. Importantly, $\hat{\beta}_1$ and $\hat{\beta}_2$ remain the same.

War causes destruction, but damages vary greatly depending on the location of military engagement. The tax base can be badly hurt when military conflict takes place within national boundaries, thus inhibiting investment in fiscal capacity. The location of war is thus likely to be a confounding variable. To address this logic, Column 4 in Table 4 controls

[102] Appendix Q reports models including endogenous controls. Results hold.

[103] Austria and Hungary are treated as two independent countries. Refer to Appendix B for further details on country splits and merges.

for the location of conflict. In particular, *War Location* is the sum of the years at war fought abroad minus the years at war fought at home from 1816 to 1913. This variable is positive when a country fights more wars abroad than at home; negative, when military disputes at home are more frequent than abroad; or zero, when countries never go to war.[104] The coefficient for this variable is positive, as one would expect, but not statistically significant. The coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ remain unchanged.

All wars are not created equal. Bloodier and longer wars might overcome resistance to taxation while maximizing the ruler's incentives to invest in fiscal capacity. To address this possibility, Column 5 and 6 include a control for the intensity of warfare, measured by the number of battle deaths during the period, *Casualties from 1816 to 1913*,[105] and the average *War Duration* during the period. These variables are not statistically significant even though their presence, if only marginally, pushes up the magnitude of $\hat{\beta}_1$, the effect of war fought while having no access to external credit. Drawn from a different dataset, *War Outcome* is addressed in Table 7.

Next, I consider the reputation of each country in the international market. That is, on top of capital flowing in London, a state's ability to finance war with external loans might depend on its reputation of default.[106] To account for that, Column 7 adjusts for the *Number of Years in Default between 1816 and 1913* of each country. This variable is not statistically different from zero, which is consistent with the "lending frenzy" that characterizes this period.[107] Importantly, the two coefficients of interest remain unchanged.

Columns 8 and 9 control for *Ethnic Fractionalization* and nonsecessionist *Civil Wars*. Ethnic fractionalization might be an impediment to invest in fiscal capacity[108] while increasing vulnerability to foreign intervention. In the absence of better data, ethnic fractionaliza-

[104]To capture any potential negative effect of war within the domestic territory, both inter-state and secessionist wars are considered in computing War Location. Only one country fought the same number of wars at home and abroad. Results are virtually identical if the total number of wars fought abroad or at home are fitted separately.

[105]Dincecco and Prado 2012.

[106]Tomz 2007.

[107]Taylor 2006.

[108]Besley and Persson 2011.

tion is measured as of the 2000s and is potentially endogenous to war.[109] A long history of *Civil War* is a strong predictor of negative patterns of development,[110] but lacking political stability might be penalized by the credit markets. Controlling for civil war, however, is far from ideal because sometimes it results from interstate war. At the risk of incurring in posttreatment bias, Columns 8 and 9 control for the level of ethnic fractionalization today and the number of years at civil wars between 1813 and 1916, respectively.[111] The marginal effect of both controls is positive and in the case of civil wars, also statistically different from zero. Key for the theoretical argument, the inclusion of these variables does not modify the point estimates of β_1 and β_2.

Scheve and Stasavage show that progressive taxation, including PIT, accelerated dramatically among WWI participants.[112] Including the latter covariate in the empirical model might lead to posttreatment bias if countries that frequently went to war in the nineteenth century and developed higher fiscal capacity by 1913 selected into WWI. Still, one might be tempted to include a *WWI Participant* indicator to check whether the coefficients of interest survive this control. Column 10 indicates that they do. The coefficient for WWI sets a meaningful benchmark to compare conventional war making in the long nineteenth century with. WWI's marginal effect on fiscal capacity is 1.3 points, whereas a one standard deviation increase in the number of years at war while having no access to external credit increases average PIT today by 1.4 points. Results are virtually equivalent, meaning that sufficient years of conventional warfare—as long as they are (at least partially) financed with taxes—exert lasting effects equivalent to participation in total war.

Finally, under a median voter framework, one should observe higher tax rates in democracies than in autocracies, everything else constant. Democracies also present a comparative advantage in external financing.[113] In addition, Section 2 showed that the ruler's decision

[109]Table A-11 in Appendix G includes a control for the *Federal* structure of the state as of today, which might reflect accumulated ethnic fractionalization.
[110]Besley and Reynal-Querol 2014.
[111]To minimize bias, civil wars that take place simultaneously with interstate wars are not considered.
[112]Scheve and Stasavage 2010.
[113]Schultz and Weingast 2003.

also depended on initial political institutions. Either reason recommends controlling for *initial* democracy levels. Except for a handful of cases, however, democracy scores by 1820 are unavailable in any systematic way. In view of this limitation, Appendix L shows results for the subsample of cases for which these data are available. Despite the reduced sample size, results hold.

To sum up, Table 4 suggests that war does not necessarily make states. It all depends on the incentives that rulers have to invest in fiscal capacity, which, I argue, are weaker when they have access to external loans and strong when the do not. Before discussing the implications of this result, Tables 5–7 address additional measurement and endogeneity considerations.

4.1 Military Powers, Sovereign States, Secessionist War, and Alternative Outcome Variables

This section addresses four potential issues: Are results driven by big military powers? Is the effect of war equivalent in sovereign and nonsovereign states? Does the theory apply to wars of independence? Does war finance shape *infrastructural transformations* in the long run?

Rulers decide whether to finance the means of war with taxes or external loans. As I argue above, this decision is a function of opportunity (i.e., the political economy of war-finance) and possibility. In that respect, less capable states should be most tempted to finance the means of war externally, specially in a context of massive cross-border lending. By the same token, they should be particularly exposed to the perverse incentives of external financing. To address this point, Columns 1 and 2 in Table 5 rerun the baseline models, dropping first the Great Powers, and then, four additional wealthy countries: the Netherlands, the United States, Canada, and Japan. In both specifications, the point estimate for β_1 remains unchanged with respect to Table 4, suggesting that war makes states in the periphery as long

as it is not financed with external loans.[114]

So far, wars are attributed to the corresponding 2001 nation-state irrespective of whether that territory had achieved statehood by 1913. One could argue that war fought by states unrecognized by the international system exerts a different (or null) effect on fiscal capacity. For instance, colonies might not invest in their military campaigns as much as the metropolis.[115] To address this possibility, Columns 3 and 4 in Table 5 rerun the analysis considering only countries that were sovereign by war time. Results, despite the reduction of the sample size, are similar to those reported in Table 4. *Sovereign states* that waged war while international lending flowed are not associated with high fiscal capacity today. To the contrary, sovereign states that waged war in the midst of a sudden stop have, on expectation, higher tax capacity today.

Some countries in the periphery waged secessionist war in the nineteenth century. The object of these wars was the formation of an independent modern nation-state. Financing secessionist war might exert effects similar to regular warfare; moreover, including them in the analysis might better reflect the universe of states at war during the period under consideration. Consistently, Columns 5 to 6 in Table 5 suggest that waging war, either interstate or secessionist, when international lending stops, is associated with long-run fiscal capacity.

A fourth battery of sensitivity tests addresses the choice of the outcome variable. PIT ratios arguably capture both capacity and willingness to tax, moreover, they vary with the economic cycle. To address both issues, I use an alternative proxy of fiscal capacity, one that emphasizes administrative capacity over willingness: the *Size of the Tax Administration*. First, this variable is a strong predictor of tax yields as shown in Appendix B. Second, unlike tax ratios, the size of the tax administration does not change with the economic cycle, not in the short run. Third, tax bureaucracies are filled with public servants, subject to stricter controls, and relatively sheltered from spurious fleeting interests of passing incum-

[114] Appendix G shows that results hold when all foundational OECD members are dropped.
[115] Gardner 2012.

Table 5: **Sensitivity Analysis.** Long-Run Fiscal Capacity as a Function of War and Exogenous Credit Access in the Long Nineteenth Century. These Models Account for Sample Changes, Conservative State Definition, Secessionist War, and an Alternative Outcome Variable.

DEPENDENT VARIABLE →	**PIT 2000s**						**Tax Staff 2000s**	
SAMPLE →	**Great Powers Excluded**	**Wealthiest† Countries Excluded**	**Non-Sovereign Countries Excluded**		**Secessionist War Included**		**Full‡ Sample**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Years at War while Credit Stops in 1816-1913	0.273***	0.283***	0.150***	0.161***	0.181***	0.161***	0.036**	0.035**
	(0.083)	(0.091)	(0.052)	(0.057)	(0.050)	(0.054)	(0.015)	(0.014)
# Years at War while Credit Flows in 1816-1913	-0.151	-0.166	-0.146**	-0.191**	-0.069	-0.091	-0.018	-0.021
	(0.118)	(0.120)	(0.060)	(0.085)	(0.074)	(0.085)	(0.019)	(0.019)
Population Density in 1820	0.826	0.662	4.399*	3.859	1.458	1.128	0.217	0.188
	(1.398)	(1.500)	(2.419)	(2.845)	(1.349)	(1.437)	(0.239)	(0.255)
Oil Producer	-0.056	-0.084	0.311	0.302	0.015	0.029	-0.106	-0.104
	(0.449)	(0.447)	(0.589)	(0.620)	(0.471)	(0.472)	(0.097)	(0.097)
Sea Access	0.029***	0.028***	0.027**	0.029**	0.027***	0.028***	0.002	0.002
	(0.007)	(0.008)	(0.011)	(0.011)	(0.007)	(0.007)	(0.001)	(0.001)
Desert Territory	0.013	0.013	0.044	0.060	0.013	0.013	0.000	0.001
	(0.046)	(0.046)	(0.064)	(0.057)	(0.045)	(0.045)	(0.005)	(0.005)
Great Power				1.432		1.964		0.136
				(1.552)		(1.257)		(0.237)
Constant	1.043	0.925	1.999*	1.842*	1.158	1.111	-0.116	-0.128
	(0.825)	(0.809)	(1.173)	(1.062)	(0.851)	(0.842)	(0.136)	(0.140)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Colonial Origins	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100	96	49	49	106	106	79	79
R-squared	0.590	0.564	0.825	0.831	0.584	0.597	0.669	0.672

Great Britain is always excluded. †Great Powers plus USA, Canada, Netherlands and Japan. Robust standard errors in parentheses.
‡Full Sample includes Great Powers, Wealthy Countries, Sovereign and Non-Sovereign States. *** p<0.01, ** p<0.05, * p<0.1

bents. These characteristics suggest that the size of the tax apparatus genuinely captures the underlying capacity to monitor and assess private income.[116]

Next, I regress the size of the tax administration, measured by the *Tax Staff per Thousand Capita* circa 2005, on the same set of covariates used to model long-term income tax ratios. Results in Columns 7 and 8 in Table 5 mimic previous ones: Waging war without access to external credit (exogenized by instances of sudden stops) is associated with a more staffed tax administration today; war waged while credit flows is not. For additional robustness tests, Appendix Table A-18 shows models of VAT as a percentage of GDP. Results are equivalent.

5 Addressing Endogeneity in War Participation

The decision to go to war can also be endogenous. First, countries that go to war might have greater administrative capacity to begin with (i.e., omitted variable bias). Second, the type of country that goes to war when credit is tight may differ in ways that are relevant to future tax capacity from those that wait until loans are available (i.e., selection bias). I address both issues stepwise.

5.1 Initial State Capacity

Countries that are frequently at war may have greater capacity to conscript and tax. Importantly, the ruler's foregone utility of financing war with tax revenue is smaller when initial state capacity is higher as shown in Section 2. These capacities may already be captured by the *Great Power* indicator and the proxy of initial wealth: *Population Density as of 1820*. After all, we know that the income level is a strong predictor of war participation.[117] Next, I further minimize bias by considering two covariates associated with *initial* state capacity: Bockstette, Chanda, and Putterman's *State Antiquity Index*;[118] and *Census Capacity*. The

[116]Despite the smaller N, the sample includes countries on five continents.
[117]Gennaioli and Voth 2015.
[118]Bockstette, Chanda, and Putterman 2002.

Table 6: **Addressing Endogeneity in War Participation.** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long Nineteenth Century with Special Attention to Omitted Variable Bias and Selection into War. Refer to Appendix O for additional models using the *Ongoing War* filter.

DEPENDENT VARIABLE →	**PIT 2000s**				
	Controlling for Initial Capacity		**Considering Ongoing Wars only**		
	(1)	(2)	(3)	(4)	(5)
# Years at War while Credit Stops in 1816-1913	0.222***	0.239***	0.164**	0.118*	0.166**
	(0.064)	(0.054)	(0.073)	(0.070)	(0.070)
# Years at War while Credit Flows in 1816-1913	-0.243***	-0.241***	-0.073	-0.085	-0.077
	(0.067)	(0.068)	(0.080)	(0.075)	(0.078)
Population Density in 1820	0.921	0.696	1.083	1.226	0.897
	(1.438)	(1.381)	(1.440)	(1.485)	(1.408)
Oil Producer	0.105	0.156	0.206	0.161	0.178
	(0.465)	(0.450)	(0.479)	(0.474)	(0.459)
Sea Access	0.027***	0.030***	0.029***	0.026***	0.030***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Desert Territory	0.016	-0.016	0.009	0.011	-0.024
	(0.046)	(0.032)	(0.045)	(0.046)	(0.033)
Great Power	2.785**	2.672**	3.153**	3.180**	3.101**
	(1.189)	(1.140)	(1.232)	(1.261)	(1.189)
Modern Census by 1820	1.504			2.085	
	(1.370)			(1.368)	
State Antiquity		0.001			0.002
		(0.001)			(0.001)
Constant	1.272	0.564	1.372	1.345	0.423
	(0.813)	(0.984)	(0.850)	(0.846)	(1.035)
Region FE	Yes	Yes	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes
Observations	106	103	106	106	103
R-squared	0.617	0.646	0.577	0.592	0.617

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

former should correlate with cumulative military and administrative capacity because older states exist as a result of winning war in the past. The latter should correlate (if not facilitate) preparation for war if only because modern censuses tend to follow earlier enumerations in which taxable wealth and the conscription base are assessed. To this end, I have coded the date of the first modern census ever conducted for every country in the sample. To control for *initial* administrative capacity, I create the indicator variable *Modern Census by 1820*, which equals 1 if country i has conducted a modern census by 1820.

Results are reported in Columns 1 and 2 of Table 6. The two new covariates hold positive coefficients, as expected, but are not statistically significant.[119] Importantly, once I control for both proxies of initial state capacity, $\hat{\beta}_1$ and $\hat{\beta}_2$ remain positive and negative, respectively, and statistically significant. That is, independent of observable initial capacity to prepare for war and raise taxes, only war waged when the international lending market was down contributed to build fiscal capacity in the long run.

5.2 Ongoing Wars

Countries that go to war despite the credit crunch may differ from countries that wait for markets to lend again. Table 3 and Appendix Figure A-7 show no evidence of strategic timing of war making once credit access is exogenized: The frequency and duration of war inside and outside sudden stop periods are virtually balanced (and war participation does not increase immediately before the onset of credit crunches). Still, following the onset of a sudden stop, states might choose whether to wage war or what kind of war to fight. Alternatively, peripheral states might have more leeway in deciding which war to fight when the metropolis is experiencing a financial crisis.[120] I address these forms of selection bias by considering only wars that are initiated while the market is still lending and eventually dries up as a result of a financial crisis. These are wars that are initiated without the expectation of a sudden stop. Thus, the decision to go to war or what type of war to fight is disconnected from external credit access.[121]

Columns 3 to 5 in Table 6 show the results of this test. The estimate for $\hat{\beta}_1$ decreases with respect to Table 4, suggesting that the latter may be somewhat upward biased. Based on the new estimate, a one standard deviation increase in the number of *ongoing wars* increases long-term average PIT by 12.5%, a sizable effect. By contrast, $\hat{\beta}_2$ is no longer negative but zero, which is still inconsistent with the unconditional interpretation of the bellicist

[119]Models including *State Antiquity* miss three observations because of data availability.

[120]I am grateful to an anonymous reviewer for this observation.

[121]The 222 country-year-wars taking place during sudden stops falls to 72 once I consider only wars that are ongoing by the onset of a sudden stop.

hypothesis (and the Ricardian equivalence).[122]

5.3 Noninitiators, War Outcome, and COW data

Another route to minimize selection is to study the effect of war making and credit access for states that choose not to go to war but are dragged into it. One could argue that countries that *initiate* war are different from those that are attacked in ways that shape long-term fiscal capacity. Based on this logic, I estimate separately the effect of war making and credit access for countries that are attacked, namely the *noninitiators*. The identification assumption is that initiators do not strike first in anticipation of a likely attack.

To conduct this test, I rely on the COW dataset, which identifies the initiator of each military conflict.[123] The COW dataset includes fewer interstate wars than Wimmer and Min because it follows stricter criteria about what a state in the nineteenth century is.[124] Accordingly, the sample of interstate wars is now made of 37 conflicts, and 172 country-year-wars in total. Seventy-eight were fought when credit flowed, and 96 when credit had suddenly stopped. Average war duration was 1.57 (sd=1.04) and 1.76 (sd=1.23) years, respectively.

COW facilitates information to control for war outcome. This is substantively compelling because military outcomes potentially affect the incentives to invest in fiscal capacity; for example, winners might extract from losers. To this end, *Net Victory* indicates the number of wars won between 1816 and 1913 by country i net of wars lost during the same period. Countries that fought no war have a value of 0.[125]

Table 7 begins by replicating the effect of war and credit access for the *entire* COW sample, including initiator and noninitiators. The effects reported in Columns 1 and 2 are slightly lower than those estimated in Table 4. Based on Column 1, a one standard deviation increase in the number of years at war fought while having no access to credit increases

[122]Appendix O shows that results hold when the ongoing war filter is implemented and the sample is limited to peripheral countries.

[123]Sarkees and Wayman 2010.

[124]Wimmer and Min 2009. Refer to Appendix B for further details.

[125]Three countries won the same number of wars that they lost: Bulgaria, Spain, and Turkey. All other zeros correspond to countries that fought no war within the period.

Table 7: **Noninitiators and War Outcome.** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long Nineteenth Century, with War Data Drawn from COW's Interstate Military Conflict Database, and Accounting for War Outcome.

DEPENDENT VARIABLE →	**PIT 2000s**			
SAMPLE →	**All countries (COW)**		**Noninitiators (COW)**	
	(1)	(2)	(3)	(4)
# Years at War while Credit Stops in 1816-1913	0.380***	0.392***	0.413***	0.412**
	(0.102)	(0.106)	(0.143)	(0.169)
# Years at War while Credit Flows in 1816-1913	0.034	0.024	0.159	0.161
	(0.167)	(0.168)	(0.202)	(0.243)
Population Density in 1820	1.380	1.413	1.469	1.468
	(1.478)	(1.507)	(1.489)	(1.520)
Oil Producer	-0.163	-0.161	-0.256	-0.257
	(0.467)	(0.472)	(0.482)	(0.485)
Sea Access	0.026***	0.027***	0.027***	0.027***
	(0.006)	(0.006)	(0.006)	(0.006)
Desert Territory	0.013	0.015	0.012	0.012
	(0.045)	(0.047)	(0.045)	(0.046)
Great Power	0.672	0.758	1.567	1.562
	(1.358)	(1.375)	(1.329)	(1.294)
Modern Census by 1820	1.748	1.781	2.025*	2.024*
	(1.239)	(1.242)	(1.092)	(1.144)
Net Victory		-0.039		0.001
		(0.091)		(0.112)
Intercept	0.969	0.936	0.991	0.992
	(0.818)	(0.836)	(0.829)	(0.840)
Colonial Origins FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	106	106	106	106
R-squared	0.617	0.617	0.609	0.609

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

average PIT today by 25.7%. Most likely, the decrease of this estimate with respect to Table 4 results from sample selection in COW, which overrepresents wealthier countries, for which additional years at war should exert a relatively smaller effect. Importantly, Columns 1 and 2 imply that results are robust to sample change.

In Columns 3 and 4, I estimate the effect of war and credit access for countries that did not choose to go to war but were pushed into it. Results are similar to the preceding ones, only bigger. Countries that were dragged into war in the midst of a sudden stop of credit present higher levels of fiscal capacity today. The effect vanishes when countries are allowed to borrow external loans to finance war. Importantly, results are robust to war outcome: Winning or losing wars does not significantly modify the differential effect of war making and credit access on long-term fiscal capacity.

Appendix P reports and additional test in which I address selection bias in war participation in a reduced-form format. Specifically, war participation by country i is replaced by that of adjacent countries, a designed inspired in Gennaioli and Voth.[126] Results hold.

One final caveat: War is assumed exogenous in the decision-theoretical model in Section 2, yet rulers may strategically initiate war to overcome resistance to taxation without granting policy concessions—a rally-round-the-flag strategy so to speak. The fiscal effect of this strategy should be transient. Maintaining high levels of taxation is hard if taxpayers cannot constrain the ruler's discretion on fiscal matters.[127] If the ruler initiates war to avoid policy concessions the mechanism that sustains high levels of taxation over time—power-sharing institutions—will not be set in motion either. Empirically, we should not observe long-term persistence of the effect of war on fiscal capacity. Endogeneity of war of this sort introduces a downward bias on the main coefficient of interest, β_1.

[126]Gennaioli and Voth 2015.
[127]Besley and Persson 2011; North and Weingast 1989.

6 Short-Term Effects

I have argued that tax-financed war exerts long-term effects on fiscal capacity because it pushes rulers to conduct fiscal reform. If fiscal capacity building is gradual, one should observe some evidence of this in the short term. In the absence of tax data for current developing economies in the early twentieth century, I work with two measures of state capacity that correlate with tax capacity: The ability to conduct a *Modern Census* and *Primary School Enrollment*, both dated as of 1913. The former measure is clearly a requirement to adopt modern forms of direct taxation because it establishes the potential tax base. The latter measure captures a cornerstone characteristic of the modern state: Public-funded mass education, which requires a solid bureaucratic structure to recruit instructors and standardize curricula.[128] Importantly, we can expect different dimensions of state capacity to correlate: They are presumed complementary, not substitutes.[129]

Columns 1 and 2 in Table 8 report a probit model in which having a modern census by 1913 is regressed on war making and exogenous credit access between 1816 and 1913 plus controls. Results suggest that waging war during the long ninteenth century increases the probability of having a modern census by 1913 only in the absence of external finance.

Columns 3 and 4 fit an OLS model in which the *Date of Adoption* of the first modern census is regressed on the baseline covariates. In this model high values of the dependent variable imply *delays* in census adoption. Results show that fighting wars in times of sudden stop accelerates adoption. Fighting wars having access to credit does not. If any, it increases delay.

Appendix R runs duration models of census adoption as a function of war and credit access between 1816 and 1913. Those models, which exploit longitudinal variation in credit access, war, and key covariates, yield similar results: Capital access discourages the adoption of census technology—a requirement of modern taxation—in war time, whereas lack of access

[128] Gellner 1983.
[129] Besley and Persson 2011.

Table 8: **Short-term Effects of War Making on State Capacity as a function of War and Exogenous Credit Access in the Long Nineteenth Century**

DEPENDENT VARIABLE →	**Modern Census**				**Primary Schooling**		
	By 1913	By 1913	Delay	Delay	By 1913	By 1913	By 1913
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Probit	Probit	OLS	OLS	OLS	OLS	OLS
# Years at War while Credit Stops in 1816-1913	0.115*	0.116**	-3.024***	-2.915***	0.855*	0.935*	0.921*
	(0.059)	(0.059)	(0.827)	(0.795)	(0.491)	(0.508)	(0.513)
# Years at War while Credit Flows in 1816-1913	-0.053	-0.052	2.233**	2.493**	-0.162	-0.135	-0.337
	(0.051)	(0.051)	(0.970)	(0.983)	(0.537)	(0.577)	(0.645)
Population Density in 1820	1.147*	1.175*	-8.516	-6.237	-0.255	2.017	0.408
	(0.675)	(0.712)	(11.975)	(11.800)	(6.521)	(6.893)	(6.825)
Oil Producer	0.689*	0.680*	-16.277**	-16.132**	-7.755	-6.316	-6.189
	(0.361)	(0.394)	(7.817)	(7.827)	(5.263)	(5.242)	(5.329)
Sea Access	0.004	0.004	-0.325***	-0.330***	0.036	0.029	0.037
	(0.005)	(0.005)	(0.120)	(0.122)	(0.056)	(0.053)	(0.052)
Desert Territory	0.025	0.025	-0.972	-0.981	0.160	0.275	0.286
	(0.039)	(0.039)	(0.687)	(0.694)	(0.340)	(0.351)	(0.360)
State Antiquity		-0.000	-0.020	-0.021		-0.019	-0.018
		(0.001)	(0.022)	(0.022)		(0.016)	(0.016)
Great Power				-13.195			8.335
				(12.933)			(8.422)
Constant	-2.609***	-2.542***	170.901***	171.295***	-0.192	6.101	4.622
	(0.687)	(0.802)	(14.579)	(14.720)	(6.082)	(8.119)	(8.427)
Initial Level of Dep Variable†	No	No	No	No	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102	99	103	103	76	76	76
R-squared	-	-	0.565	0.567	0.858	0.863	0.865

Great Britain is excluded. The *Great Power* indicator in Columns 1 and 2 cannot be estimated because of perfect collinearity. †Initial Value of Primary Schooling in 1820 is logged to account for ceiling effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

to capital speeds up adoption.

Results in Columns 5 to 7, in which I model the primary school enrollment by 1913, mimic previous results: War making while credit flows does not predict higher enrollment ratios by 1913, whereas war making while credit stops does. Importantly, results are robust to state antiquity and initial enrollment ratios.[130]

7 Evidence of Transmission

Does the effect of war finance travel over time, and why? In this section, I show evidence of transmission, then advance a political transmission mechanism. First, I show transmission by studying the effect of nineteenth-century war finance on post-WWII tax capacity. Given data constraints, to proxy fiscal capacity, I rely on the share of total tax revenue that is *not* accrued from trade taxes. This share measures the effort to raise revenue through sophisticated taxes (e.g., the income tax) instead of tariffs, a tax handle that low-capacity countries often use.

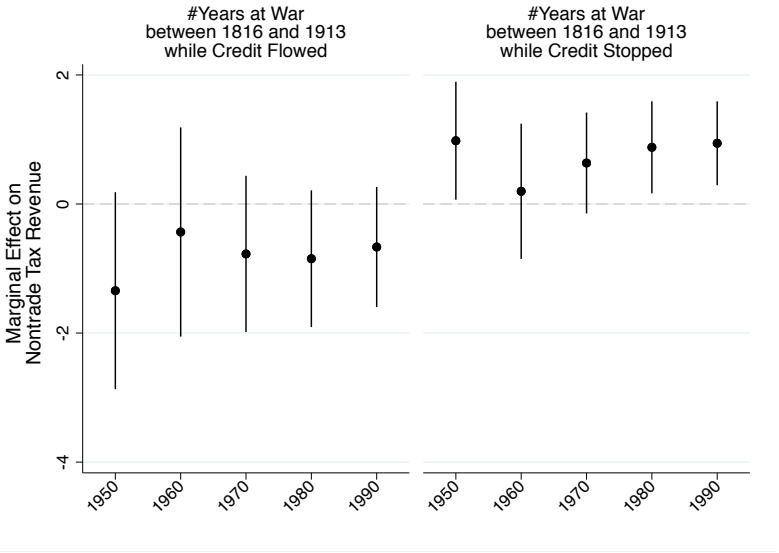
To assess transmission, first I compute decennial averages of nontrade taxes as percent of total taxation from 1945 to 1995; then, I regress those ratios on the number of years at war having and lacking access to external loans in the nineteenth century plus controls (i.e., Expression 4).[131] Figure 3 summarizes results.

The left plot shows that waging war in the nineteenth century with access to credit is not associated with post-WWII fiscal capacity, whereas waging war lacking access to external finance is (right plot). Approximately, an additional year at war in the nineteenth century lacking external finance increases post-WWII nontrade tax revenue by 1%, else constant. This result suggests that the effects reported in Tables 4–8 persist throughout the twentieth

[130] Appendix S considers a third proxy of state capacity by 1913: the length of open rail lines.

[131] Data for nontrade tax revenue is limited. The small N does not allow to fit region- and colony-fixed effects. To minimize unobserved heterogeneity across units, I include a *Former Colonial Status* indicator, which collapses the three previous dummy variables (British, Iberian, and Other Colonies) into one, and the *Great Power* indicator, which adjusts for the systematic difference of European core powers. In addition, I include a control for initial wealth (proxied by *Population Density in 1820*), *Oil Production*, and *Sea Access*. Results in regression format can be found in Appendix T.

Figure 3: **Evidence of Transmission:** Marginal effects of the Number of Years at War with and without access to External Credit between 1816 and 1913 on Nontrade Tax Revenue from 1945 to 1995 (decennial averages centered at first year of decade). 90% CI.



century.

7.1 Transmission Mechanism

The fiscal effect of war travels over time. Why? The theory section states that the effect of war on long-run fiscal capacity is transmitted via a political channel. The connection between war finance and political reform has a long tradition in the literature.[132] In order to finance the means of war, rulers may willingly share power over spending decisions to overcome taxpayers' resistance to increased taxation.

Power-sharing institutions facilitate transmission of the war effect because they transform taxation into a nonzero-sum game—revenue is secured by the ruler, whom taxpayers hold fiscally accountable—facilitating sustained investment in tax capacity.[133] The findings so far suggest, however, that incentives to finance war with taxes—thus chances of observing movements toward power-sharing institutions—are weak when countries have access to external

[132]Bates and Lien 1985; Cox 2012; Dincecco 2011; Ferejohn and Rosenbluth 2016; Hoffman and Rosenthal 2000; Stasavage 2016; Tilly 1990.
[133]Besley and Persson 2011.

finance. By contrast, war should contribute most decisively to political reform—and activate the political mechanism of transmission—when it is waged while having no access to external finance. This argument is consistent with the resource curse literature, and specifically with Downing's interpretation of state making and political reform in early modern Europe: Countries that systematically relied on ore from colonies to finance military campaigns (e.g., Spain) bypassed parliament and did not develop tax capacity.[134]

Figure 4 lends support to the political mechanisms. It shows that *Executive Constraints*—the expected outcome of the political bargaining over taxation—are positively associated with waging war while lacking external finance, both in the short and long run. A one standard deviation increase in the number of years at war while credit is tight in the nineteenth century increases average executive constraints by 16% in 1913 and 4.5% in the 2000s.[135] By contrast, war waged while having access to external credit is not associated with political change in the short or long run. If any, that relationship is negative.

In sum, Figure 4 suggests that war facilitates political reform when incumbents cannot escape the political costs of domestic taxation, namely when they lack external finance. Political reform, in turn, transforms taxation into a win–win game, facilitating long-term persistence.[136]

8 Discussion

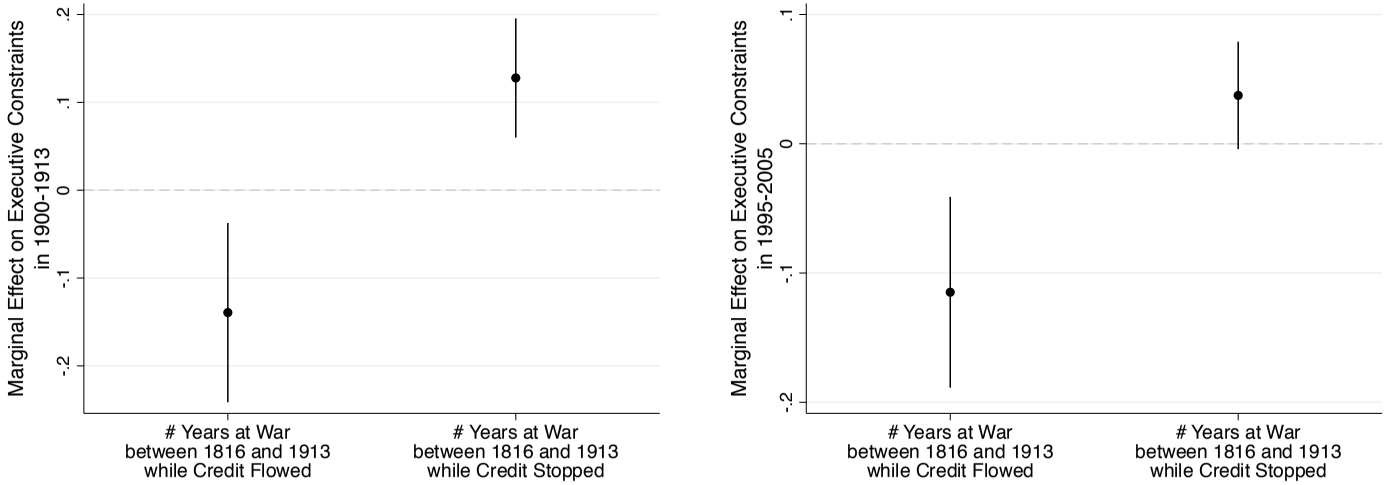
Contrary to the unconditional characterization of the bellicist hypothesis, that is, *more war, more state*, I argue—alongside Tilly's original work—that the effect of war on state building ultimately depends on how warfare is financed. Specifically, I claim that financing war with taxes is conducive to state-making, whereas financing wars with external loans may not because of two extended practices to dodge the Ricardian equivalence: debt relief and

[134]Downing 1993, p.80.

[135]Data for *Executive Constraints* is drawn from Polity IV. Estimates are drawn from models that control for *Initial Executive Constraints*. Refer to Appendix U for results in regression format.

[136]Appendix V explores an alternative, nonmutually exclusive bureaucratic mechanism.

Figure 4: **Political Mechanism:** The Effect of War Finance on Executive Constraints in the Short- (1900-13) and Long-Run (1995-2005). 90% CI.



(a) Short-Run

(b) Long-Run

the exchange of old bonds for state monopolies and nontax revenue.

Building on Centeno and Thies, I emphasize the radically different international context in which countries in the periphery are created as compared to that faced by European nations in early modern times.[137] Most states in the periphery were founded only after 1815, coinciding with the globalization of financial markets, resulting from the income growth in the wake of the industrial revolution and Britain's capacity to spin off excess savings to the rest of the world. Unlike European states, from their very inception the new states in the periphery had access to unprecedented inexpensive external finance despite their weak institutionalization, frequent government turnover, and lack of reputation in the international markets. The lending frenzy stopped abruptly in 1914. By then, however, many externally financed wars had already been fought.

Cheap external credit strengthened incentives to finance war externally while preempting the development of *domestic* credit markets, thus the formation of a corpus of domestic lenders with whom to strike bargains conducive to political reform and long-term fiscal capacity.[138] Debt relief and the exchange of old bonds for nontax revenue minimized the cost of default for developing nations; however it precluded the Ricardian equivalence, thus state building. Counterintuitively, countries in the periphery may have benefited from less dynamic international lending markets because that would have strengthened the incentives to raise taxes to finance the means of war, stimulate domestic borrowing, and conduct the political reform associated with long-term fiscal capacity—namely, what Europeans were pushed to do only centuries before, when international credit markets were oligopolistic and expensive.[139]

The perverse or unanticipated effects of inexpensive external credit resonate with Tilly's original hypothesis by emphasizing the conditional effect of war on credit access: In Europe, frequent war making and the absence of cheap external credit propelled domestic lending and

[137]Centeno 2002; Thies 2005.
[138]North and Weingast 1989; Stasavage 2011.
[139]Homer and Sylla 2005.

eventually political reform that addressed commitment problems in debt repayment.[140] Interstate war combined with domestic lending allowed territorial states to pursue the "coercive-capital intensive" (or fiscal–military) strategy that ended in the modern tax state.[141] By contrast, states that kept relying on external loans to finance war found it much harder to capitalize the effect of war on state making, for example, Spain under Phillip II.[142] Access to cheap external credit—which countries in the periphery had since their inception—distorted the causal chain of the original bellicist hypothesis. Readily available external loans and extended practice of debt relief and exchange of old bonds for nontax revenue and state monopolies weakened the incentives to finance war with taxes and ultimately preempted the opportunity to capitalize war efforts and conduct political reform. Interestingly, the perverse incentives associated with cheap loans are similar to those derived from other forms of nontax revenue: foreign aid,[143] oil,[144] and ore from colonies.[145] Altogether, I establish the scope conditions under which war exerts positive and lasting effects on state building in the era of global credit markets.

[140]Domestic markets were created twofold: By lending from merchants in commercial cities (e.g., Henry IV, 1598-1610, borrowed from Paris and marginalized increasingly expensive Italian lenders) or by coercive annexation of capital-intensive cities (Stasavage, 2011).

[141]Tilly 1990.

[142]Drelichman and Voth 2011.

[143]**?**.

[144]Ross 2001.

[145]Downing 1993.

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NOT FOR PUBLICATION

Supplementary Online Appendices

The Legacy of War on Fiscal Capacity

These appendices contain materials, results and robustness checks that supplement the main text.

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A The Exchange of Old Bonds for Equity as part of Default Settlements

Along with significant debt relief, default settlements in the first globalization of credit involved the exchange of land, state monopolies, and other assets—generally referred to as equity—for old war bonds as part of default settlement. Specifically, defaulters would handle over land and state monopolies (e.g., salt, railroads) to their lenders as part of the default settlement (Marichal, 1989). The exchange of nontax revenue for old bonds allowed countries to regain credit access without enhancing tax capacity, potentially unraveling the Ricardian equivalence. This Appendix shows that this exchange decreased the price of external loans and that strengthened the incentives to finance war externally.

First I compute the premium paid when equity (e.g. rents from copper mines) can be used both to finance war and as collateral in case of default. The lender's expected return from lending to a risk-free borrower is $(W-N)(1+r)-(W-N)$, and to a non-risk-free borrower is $(1-d)(1+r+p)(W-N)+dN-(W-N)$, where d is the risk of default and $(W-N)$ is the loan size to finance war. Notice that in case of default, equity is confiscated by the international investor. Let $\psi \in \{0,1\}$ denote the market value of equity relative to war cost, $N=\psi W$. The premium is obtained by setting the expected payoff of lending to a country with nonzero probability of default equal to the risk-free expected payoff:

$$p=\frac{d(1+r-\psi(2+r))}{(1-d)(1-\psi)} \tag{5}$$

The individual premium is a negative function of the market value of equity ($\partial p/\partial\psi < 0, \partial p/\partial^2\psi < 0$) because the latter will be confiscated in case of default. The effect of default d depends on ψ: For $\psi < (1+r)/(2+r)$, the premium increases with the risk of default. For $(1+r)/(2+r) < \psi < 1$, the premium *decreases* with the risk of default. That is, when the value of equity relative to the war cost is high, the premium will decrease in expectation of

default and confiscation of the collateral. This offers a compelling explanation for why the market would lend to economies with weak fundamentals.

In the presence of equity, the ruler's present value of taxation becomes

$$\kappa T + N - W - c_t + \delta\big[(\kappa + \eta)T + N - c_t\big] \tag{6}$$

where equity is also used to finance war. The ruler's payoff upon financing externally becomes

$$L + N - W - c_l + \delta\big[(1-d)\big(\kappa T + N - (1+i)L - c_t\big) - dN\big] \tag{7}$$

Plugging (5) into $i = r + p$, we reach the new condition of loan preference over taxes:

$$L > \kappa T - \Delta c + \delta\big[\eta T + (1-d)L(1+r) + \frac{Ld(1+r-\psi(2+r))}{1-\psi} + d(\kappa T + 2N - c_t)\big] \tag{8}$$

which is similar to Expression 3 in the main text except for one important caveat: As the market value of equity ψ increases, so does preference for external finance ($\partial RHS/\partial\psi < 0$, $\partial RHS/\partial^2\psi < 0$).

Together, Appendix A shows that once war debt can be exchanged for equity, credit is offered at lower rates (Expression 5) and the ruler's incentives to finance war with loans strengthen (Expression 8), thus expanding the states of the world in which war is financed externally rather than with taxes, preempting also the activation of the persistence mechanism of war effects.

B Data Details

1. Personal Income Tax. PIT data (normalized to GDP) is drawn from various sources. Chief among them is the IMF *Global Financial Statistics* (GFS). This source provides almost 80% of the data. Consistent with the theoretical claims, I work with PIT raised by the *central* government, as war is expected to makes states by centralizing fiscal powers. The GFS data that I work with refer to cash-accounts (as recommended by the IMF). For the few cases that these data are not available, I use non-cash values, which correlate at .97 with cash-accounts.

Personal Income Tax data is scarce, even for the IMF. Missing values are filled in with various sources. Crucially, Column 1 in Table A-1 shows that data augmentation does not change the point estimates of interest. That is, models that use GFS data only yield the same results than models that augment GFS data with additional sources.

Cases not covered by the GFS are filled as follows: for Chile, Nicaragua, Ecuador and Guatemala, data are drawn from the *Inter-American Development Bank Dataset*;[146] for Nepal, data are drawn from the Ministry of Finance;[147] For Sri Lanka, data are drawn from the Department of Fiscal Policy;[148] for Lebanon, data are available from the Ministry of Finance for the 2000-5 period;[149] For Zambia, data are for 2005 only, and are drawn from CMI Report;[150] For Guinea, Rwanda, Chad, Namibia and Yemen, Kenya, Mali, Nigeria, Philippines, Senegal and Vietnam only 2004 data are drawn from the pilot study of the *USAID Fiscal Reform and Economic Governance Project, 2004-2010*. Again, results do not change as a result of the data augmentation (refer to Column 1 in Table A-1).

To minimize influence of abnormal values, I compute average PIT values as a percentage

[146]IDB (Inter-American Development Bank) and CIAT (Inter- American Center of Tax Administrations). 2012. Latin America and the Caribbean Fiscal Burden Database, 1990-2010. Database n. IDB-DB-101. Washington, DC.

[147]Nepal Rastra Bank, Research Department Government Finance Division. 2014. *A Handbook of Government Finance Statistics*.

[148]Available at http://www.treasury.gov.lk/fiscal-operations/fiscal-data.html. Accessed, March 31, 2015.

[149]Available at ttp://www.finance.gov.lb/EN-US/FINANCE/REPORTSPUBLICATIONS/DOCUMENTSANDREPORTS Accessed on March 31, 2015.

[150]Odd-Helge Fjeldstad and Kari K. Heggstad. 2011. *The tax systems in Mozambique, Tanzania and Zambia: capacity and constraints* Bergen: Chr. Michelsen Institute (CMI Report R 2011:3) 124 p.

Table A-1: **Measurement Decisions regarding the Dependent Variable:** Long-Run Personal Income Tax (as % of GDP) as a Function of War and Exogenous Credit Access in the Long Nineteenth Century.

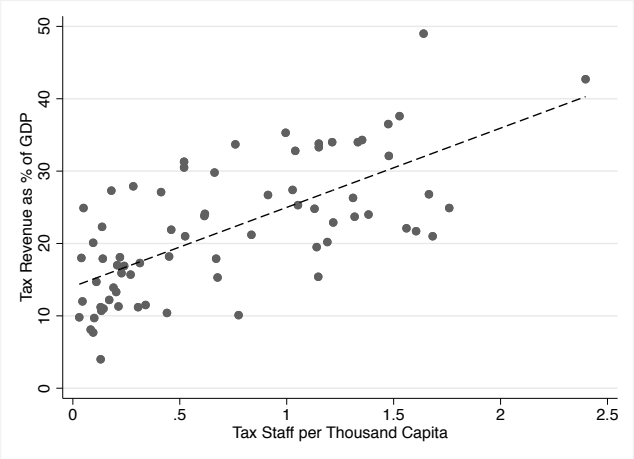
	(1) Non-Augmented Dep Variable, 1995-2005	(2) Dep Variable dated as of 1990-2000	(3) Dep Variable dated as of 2000-10
# Years at War while Credit Stops in 1816-1913	0.280***	0.191**	0.226***
	(0.069)	(0.073)	(0.052)
# Years at War while Credit Flows in 1816-1913	-0.254***	-0.182**	-0.228***
	(0.072)	(0.076)	(0.073)
Population Density in 1820	0.742	2.278	1.057
	(1.600)	(1.540)	(1.434)
Oil Producer	-0.014	0.166	0.133
	(0.527)	(0.692)	(0.427)
Sea Access	0.031***	0.033***	0.029***
	(0.008)	(0.009)	(0.007)
Desert Territory	-0.055	-0.041	0.026
	(0.056)	(0.055)	(0.038)
Great Power	2.587**	2.047	2.850**
	(1.218)	(1.303)	(1.132)
Constant	1.442	0.446	1.322*
	(0.901)	(1.140)	(0.712)
Region FE	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes
Observations	87	83	104
R-squared	0.656	0.601	0.625

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

of GDP for the 1995-2005 period. This decade maximizes the sample size compared to earlier and later decades. For robustness, Columns 2 and 3 in Table A-1 show results for slightly different time periods: 1990-2000 and 2000-2010. Results are the same.

2. Tax staff. The size of the tax administration is drawn from the USAID *Fiscal Reform and Economic Governance Project, 2004-2010*. To maximize the sample size, I combine the values for 2004, 2007-10. This variable is a strong predictor of total tax revenue to GDP (as drawn from US AID 2008), as Figure A-1 shows. Additionally, it presents advantages discussed in the main text: e.g., it does not vary with the economic cycle, unlike tax ratios.

Figure A-1: **Total Tax Revenue vs. Size of the Tax Administration**



3. Census. I coded the date of the first modern census based on Goyer and Draaijer (1992a,b,c) (abc). Specifically, a modern census requires periodicity, universality, and individual enumeration by means of house-to-house visitation.

4. WWI participation. The WWI indicator takes value 1 for all countries that *actively* participated in WWI (i.e., suffered military casualties).

5. War and Geographical Mapping. The main source of war data is Wimmer and Min (2009). All inter-state wars included in the analysis are listed in Table A-2. In the few cases that a country fights more than one war in the same year, I keep the longest war in the sample. This change fundamentally affects Great Britain (which is always excluded from the analysis to maximize exogeneity of the sudden stops) and France. Table 5 in the main text shows that results hold even when France (and other Great Powers) is dropped from the sample.

Most wars can be easily matched to current state borders thanks to the geographical location provided in this dataset. For non-obvious matches, I make the following assumptions:

i. **Country Splits:** This refers to wars attributed by Wimmer and Min (2009) to former political entities that eventually split. Countries affected are: Austria-Hungary,

Czechoslovakia, Korea, Peru-Bolivia, and Yugoslavia. To facilitate matching, entries have been duplicated and attributed evenly across current political units: Austria and Hungary, Czech Republic and Slovakia, North Korea and South Korea, and Peru and Bolivia, respectively. Example: suppose that Austria-Hungary fought 5 wars within 1816-1913, then I assign 5 wars to Austria and 5 wars to Hungary. The assumption is that both entities evenly inherit the fiscal burden and consequences of warfare. Data for the outcome variable for the constituent parts of former Yugoslavia are missing. This case is not considered.

ii. **Region-to-State Match**: see Table A-3

iii. **Tentative Match:** see Table A-4

iv. **Unmatched Units:** These are former polities that overlap with more than one state today. These are not considered in the analysis: Bornu (modern Chad, Niger and Cameroon), Khanate of Kokand (Kazakhstan and Uzbekistan), Mandingo Empire (eleven states in West Africa), Oyo (various states in West Africa), Zuku, Tukolor Empire (Mali, Nigeria and Guinea), Bambara Empire (Guinea and Bali), and Principality of Jammu (China, Tibet, Pakistan).

Table A-2: **List of Inter-State Wars, 1816-1913.** This table reproduces the war list in Wimmer and Min 2009 for this period. To this list, I apply *country splits* (explained above, followed by a *) and *region-to-state matches* (explained above, followed by a †). Units that are *tentatively matched* (listed in Table A-4, not considered in the main analysis) are followed by a ‡. This table does not include *secessionist war* (considered only in Columns 5 and 6 in Table 5 in the main text); nor war by *unmatched units* (listed above, and followed by a *). Notice that some states are not included in the final sample (e.g., Afghanistan) because of data availability for the rest of covariates.

Onset	**War Name**	**Participants**
1816-1818	Egypt vs. Wahabis	Egypt, Saudi Arabia
1816-1825	Russia vs. Georgians	Russia, Georgia (Kingdom of Kartli-Kakheti)†
1817-1818	British-Mahrattan	United Kingdom, Maratha Empire
1817-1818	British-Kandyan	United Kingdom, Sri Lanka (Kingdom of Kandy)†
1820-1820	Egypt's conquest of Sudan	Egypt, Sudan (Kingdom of Sinnar)†
1821-1823	Turko-Persian	Turkey, Iran
1823-1823	Franco-Spanish	France, Spain
1823-1826	British-Burmese of 1823	United Kingdom, Myanmar
1824-1826	British-Ashanti of 1824	United Kingdom, Ashanti Kingdom‡
1825-1826	British-Bharatpuran	United Kingdom, Kingdom of Bharatpur‡
1826-1828	Russo-Persian	Russia, Iran
1827-1829	Bolivia vs Peru	Bolivia, Peru
1828-1829	Russo-Turkish	Russia, Turkey
1829-1840	Russia vs. Circasians	Russia
1831-1832	Ottoman Empire vs. Egyptians	Turkey, Egypt
1831-1834	Thailand vs. Cambodia	Thailand, Cambodia
1835-1835	Bolivia vs. Peru	Bolivia, Peru
1838-1838	Iran vs. Afghanistan	Iran, Afghanistan
1838-1840	British-Zulu of 1838	United Kingdom, Zulu‡
1838-1842	British-Afghan of 1838	United Kingdom, Afghanistan
1839-1839	Russo-Khivan	Russia, Khanate of Kiva‡
1839-1839	War of the Bolivian confederation	Peru-Bolivia*, Chile, Argentina
1839-1840	Ottoman Empire vs. Mehmet Ali	Turkey, United Kingdom
1839-1842	First Opium	United Kingdom, China
1839-1847	Franco-Algerian of 1839	France, Algeria (Barbary states)†
1841-1841	Peruvian-Bolivian	Peru, Bolivia
1841-1841	Dogra Invasion of Tibet	Tibet†, Principality of Jammu*
1841-1845	Thailand vs. Vietnam over Cambodia	Thailand, Vietnam
1843-1843	British-Baluchi	United Kingdom, Kingdom of Sindh‡
1844-1844	Franco-Moroccan	France, Morocco
1845-1846	British-Sikh of 1845	United Kingdom, Kingdom of Lahore‡

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Table A-2 – *Continued from previous page*

Years	War Name	Participants
1845-1852	Uruguyan Dispute	Argentina, Brazil, France, United Kingdom
1846-1847	British-Kaffir of 1846	United Kingdom
1846-1848	Mexican-American	Mexico, United States of America
1848-1849	First Schleswig-Holstein	Denmark, Germany
1848-1849	British-Sikh of 1848	United Kingdom, Kingdom of Lahore‡
1849-1849	Roman Republic	Austria-Hungary*, France, Papal States†, Two Sicilies†
1850-1853	British-Kaffir of 1850	United Kingdom
1852-1852	Siege of Montevideo	Uruguay, Brazil, Argentina, France, Great Britain
1852-1853	British-Burmese of 1852	United Kingdom
1853-1856	Crimean	France, Italy, Russia, Turkey, United Kingdom
1856-1857	Anglo-Persian	Iran, United Kingdom
1856-1857	Kabylia Uprising	France
1856-1857	Nicaragua vs. Walker	Nicaragua
1856-1860	Second Opium	France, United Kingdom, China
1857-1857	Franco-Senegalese of 1857	France, Kingdom of Waalo‡
1858-1862	Franco-Indochinese of 1858	France, Vietnam
1859-1860	Spanish-Moroccan	Morocco, Spain
1860-1870	British-Maorin	United Kingdom
1862-1867	Franco-Mexican	France, Mexico
1863-1863	Ecuadorian-Columbian	Colombia, Ecuador
1864-1864	Second Schleswig-Holstein	Austria-Hungary*, Denmark, Germany
1864-1866	Russia vs. Kokand and Bokhara	Russia, Khanates of Kokand and Bokhara⋆
1864-1870	Lopez	Argentina, Brazil, Paraguay
1865-1865	British-Bhutanese	United Kingdom, Bhutan
1865-1866	Spanish-Chilean	Chile, Peru, Spain
1866-1866	Seven Weeks	Austria-Hungary*, Baden†, Bavaria†, Germany, Hanover†, Hesse Electoral†, Hesse Grand Ducal†, Italy, Mecklenburg Schwerin†, Saxony†, Wuerttemburg†
1867-1868	British-Ethiopian	United Kingdom, Ethiopia
1870-1871	Franco-Prussian	Baden†, Bavaria†, France, Germany, Wuerttemburg†
1873-1874	British-Ashanti of 1873	United Kingdom, Ashanti Kingdom‡
1873-1878	Dutch-Achinese	Netherlands, Aceh Sultanate‡
1873-1885	Franco-Tonkin	France, Vietnam, China
1875-1876	Egypto-Ethiopian	Egypt, Ethiopia
1876-1876	First Central American	El Salvador, Guatemala
1877-1878	Russo-Turkish	Russia, Turkey
1877-1878	British-Kaffir of 1877	United Kingdom
1878-1880	British-Afghan of 1878	United Kingdom

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Table A-2 – *Continued from previous page*

Years	War Name	Participants
1878-1881	Russo-Turkoman	Russia
1879-1879	British-Zulu of 1879	United Kingdom, Zulu‡
1879-1883	Pacific	Bolivia, Chile, Peru
1881-1881	Russia vs. Turkmen	Russia
1881-1882	Franco-Tunisian of 1881	France, Tunisia
1882-1882	Anglo-Egyptian	Egypt, United Kingdom
1882-1884	Franco-Indochinese of 1882	France, China, Vietnam
1883-1885	Franco-Madagascan of 1883	France, Madagascar (Merina Kingdom)†
1884-1885	Sino-French	China, France
1885-1885	Second Central American	El Salvador, Guatemala
1885-1885	Russo-Afghan	Russia, Afghanistan
1885-1885	Serbo-Bulgarian	Yugoslavia (Kingdom of Serbia)†, Bulgaria
1885-1886	British-Burmese of 1885	United Kingdom, Myanmar
1885-1886	Mandigo	France, Mandingo Empire
1887-1887	Italo-Ethiopian of 1887	Italy, Ethiopia
1889-1889	Sudan vs. Ethiopia	Sudan (Mahdiyya state)†, Ethiopia
1889-1892	Franco-Dahomeyan	France, Benin (Kingdom of Dahomey)†
1890-1891	Franco-Senegalese of 1890	France, Senegal (Kingdoms of Jolof and Futa Toro)†
1891-1891	French vs. Tukulor Empire	France, Mali (Tukulor Empire)†
1892-1892	Belgian-Congolese	Belgium
1893-1893	Franco-Thai	France, Thailand
1893-1893	Invasion of Bornu near Lake Chad	Bornu
1893-1893	British vs. Matabele	United Kingdom, Ndebele Kingdom‡
1893-1894	British-Ashanti of 1893	United Kingdom, Ashanti Kingdom‡
1894-1894	Dutch-Balian	Netherlands, Balinese Kingdom of Lombok‡
1894-1895	Sino-Japanese	China, Japan
1894-1895	Franco-Madagascan of 1894	France, Madagascar (Merina Kingdom)†
1895-1896	Italo-Ethiopian of 1895	Italy, Ethiopia
1896-1899	Mahdi Uprising	France, United Kingdom, Sudan (Mahdiyya state)†
1897-1897	Greco-Turkish	Greece, Turkey
1897-1897	British-Nigerian	United Kingdom, Benin Empire†
1898-1898	Spanish-American	Spain, United States of America
1899-1902	Boer War of 1899	United Kingdom, Orange Free State†, South African Republic†
1900-1900	Boxer Rebellion	China, France, Japan, Russia, United Kingdom, United States of America
1900-1900	Sino-Russian	China, Russia
1903-1903	British Conquest of Kano & Sokoto	United Kingdom, Emirates of Kano‡, Sokoto‡

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Table A-2 – *Continued from previous page*

Years	**War Name**	**Participants**
1903-1904	United Kingdom vs. Tibet	United Kingdom, Tibet[†]
1904-1905	Russo-Japanese	Japan, Russia
1904-1905	South West African Revolt	Germany
1906-1906	Third Central American	El Salvador, Guatemala, Honduras
1907-1907	Fourth Central American	El Salvador, Honduras, Nicaragua
1909-1910	Spanish-Moroccan	Morocco, Spain
1911-1912	Italo-Turkish	Italy, Turkey
1911-1912	First Moroccan	France, Spain
1912-1913	First Balkan	Bulgaria, Greece, Turkey, Yugoslavia (Kingdom of Serbia)[†]
1913-1913	Second Balkan	Bulgaria, Greece, Romania, Turkey, Yugoslavia

Table A-3: **Region-to-State Matches between Political Units listed in Wimmer-Min 2009 and Modern Nation-States.** Table A-3 lists political units in Wimmer and Min (2009) that were eventually incorporated to a larger unit (or merged into one). These are nonstate and substate actors that can be easily matched to current nation-states. All these cases are considered in the main analysis.

Original unit →	**Matched to**	**Original unit →**	**Matched to**
Hanover	Germany	Syria (Arab Kingdom of Syria)	Syria
Hesse Electoral	Germany	Algeria (Barbary states)	Algeria
Hesse Grand Ducal	Germany	Afghanistan (Durrani Kingdom)	Afghanistan
Baden	Germany	Benin (Kingdom of Dahomey)	Benin
Bavaria	Germany	Benin Empire	Benin
Wuerttemburg	Germany	Argentina (United Provinces of Rio de la Plata)	Argentina
Saxony	Germany	Georgia (Kingdom of Kartli-Kakheti)	Georgia
Mecklenburg Schwerin	Germany	Madagascar (Merina Kingdom)	Madagascar
Modena	Italy	Mali (Tukulor Empire)	Mali
Papal States	Italy	Yugoslavia (Kingdom of Serbia)	Serbia
Tuscany	Italy	South African Republic	South Africa
Two Sicilies	Italy	Orange Free State	South Africa
Senegal (Kingdoms of Jolof and Futa Toro)	Senegal	Tibet	China
Sri Lanka (Kingdom of Kandy)	Sri Lanka	Transvaal	South Africa
Sudan (Kingdom of Sinnar)	Sudan	Xhosa	South Africa
Sudan (Mahdiyya state)	Sudan	Republic of Vietnam	Vietnam

Table A-4: **Tentative Matches.** These are political units listed in Wimmer-Min that cannot be directly matched to current states. They are not considered in the main analysis, but results are robust to their inclusion, as shown in Columns 2 and 3 in Table A-11.

Original unit	**Matched to**
Aceh Sultanate	Indonesia
Ashanti Kingdom	Ghana
Buganda	Uganda
Emirates of Kano	Nigeria
Khanate of Kiva	Uzbekistan
Kingdom of Bharatpur	India
Kingdom of Lahore	Pakistan
Balinese Kingdom of Lombok	Indonesia
Maratha Empire	India
Sanusi Empire	Lybia
Sokoto	Nigeria
Zulu	South Africa
Zulu Kingdom	South Africa
Ndebele Kingdom	Zimbabwe
Kingdom of Sindh	Pakistan
Kingdom of Waalo	Senegal

5. Civil War. Wimmer and Min (2009) differentiate between secessionist and non-secessionist war.

- **Secessionist War:** Wimmer and Min's (2009) dataset attributes war participation to the colonial power only. I extend their code by attributing war participation to the territory that seeks independence. After this change the variable remains as listed in Table A-5. Analysis including these cases in the count of the *# of years at war and credit access* are only found in Columns 5 and 6 in Table 5 in the main text.

- **Non-Secessionist War:** These are considered only as a control. Civil war's contribution to state building is yet to be established. Porter (1994) argues that civil war was positive for state-building in early modern Europe. Similarly, Balcells and Kalyvas (2014) suggest that irregular warfare might serve to state building. However, others find opposite evidence in Africa (Herbst 2000) and Latin America (Cardenas 2010, Centeno 2002).

6. A note on COW vs Wimmer-Min: To enter the Correlates of War interstate war dataset prior to 1920, territorial units must possess diplomatic relations with both Britain and France. A considerable large number of states that went to war during the nineteenth century—mainly outside Europe—had not yet established sufficient relations with both of these states (Butcher and Griffiths 2015). As a result, they are excluded from the COW inter-state dataset. Wars against or between colonies and other non-internationally recognized states entities enter three auxiliary datasets in COW. But, unlike Wimmer and Min (2009), those wars are not mapped onto current state boundaries, preventing a clear match between past warfare and current nation-states.

Lastly, Table A-6 reports the summary statistics and sources of all variables.

Table A-5: **List of Secessionist Wars, 1816-1913.** This list draws from Wimmer and Min 2009 but also attributes participation to the state seeking independence, not just the colonial power. To this list, I apply *country splits* (explained above, followed by a *) and *region-to-state matches* (explained above, followed by a †).

Years	**War Name**	**Participants**
1816-1817	Portuguese vs. Latin American patriots	Uruguay, Portugal
1817-1818	Spanish vs. Mexican nationalists	Mexico, Spain
1817-1818	Chilean war of independence	Chile, Spain
1818-1823	Bolivar vs. Royalists	Colombia, Ecuador, Venezuela, Spain
1821-1828	Ottoman Empire vs. Greeks	Greece, Turkey
1824-1824	Bolivia's war of independence	Bolivia, Spain
1824-1824	Spain vs. Latin American patriots	Peru, Spain
1825-1828	Argentinian-Brazilian	Uruguay, Brazil, Argentina (United Provinces of Rio de la Plata)†
1825-1830	Dutch-Javanese	Indonesia, Netherlands
1830-1831	Netherlands vs. Belgians	Belgium, France, Netherlands, United Kingdom
1831-1831	Russia vs. Poles of 1831	Poland, Russia
1835-1836	Mexico vs. Texans	Mexico, United States of America
1844-1844	Dominican war of independence	Dominican Republic, Haiti
1846-1846	Cracow Revolt	Poland, Austria-Hungary*
1848-1849	Austro-Sardinian	Italy, Austria-Hungary*, Modena†, Tuscany†
1848-1849	Austria-Hungary vs. Magyars	Romania, Austria-Hungary*, Russia
1852-1853	Ottoman Empire vs. Montenegrins of 1852	Yugoslavia, Turkey
1858-1859	Ottoman Empire vs. Montenegrins of 1858	Yugoslavia, Turkey
1859-1859	Italian Unification	Italy, Austria-Hungary*, France
1862-1862	Turkey vs. Montenegro	Yugoslavia, Turkey
1863-1864	Russia vs. Poles of 1863	Poland, Russia
1863-1865	Spanish-Santo Dominican	Dominican Republic, Spain
1866-1867	Ottoman Empire vs. Cretans of 1866	Greece, Turkey
1868-1878	Spanish-Cuban of 1868	Cuba, Spain
1875-1877	Ottoman Empire vs. Christian Bosnians	Yugoslavia, Turkey
1880-1881	Boer War of 1880	South Africa, United Kingdom
1888-1889	Ottoman Empire vs. Cretans of 1888	Greece, Turkey
1895-1895	Japano-Taiwanese	Taiwan, Japan
1895-1898	Spanish-Cuban of 1895	Cuba, Spain
1896-1897	Ottoman Empire vs. Cretans of 1896	Greece, Turkey
1896-1898	Spanish-Philippino of 1896	Philippines, Spain
1899-1902	American-Philippino	Philippines, United States of America
1903-1903	Ottoman Empire vs. VMRO Rebels	Macedonia, Turkey

Table A-6: **Summary Statistics and Data Sources**

Variable	Mean	Std. Dev.	Min.	Max.	N	Source
Personal Income Tax as % of GDP 1995-2005	2.999	3.258	0	15.058	107	Various Sources, see above
Valued Added Tax as % of GDP 1995-2005	4.959	2.898	0	12.05	106	US AID (2012)
Tax Staff per 1000 capita 2004-10	0.702	0.557	0.03	2.398	80	IMF GFS and US AID (2012)
Modern Census by 1820	0.056	0.231	0	1	107	coded by author from Goyer and Draaijer (1992a,b,c)
Modern Census by 1914	0.533	0.501	0	1	107	coded by author from Goyer and Draaijer (1992a,b,c)
First Modern Census Date	1901.374	50.771	1751	1984	107	coded by author from Goyer and Draaijer (1992a,b,c)
Primary Education Enrollment	41.99	34.764	0.09	100	77	Lee and Lee (2016)
ln(Rail Lines)	7.804	2.125	0	12.908	63	Comin and Hobijn (2010)
Non-Trade Tax 1945-1955	84.261	11.949	53.729	99.571	34	Cagé and Gadenne (2016)
Non-Trade Tax 1955-1965	84.041	10.051	60.659	98.92	37	Cagé and Gadenne (2016)
Non-Trade Tax 1965-1975	80.252	15.68	29.341	98.665	56	Cagé and Gadenne (2016)
Non-Trade Tax 1975-1985	79.689	17.292	21.668	99.39	72	Cagé and Gadenne (2016)
Non-Trade Tax 1985-1995	83.131	13.982	37.124	99.687	85	Cagé and Gadenne (2016)
# Years at War 1816-1913	4.346	9.851	0	60	107	Wimmer and Min (2009)
# Years at War while Credit Flows in 1816-1913	2.075	4.718	0	27	107	Wimmer and Min (2009) and Reinhart and Rogoff (2009)
# Years at War while Credit Stops in 1816-1913	2.271	5.485	0	36	107	Wimmer and Min (2009) and Reinhart and Rogoff (2009)
# Years at War while Credit Flows in 1816-1913 [COW sample]	0.729	1.588	0	9	107	Sarkees and Wayman (2010) and Reinhart and Rogoff (2009)
# Years at War while Credit Stops in 1816-1913 [COW sample]	0.879	2.022	0	8	107	Sarkees and Wayman (2010) and Reinhart and Rogoff (2009)
# Years at War while Credit Stops in 1816-1913 (Ongoing Criterium)	0.673	2.298	0	12	107	Wimmer and Min (2009) and Reinhart and Rogoff (2009)
# Years at War with Access to Credit in 1816-1913 [Reinhart-Rogoff sample]	5.524	11.837	0	60	63	Wimmer and Min (2009) and Reinhart and Rogoff (2009)
# Years at War while in Default in 1816-1913 [Reinhart-Rogoff sample]	0.810	2.177	0	11	63	Wimmer and Min (2009) and Reinhart and Rogoff (2009)
Oil Producer	0.692	0.464	0	1	107	calculated from Wimmer and Min (2009)
# Years at Civil War 1816-1913	1.607	3.838	0	18	107	calculated from Wimmer and Min (2009)
Population Density in 1820	0.205	0.289	0	1.635	107	World Mapper www.worldmapper.org
Great Power	0.065	0.248	0	1	107	Flandreau and Flores (2012)
War Casualties 1816-1913	0.111	0.275	0	1.512	88	Dincecco and Prado (2012)
War Location 1816-1913	1.953	9.450	-13	61	107	calculated from Wimmer and Min (2009)
Ethnic Fractionalization	0.37	0.273	0.004	0.9	106	Wimmer and Min (2009)
Sea Access	36.57	35.594	0	100	107	Wimmer and Min (2009)
Desert	1.862	5.016	0	26.132	107	Nunn and Puga (2012)
Rugged Terrain	1.528	1.313	0.037	6.74	107	Nunn and Puga (2012)
Land Area	100.142	239.182	0.924	1638.134	107	Nunn and Puga (2012)
State Antiquity	445.054	210.295	25	860.975	104	Bockstette et al. (2002)
Size of Financial Admin per 100 capita in 1980	0.1	0.097	0.01	0.4	23	Tait and Heller (1983)
Wage Premium of Financial Admin in 1980	117.473	32.972	68.099	206.2	1	Tait and Heller (1983)
Executive Constraints 1800-1830	1.959	1.699	1	7	30	Marshall and Jaggers (2000)
Executive Constraints 1900-1913	4.073	2.36	1	7	50	Marshall and Jaggers (2000)
Executive Constraints 1995-2005	5.396	1.769	1.091	7	104	Marshall and Jaggers (2000)
Region	2.636	1.152	1	6	107	coded by author
British Colony	0.187	0.392	0	1	107	coded by author
Iberian Colony	0.187	0.392	0	1	107	coded by author
Other Colony	0.327	0.471	0	1	107	coded by author
WWI Participant	0.421	0.496	0	1	107	coded by author

C Cross-Sectional Distribution of Warfare and Access to Credit

1. Table A-7 reports the breakdown of war participation while credits flows and stops (i.e., sudden-stops). This sample is upper bounded by data availability of the outcome variables: PIT, VAT and Tax Administration Size.

2. Figure A-2 plots the *location* of warfare. For exposition purposes only, all inter-state and secessionist warfare for all countries—immediately and tentatively matched (see Table A-4)—are considered. Darker areas indicate higher frequency of war in territory x.

3. Figure A-3 plots the distribution of *war participants* regardless of war location. For exposition purposes only, all inter-state warfare and secessionist warfare for all countries—immediately and tentatively matched (see Table A-4)—are considered. Darker areas indicate higher rates of participation.

   Notice that Figures A-2 and A-3 show that most wars were fought outside Europe but involved at least one European power.

Table A-7: **Exogenous access to Credit and War Participation:** This table lists the # Years at War while Credit Flows between 1816 and 1913 **(W&F)**, and # Years at War while Credit Stops between 1816 and 1913 **(W&S)**. N = 107

	W&F	**W&S**		**W&F**	**W&S**		**W&F**	**W&S**
Albania	0	0	Germany	3	5	Norway	0	0
Argentina	3	13	Greece	1	2	Pakistan	0	0
Armenia	0	0	Guatemala	2	1	Panama	0	0
Australia	0	0	Guinea	0	0	Paraguay	1	6
Austria	1	2	Honduras	2	0	Peru	6	7
Azerbaijan	0	0	Hungary	1	2	Philippines	0	0
Bangladesh	0	0	Iceland	0	0	Poland	0	0
Belarus	0	0	India	0	0	Portugal	0	0
Belgium	0	1	Indonesia	0	0	Romania	0	1
Bhutan	0	1	Iran	4	5	Russia	27	14
Bolivia	6	5	Ireland	0	0	Rwanda	0	0
Brazil	3	12	Israel	0	0	Senegal	0	2
Bulgaria	1	2	Italy	5	8	Slovakia	0	0
Burundi	0	0	Ivory Coast	0	0	Slovenia	0	0
Cambodia	4	0	Japan	4	1	South Africa	4	0
Canada	0	0	Kazakhstan	0	0	South Korea	0	0
Chad	0	0	Kenya	0	0	Spain	3	7
Chile	5	3	Latvia	0	0	Sri Lanka	2	0
China	13	14	Lebanon	0	0	Swaziland	0	0
Colombia	1	0	Lesotho	0	0	Sweden	0	0
Congo	0	0	Lithuania	0	0	Switzerland	0	0
Costa Rica	0	0	Macedonia	0	0	Tajikistan	0	0
Croatia	0	0	Madagascar	4	1	Thailand	5	5
Cyprus	0	0	Malaysia	0	0	Tunisia	2	0
Czech Republic	0	0	Mali	0	1	Turkey	9	10
Democratic Republic of the Congo	0	0	Mexico	4	5	Ukraine	0	0
Denmark	1	2	Moldova	0	0	United Kingdom	26	32
Dominican Republic	0	0	Mongolia	0	0	United States of America	3	2
Ecuador	1	0	Morocco	1	4	Uruguay	0	1
Egypt	7	2	Myanmar	4	2	Venezuela	0	0
El Salvador	3	1	Namibia	0	0	Vietnam	10	13
Estonia	0	0	Nepal	0	0	Yemen	0	0
Ethiopia	4	4	Netherlands	1	6	Zambia	0	0
Finland	0	0	New Zealand	0	0	Zimbabwe	0	0
France	24	36	Nicaragua	2	1			
Georgia	9	1	Nigeria	0	0			

Figure A-2: **The Geography of Military Conflict in the Long-Nineteenth Century.** Colors indicate the total number of years at war. Source: Wimmer and Min (2009).

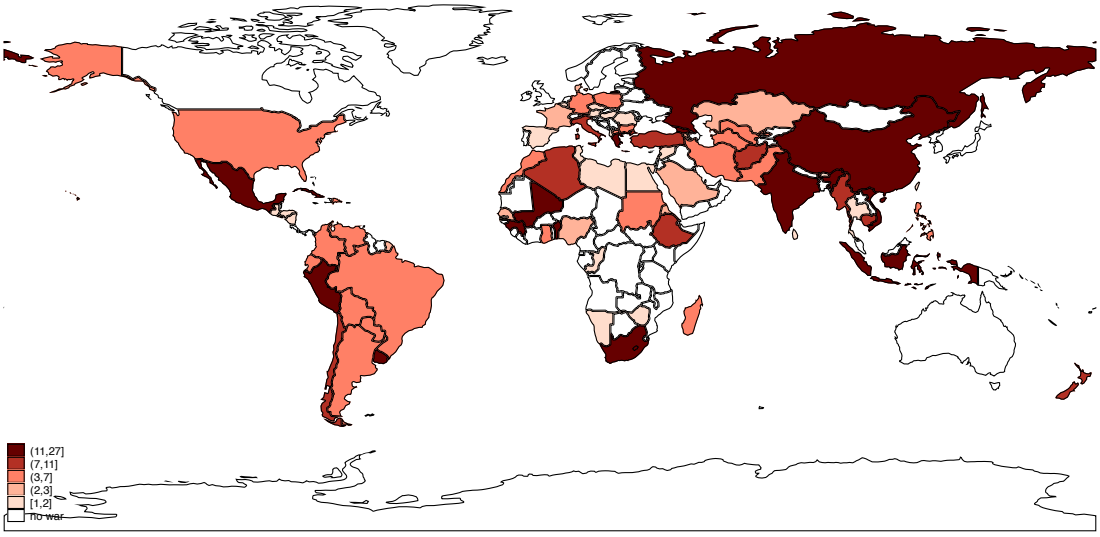
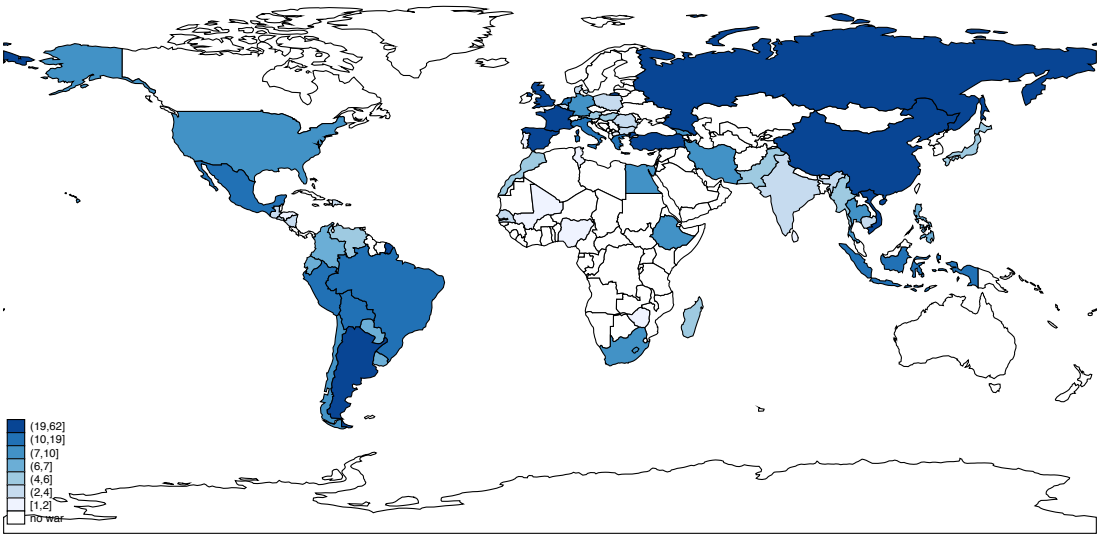


Figure A-3: **Frequency of War Participation in the Long-Nineteenth Century.** Colors indicate the total number of years at war. Source: Wimmer and Min (2009).



D Estimating β_1 and β_2 Separately

The number of years at war having and lacking access to credit are correlated. Table A-8 fits both predictors separately to assess whether results are driven by collinearity issues. In every model, credit access is exogenized based on sudden stops. Results replicate the main article's finding. War makes states when credit dries up and incentives to tax are strong, while it does not when states have access to external lending.

Table A-8: **Estimating β_1 and β_2 separately:** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

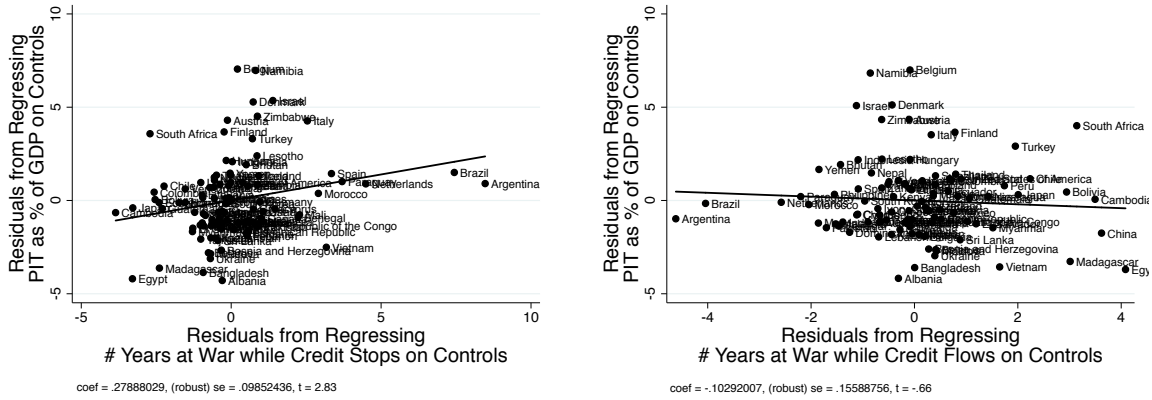
	(1)	(2)	(3)	(4)
# Years at War while Credit Stops in 1816-1913	0.131***	0.087**		
	(0.038)	(0.041)		
# Years at War while Credit Flows in 1816-1913			0.046	-0.038
			(0.072)	(0.077)
Population Density in 1820	1.496	1.220	1.696	1.134
	(1.344)	(1.426)	(1.378)	(1.446)
Oil Producer	0.030	0.013	0.225	0.219
	(0.468)	(0.464)	(0.486)	(0.479)
Sea Access	0.028***	0.028***	0.026***	0.028***
	(0.007)	(0.007)	(0.007)	(0.007)
Desert	0.007	0.007	0.003	0.006
	(0.045)	(0.044)	(0.045)	(0.044)
Great Power		1.955		3.129**
		(1.479)		(1.232)
Constant	1.170	1.102	1.417	1.348
	(0.846)	(0.835)	(0.877)	(0.852)
Colonial Origins FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	106	106	106	106
R-squared	0.566	0.579	0.539	0.570

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

E Influence of Outliers

Figure 2 in the main text shows three potential outliers in the sample: Russia, Georgia and France. The partial-correlation plot between PIT in the 2000s and Years at War in the long ninteenth century as a function of credit access *once the three outliers are dropped* is plotted in Figure A-4.

Figure A-4: **Partial Correlations of Personal Income Tax and Exogenous War-Financing once Outliers are dropped: Russia, Georgia, and France.** Estimates drawn from Column 1 in Table A-9.



(a) War while Credit Stops

(b) War while Credit Flows

Column 1 in Table A-9 reports the same information in regression format. In Column 2, I use a non-visual criterion to identify outliers: namely, Cook's distance. Accordingly, I drop 11 observations with unusually high distances. Column 2 also confirms that war makes states when credit dries and incentives to resort to taxes are strong, while it does not when states have access to external lending. Results are not driven by outliers.

Table A-9: **Dropping Influential Outliers.** PIT as % of GDP Today as a Function of War and Exogenous Access to Credit in the Long Nineteenth Century once Outliers are excluded.

	Russia, Georgia and France Excluded (1)	**Cook's Distance Outliers Excluded** (2)
# Years at War while Credit Stops in 1816-1913	0.279***	0.302***
	(0.099)	(0.079)
# Years at War while Credit Flows in 1816-1913	-0.103	-0.210***
	(0.156)	(0.047)
Population Density in 1820	1.232	1.786**
	(1.305)	(0.713)
Oil Producers	0.011	0.016
	(0.464)	(0.410)
Sea Access	0.028***	0.028***
	(0.007)	(0.007)
Desert Territory	0.010	0.012
	(0.046)	(0.028)
Constant	1.185	1.178**
	(0.853)	(0.570)
Region FE	Yes	Yes
Colonial Origins FE	Yes	Yes
Observations	103	95
R-squared	0.580	0.529

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

F Influence of Fixed Effects

Region- and Colonial Origins fixed effects (6 and 4 categories, respectively) minimize unobserved cross-sectional heterogeneity. However, if covariates are highly correlated within region/colonial origins groups, adding fixed effects might induce high multicollinearity and outliers. Based on the simplest specification of the exogenous access to credit model, I stepwise drop fixed effect batteries. Column 1 in Table A-10 drops Colonial Origins Fixed Effects. Column 2 drops Region Fixed Effects. And Column 3 drops both sets of fixed effects. Results hold across specifications.

Table A-10: **Fixed Effects Influence:** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

	(1)	(2)	(3)
# Years at War while Credit Stops in 1816-1913	0.227***	0.283***	0.157*
	(0.056)	(0.068)	(0.092)
# Years at War while Credit Flows in 1816-1913	-0.181***	-0.265***	-0.185**
	(0.060)	(0.077)	(0.082)
Population Density in 1820	1.335	0.511	1.466
	(1.386)	(1.545)	(1.539)
Oil Producer	0.214	0.851	0.784
	(0.508)	(0.521)	(0.615)
Sea Access	0.031***	0.020**	0.020**
	(0.007)	(0.009)	(0.010)
Desert Territory	0.012	0.018	0.056
	(0.046)	(0.055)	(0.057)
Intercept	2.290***	1.101*	1.310**
	(0.781)	(0.615)	(0.605)
Region FE	Yes	No	No
Colonial Origins FE	No	Yes	No
Observations	106	106	106
R-squared	0.533	0.298	0.118

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

G Sub-Sample Analysis, Attrition Bias, and Federal States

Table A-11 investigates the extent to which results hinge on particular regions, matching decisions, or territorial configuration of the state.

Keeping Developing Nations Only. As it is argued in the Introduction, the bellicist hypothesis receives broad support in Europe. But these countries are wealthier than average, thus are more prone to participate in war. Columns 1 and 2 in Table 5 in the main text show that results are robust to dropping the Great Powers and other economic powers in the nineteenth-century. Next, Column 1 in Table A-11 applies a stricter test by dropping all OECD foundational economies. Results, despite the sample size reduction, hold.

Attrition Bias. Most wars can be easily matched to current states (further details in Appendix Section B). A minority cannot: These are extinct political entities the territory of which overlap with more than one modern state. Table A-4 lists past polities that cannot be matched with current state-borders without making various assumptions. The analyses in the main text do not consider these polities, but Columns 2 and 3 in Table A-11 do in order to minimize any potential attrition bias. Results hold.

Federal Structure. A federal constitutional structure might limit *central* government tax yields while correlate with past warfare if non-unitary states result from a history of ethnic civil wars. Column 4 and 5 in Table A-4 include a control for Federal Structure circa 2000. Data on Federal Structure is drawn from Treisman (2000).

Table A-11: **Sub-Sample Analysis, Attrition Bias, and Federal States:** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

SAMPLE →	**Foundational OECD Excluded** (1)	**Tentative Match Included** (2)	**Tentative Match Included** (3)	**Federal Control** (4)	**Federal Control** (5)
# Years at War while Credit Stops	0.124*	0.259***	0.243***	0.242***	0.226***
	(0.070)	(0.051)	(0.061)	(0.056)	(0.067)
# Years at War while Credit Flows in 1816-1913	-0.055	-0.263***	-0.265***	-0.248***	-0.247***
	(0.111)	(0.059)	(0.059)	(0.066)	(0.066)
Population Density in 1820	-1.165	0.719	0.948	0.705	0.944
	(0.740)	(1.370)	(1.428)	(1.423)	(1.441)
Oil Producer	-0.016	0.126	0.086	0.188	0.139
	(0.403)	(0.442)	(0.460)	(0.458)	(0.477)
Sea Access	0.016**	0.030***	0.027***	0.029***	0.026***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Dessert Territory	-0.025	-0.013	0.019	-0.017	0.016
	(0.033)	(0.032)	(0.046)	(0.032)	(0.046)
State Antiquity	-0.002	0.001		0.001	
	(0.001)	(0.001)		(0.001)	
Census in 1820			1.460		1.454
			(1.363)		(1.390)
Great Power†		2.632**	2.754**	2.804**	2.860**
		(1.141)	(1.188)	(1.217)	(1.271)
Federal Structure				-0.453	-0.277
				(0.786)	(0.806)
Constant	1.437*	0.528	1.274	0.513	1.303
	(0.854)	(0.970)	(0.803)	(0.952)	(0.845)
Region FE	Yes	Yes	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes
Observations	83	103	106	103	106
R-squared	0.702	0.655	0.625	0.649	0.618

Great Britain in Excluded. †In Column 1, Great Power is dropped because all of them were European. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

H Cluster Standard Errors

War in country i might affect the likelihood of war in a neighbor state. To account for such error correlation, Table A-12 fits models with clustered standard errors at the regional level. Because the number of clusters is low, I compute Wild-Bootstrap cluster standard error. I report 95% CI. Results suggest again that war makes states when incentives to tax are strong (i.e., during sudden-stop of credit) but it does not when countries can finance war externally.

Table A-12: **Wild-Bootstrap Cluster Standard Errors:** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

	(1)	(2)	(3)	(4)
# Years at War while Credit Stops in 1816-1913	0.272***	0.250***	0.261***	0.246***
	[0.189,0.354]	[0.184,0.324]	[0.188,0.333]	[0.165,0.326]
# Years at War while Credit Flows in 1816-1913	-0.198***	-0.250***	-0.189***	-0.189***
	[-0.287,-0.108]	[-0.341,-0.159]	[-0.264,-0.112]	[-0.269,-0.108]
Great Power	No	Yes	No	No
State History	No	No	Yes	No
Census by 1820	No	No	No	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes
Observations	106	106	103	106
R-squared	0.587	0.609	0.623	0.592

Great Britain is excluded. Baseline controls are: Population Density as of 1820, Oil Producer, Sea Access, Desert Territory. Intercept not reported. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

I The Nature, Timing, and Length of Sudden-Stops

Stock Market Crash. The 1910 crisis is a stock-market crash, not a banking panic. Based on Figure 1, the stock-market crash might not cause comparable capital dry shocks. Accordingly, Column 1 in Table A-13 treats the 1910 stock-market crisis as a non-crisis, and investigates whether this has any impact on the estimates of interest. It does not.

The 1893 Crisis. Reinhart and Rogoff (2009) do not list the 1873 banking crisis for Great Britain, despite it being a major crisis in the nineteenth century (Kindleberger and Aliber 2005). Technically, the 1873 crisis originated in Austria and Germany. But, it was only a matter of months that the crisis reached London, causing a sudden-stop of credit (Bordo 1986), as Figure 1 reflects. Based on the relevance of this crisis, I include it in the main analysis. For the sake of robustness, Column 2 in Table A-13 excludes the 1873 banking crisis as a cause of sudden-stop. Results hold

Longer Spells [or Placebo Test]. Columns 3 and 4 allow for longer spells of sudden-stops. Specifically, Columns 3 and 4 replace the four-year rule of credit stop based on Catao (2006) for five and six years spells, respectively. The effect of fighting war during these longer periods is still positive. Longer windows can be interpreted as placebo tests. Accordingly, results hold but turn weaker as windows expand. Results hold.

Table A-13: **Nature, Timing and Length of Crises:** Personal Income Tax Today (as % of GDP) as a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

	(1)	(2)	(3)	(4)
	1910 Crisis Dropped	1873 Crisis Dropped	5-year Sudden-Stop Windows	6-year Sudden-Stop Windows
# Years at War while Credit Stops in 1816-1913	0.203***	0.288***	0.176***	0.165***
	(0.069)	(0.068)	(0.047)	(0.045)
# Years at War while Credit Flows in 1816-1913	-0.179*	-0.199***	-0.244***	-0.300***
	(0.101)	(0.064)	(0.079)	(0.086)
Population density in 1820	0.738	0.939	0.680	0.681
	(1.376)	(1.361)	(1.386)	(1.372)
Oil Producer	0.180	0.119	0.169	0.197
	(0.450)	(0.455)	(0.450)	(0.449)
Sea Access	0.031***	0.031***	0.029***	0.029***
	(0.007)	(0.007)	(0.007)	(0.007)
Desert Territory	-0.022	-0.012	-0.015	-0.016
	(0.033)	(0.031)	(0.033)	(0.032)
Great Power	2.574**	2.084*	2.633**	2.535**
	(1.246)	(1.179)	(1.104)	(1.052)
State Antiquity	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Intercept	0.491	0.534	0.477	0.512
	(0.990)	(0.979)	(0.991)	(0.980)
Region FE	Yes	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes
Observations	103	103	103	103
R-squared	0.631	0.651	0.642	0.646

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

J Models Using an Endogenous Measure of Credit Access: Default Episodes

The analysis in this section identifies periods of access to international credit markets based on default episodes, as listed in Reinhart and Rogoff (2009). These authors define sovereign default as the failure of a government to meet a principal or interest payment on the due date (or within the specified grace period). Among the main causes of default, there is war, which reinforces the main insight of the theoretical discussion: financing war with loans does not *guarantee* an improvement in the fiscal capacity of the state with respect to prewar levels.

Reinhart and Rogoff (2009) code periods of external default starting as early as 1800 for 68 countries, as defined by their current territory. Next, I work with 63 out the 68 countries in their sample, all for which full data is available.[151] The sample includes countries of the five continents and accounts for approximately 90% of world GDP by 1913. The median duration of default episodes in the period under consideration is six years (Reinhart and Rogoff 2009:81). Critically, while in default, countries are excluded from the international lending market (Tomz 2007), which I expect to strengthen the ruler's incentives to invest in the tax capacity of the state.

The empirical specification follows the same form as Expression 1. However, instead of using sudden-stops of credit to establish when a given country has no access to international lending, here I use default episodes, an intuitive but endogenous variable. To establish a benchmark, Column 1 in Table A-14 tests for the unconditional version of the bellicist hypothesis for the 63 states sampled in (Reinhart and Rogoff 2009). Results are mixed (consistent with what many have found): the coefficient for *# of Years at War between 1816-1913* in Column 1 is positive but not significant.

Column 1 should be compared to Column 2 and remaining specifications, in which I

[151] The five countries excluded due to tax-data limitations are: Algeria, Angola, Central African Republic, Ghana and Taiwan.

Table A-14: **Using Default Episodes to Identify Lack of International Finance:** Personal Income Tax Today (as % of GDP) as a Function of War and Endogenous Credit Access in the Long-Nineteenth Century.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# Years at War 1816-1913	0.037								
	(0.024)								
# Years at War while in Default		0.150**	0.167**	0.168**	0.175**	0.137*	0.159*	0.144*	0.171**
		(0.071)	(0.074)	(0.076)	(0.072)	(0.072)	(0.081)	(0.075)	(0.077)
# Years at War with Access to Credit		0.034	0.032	0.028	0.021	0.020	0.031	0.018	0.027
		(0.025)	(0.026)	(0.027)	(0.071)	(0.043)	(0.026)	(0.028)	(0.025)
Population Density in 1820	3.389**	3.493**	3.420**	3.378*	3.370**	3.172*	3.473**	3.351*	2.983
	(1.578)	(1.597)	(1.656)	(1.723)	(1.616)	(1.839)	(1.707)	(1.668)	(1.831)
Oil Producer	-0.822	-0.945	-0.919	-0.928	-0.916	-0.632	-0.944	-1.060	-0.874
	(0.640)	(0.659)	(0.673)	(0.687)	(0.676)	(0.913)	(0.749)	(0.687)	(0.667)
Sea Access	0.021**	0.021**	0.022**	0.022**	0.021**	0.022*	0.022**	0.024***	0.024***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.011)	(0.008)	(0.009)	(0.008)
Desert	-0.051	-0.057	-0.060	-0.059	-0.059	-0.078	-0.053	-0.059	-0.050
	(0.062)	(0.060)	(0.060)	(0.060)	(0.063)	(0.085)	(0.074)	(0.061)	(0.055)
# Years in Default			-0.009	-0.008	-0.009	-0.008	-0.008	-0.017	-0.011
			(0.013)	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)	(0.014)
Great power				0.317					
				(1.549)					
War Location					0.015				
					(0.070)				
War Casualties 1816-1913						0.906			
						(1.868)			
Ethnic fractionalization							0.325		
							(1.546)		
# years at Civil War 1816-1913								0.090*	
								(0.053)	
WWI Participant									0.752
									(0.906)
Intercept	3.269**	3.390**	3.496**	3.458**	3.566**	3.633**	3.338*	3.509**	2.813*
	(1.361)	(1.359)	(1.418)	(1.448)	(1.491)	(1.696)	(1.822)	(1.411)	(1.604)
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63	63	63	63	63	54	62	63	63
R-squared	0.756	0.759	0.760	0.761	0.761	0.723	0.759	0.768	0.764

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

distinguish the effect of war fought while in default, β_1, from war fought while having access to international credit markets, β_2. Both point estimates are positive, but, consistent with the political economy of war finance, only the former is significantly different from zero. A one-standard deviation increase in the number of years at war while in default increases income tax to GDP in 0.41 points. This is a 15% increase with respect to the PIT's sample mean.

On the contrary, Column 2 suggests that wars that are fought when countries have access to international markets do not exert any persistent effect on fiscal capacity. This is consistent with the commitment problem above indicated. Nothing guarantees that once war is over, countries service debt within the pre-established timeframe and conditions. Some countries honor their debt (by enhancing its fiscal capacity as to amass the required funds), others do not.

Column 3 controls for the baseline propensity to default. To this end, I include the *# Years in Default between 1816 and 1913* of each observation. The two coefficients of interest remain virtually identical. The remaining of Table A-14 considers potential confounders, while making sure not to control for endogenous covariates (e.g., Current per Capita GDP or Democracy levels).[152] Models include: Being a Great Power, War Location, War Casualties, Ethnic Fractionalization, Contemporaneous Civil War, and WWI participation. Across specifications, $\hat{\beta}_1$ and $\hat{\beta}_2$ remain the same as in Columns 2 and 3.

[152]For reference, Appendix Table A-22 reports models including endogenous controls. Results hold.

K Alternative War-Financing Policy

There are (at least) three other ways to finance war: domestic loans, expanding money supply, and financial repression. I address them stepwise:

K.1 Domestic Borrowing

Domestic borrowing requires a developed financial market, something that, *in the period under consideration*, was only guaranteed in a few European countries (Reinhart and Rogoff 2009: ch.7). The pool of domestic investors in the periphery tended to be small, and loans to government represented a large share of their portfolio. This implied expensive credit relative to other options overseas (Della Paolera and Taylor 2013, Flandreau and Flores 2012, Kuran and Rubin 2017). Not surprisingly, countries in the periphery resorted to international markets for financing.

Columns 1-3 in Table A-15 address the possibility of fighting wars while having access to either *domestic* or external credit, or none.. The first row shows the coefficient of *having no access to the domestic or international markets* (i.e., domestic and external default), while the fourth row shows the effect of *having access to either to the domestic or international markets*. In the former case, I expect the incentives to invest in fiscal institutions to be maximum. Consistent with this expectation, the magnitude of the coefficients grows with respect to those reported in Table A-14 (external default only). Column 2 adds a *Great Power* indicator to control for differences in domestic credit markets, and Column 3 controls for the *War Location*, as it could influence the capacity to mobilize resources domestically. The point estimates of the two coefficients of interest, $\hat{\beta}_1$ and $\hat{\beta}_2$, remain fairly stable.

K.2 Expanding Money Supply

A second means to financing war is expanding the money supply (also known as *printing money*). Except as an extreme measure of last resort, printing money occupied a "subor-

Table A-15: **PIT as % of GDP Today as a Function of War and Endogenous Credit Access in the Long Nineteenth Century, with Special Attention to Domestic Default Episodes and Money Printing**

	Accounting for Domestic Default			**Accounting for Money Printing**		
	(1)	(2)	(3)	(4)	(5)	(6)
# Years at war while in external and domestic default	0.171**	0.172**	0.180**			
	(0.073)	(0.075)	(0.069)			
# Years at war while in external default but no money printing				0.171*	0.172*	0.185**
				(0.092)	(0.094)	(0.089)
# Years at war while in external default and money printing				0.154***	0.157***	0.152***
				(0.055)	(0.056)	(0.056)
# Years at war with access to credit[a]	0.032	0.028	0.020	0.032	0.028	0.020
	(0.026)	(0.027)	(0.071)	(0.026)	(0.028)	(0.073)
# Years in default[b]	-0.009	-0.008	-0.010	-0.009	-0.008	-0.010
	(0.013)	(0.014)	(0.013)	(0.013)	(0.014)	(0.013)
Population Density in 1820	3.421**	3.377*	3.366**	3.422**	3.380*	3.368**
	(1.655)	(1.723)	(1.613)	(1.673)	(1.741)	(1.634)
Oil Producer	-0.917	-0.926	-0.912	-0.919	-0.927	-0.915
	(0.671)	(0.685)	(0.673)	(0.680)	(0.695)	(0.683)
Sea Access	0.022**	0.022**	0.021**	0.022**	0.022**	0.021**
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.010)
Desert Territory	-0.060	-0.059	-0.059	-0.061	-0.059	-0.059
	(0.060)	(0.060)	(0.063)	(0.060)	(0.061)	(0.063)
Great Power		0.332			0.314	
		(1.551)			(1.567)	
War Location 1816-1913			0.016			0.016
			(0.069)			(0.072)
Constant	3.464**	3.423**	3.534**	3.497**	3.459**	3.575**
	(1.403)	(1.434)	(1.466)	(1.433)	(1.464)	(1.511)
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63	63	63	63	63	63
R-squared	0.761	0.761	0.761	0.760	0.761	0.761

Great Britain is excluded. [a] In Columns 1-3, access to credit refers to either domestic or international markets, or both. [b] Years in default refer to external default. Robust Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

dinate position" in pre-1913 war finance (Sprague 1917). The reason is that expanding the money supply has inflationary consequences. A sudden expansion of the money supply gives the government a temporary relief with which to pay bills and purchase additional weapons, but this gain is rapidly dissipated by the costs of inflation (Rockoff 1998, Schumpeter 1938). Nevertheless, it is worth checking what the effect of printing money is on long-term fiscal capacity.

In the absence of direct data of instances of money printing, I rely on episodes of inflationary crises, as coded by Reinhart and Rogoff (2009). Specifically, this test assumes that inflationary crises are related to episodes of money supply expansions. Inflation does not dissipate soon. To account for these lags, I add four year leads to the onset of an inflationary crisis. Based on that, I estimate the effect of being at war and in external default in the presence and absence of an inflationary crises. I expect inflationary crises (i.e., the proxy of money printing) to weaken the incentives to invest in fiscal capacity while being at war and excluded from international financial markets.

The results in Columns 4-6 in Table A-15 reinforce and qualify previous findings. First, they confirm that waging war while being in default is related to higher fiscal capacity in the long-run *regardless of money printing*: both coefficients are positive. However, based on the coefficients' magnitude, if inflation is kept under control (i.e., the ruler does not print money), fiscal capacity might be even higher in the long-run. This result implies that incumbents that are not tempted to print money while being at war and in default are those investing more decisively in the fiscal capacity of the state, holding everything else constant.

K.3 Fiscal Repression

A third way to finance war is financial repression. Calomiris and Haber (2014), Menaldo (2016) and Reinhart (2012) show that, if anything, financial repression is a *substitute* of fiscal capacity building. I lack systematic data about instances of financial repression, and cannot test this proposition here. However, financial repression (or office selling or confiscation)

introduces a downward bias, if any, on the main coefficient of interest, β_1. That is, if rulers prioritize fiscal repression when they lack access to external finance, we should not expect a positive coefficient for the *# Years at War while Credit Stops*, precisely because fiscal repression is implemented as to avoid fiscal capacity building.

L Initial Political Conditions

L.1 Direct Measures

Expression 3 in the main paper suggests that the preference for loans over taxes is a function of Δc, the distance between c_t and c_l, the political costs of taxing and borrowing, respectively. That comparative static suggests accounting for initial levels of power-sharing institutions. Few countries can be characterized as democracies by 1820, but they had different levels of executive constraints. To account for these, I employ the *Executive Constraint* component in the Polity IV dataset (Marshall and Jaggers 2000). To slightly broadening the sample while not departing from *initial* conditions in excess, I compute Executive Constraint averages for two periods: 1800-1830 and 1800-1850, as reported in Columns 1 to 2 in Table A-16. To maximize degrees of freedom, I keep a minimum set of economic and geographic controls (refer to fn. 131 in the main text).

For robustness, Column 3 fits average democratic status between 1800 and 1850 as established in Boix et al. (2013). In Column 4 I fit a country-level average of *Traditional Local Democracy* for the 1800-1850 period, as coded by Giuliano and Nunn (2013) based on the Ethnographic Atlas. In Column 5, long-run fiscal capacity is regressed on levels of *democratization*, as measured by Vanhanen (2003). Across specifications, and despite the strong reduction in the sample size, the main coefficient of interest, $\hat{\beta}_1$ is positive and almost always statistically significant, while $\hat{\beta}_2$, is negative and often statistically significant.

Table A-16: **Direct Initial Political Conditions:** PIT as % of GDP Today a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

	(1)	(2)	(3)	(4)	(5)
# Years at War while Credit Stops in 1816-1913	0.172**	0.159**	0.158*	0.135	0.153*
	(0.074)	(0.068)	(0.078)	(0.082)	(0.075)
# Years at War while Credit Flows in 1816-1913	-0.322***	-0.285***	-0.214*	-0.215	-0.223*
	(0.088)	(0.085)	(0.120)	(0.127)	(0.120)
Executive Constraints 1800-1830 [*Polity IV*]	1.057***				
	(0.213)				
Executive Constraints 1800-1850 [*Polity IV*]		0.434			
		(0.407)			
Democracy Status 1800-1850 [*Boix et al. 2013*]			2.694		
			(3.350)		
Local Democracy 1800-1850 [*Giuliano-Nunn 2013*]				0.177	
				(1.397)	
Democratization in 1858 [*Van Hanen 2003*]					0.514*
					(0.258)
Great Power	4.304***	3.563***	1.717	1.371	1.883
	(1.062)	(1.099)	(2.062)	(2.364)	(2.034)
Colonial Past	-1.781*	-1.232	-1.027	-1.433	-1.090
	(0.879)	(1.072)	(1.117)	(1.370)	(1.124)
Population Density in 1820	3.238	4.175	4.473	4.172	4.884
	(2.570)	(3.027)	(3.694)	(4.053)	(3.368)
Oil Producer	0.674	1.858**	1.621**	1.970***	1.764**
	(0.607)	(0.709)	(0.627)	(0.662)	(0.661)
Sea Access	0.041**	0.025*	0.027*	0.024	0.028**
	(0.016)	(0.013)	(0.013)	(0.016)	(0.012)
Intercept	-0.541	-0.787	-0.294	0.105	-0.700
	(0.838)	(0.888)	(0.810)	(1.088)	(0.921)
Observations	29	37	37	36	37
R-squared	0.740	0.617	0.572	0.532	0.614

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

L.2 Indirect Measures

An alternative way to address initial political conditions is to focus on geographic determinants of the central ruler's authority across the territory and *vis-à-vis* regional elites. Well until the nineteenth century, the difficulties of transportation, military technology and demographic realities placed sharp limits on the reach of even the most ambitious states (Scott 2009:4). Central rulers' authority was particularly challenged in mountainous territory, where *rebel communities* were protected by natural barriers to state presence. We could expect the central ruler's capacity to raise taxes to finance the means of war to be undermined by unfavorable local geographic condition. To account for this possibility, Column 1 in Table A-17 controls for *Average Ruggedness*, as coded in Nunn and Puga (2012).

Prior to the transportation revolution, central rulers in big states benefited from weaker monitoring (or political constraints) by regional elites (Stasavage 2011). Large territorial states might have exacerbated commitment problems in debt repayment and fiscal centralization. Columns 2 and 3 in Table A-17 accounts for this possibility by controlling for *Land Area* and *ln(Land Area)*, respectively.

None of the two politically relevant geographic covariates turn to be statistically significant. Importantly, the point estimates for β_1 and β_2 remain unchanged after their consideration.

Table A-17: **Indirect Initial Political Conditions:** PIT as % of GDP Today a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

	(1)	(2)	(3)
# Years at War while Credit Stops in 1816-1913	0.278***	0.263***	0.274***
	(0.057)	(0.062)	(0.058)
# Years at War while Credit Flows in 1816-1913	-0.201***	-0.159**	-0.199***
	(0.057)	(0.078)	(0.058)
Population Density in 1820	1.278	1.217	1.230
	(1.316)	(1.324)	(1.357)
Oil Producer	0.164	0.167	0.137
	(0.483)	(0.476)	(0.598)
Sea Access	0.028***	0.026***	0.028***
	(0.007)	(0.008)	(0.008)
Dessert Territory	0.018	0.017	0.014
	(0.045)	(0.045)	(0.045)
Rugged Terrain	0.113		
	(0.173)		
Land Area		-0.001	
		(0.001)	
ln(Land Area)			-0.008
			(0.216)
Constant	1.045	1.347	1.356
	(0.930)	(0.832)	(0.901)
Observations	106	106	106
R-squared	0.589	0.590	0.587

Great Britain is excluded. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

M VAT as Outcome Variable

Value-Added Tax (VAT) is arguably easier to implement than the income tax (Bird and Gendron 2007), and it may not capture cumulated investment in fiscal capacity as precisely as income tax ratios do. Still, Table A-18 fits models of current VAT (as % of GDP) as a function of war and credit access in the long ninteenth century. VAT data is drawn from IMF Government Financial Statistics. The sample size is limited by data availability. Column 1 regresses average VAT revenue between 1995 and 2005 on the benchmark regressors. We can augment VAT data by replacing missing values for those reported in *USAID Fiscal Reform and Economic Governance Project, 2004-10*, as I did with PIT data.[153]

Results with augmented VAT are reported in Column 2 in Table A-18.[154] Columns 3 and 4 add two controls for initial state capacity, one at a time. Results hold, although β_1 in column 3 is statistically significant only at 88% confidence: War fought while having no access to external finance—when incentives to enhance taxes are expected to be strong—is associated with long-term fiscal capacity. War waged while having access to external finance is not.

[153]Recall, PIT data augmentation does not change results. Refer to Table A-1.
[154]Descriptive statistics for augmented VAT variable can be found in Table A-6.

Table A-18: **Value-Added Tax.** VAT as % of GDP Today as a Function of Years at War and Exogenous Access to External Credit in the Long Nineteenth Century

	(1)	(2)	(3)	(4)
# Years at War while Credit Stops in 1816-1913	0.229*	0.097	0.102	0.097*
	(0.124)	(0.059)	(0.065)	(0.057)
# Years at War while Credit Flows in 1816-1913	0.065	0.047	0.046	0.037
	(0.104)	(0.079)	(0.080)	(0.077)
Population Density in 1820	0.326	-0.260	-0.285	-0.371
	(1.098)	(0.784)	(0.767)	(0.839)
Oil Producer	-1.165	-1.018	-1.013	-1.188
	(0.761)	(0.684)	(0.685)	(0.733)
Sea Access	0.005	0.008	0.009	0.008
	(0.013)	(0.008)	(0.008)	(0.008)
Dessert Territory	0.097*	0.029	0.029	0.022
	(0.051)	(0.054)	(0.055)	(0.058)
Great Power	-3.416**	-0.420	-0.432	-0.309
	(1.355)	(1.375)	(1.386)	(1.364)
Modern Census by 1820			-0.259	
			(1.578)	
State Antiquity				0.000
				(0.002)
Intercept	1.285	2.207**	2.207**	2.202**
	(1.182)	(0.861)	(0.864)	(0.958)
Augmented Dependent Variable	No	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes
Observations	65	105	105	102
R-squared	0.439	0.388	0.388	0.381

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

N Military Alliances, British Colonies, and British Wars

This section examines the effect of (1) military alliances in the international system, (2) the effect of being a British colony, and (3) British active participation in war. Do results change when we account for these potential confounders?

N.1 Military Alliances

Military alliances might change the incentives to wage war and facilitate access to external credit. To account for this source of endogeneity, I control for *Military Alliances* that countries may have with any of the four credit capitals in the long ninteenth century: the British, the French, the German, and the USA. Despite having uneven weight in global finances (refer to Table 1 in the main text), any of these four economies had both the capacity to finance third countries and coordinate military interventions with them.

To code military alliances, I rely on Gibler (2009). This dataset offers dyads of military alliances between independent countries since 1816. Some of these alliances were short-lived while others were enduring. To account for this heterogeneity, I compute the share of years between 1816-1913 in which a given country had any form of military alliance (defense, neutrality, non-aggression, and entente) with each of the four credit capitals separately. For instance, Portugal had a military alliance with Britain for the whole period. Accordingly, for Portugal, *Alliance with Britain* holds the maximum value: 100%. Other countries (e.g., Belgium) stroke no military alliance with Britain during the long ninteenth century. Accordingly, the value for Belgium for this variable is zero. Results are reported in Columns 1 and 2 of Table A-19. Results hold.

N.2 Excluding British Colonies

It is argued that British colonies had access to external credit in more favorable conditions than other colonies (Accominotti et al. 2011). Since Britain was the credit capital and the

military superpower of the long ninteenth century, the decision to go to war of British colonies may be different from other countries'. The British colonial origins fixed effect might not address this source of heterogeneity well enough. To address this issue, Columns 3 and 4 in Table A-19 re-run Expression 4 excluding all British colonies. Results hold.

N.3 Excluding Wars Fought by Britain

Having already addressed strategic considerations with respect to British colonies, we might wonder whether wars in which Britain was directly involved are comparable to other wars. Kirshner (2007:206) argues that countries would be penalized in the international markets if they fought against the financial center. Wars involving the British army might thus be different from others in ways that affect capital access and fiscal capacity. To address this issue, Columns 5 and 6 in Table A-19 report models excluding all wars in which the British explicitly participated. To that end, *# years at war without external finance* and *# years at war with external finance* are recalculated for remaining countries. Results hold across specifications.

Table A-19: **Military Alliances, British Colonies, and Britain's Wars.** PIT as % of GDP Today as a Function of War and Exogenous Access to Credit in the Long Nineteenth Century. Revised April 24, 2019

Sample →	**All Countries Included**		**British Colonies Excluded**		**British Wars Excluded**	
	(1)	(2)	(3)	(4)	(5)	(6)
# Years at War while Credit Stops in 1816-1913	0.285***	0.298***	0.186***	0.197***	0.320***	0.339***
	(0.068)	(0.061)	(0.056)	(0.037)	(0.081)	(0.072)
# Years at War while Credit Flows in 1816-1913	-0.310***	-0.290***	-0.226***	-0.238***	-0.316***	-0.313***
	(0.089)	(0.087)	(0.048)	(0.042)	(0.056)	(0.053)
Population Density in 1820	0.926	0.648	2.696	1.928	0.888	0.699
	(1.484)	(1.415)	(1.767)	(1.619)	(1.420)	(1.374)
Oil Producer	-0.048	0.025	0.185	-0.009	0.042	0.118
	(0.464)	(0.454)	(0.466)	(0.419)	(0.456)	(0.433)
Sea Access	0.024***	0.029***	0.025***	0.029***	0.027***	0.030***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
Desert Territory	0.023	-0.010	0.086*	0.042	0.018	-0.013
	(0.049)	(0.034)	(0.050)	(0.027)	(0.046)	(0.032)
Alliance with Britain	-0.012	-0.000				
	(0.017)	(0.008)				
Alliance with France	0.189**	0.138**				
	(0.076)	(0.068)				
Alliance with Germany	-0.011	-0.007				
	(0.021)	(0.021)				
Alliance with USA	0.956	0.811				
	(0.644)	(0.625)				
Great Power	0.601	0.806	2.454**	2.525**	2.747**	2.665**
	(1.285)	(1.155)	(1.226)	(1.115)	(1.156)	(1.115)
Modern Census by 1820	2.457		1.729		1.476	
	(1.574)		(1.499)		(1.321)	
State Antiquity		0.001		0.004***		0.001
		(0.001)		(0.001)		(0.001)
Intercept	1.162	0.468	0.403	-1.624*	1.243	0.644
	(0.788)	(0.975)	(0.770)	(0.835)	(0.818)	(0.986)
Colonial Origins FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	106	103	86	83	106	103
R-squared	0.648	0.668	0.568	0.656	0.629	0.658

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

O Ongoing War and Periphery Countries

This Appendix is a follow-up of Columns 3 to 5 in Table 6. Specifically, Table A-20 considers *ongoing wars* only (i.e., wars that are initiated while the market is still lending and eventually dries up as a result of a financial crisis) while putting the spotlight on peripheral countries.

These models drop Great Powers, the USA, Canada, and the Netherlands. Results suggest that after addressing (1) selection issues in war participation (i.e., *ongoing wars*) and (2) endogeneity in war finance (i.e., *sudden-stops)*, war makes states with certainty *in peripheral countries* as long as war is not financed with external loans. This coincides with periods in which incentives to tax are strongest.

Table A-20: **Ongoing Wars in the Periphery.** Models of Personal Income Tax Today (as % of GDP) for Wars that are initiated right before the Exogenous Shock of Credit. Sample limited to Peripheral Countries.

	(1)	(2)	(3)
# Years at War while Credit Stops in 1816-1913	0.116**	0.108**	0.117**
	(0.056)	(0.054)	(0.058)
# Years at War while Credit Flows in 1816-1913	0.048	0.057	0.056
	(0.109)	(0.108)	(0.120)
Population Density in 1820	0.742	0.949	0.723
	(1.563)	(1.621)	(1.539)
Oil Producer	0.026	-0.077	0.102
	(0.455)	(0.449)	(0.435)
Sea Access	0.026***	0.023***	0.027***
	(0.008)	(0.007)	(0.008)
Dessert Territory	0.003	0.005	-0.027
	(0.045)	(0.046)	(0.033)
Census in 1820		2.316	
		(1.900)	
State Antiquity			0.001
			(0.001)
Constant	1.051	1.010	0.444
	(0.830)	(0.830)	(1.032)
Region FE	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes
Observations	96	96	93
R-squared	0.538	0.553	0.580

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

P A Reduced-Form Model of War-Making

This section addresses the endogeneity of war in a reduced-form framework. In analyzing the effect of war in Europe, Gennaioli and Voth (2015) instrument war frequency of country i based on war participation by adjacent countries against third countries. The logic behind this instrument is that contextual circumstances that lead neighboring countries to war might increase the likelihood of country i going to war against a third country. The exclusion restriction is that there is no effect of war in neighboring countries on fiscal capacity that is not the result of the risk of war (*ibid.*).

War in neighboring countries is not a strong instrument outside Western Europe; the presence of oceans separating units weakens the relationship between the instrument and the endogenous variable. The exclusion restriction is also harder to hold in the presence of wars of conquest of entire regions by colonial powers. For both reasons, I take a different path here. I interpret war in a neighboring country as a threat that potentially yields an impact on fiscal development.[155] To that end, I replace *inter-state war fought by country i while credit stops (flows)* for *inter-state wars fought by immediately adjacent neighbors while credit stops (flows)*. Notice that I can implement this test only because sudden-stops are common to every country. Importantly, wars of i against adjacent countries are excluded to maximize exogeneity. Expression 4 becomes:

$$\begin{aligned} PIT_{i,1995-2005} = \quad & \alpha + \beta_1(\text{\#years at war by i's-adjacent neighbors between 1816-1913} \mid \text{external lending stops}) \\ & + \beta_2(\text{\#years at war by i's-adjacent neighbors between 1816-1913} \mid \text{external lending flows}) \\ & + \boldsymbol{X}_i\delta + \gamma + \rho + \epsilon_i \end{aligned}$$

where controls and fixed effect batteries remain the same. I exclude all islands from the analysis (Australia, Iceland, Madagascar, Philippines, and New Zealand) because they do not have clear adjacent neighbors. I report the estimates in Table A-21 and plot the coefficients of interest, $\hat{\beta}_1$ and $\hat{\beta}_2$, in Figure A-5. These hold the expected sign: that is, the reduced-form version of waging war while having access to external credit is not associated with long-term

[155] I am grateful to an anonymous reviewer for this suggestion.

fiscal capacity, whereas the reduced-form version of waging war while having no access to external loans is. The main difference with Table 6 in the main text is the size of the effects: Here they attenuate because of the imperfect match between war making by country i and that of its adjacent neighbors.

Figure A-5: **Estimates from Reduced Form Model**

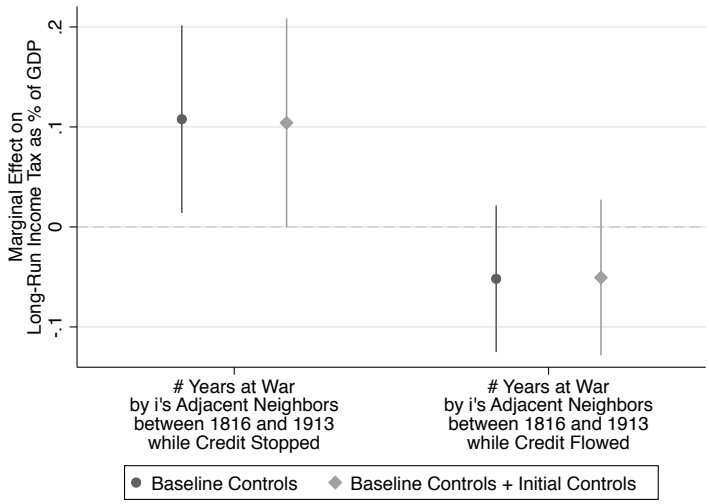


Table A-21: **Reduced-Form Models.** Personal Income Tax as % of GDP Today as a Function of War and Exogenous Access to Credit in the Long Nineteenth Century, with War Participation of Country i replaced by War Participation by Adjacent Countries

	(1)	(2)
years at war by i's-adjacent neighbors between 1816-1913 while external lending stops	0.108*	0.104
	(0.056)	(0.063)
years at war by i's-adjacent neighbors between 1816-1913 while external lending flows	-0.052	-0.051
	(0.044)	(0.047)
Population Density	1.299	1.112
	(1.187)	(1.323)
Oil Producer	0.234	0.160
	(0.524)	(0.513)
Sea Access	0.031***	0.029***
	(0.009)	(0.007)
Desert Territory	-0.020	-0.046
	(0.047)	(0.033)
Great Power		2.059
		(1.380)
Modern Census by 1820		2.410*
		(1.229)
State Antiquity		0.000
		(0.001)
Intercept	1.783**	1.127
	(0.893)	(1.096)
Region FE	Yes	Yes
Colonial Origins FE	Yes	Yes
Observations	100	97
R-squared	0.451	0.549

Great Britain and all islands occupied by one country only are excluded. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Q Including Endogenous Controls

Covariates that result from treatment are known as endogenous controls (or *bad controls*). Their inclusion in empirical models biases the estimates of interest, in this case β_1 and β_2. This problem is also known as post-treatment bias. Here I consider four potential bad controls: democracy, preferences for redistribution, GDP per capita, and trade openness. Bates and Lien (1985) claim that democratic institutions may result from tax-financed war participation. The Transmission Section in the main paper lean support to this argument. Scheve and Stasavage (2010) suggest that preferences for the size of government is endogenous to war participation. Dincecco and Prado (2012) show that long-term GDP is a function of participation in war in the past. Queralt (2015) claims that trade openness follows fiscal capacity building, which results from war participation.

Table A-22 corroborates that the inclusion of bad controls impact the size of the coefficients of interest, specially when the model includes current per Capita GDP. Still, both $\hat{\beta}_1$ and $\hat{\beta}_2$ hold the expected sign and achieve statistical significance within conventional levels.

Table A-22: **Models of PIT as % of GDP Today as a Function of Exogenous Credit Access and War-Making in the Long Nineteenth Century including *Bad Controls*.**

	(1)	(2)	(3)	(4)
# Years at War while Credit Stops in 1816-1913	0.224***	0.235***	0.147***	0.239***
	(0.054)	(0.055)	(0.053)	(0.054)
# Years at War while Credit Flows in 1816-1913	-0.216***	-0.233***	-0.137*	-0.240***
	(0.071)	(0.071)	(0.076)	(0.070)
Democracy 1995-2005	1.327**			
	(0.656)			
Government Size 1995-2005		-3.307		
		(2.442)		
ln(Per Capita GDP) 1995-2005			1.078***	
			(0.204)	
Trade Openness 1995-2005				0.001
				(0.008)
Population Density in 1820	0.261	0.648	0.740	0.715
	(1.441)	(1.409)	(1.076)	(1.432)
Oil Producer	0.170	0.091	-0.331	0.162
	(0.454)	(0.464)	(0.360)	(0.455)
Sea Access	0.026***	0.028***	0.012*	0.030***
	(0.007)	(0.008)	(0.007)	(0.007)
Desert Territory	-0.023	-0.004	-0.033	-0.015
	(0.038)	(0.037)	(0.027)	(0.033)
State Antiquity	0.001	0.001	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Great Power	2.281*	2.571**	1.417	2.669**
	(1.156)	(1.155)	(1.209)	(1.152)
Constant	0.505	1.230	-4.913***	0.458
	(0.997)	(1.203)	(1.385)	(1.317)
Region FE	Yes	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes	Yes
Observations	102	101	103	103
R-squared	0.666	0.652	0.755	0.647

Great Britain is excluded. Sources of bad controls: Democracy: Boix et al. (2013); Per Capita GDP and Trade Openness: World Bank Indicators; Government Size: Feenstra et al. (2013). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

R Short-Term Effects: A Duration Model

In Section 6, *Short-Term Effects*, I assess short-run effects using cross-sectional data. Here I model the adoption of a modern census exploiting duration dependence. Census technology is a requirement to develop a modern tax system capable of assessing wealth and monitoring compliance of an atomized tax base. Hence, it is worth investigating further whether war while lacking external finance stimulates modern census adoption. This time, however, I exploit longitudinal variation of this variable and fit a duration model.

The structure of the data for this test is Binary Time-Series Cross-Sectional (BTSCS). Beck, Katz, and Tucker (1998) show the equivalence between BTSCS and survival models. Hence, the adoption of a modern census can be fitted with a standard probit model. I follow Carter and Signorino (2010) and use a cubic polynomial approximation to the hazard. Four observations are left-censored: the USA, the UK, Norway and Sweden, which adopted a modern census before 1816. I exclude these cases from the analysis. The hazard rate is computed with respect to the adoption of the first modern census by Sweden, 1751. That is, since the implementation of the first modern census, all countries are at risk of adopting that head counting technology. See Aidt and Jensen (2009) for a similar design.[156]

Some countries had not adopted a modern census by 1913. Right-censoring, however, is not problematic. Since adoption of census technology happens only once, time-invariant country-specific characteristics cannot be fitted. Hence, I only include time-varying economic controls (per Capita GDP and Oil Production) plus region and colonial origins fixed effects. In Column 2 I replace the colonial origins fixed effects for time-varying country-specific colonial status between 1816 and 1913. The sample is limited to 50 panels because of data availability of key controls: ln(per cap GDP) and Oil Production.

Results are reported in Table A-23. Fighting war having access to international finance has a large negative effect, as shown in the first row. That is, having access to external finance

[156]If I use country-specific hazards, results are more favorable to the working hypothesis. However, the initial year of country-specific hazards is driven by data availability, not a substantive reason. That is why I use a common starting point.

discourages the adoption of a modern census, a requirement for high tax capacity. The effect of waging inter-state war without access to international finance is positive, approximately 0.5 points, and statistically significant at 90%. This result confirms the opposite incentives to adopt a modern census during war—thus the capacity to strengthen tax capacity—depending on external finance access.

Table A-23: **Duration Model: Adoption of Modern Census as a Function of War and Exogenous Access to Capital from 1816 to 1913**

	(1)	(2)
Inter-State War	-3.563***	-3.663***
	(0.168)	(0.211)
Sudden-Stop of Credit	0.026	0.063
	(0.173)	(0.167)
Inter-State War × Sudden Stop	4.106***	4.157***
	(0.342)	(0.377)
ln(Per Capita GDP)	0.540***	0.538***
	(0.161)	(0.181)
ln(Oil Production)	0.098	-0.039
	(0.377)	(0.390)
British Colony (Time-Varying)		0.597*
		(0.307)
Iberian Colony (Time-Varying)		0.748
		(0.624)
Other Colony (Time-Varying)		-0.626**
		(0.309)
Constant	-7.135	-10.226
	(5.577)	(7.372)
Joint effect: War + War×Sudden Stop	0.543*	0.494*
	(0.298)	(0.294)
Hazard Rate	Yes	Yes
Colonial Origins	Yes	No
Region FE	Yes	Yes
Panels	50	50
Observations	3,035	3,035

The hazard is approximated with a cubic polynomial (Carter and Signorino 2010). Per Capita GDP (Maddison-Extended Dataset) and Oil Production (Wimmer and Min). Countries with full information (50): Albania, Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Bulgaria, Canada, Chile, China, Colombia, Ecuador, Egypt, Finland, Germany, Greece, Hungary, India, Indonesia, Iran, Ireland, Israel, Italy, Japan, Lebanon, Malaysia, Mexico, Morocco, Myanmar, Nepal, Netherlands, New Zealand, Panama, Peru, Philippines, Poland, Portugal, Romania, South Africa, South Korea, Spain, Sri Lanka, Switzerland, Thailand, Tunisia, Turkey, Uruguay, Venezuela, and Vietnam. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

S Additional Evidence of Short-Term Effects: Railroad Density as of 1913

The Short-Run Effects Section in the main text show evidence that war-finance has effects on two proxies of state capacity: School Enrollment Ratios and Census Technology. This section considers a third proxy: *Rail lines length*, which captures Mann's (1984) notion of "infrastructural power" of the state. Rail lines facilitate the state's presence throughout the territory. Importantly, Dincecco, Fenske and Onorato (2016) and Queralt (2015) show that the railroad network correlates with fiscal capacity.

Next, I regress Rail Line Length By 1913 on war and exogenous credit access in the long ninteenth century. Due to data limitations, the initial value of Railroads correspond to 1850. To fully account for the topographical characteristics of rail line building, models include three additional controls: land area, tropical weather, and terrain ruggedness.

Table A-24: **Additional Evidence of Short-Term Effects:** Railroad Length by 1913 as a function of War and Exogenous Credit Access

	(1)	(2)	(3)
# Years at War while Credit Stops in 1816-1913	0.095*	0.094*	0.092*
	(0.049)	(0.050)	(0.049)
# Years at War while Credit Flows in 1816-1913	-0.096	-0.093	-0.118
	(0.070)	(0.075)	(0.071)
ln(Railroad Length by 1850)	0.176	0.173	0.001
	(0.176)	(0.182)	(0.256)
Population Density as of 1820	0.549	0.594	1.076
	(1.798)	(1.803)	(1.847)
Oil Producer	-0.137	-0.104	-0.080
	(0.517)	(0.592)	(0.599)
Sea Access	-0.001	-0.001	0.001
	(0.006)	(0.007)	(0.007)
Desert Territoy	0.080	0.078	0.076
	(0.052)	(0.053)	(0.052)
Land Area	0.003***	0.002**	0.003**
	(0.001)	(0.001)	(0.001)
Rugged Terraing	0.070	0.071	-0.016
	(0.189)	(0.190)	(0.196)
Tropical Weather	-0.011	-0.011	-0.012
	(0.011)	(0.012)	(0.012)
State Antiquity		-0.000	-0.000
		(0.001)	(0.001)
Great Power			1.743
			(1.175)
Constant	5.807***	5.916***	5.974***
	(1.230)	(1.868)	(1.841)
Region FE	Yes	Yes	Yes
Colonial Origins FE	Yes	Yes	Yes
Observations	62	61	61
R-squared	0.620	0.620	0.633

Great Britain is excluded. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

T Transmission Effects in Regression Framework

Table A-25 presents Figure 3's information in the main text in regression format. Accordingly, fiscal capacity is proxied by nontrade tax revenue as a percentage of tax revenue. For each decade between 1945 and 1995, I compute the average value of the dependent variable. Given the small N, fewer controls are considered, as explained in fn. 131 in the main text. Some of the estimates for $\hat{\beta}_1$ do not reach standard levels of statistical significant, but they are reasonably close given the sample size, as shown in Figure 3 in the main text.

Table A-25: **Transmission Effects:** Non-Trade Tax Revenue as a Percentage of Total Tax Revenue from 1946 to 1995 as a Function of War and Credit Access in the Long-Nineteenth Century. Decade by Decade Models.

	(1) 1946-1955	(2) 1956-1965	(3) 1966-1975	(4) 1976-1985	(5) 1986-1995
# Years at War while Credit Stops in 1816-1913	0.981*	0.197	0.636	0.878**	0.942**
	(0.536)	(0.617)	(0.466)	(0.427)	(0.390)
# Years at War while Credit Flows in 1816-1913	-1.343	-0.434	-0.773	-0.848	-0.666
	(0.895)	(0.954)	(0.721)	(0.635)	(0.559)
Population Density in 1820	-3.141	-6.317	2.545	-3.251	-3.446
	(9.439)	(5.937)	(5.691)	(6.250)	(5.365)
Oil Producer	-7.560	-5.112*	12.337*	18.279***	14.788***
	(5.182)	(2.540)	(6.305)	(4.420)	(3.057)
Sea Access	0.042	0.065	0.004	0.022	0.019
	(0.067)	(0.062)	(0.064)	(0.043)	(0.038)
Colonial Past	-6.769	0.278	-0.853	-1.255	-1.821
	(5.094)	(4.515)	(4.447)	(3.430)	(2.682)
Great Power	9.501	14.081**	10.773**	11.649***	5.687
	(5.945)	(5.348)	(5.079)	(4.121)	(4.097)
Intercept	92.313***	86.448***	69.119***	65.954***	72.090***
	(7.031)	(5.388)	(6.827)	(5.156)	(3.774)
Observations	34	37	55	71	84
R-squared	0.261	0.163	0.207	0.340	0.342

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

U Political Mechanism in Regression Format

This section shows information in Figure 4 in the main text in regression format. Table A-26 includes two dependent variables: Average Executive Constraints in 1900-1913 and 1995-2005, respectively. Two clarifications are in order: First, I rely on Executive Constraints instead of the standard Polity 2 score (which includes also measures of executive recruitment, and political competition) because Executive Constraints genuinely captures the outcome of the political negotiation around taxation: namely power-sharing institutions.[157] Second, I calculate average values to minimize the influence of abnormal cases.

Initial Executive Constraints is a key confounder in this test, as it influences access to external credit in the past (Schultz and Weingast 2003) and it might condition future Executive Constraints. However, very few countries hold a value for early initial constraints—29 exactly, once I drop Great Britain from the sample: Argentina, Austria, Belgium, Bolivia, Brazil, Chile, China, Denmark, Ecuador, France, Greece, Iran, Japan, Mexico, Morocco, Nepal, Netherlands, Norway, Paraguay, Peru, Portugal, Russia, Spain, Sweden, Thailand, Turkey, United Kingdom, United States of America, Uruguay and Venezuela. Most of these countries are sovereign by 1830, thus non-sovereign countries (e.g., colonies) are under-represented in this test.[158]

The introduction of *Initial Executive Constraints* reduces the sample size dramatically. The small N does not allow for a full battery of Region and Colonial Origins fixed effects. To minimize unobserved heterogeneity across units, I include six controls, as explained in fn. 131 in the main text. Results in Table A-26 suggest that going to war while credit flows in the long ninteenth century is negatively related to executive constraints in the short- and long-run. External credit saves the ruler the political costs of undertaking political change, allowing the persistence of low executive constraints. By contrast, going to war while credit stops is positively related to short- and long-run executive constraints. The coefficient for the

[157] See Besley and Persson 2011 for a similar approach.

[158] This issue is addressed in the bureaucratic mechanism section.

Table A-26: **Political Mechanism in Regression Format:** Executive Constraints in 1900-1913 (short-run) and 1995-2005 (long-run) as a Function of War and Exogenous Credit Access in the Long-Nineteenth Century.

	(1) **Executive Constraints 1900-1913**	(2) **Executive Constraints 1995-2005**
# Years at War while Credit Stops in 1816-1913	0.128***	0.037
	(0.039)	(0.024)
# Years at War while Credit Flows in 1816-1913'	-0.139**	-0.115**
	(0.059)	(0.043)
Population Density in 1820	0.799	-0.598
	(0.843)	(0.496)
Oil Producer	-0.266	-0.629
	(0.659)	(0.911)
Sea Access	0.049***	0.023***
	(0.014)	(0.008)
Executive Constraints 1800-1913	0.775***	0.328**
	(0.164)	(0.123)
Former Colony	-0.370	-0.205
	(0.790)	(0.546)
Great Power	1.465**	1.554*
	(0.569)	(0.863)
Constant	0.689	5.548***
	(0.727)	(0.977)
Observations	29	29
R-squared	0.632	0.407

Great Britain is excluded. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

long-run does not reach standard levels of statistical significance by a small margin (p-value = 0.136). However, Figure 4 in the main text, which plots this coefficient with 90% CI, does suggest that war increases short- and long-term executive constraints as long as it is waged in periods in which rulers have strong incentives to expand tax capacity.

Overall, results suggest that war-financing has important implications on the origins of power-sharing institutions. Tax-financed war facilitates political reform, whereas external debt-financed war does not. This is a novel result that will be fully developed elsewhere.

V Bureaucratic Mechanism of Persistence

Arguably, the political mechanism analyzed in the main manuscript is most compelling among sovereign countries, in which genuine tax bargaining between the ruler and taxpayers may naturally arise. Political conditions in colonies and occupied territories might not be conducive to such negotiations.[159] For such cases—and for every other case, that is, regardless of political status—there is a second, nonmutually exclusive mechanism that facilitates transmission over time, namely bureaucratic survival.

Modern tax administrations are created for and by war.[160] Professionalized bureaucracies are necessary to assess wealth and collect taxes as well as to resist the natural aversion to having one's sources of income monitored. However, once created, bureaucracies entrench, grow larger, and, arguably, became states within states (Tilly, 1990, p.115).

Bureaucracies maximize institutional survival by increasing their size and financial endowment (Niskanen, 1994). Accordingly, we can expect tax bureaucracies to oppose disinvestment in administrative capacity, ultimately carrying on the effect of war making on long-run fiscal capacity. Columns 7 and 8 in Table 5 in the main text, in which the size of the tax administration circa 2005 is regressed on past warfare and credit access, lend support to this mechanism.

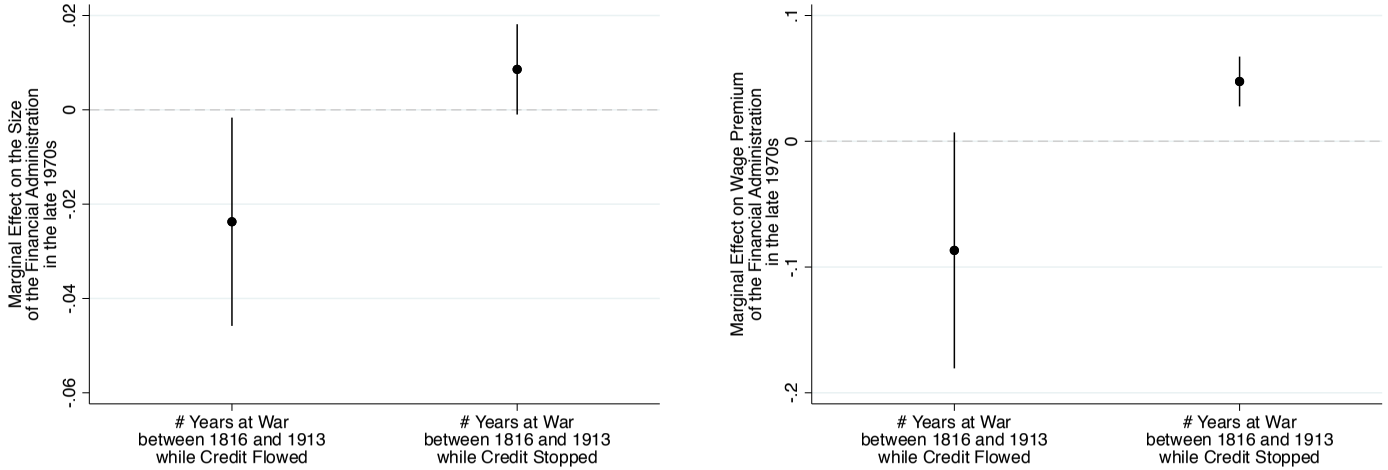
To show earlier cross-national evidence, Figure A-6 plots the effect of nineteenth-century war finance on two proxies for administrative capacity in the late 1970s: the *Size of the Finance Administration* and its *Wage Premium* relative to other branches of government.[161] Despite the small sample size (see below for details), Figure A-6 shows that nineteenth century war waged without access to external finance is associated with bigger and well-funded finance administrations, whereas war waged with access to external finance is not. In particular, a one standard deviation increase in the number of years at war when credit

[159]This opinion is contested: Bräutigam (2008) and Makgala (2004) show evidence of tax-based political bargain between local elites and colonial powers.

[160]See Brewer (1988) for Europe and Young (1994) for colonial Africa.

[161]Earlier crossnational data are not available.

Figure A-6: **Bureaucratic Mechanism:** The Effect of War Finance on the Size and Wage Premium of the Finance Administration in the Late 1970s. 90% CI.



(a) Size Relative to Population

(b) Wage Premium

is tight in the nineteenth century increases average size and wage premium of the finance administration in the late 1970s by 49% and 22%, respectively.[162]

Data Details for Bureaucratic Mechanism. Historical, cross-national data for public administration characteristics are virtually non-existent. As far as I know, Tait and Heller (1983) is the one exception. They code key characteristics of the public administration of 49 countries in the late 1970s. Tait and Heller's (1983) sample includes developed economies as well as former colonies. Their data do not include information of the Size of the Tax Administration, specifically. Instead I work with data of the Size of the Finance and Planning Administration (normalized to 100 inhabitants).[163]

The Size of the Finance Administration measures the extensive margin of the effect of war. According to Niskanen (1994), we should also observe an effect of war on the intensive margin of bureaucratic development. In the absence of budget data, I measure the intensive margin by the Wage Premium of the Finance Administration Employees relative to other branches of central government.[164]

The effective sample is fairly small. To minimize unobserved heterogeneity across units, I include six controls, as explained in fn. 131 in the main text. Despite the small N, results move in the expected direction. Three out of the four coefficients of interest hold the expected sign and are statistically different from zero. $\hat{\beta}_1$ in Column 1 of Table A-27 almost reaches conventional levels of statistical significance (p-value = 0.112, N = 23). This is clearly seen in Figure A-6 in the main text. Altogether, these results suggest that war finance has effects on long-term bureaucratic development.

[162]The prediction for Size is unusually high because both this variable and the key predictor are skewed.

[163]With respect to the Size of the Finance Administration, the following countries can be matched to the main dataset of this article: Argentina, Belgium, Congo, Cyprus, Ecuador, El Salvador, Germany, Guatemala, Iceland, Ireland, Japan, Netherlands, New Zealand, Panama, Senegal, South Africa, South Korea, Sri Lanka, Swaziland, Sweden, United States of America, Zambia, and Zimbabwe.

[164]With respect to the Wage Premium of the Finance Administration, the following countries can be matched to the main dataset of this article: Argentina, Cyprus, Ecuador, El, Salvador, Iceland, Japan, New Zealand, Panama, South Africa, South Korea, Sri Lanka, Swaziland, United States of America, Zambia, and Zimbabwe. All remaining countries have missing information in some key variable. At any point, both effective samples offer a good balance of developing and developed countries.

Table A-27: **Bureaucratic Capacity in the late 1970s** as a function of war and access to external finance in the long ninteenth century.

	Size of the Finance Administration (1)	**Wage Premium** (2)
# Years at War while Credit Stops in 1816-1913	0.009	0.048***
	(0.005)	(0.011)
# Years at War while Credit Flows in 1816-1913	-0.024*	-0.087
	(0.013)	(0.050)
Former Colony	-0.024	-0.256
	(0.034)	(0.168)
Population Density in 1820	0.070	0.201
	(0.120)	(0.259)
Oil Producer	-0.020	-0.272
	(0.036)	(0.173)
Sea Access	0.000	0.001
	(0.001)	(0.002)
Great Power†	-0.052	
	(0.079)	
Constant	0.118**	1.451***
	(0.048)	(0.247)
Observations	23	15
R-squared	0.230	0.516

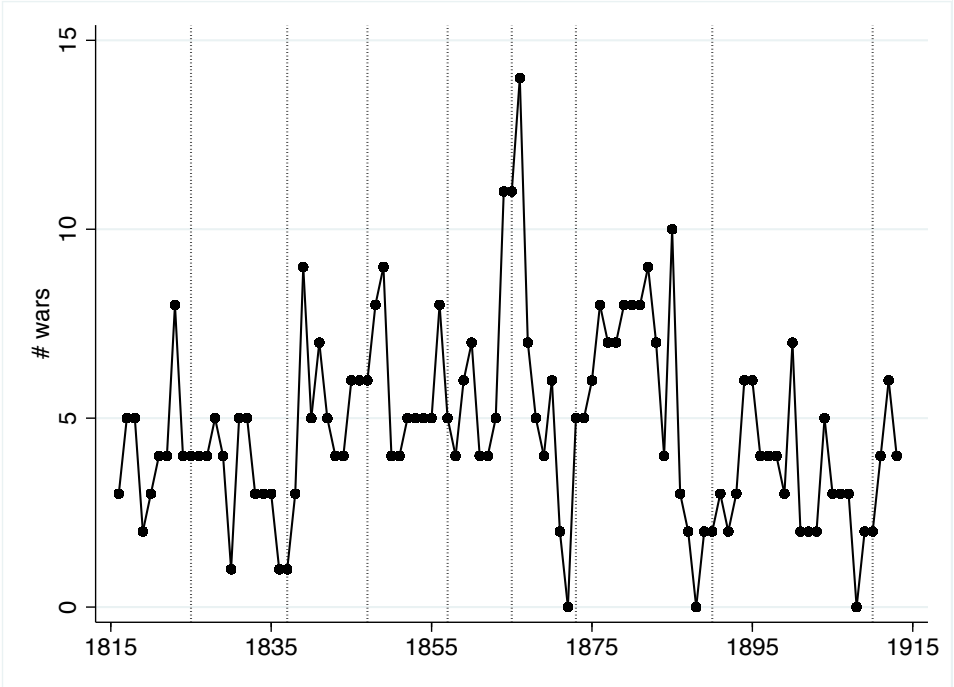
† There is no Great Power in the Wage Premium sample. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

W Further Evidence of Exogeneity of Sudden-stops

Table 3 suggests that the frequency and length of war in and outside sudden-stop periods are virtually identical (or balanced). Figure A-7 shows this differently. In particular, it plots the *Total Number of Wars per Year* in the sample, and identify the onset of sudden-stops. Financial crises that begin within four years of the last sudden-stop (the average duration) are not plotted.

If sudden-stops are anticipated, we should observe a *systematic* increase in the frequency of war right before the onset of the credit crunch. However, Figure A-7 does not show such a pattern. Wars take place before and after sudden-stops, almost evenly, consistent with Table 3 in the main text.

Figure A-7: **Total Number of Wars per Year and Sudden-Stops Onset (vertical line)**



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