NOT FOR PUBLICATION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.

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*;[149] for Nepal, data are drawn from the Ministry of Finance;[150] For Sri Lanka, data are drawn from the Department of Fiscal Policy;[151] for Lebanon, data are available from the Ministry of Finance for the 2000-5 period;[152] For Zambia, data are for 2005 only, and are drawn from CMI Report;[153] 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

[149] 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.

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

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

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

[153] 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.

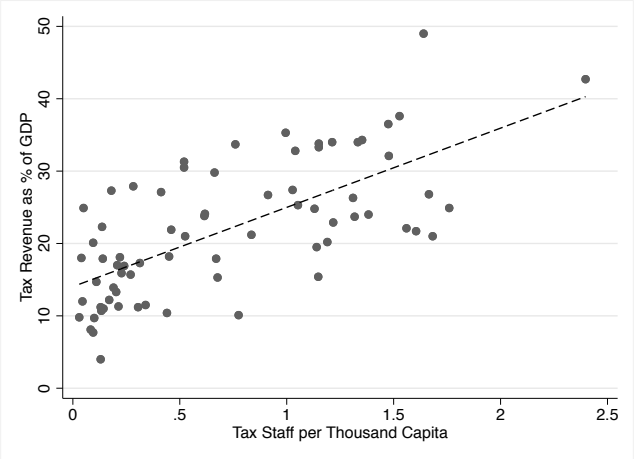
	(1) Non-Augmented Dep Variable, 1995-2005	(2) Dep Variable dated as of 1990-2000	(3) Dep Variable dated as of 2000-10
# Years at War while Credit Stops in 1816-1913	0.280***	0.191**	0.226***
	(0.069)	(0.073)	(0.052)
# Years at War while Credit Flows in 1816-1913	-0.254***	-0.182**	-0.228***
	(0.072)	(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).

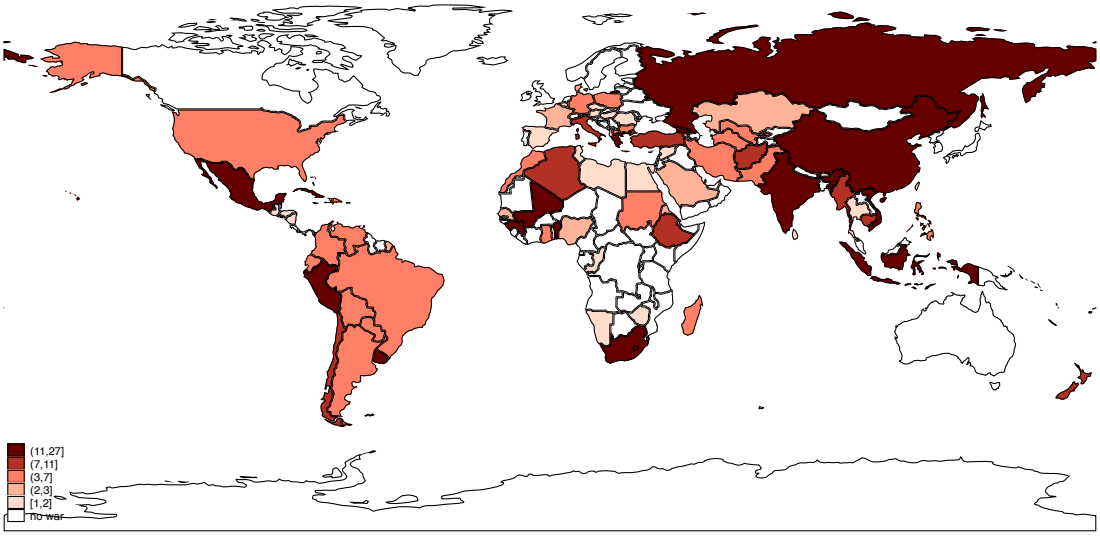
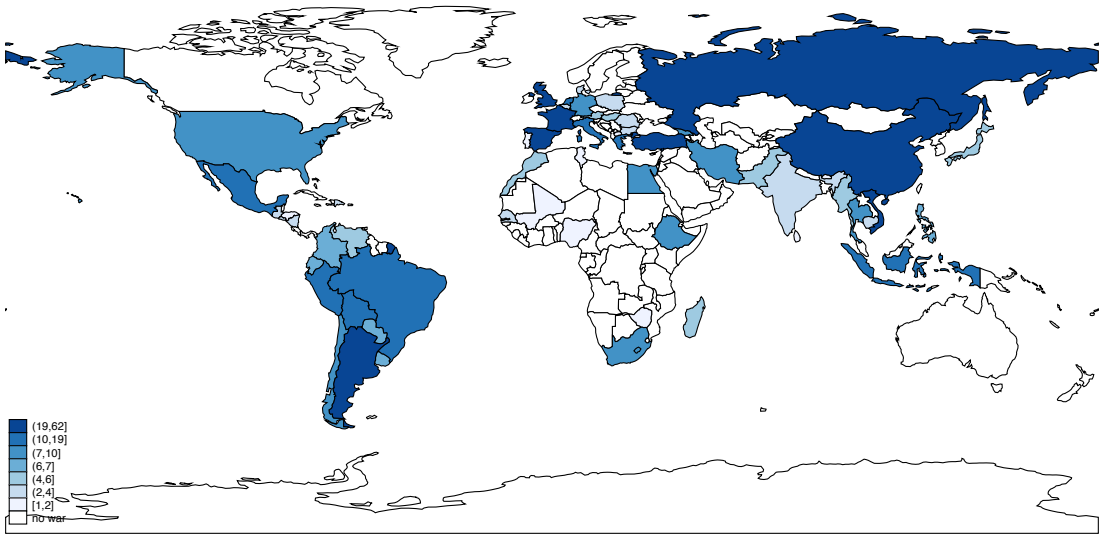


Figure A-3: **Frequency of War Participation in the Long-Nineteenth Century.** Colors indicate the total number of years at war. Source: Wimmer and Min (2009).



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.

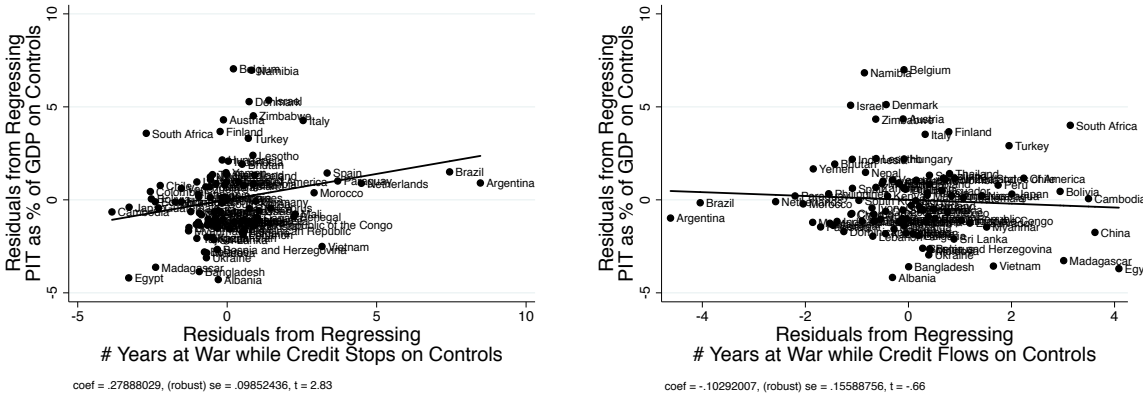
	(1)	(2)	(3)	(4)
# Years at War while Credit Stops in 1816-1913	0.131***	0.087**		
	(0.038)	(0.041)		
# Years at War while Credit Flows in 1816-1913			0.046	-0.038
			(0.072)	(0.077)
Population Density in 1820	1.496	1.220	1.696	1.134
	(1.344)	(1.426)	(1.378)	(1.446)
Oil Producer	0.030	0.013	0.225	0.219
	(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.[154] 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

[154]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).[155] 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.

[155]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. 134 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.[156]

Results with augmented VAT are reported in Column 2 in Table A-18.[157] 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.

[156]Recall, PIT data augmentation does not change results. Refer to Table A-1.
[157]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.[158] 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

[158] 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**

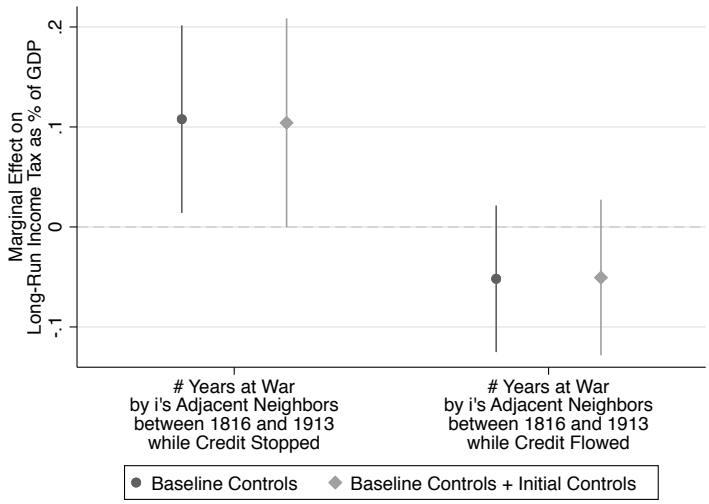


Table A-21: **Reduced-Form Models.** Personal Income Tax as % of GDP Today as a Function of War and Exogenous Access to Credit in the Long Nineteenth Century, with War Participation of Country i replaced by War Participation by Adjacent Countries

	(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 , *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.[159]

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

[159]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. 134 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.[160] 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 underrepresented in this test.[161]

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. 134 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

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

[161] 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.[162] 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.[163] 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.

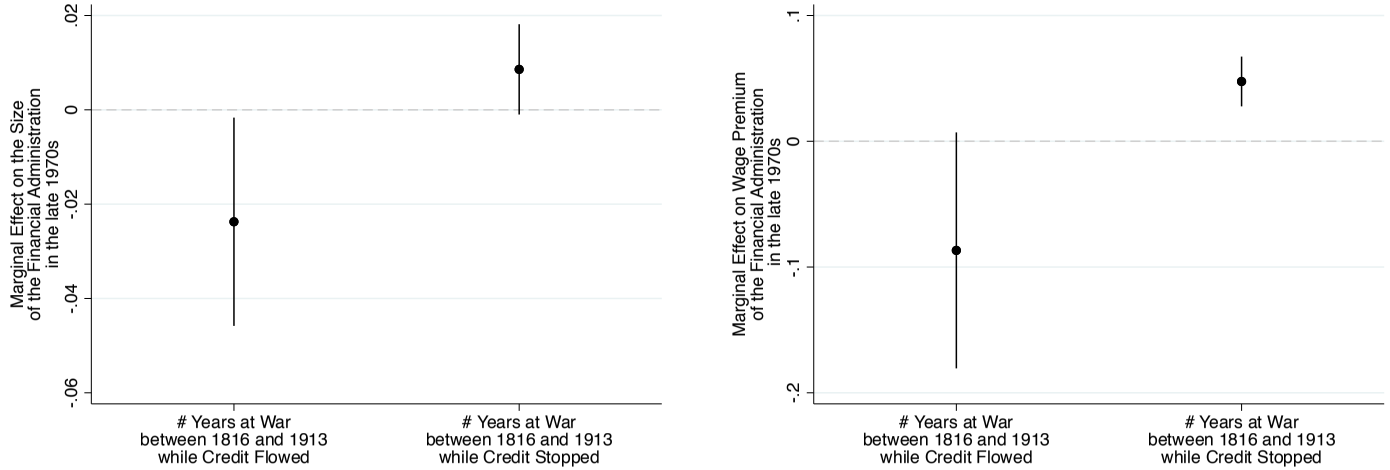
To show earlier cross-national evidence, Figure A-6 plots the effect of nineteenth-century war finance on two proxies for administrative capacity in the late 1970s: the *Size of the Finance Administration* and its *Wage Premium* relative to other branches of government.[164] Despite the small sample 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

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

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

[164]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.[165]

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).[166]

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.[167]

The effective sample is fairly small. To minimize unobserved heterogeneity across units, I include six controls, as explained in fn. 134 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.

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

[166]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.

[167]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

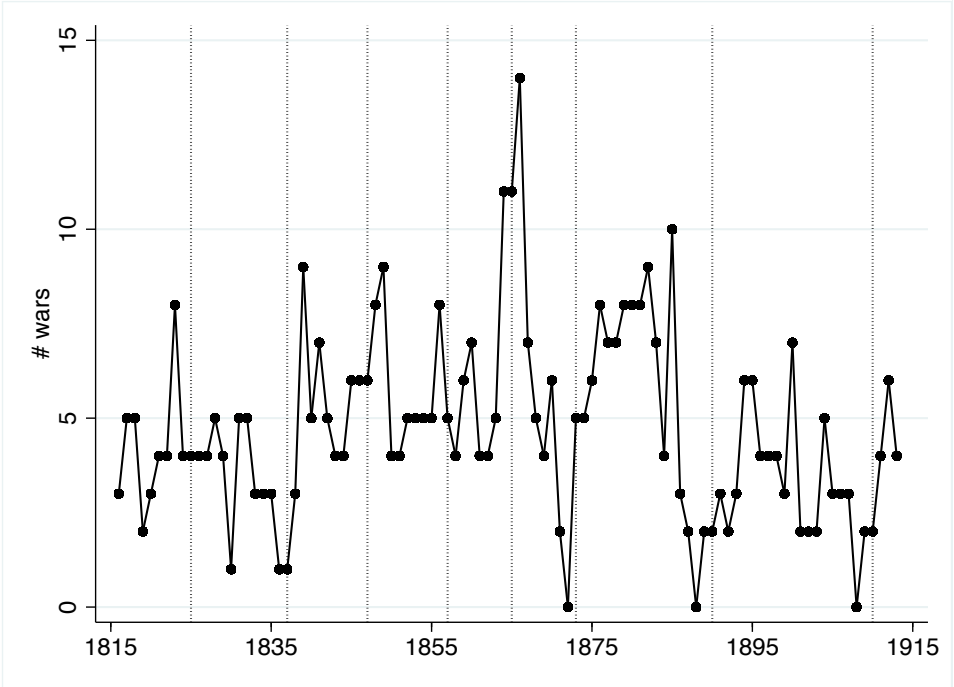
† There is no Great Power in the Wage Premium sample. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

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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