Democracy, autocracy, and sovereign debt: How polity influenced country risk on the peripheries of the global economy, 1870–1913

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ABSTRACT

This article tests the influential democratic advantage hypothesis – that democratic governments have historically borrowed more cheaply than autocratic governments – in the context of the first financial globalization, from circa 1870 to 1913. We construct indicators of political regime types, then regress government bond spreads of 27 independent capital-importing countries on them. In contrast with the mainstream literature, the results suggest that democracies were associated with higher country risk. Our findings indicate that autocratic regimes had a significant advantage: democracies paid 5.7% more on their debt than autocracies, controlling for several financial and political variables. This gap is the equivalent of 35.4% of the negative effect defaults had on credit costs. Our conclusions hold when allowing for different definitions of political regime type and bond spreads. The correlations identified also find support in qualitative evidence, according to which creditors favored autocracies for being politically more stable than democracies.

1. Introduction

The study of sovereign debt is ultimately a study of power. The capacity of states to govern, tax, and promote economic growth is correlated with the risk of default. Creditworthiness, therefore, says a great deal about political conditions. The existing literature often argues that parliamentary democracies produce better borrowers because the legislature can limit the power of the executive, forcing it to credibly commit to its contracts and respect the property rights of creditors (North and Weingast, 1989). This is allegedly not the case for autocracies, whose powerful executives are under the inevitable temptation of expropriating their creditors.

The idea of a democratic advantage, however, is debatable, and there is no consensus on the topic today. The historical literature challenges the assumption that Britain was a democracy in the century that followed the Glorious Revolution and argues that a particular form of party politics rather than polity explains Britain's capacity to tax and to borrow in that period (O'Brien, 1999; Horwitz, 1997; Stasavage, 2008). Studies in modern political science also show that policy consistency and stability rather than regime type explain country risk, and autocratic regimes tend to depend more on creditors to maintain the stability of their regimes, creating incentives for good borrowing behavior (Archer et al., 2007; DiGiuseppe and Shea, 2016). While Beaulieu et al. (2012) found that autocracies were excluded from the credit market in the postwar period, Block and Vaaler (2004) show that creditors and credit rating agencies tend not to like elections in developing countries.

The democratic advantage hypothesis, however, is almost virtually accepted without debate for the period of the first financial globalization – from the 1870s to 1913, when most countries in the world borrowed in London. Most authors who study that period focus on financial rather than political conditions in borrowing countries. Such a financial approach may be due to the debate on

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the gold standard around which that literature evolved (Flandreau and Zumer, 2004; Ferguson and Schularick, 2006; Mitchener and Weidenmier, 2010; Mauro et al. 2006). The lack of studies on polity is striking because not only capital but also political institutions were globalized at the turn of the twentieth century. Independent states across the world extended suffrage and introduced parliamentary checks and balances, while other countries followed a more authoritarian model based on Bismarck's Germany (Huntington, 1993; Ziblatt, 2017). This heterogeneity in political institutions makes the 1870–1913 period an ideal setting to test the democratic advantage hypothesis in the context of foreign debt.

To assess the relationship between polity and the cost of credit, we perform regressions on the government bond spreads in London of 27 independent capital-importing countries between 1870 and 1913. Our regressors of interest are indicators of political regime type (measures that capture the power of the legislature and how the heads of states are chosen) derived from the *Statesman's Yearbook* (1870-1913), a contemporary British publication. We include a number of control variables that capture political stability, defined as years that countries had been at peace, together with several financial indicators widely applied in the literature.

The overall conclusion of this article is that the democratic advantage did not apply to the peripheries of the first financial globalization. Without implying causality, our regressions show that democracies were associated with higher spreads.[1] Results indicate that democracies paid about 5.7% more on their debt than autocracies. This gap is significant: it is the equivalent of 35.4% of the notoriously negative effect defaults had on credit costs. This "autocratic advantage" holds with several political and financial controls; it is also robust allowing for different definitions of polity and spreads. A similar result stands out from a panel event study that indicates that the spreads were significantly higher when countries moved from more to less autocratic polities vis-à-vis the opposite process – respectively democratization and autocratization.

These correlations are in line with qualitative evidence from primary sources, which indicate that creditors praised what the press called "true autocrats" for providing political stability. In contrast with today, the term "dictator" did not necessarily have a negative connotation, and democracies were not always regarded positively. This is consistent with the fact that democracies were nascent polities in the period, and thus on average less stable than today.

2. The literature on polity and sovereign risk

The historical literature that relates sovereign debt to polity focuses on the evolution of fiscal institutions in early modern Europe. The influential work by North and Weingast (1989) proposes that the British parliamentary system that emerged from the Glorious Revolution in the 1680s limited the executive's power to impose lump-sum tributes and forced loans. The Parliament represented taxpayers and creditors, who became more willing to pay taxes and to lend to the government. Thanks to the power of the legislature, the executive was able to tax consistently and to issue relatively cheap long-term debt.

The work of several scholars supports this democratic advantage hypothesis. Some point out that legislators with ministerial responsibilities made it easier for governments to raise taxes (Daunton, 2010, 2012; Cox, 2011; Hoffman and Rosenthal, 1997). Others present democracy as the main driver through which governments introduced progressive direct taxation, such as an income tax (Bordo and Cortés Conde, 2001; Acemoglu and Robinson, 2006; Aidt and Jensen, 2009; Ardanaz and Scartascini, 2011). In a cross-country exercise for pre-modern Europe, Dincecco (2011) finds that limited government in centralized states achieve better fiscal results than those that were fragmented and absolutist.

Nevertheless, the democratic advantage hypothesis has also received criticism. Stasavage (2008) asserts that party politics rather than a limited government system increased taxation and reduced the risk of default in early modern Britain. Several scholars highlight the role of external wars rather than polity in making taxation politically easier for the state (Tilly, 1990; O'Brien, 2012; Bonney, 1999; Besley and Persson, 2009). Karaman and Pamuk (2013) combine war and polity in a cross-country study that covers Europe from the sixteenth to the eighteenth centuries. They find that war intensity raised taxation both in autocratic rural states and in representative urban states: hence, the success of the British system was a subcase. Others find no causality between democracy and taxation in recent times (Mulligan et al., 2004; Profeta et al., 2013).

Another stream of the financial history literature studies the determinants of sovereign risk during the first financial globalization, from circa 1870 to 1913, when a record amount of capital flew from Europe to the rest of the world. Britain was at the center of that process and sovereign debt was one of its main drivers. In contrast with the works quoted above, however, this specific literature overlooks the role of polity, emphasizing instead financial and monetary factors such as the gold standard. The pioneering work by Bordo and Rockoff (1996) regresses government bond spreads of nine countries on variables that capture financial conditions, including a dummy that controls for adherence to the gold standard. The authors find that countries on gold borrowed at lower rates. Applying panel regression methodologies on larger samples, several scholars confirm these results (Obstfeld and Taylor, 2003; Mauro et al., 2006; Esteves and Tovar Jalles, 2016). Others find that variables such as credit record, fiscal sustainability, and association to the British Empire were more important than the gold standard in determining credit rates (Flandreau and Zumer, 2004; Ferguson and Schularick, 2006; Alquist and Chabot, 2011; Mitchener and Weidenmier, 2015).

The work by Flandreau and Zumer (2004) is particularly important for two reasons. Firstly, it avoids ahistorical analysis by limiting its empirical exercise to information available to contemporaries. For instance, modern reconstructed series such as GDP are not included in the regressions.[2] Secondly, despite the lack of changes in political institutions in their country set during 1870–

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[1] We use the term "democracy" to characterize polities that were far more authoritarian than the democracies that exist today – a point discussed in detail below.

1913, the authors control their regressions for franchising rate, which appears to be significantly associated with lower credit costs (Flandreau and Zumer, 2004: 37). However, a more recent paper by Dasgupta and Ziblatt (2021) shows that the impact of franchise extension on bond spreads was in fact the opposite during 1800–1920, as franchise extension contributed to large increases in bond premiums.

Despite being valid steps towards the study of polity in the sovereign debt market, the franchising rate is a problematic proxy for democracy, since rigged elections in authoritarian countries could include a significant portion of the population, who *de jure* had a say, but *de facto* were excluded from the political process.[3] Moreover, the contemporary bondholders had very limited information on the share of adult population with the right to vote during the period we study, which is only one of the dimensions of polity. Instead, as we show in the next chapter, they were more likely to be informed about institutional arrangements that could limit the power of the executive, such as the existence of elections or effective legislatures.

Although the first financial globalization literature does not focus on polity in great detail, some works include variables that capture political stability. Both the definitions of political stability and the results vary. Flandreau and Zumer (2004) show that a political crisis dummy was associated with higher spreads. Mauro et al. (2006) and Ferguson and Schularick (2006) find similar results in relation to interstate and civil wars, respectively. Mitchener and Weidenmier (2015) and Esteves and Tovar Jalles (2016) find no significant results whatsoever. It is important to note that these variables on political instability capture short-term events such as wars and uprisings. They may be related to but are different from the polity variables we construct in this article, which capture long-term political systems.

Overall, by assessing the effect of polity on credit cost, we contribute to the literature on the first financial globalization. We focus on 1870–1913, when countries around the world borrowed extensively in London. Our sample includes 27 independent peripheral countries: Argentina, Austria-Hungary, Brazil, Bulgaria, Chile, China, Colombia, Costa Rica, Denmark, Ecuador, Greece, Guatemala, Italy, Japan, Mexico, Nicaragua, Norway, the Ottoman Empire, Peru, Portugal, Romania, Russia, Serbia, Spain, Sweden, Uruguay, and Venezuela. These countries were all on the "peripheries", in that they were independent capital-importing countries in Latin America, Southern and Eastern Europe, Scandinavia, and Asia.[4] In the next section, we provide a historical explanation of our focus on these regions.

3. Polity and the peripheries of the first financial globalization

During 1870–1913, political institutions such as elections and parliamentary power over the executive spread around the world. Purely autocratic polities, in which the executive was the only relevant power, had become a minority by the turn of the century. This does not mean, though, that democracies spread across all countries in the same way. Some countries followed Britain, building effective parliaments and promoting free and fair elections. Yet others had strong governments that tried to emulate variations of the Prussian model, with dysfunctional or weak legislative powers and limited or rigged elections. To make matters more complex, a few countries fluctuated between systems, becoming more or less democratic throughout our period of analysis (Huntington, 1993; Ziblatt, 2017).

The uneven spread of democratic and autocratic institutions appears in contemporary print-resources available to the bondholders. These sources occasionally reviewed the major differences in political regime types across the world. For instance, the Economist (1894a: 1340) highlighted key defining characteristics of autocratic regimes: "the true autocrat is a man whose order has the full force of law, and who can therefore not only order a change in the number of a jury by decree, but can direct the arrest of a subject [...] without appeal or responsibility". Yet only a few would fall under this category, such as the Russian Tsar, Montenegrin Prince, and the rulers of some British dependencies like the Sarawak Raj and Indian Viceroy. These "true autocracies" were seen to be different from the constitutional monarchies of continental Europe, where personal power could only be exercised within the constraints of the law. Moreover, the discrepancies in the effectiveness of democratic legislatures were also obvious to contemporary observers. For instance, the Spanish Cortes was seen as "a farce ever since it was constructed" and the Austrian Reichsrat was regarded as incapable of governing the entire Austro-Hungarian Empire. Similarly, the "shadowy legislative bodies of South and Central America" were also seen as weak and "few either know or care anything of their proceedings" (Economist, 1898b: 867).

This awareness of different degrees of democratic and autocratic rule was part of the information set of the contemporary bondholders. Moreover, contemporary observers did not necessarily have a negative attitude towards autocracies. The assumption that a representative legislative body was "the final product of political wisdom" was increasingly questioned in the light of historical

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[2] We follow a similar approach because our intention is not to test a modern economic theory with historical data, but rather to understand whether contemporary perceptions of polity mattered in determining historical bond prices.

[3] Russia had a franchise rate of 253 per 1,000 inhabitants in 1910, higher than the rate for Britain (171) and close to the United States (292) (Banks and Wilson, 2020). Yet Russia was a notorious autocracy ruled by an unelected tsar. More moderate – but still authoritarian – dictators often used rigged elections to legitimize their rule. A famous example is Porfirio Díaz, who "won" six elections during his 27 year-long presidency in Mexico. Francisco Madero was arrested for attempting to run as a contender in 1910. In the following year, he led the revolution that toppled Díaz and allowed the first free and fair elections in the country's history (Speckman-Guerra, 2013: 196, 209-210).

[4] We do not include formal or informal colonies in our analysis, as we are interested in understanding the impact of political institutions that emerged locally, rather than those enforced or transplanted by foreign powers. Moreover, the literature shows that it is methodologically problematic to pool together colonies and sovereign states in the analysis of borrowing costs (Accominotti et al., 2010, 2011).

events (Economist, 1898b: 867). For example, Bulgaria was governed by a Western-style liberal constitution, universal male suffrage, and parliamentary power in a one-chamber system since its independence in 1878. Nonetheless, the Bulgarian prince was dissatisfied with the democratic constitution, since it gave extensive powers to the local population and peasantry at the level of communes. Following a series of political struggles and turmoil between the parliament and the monarchy, the latter eventually came to dominate the political arena in 1887, and the parliamentary system of Bulgaria lost its importance (Pammer and Tunçer, 2021).

In assessing the financial condition of Bulgaria in 1894, the *Economist* argued that Bulgarian bonds could potentially be a good investment. In addition to Bulgaria's rich natural resources and strong state finance, its political regime was also praised as a positive factor, because it was seen as having the "good fortune to be ruled successively by two remarkably sagacious and high-minded young Princes". Moreover, the newspaper depicted the country's long-reigning prime minister Stefan Stambolov as someone who "has labored for nearly twenty years for the welfare of his native country, earning the well-merited title of 'Dictator of Bulgaria'" (Economist, 1894b: 16–17). A similar sympathetic attitude towards autocratic rule can also be observed for Russia. In discussing the corruption claims of the new Russian Emperor, Nicholas II, the Economist (1895: 2684) recognized that a representative body would be the best check on corruption, but it also argued that in other areas of economic and political reform, the Tsar's autocracy could deliver results more efficiently. Likewise, the Times (1899: 10) praised the "military dictatorship" Porfirio Díaz imposed on Mexico, thanks to which "order and progress" replaced "revolutionary outbreaks." Based on his "absolute powers," the dictator was able to "turn his attention to the best means of increasing the national wealth by the developing of the latent natural wealth of Mexico and by the encouragement of private industrial enterprise wherever opportunity offered." That praise was given in the same year the country issued a conversion loan in Europe at record low rates (Weller, 2015).

These examples imply that, in contemporary public opinion, autocracies could potentially offer efficiency and political stability. As for the opposite side of the political spectrum, democratic regimes with parliamentary systems were not regarded as inherently positive. For instance, during the political crisis of 1907 in Portugal, which eventually led to the assassination of the King in 1908, the Economist (1907: 2077) reported that "parliamentary government, as in Spain, has consisted in a see-saw of two parties, rejoicing in particularly pretentious names, but resting on no real support of intelligent public spirit". Following the failure of parties to come up with a coalition government and get the budget through the Parliament, the King's suspension of the parliament appeared a positive step: "the nation, as well as its creditors, might well be thankful for any regime which would fulfil the promises of the government". The Economist (1898a: 934) made a similar remark about the political and fiscal crisis in Italy in 1898, which, according to the newspaper, resulted from the "vexatious meddling of the Chamber". The newspaper argued that the executive temporarily should be free from the "shifting intrigues of an incapable Chamber", even though this could be considered "absolutism". Such a step was a "necessity at times in countries in which the Parliamentary system does not respond to the national requirements", and "only those who worship of Parliament as a fetish, rather than […] as a means to an end, will dispute this proposition".

This cautious attitude towards democracy was particularly pronounced during transition periods, which were commonly turbulent in the peripheries of the late nineteenth century. For example, the Economist (1909a: 809) did not have a positive view of the new regime that appeared in Nicaragua in 1909 after the collapse of the 15-year-long dictatorship of President Jose Zelaya Lopez, "however cruel to individuals". The reason behind this was the fear of civil unrest that could follow the removal of the dictator, who at least ensured "a long immunity from serious wars and revolutions". Another article published in 1909, titled "violence as a means of persuasion", affirmed that democratic governments were more unlikely to be brought about "by peaceful means than by violence". Although military force could overthrow autocratic regimes, the Economist (1909b: 736) recognized that this would often lead to further political instability.

It is possible to extend the list of these historical narratives from print-sources available to contemporary investors. One conclusion emerging out of these views is that the contemporary observers did not regard democracy or autocracy as inherently good or bad. Rather, they were interested in the potential of polities to produce political stability. In the context of capital-importing countries, creditors were inclined to have a more favorable view of autocratic regimes. That was a reaction to political instability, since the relatively young parliamentary democracies of peripheries were usually plagued by civil wars, social unrest and deadlocks between opposing parties and competing interest groups. Similarly, transitions to a democratic regime could potentially bring instability in the short term, which creditors regarded warily. In the next section, we rely on this historical account while building our variables, model, and hypotheses.

4. Polity and sovereign risk: data and model

In order to analyze the impact of political regime type on sovereign risk, we construct a cross-sectional time series dataset covering the period 1870–1913 for 27 independent countries that borrowed in London. We regress government bond spreads on variables that capture polity, political stability, and economic conditions.

4.1. Spreads

Our dependent variable is government bond spreads, defined as the difference in yield between borrower governments' bonds and the British consol. We follow Alquist and Chabot (2011: 265) and define the bond yield of a given country i as the average of the coupon yield of their bonds j floating in London in time t, weighted according to their respective capitalization or outstanding

value:[5]

$$R_{it} = \sum_{j=1}^{j} \left(w_{ijt} r_{ijt} \right) - r_{UKt} \tag{1}$$

where:

w_{ijt} is the market-value weight of bond j in country i in time t;

r_{ijt} is the coupon yield of bond j in country i in time t, which is given by the ratio of that bond's interest rate and the discount at which it is traded;

r_{UKt} is the British consol yield in time t.

In Appendix 3, we list the bonds used to calculate the weighted-average spreads defined in Eq. (1). We have included all bonds issued by our countries that follow three criteria: (1) they are denominated in gold-pegged currencies, almost always in British pounds; (2) they do not have special guarantees from third parties, such as railway companies or other governments, and (3) they have been issued by the central rather than provincial governments. This is crucial to avoid biases from peculiarly senior or junior bonds.

4.2. Polity variables

The variables of interest measure polity, capturing whether countries were more or less democratic. They are derived from the *Statesman's Yearbook*, an annual British compendium, which is arguably the most consistent and reliable contemporary source of general information on countries around the world.[6] The *Statesman's Yearbook* printed information on features such as population, area, economic conditions, and – crucially for us – regime type. For each country, under the heading of "constitution and government", the source outlined key political characteristics of the country, including its constitution, selection methods of the executive, the effectiveness of the legislatures, and other relevant information. For instance, Russia was referred to as "an absolute hereditary monarchy" where the "whole legislative, executive and judicial power is united in the emperor whose will alone is law" (*Statesman's Yearbook*, 1881: 365). At the other end of the spectrum, Costa Rica was "governed under a constitution […]. [B]y its terms the legislative power is vested in a congress of one chamber […] chosen in electoral assemblies, the members of which are returned by universal suffrage […]. The executive authority is in the hands of a President elected in the same manner as the Congress for the term of four years" (*Statesman's Yearbook*, 1881: 533). Moreover, the source reported major regime changes whenever and wherever they happened.

The Cross-National Time-Series (CNTS) database has codified the polity information available in the *Statesman's Yearbook* according to two dummy variables, *effective legislature* and *elected executive* (Banks and Wilson, 2020), which are defined as follows:

$$Effective\ legislature_{it} = \begin{cases} 1 & \text{if a legislature effectively limits the executive in country } i \text{ in time } t; \\ 0 & \text{otherwise.} \end{cases}$$

$$Elected\ executive_{it} = \begin{cases} 1 & \text{if an executive is elected by popular vote in country } i \text{ in time } t; \\ 0 & \text{otherwise.} \end{cases}$$

Effective legislature thus captures the power of the legislative branch, while *elected executive* reflects the electoral processes. In respect to this latter variable, it takes the value of 1 if elections are both direct and indirect. Indirect elections happened in parliaments and electoral colleges. Polities are more democratic when these variables take the value of 1, and autocratic when they equal 0.[7] Fig. 1 shows the values of *elected executive* and *effective legislature* for our sample.

As described in the previous section, contemporary reports often took into account these two polity characteristics (legislature and elections) together to characterize different political systems across the world. Given the two dummy variables shown in Fig. 1, we have four possible combinations, which we label by mirroring the contemporary language as follows:

(1) True autocracy: the head of state is not elected by popular vote and there is no effective legislature limiting his powers.
(2) Presidential democracy: the head of state is elected by popular vote but there is no effective legislature to limit his powers.
(3) Constitutional monarchy: the head of state is not elected by popular vote but there is an effective legislature limiting his powers.
(4) True democracy: the head of state is elected by popular vote and there is an effective legislature limiting his powers.

We should emphasize that these polity groups are historically conceived: contemporaries used the terms above to describe political institutions, rather than comment on their *de facto* quality. Due to non-transparent elections and relatively weak legislatures, some of the "true democracies" were well below the standards of modern democracies. In particular, the "presidential democracies" of the

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[5] We differ from the literature that analyzes supply-side factors, such as the liquidity of the market for colonial debt, where the bond-based approach may be justifiable (Chavaz and Flandreau, 2017). Although such a bond-based approach increases the number of observations significantly, it also introduces biases towards countries with many bond issues. As shown in Appendix 3, the number of bonds per country varies greatly in our sample. Moreover, an average-based approach is in line with our aim to assess the role of country-defined polities on credit cost. As a robustness check, we run the variations of our main model in a bond-based specification. As shown in Table E in Appendix 4.2, our main findings still hold.

[6] See Flandreau and Zumer (2004) for an application and discussion of this source in the context of the first financial globalization.

[7] We have reviewed these polity dummy variables by going through the various issues of the *Statesman's Yearbook*. We found that the CNTS codification is consistent with the primary sources most of the time.
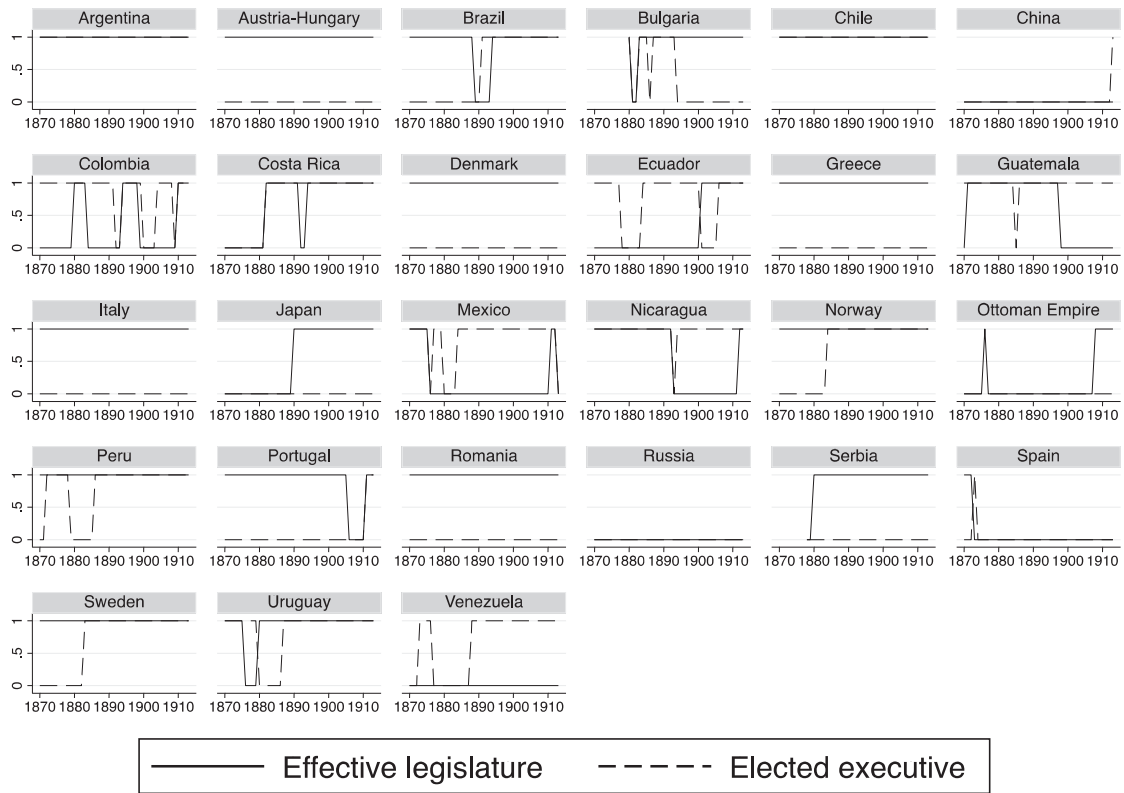
Fig. 1. Dummy variables on polity, 1870–1913. *Note: Effective legislature* takes the value of one when there is a legislature that effectively limits the power of the executive, and zero otherwise. *Elected executive* takes the value of one when the executive is elected directly or indirectly, and zero otherwise. *Source*: Banks and Wilson (2020), *Statesman's Yearbook* (1870–1913).

past resemble today's illiberal systems such as Turkey and Venezuela, which contrast with modern democracies headed by presidents, such as the United States. What is more, presidential democracies would be autocratic under the criterium adopted by North and Weingast (1989) and the literature that followed it, which focuses on the legislature's power to limit the executive rather than on elections. For these reasons, we consider presidential democracy more authoritarian than constitutional monarchy. We thus work with a democracy ranking that follows the order in which these polity groups were presented above: ranging from (1) true autocracy, the least democratic, to (4) true democracy, the most democratic. We use this ranking in an event study shown at the end of this article.

Following this group typification, we condense the variables *elected executive* and *effective legislature*, shown in Fig. 1, into the following new dummy variables:

$$\textit{True autocracy}_{it} = \begin{cases} 1 & \text{when } \textit{elected executive}_{it} = 0 \text{ and } \textit{effective legislature}_{it} = 0; \\ 0 & \text{otherwise}. \end{cases}$$

$$\textit{Presidential democracy}_{it} = \begin{cases} 1 & \text{when } \textit{elected executive}_{it} = 1 \text{ and } \textit{effective legislature}_{it} = 0; \\ 0 & \text{otherwise}. \end{cases}$$

$$\textit{Constitutional monarchy}_{it} = \begin{cases} 1 & \text{when } \textit{elected executive}_{it} = 0 \text{ and } \textit{effective legislature}_{it} = 1; \\ 0 & \text{otherwise}. \end{cases}$$

$$\textit{True democracy}_{it} = \begin{cases} 1 & \text{when } \textit{elected executive}_{it} = 1 \text{ and } \textit{effective legislature}_{it} = 1; \\ 0 & \text{otherwise}. \end{cases}$$

Fig. 2 shows how, following the criteria above, we characterize the countries in our sample by polity groups from 1870 to 1913. Notoriously autocratic polities such as Russia and the Ottoman Empire are unsurprisingly classified as *true autocracies*.[8] Countries

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[8] This result finds support in the contemporary press. We use the "term frequency" tool of Gale and include variations of dictatorship, autocracy, despotism, and tyranny as our choice of words because they reflect the historical terminology about autocratic regimes. These terms appear in the *Economist* in 96 articles, from 1870 to 1913, mainly in respect to Russia (30% of the total), but also substantially to the Ottoman Empire (13%).
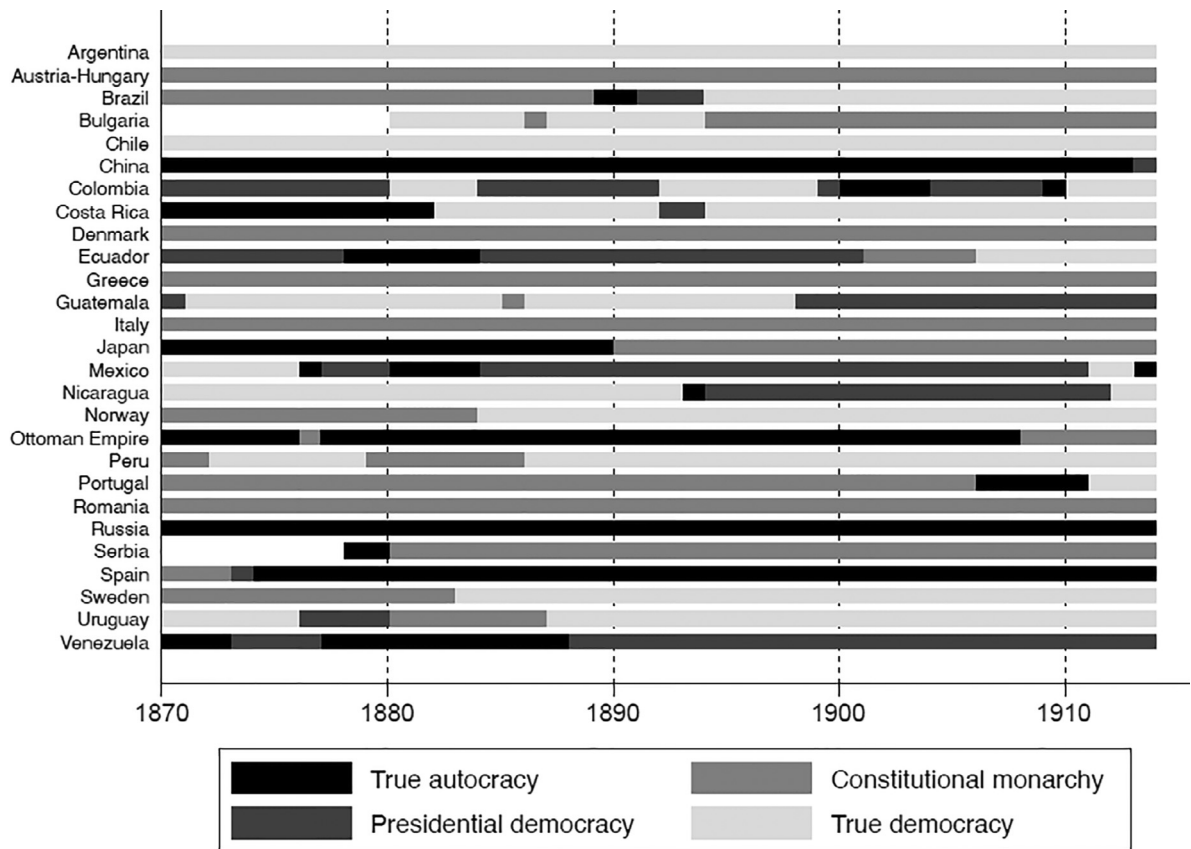
Fig. 2. Polity per country: 1870–1913. *Note:* True autocracy: unelected head of state governs without legislature constraint; Presidential democracy: head of state is elected but is not limited by the legislature; Constitutional monarchy: the head of state is not elected but an effective legislature limits his powers; True democracy: elected head of state limited by the legislature. *Source:* Banks and Wilson (2020); *The Statesman's Yearbook* (1870–1913).

under typical dictators who staged elections but governed without legislative tutelage, such as Porfirio Díaz's Mexico and Zelaya's Nicaragua, appear as *presidential democracies.* Austria-Hungary is a famous example of *constitutional monarchy.* As stressed above, *true democracies* may not have been democratic by today's standards, which explains why this typification is attributed to countries like Argentina and Costa Rica. Although imperfect, however, elections and Congress did play significant roles in those countries' political processes (Botana, 1994; Alonso, 2000).

Two conclusions stand out from Fig. 2. Firstly, the number of true democracies increased substantially over the years. Constitutional monarchies were the most stable polity group, even though their numbers declined slightly. Yet the most considerable decline happened with true autocracies, which fell from nine to three. Some of these extremely authoritarian polities became presidential democracies, which is the smallest group in our sample and only involved Latin American countries. The second conclusion imposes a limitation to our methodology: in most cases, regimes did not vary substantially. Across the 44 years we study, eight countries maintained their polities and thirteen had a dominant regime type.[9] The remaining six countries (Brazil, Bulgaria, Colombia, Ecuador, Guatemala, and Nicaragua) switched between three or four regimes throughout the period.

4.3. Political stability variable

We construct a variable that captures the duration of political stability in every country across time. The variable is defined as the years since the last time a civil war or coup d'état took place. More formally, we set the variable *years of domestic peace*$_{it}$ = 1 when a coup or a civil war happened in country i in year $t = t_0$. Assuming that such events do not happen in the year $t = t_0 + n$, where $n = \{1, 2, \ldots\}$, then *years of domestic peace*$_{it}$ = $n + 1$ for every t.

Fig. 3 shows the value of *years of domestic peace* for our sample during the period of analysis. The dataset goes back to the 1810s, which allows us to capture political stability in the long run. It also explains why *years of domestic peace* takes values well above one for some countries in 1870. In the Portuguese case, for example, *years of domestic peace*$_{1870}$ = 36, which is the number of years since

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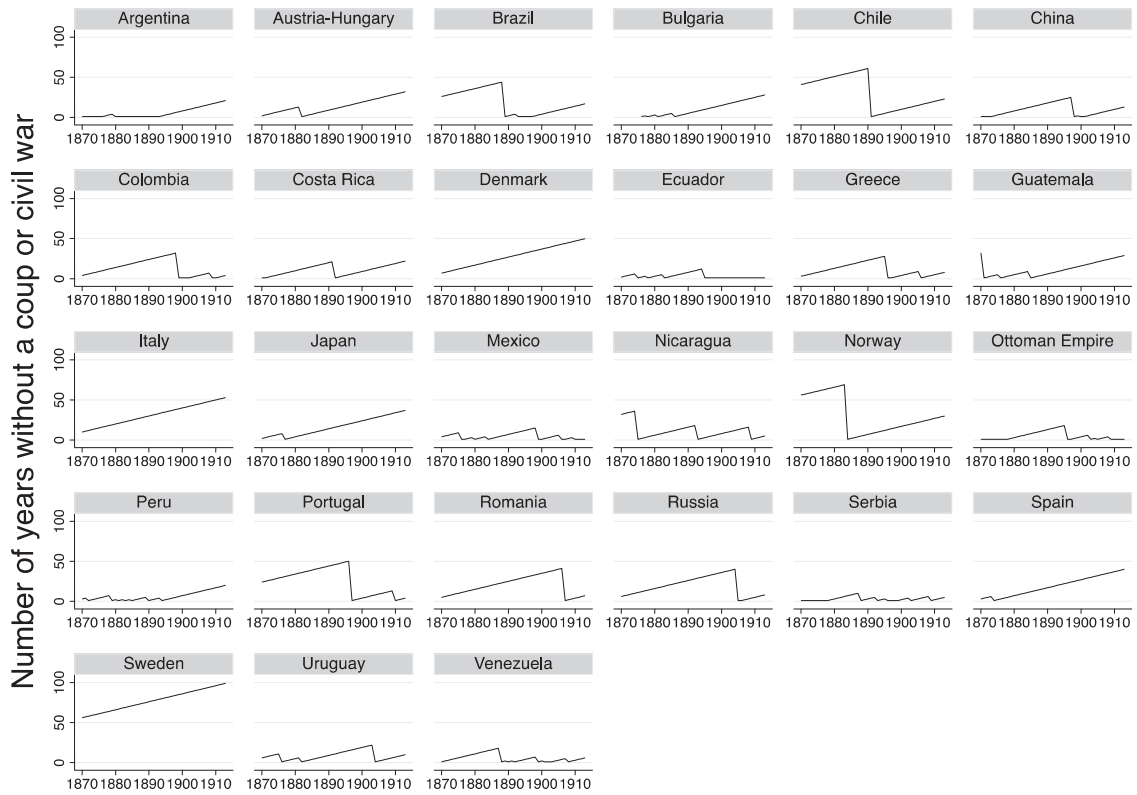[9] This is defined as having more than 30 years with one regime type.

Fig. 3. Political stability: *Years of domestic peace*, 1870–1913. *Note: Years of domestic peace* takes the value of one whenever countries are in civil war or a coup d'état takes place, two in the first peaceful year after any of these events, three in the second peaceful year, and so on. *Source*: Calculated from Sarkees and Wayman (2010), Banks and Wilson (2020).

the end of the 1832–34 Portuguese Civil War. *Years of domestic peace* fluctuates close to one in particularly unstable countries, such as Serbia. On the other extreme, Sweden is the most stable country in our sample: it was undisturbed by either coups or civil wars from the beginning of the series. For Sweden, then, we set *years of domestic peace*$_{1870}$ = 54, meaning that the variable took the value of one just after the end of the Napoleonic Wars.[10]

4.4. Financial variables

We control our model for a number of financial variables. In line with the literature, we control for default by introducing the dummy *years on default*$_{it}$ = 1 when the government in country i fails to meet its external financial obligations in time t, and 0 otherwise. The analogous dummy variable *years on gold*$_{it}$ controls for the time t when country i is on the gold standard. *Debt/tax*$_{it}$ is the ratio between the stock of sovereign debt the central government in country i owes in London and the tax revenue it collects in time t. The higher this ratio, the heavier the debt burden. *Exports/population*$_{it}$ is a measure of openness of the economy. It is defined as the ratio between exports and population of i in t. *Share of exports to Britain*$_{it}$ is the exports the country i sells to Britain in proportion to its total exports. It captures any special relationship between the borrowing countries and Britain.[11] Finally, *market spread*$_{it}$ controls for global market conditions; it is the debt-weighted average of spreads for 27 countries in our sample for each year t, excluding the country i from the sample.[12] We report the summary statistics and data sources in Appendix 2.

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[10] One issue with the political stability variable may be the potential for endogeneity with the political regime indicators. We address this issue by not including the polity and political stability indicators in the same model in section 5.2.

[11] As noted in footnote 2, we do not include GDP as a control variable in our main specifications. Yet we do so as a robustness check (Table 3), and the results remain the same overall. Ferguson and Schularick (2006) control for trade openness with exports/population whereas Flandreau and Zumer (2004) and Accominotti et al. (2011) compute it relative to Britain. We adopt both approaches.

[12] We have also conducted our analysis with the market-capitalization weighted average of bond spreads from Mauro et al. (2006), as well as British capital calls from Stone (1999), and the results do not change significantly.

4.5. Baseline model and empirical limitations

Our baseline model is:

$$log\,R_{it} = \ \theta_1 Polity_{it} + \theta_2 Peace_{it} + \theta_3 \chi_{it} + \alpha_{region} + \beta_t + \varepsilon_{it} \tag{2}$$

where bond spread is the dependent variable as in Eq. (1); it enters into the model with logarithmic values to symmetrize the residuals, given the wide variation in spreads across time and countries. $\alpha_{regions}$ and β_t are the regional and time fixed effects, respectively. *Polity* refers to the polity dummy variables, both non-interacted (*elected executive* and *effective legislature*) and interacted in polity groups (*true autocracy, presidential democracy, constitutional monarchy*, and *true democracy*). *Peace* is the value of *years of domestic peace* for country j at year t, χ are the financial control variables,[13] and ε_{jt} is the error term.

We face two main empirical challenges. Firstly, most polities were stable across time in our sample. Hence, time-invariant polity variables prevent the model from being controlled by country fixed effects. Including country fixed effects would only take into account within country variance, disregarding between country variation (Beck, 2011; Plümper et al., 2005).[14] Moreover, a simple random effects model shows serial and contemporaneous correlation of errors and groupwise heteroscedasticity.[15] We partially address this problem by including regional fixed effects. The regions are defined based on geographical proximity: Asia (China and Japan), Caribbean Latin America (Colombia, Costa Rica, Guatemala, Mexico, Nicaragua, and Venezuela), non-Caribbean Latin America (Argentina, Brazil, Chile, Ecuador, Peru, and Uruguay), Eastern Europe (Austria-Hungary, Bulgaria, Greece, the Ottoman Empire, Romania, Russia, and Serbia), Northern Europe (Denmark, Norway, and Sweden), and Southern Europe (Spain, Italy, and Portugal).[16]

The second problem is potential endogeneity between our polity and peace variables. Dictators often took power through coups and, once in office, threw their countries into civil wars, using violence to make their regimes more repressive. Inversely, those fighting for democracy often needed to take up arms to get rid of autocratic regimes.[17] This is reflected in our dataset: 20.7% of the polity changes we analyze happened during civil wars and 17.2% when coups took place in the respective country. Although most polity changes happened in peaceful years, a large proportion coincided with violence (when *years of domestic peace* = 1). We do not claim that there was always a causal relationship between political violence and polity change in these cases, but the numbers indicate that there is room for endogeneity. In Section 5.2 we address this problem by running our baseline regression without *years of domestic peace* and with an alternative variable that captures external rather than domestic wars. These changes do not alter results significantly, indicating that endogeneity is unlikely to be a driver of our main conclusions.

5. Results and discussion

5.1. Baseline model

Table 1 shows the results of our baseline specifications with standardized beta coefficients and standard errors in parentheses.[18] In specifications 1 and 2, we regress spreads on the two polity dummy variables *effective legislature* and *elected executive*, which we have described in Fig. 1. The coefficient of *effective legislature* is positive and significant, which suggests that limited governments borrowed more expensively in London. The size of the coefficient suggests that spreads were 5.5% higher in countries where the legislature was effective. This effect is considerable: it is equivalent to 34.1% of being in default, which notoriously damages countries' credit.[19] However, *elected executive* is not significant, which implies that the impact of elections on credit costs may be conditional upon the presence of a legislature.

In specifications 3 to 5, we introduce the four polity groups described in the previous section. With respect to *true autocracy* (the omitted group), the coefficients of *true democracy* and *constitutional monarchy* are positive and significant. These are the two polity

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[13] The financial control variables are *debt/tax revenue, exports/population*, and *share of exports to Britain* at 3-years moving averages.

[14] There is an extensive literature on this issue within the empirical political science literature beyond what is cited in the text.

[15] We run a Hausman test to see whether unique errors are correlated with regressors in order to decide between fixed or random effects.

[16] We divide Latin America using the Caribbean because of the growing influence of the United States in that region during our period of analysis. In comparison, the United States was less present elsewhere in the continent, where Britain remained the main trade and financial player (Smith, 1986).

[17] A good example is Nicaragua under the authoritarian rule of José Santos Zelaya (1893–1909). Zelaya was the leader of a liberal alliance against the unconstitutional reelection of Roberto Sacasa (1889–1893). Once in power, Zelaya sent troops to invade Mosquito, then a British protectorate, and Honduras. Meanwhile, at home, he imposed an autocratic constitution and persecuted his former liberal allies. Zelaya continued to wage wars for the rest of his presidency, instrumentalizing these conflicts to build a regime he referred to as "honest dictatorship" (Cruz, 2005: 94). Another example is the Young Turk Revolution of 1908, a revolt initiated by army officers that brought an end to the autocratic rule of the Abdulhamid II and gave the representative assembly the power to pass legislation over the Sultan's authority (Brown 2002: 23–26).

[18] Standardized or beta coefficients are calculated as $B_i = \frac{s(x_i)}{s(y)} \times \theta(x_i)$, where y and x_i are the dependent and the independent variables, respectively. $\theta(x_i)$ is the unstandardized regression coefficient, $s(x_i)$ is the standard deviation of the independent variable x_i, and $s(y)$ is the standard deviation of the dependent variable y. Because beta coefficients are all in the same standardized units, they express the number of standard deviations by which the dependent variable changes with one standard deviation increase in the independent variable.

[19] The elasticity of our variables of interest in relation to the dependent variable *spread* is given by: $\epsilon_i = (e^{B_i \times \frac{s(x_i)}{s(y)}}) - 1$, where B_i is the standardized coefficient reported in Table 1, $s(x_i)$ is the standard deviation of the independent variable x_i, and $s(y)$ is the standard deviation of the dependent variable y, in our case log bond spreads. Appendix 2 reports the standard deviations for all variables.

Table 1
Baseline OLS regression results.

Variables	(1)	(2)	(3)	(4)	(5)
Effective legislature	0.123***				
	(0.057)				
Elected executive		-0.018			
		(0.058)			
True democracy			0.098***	0.115***	0.130***
			(0.087)	(0.082)	(0.069)
Presidential democracy			0.012	0.026	0.001
			(0.085)	(0.079)	(0.075)
Constitutional monarchy			0.140***	0.138***	0.170***
			(0.069)	(0.066)	(0.064)
Years of domestic peace	−0.301***	−0.280***	−0.304***	−0.302***	−0.081***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years on default	0.487***	0.485***	0.492***	0.471***	0.403***
	(0.076)	(0.081)	(0.081)	(0.078)	(0.062)
Years on gold	−0.095***	−0.082***	−0.093***	−0.069**	0.026
	(0.070)	(0.069)	(0.069)	(0.069)	(0.054)
Debt/tax	0.098***	0.092***	0.092***	0.080***	0.082***
	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)
Exports/population	−0.077***	−0.025	−0.063**	−0.050*	0.149***
	(0.010)	(0.011)	(0.013)	(0.011)	(0.013)
Share of exports to UK	−0.081***	−0.065**	−0.080***	−0.100***	0.108***
	(0.125)	(0.129)	(0.123)	(0.125)	(0.109)
Market spread	0.196***	0.198***	0.196***		
	(0.054)	(0.054)	(0.053)		
Observations	974	974	974	974	974
R-squared	0.548	0.537	0.549	0.598	0.744
Time FE	NO	NO	NO	YES	YES
Region FE	NO	NO	NO	NO	YES

Note: Dependent variable is log values of market-weighted bond spreads at annual frequency (mean = 1.30). Standardized beta coefficients. Robust standard errors in parentheses. Constants are significant at *** $p < 0.01$, **$p < 0.05$, * < 0.1.
Source: Appendix 1.

groups in which the legislature limits the executive, in line with the result for *effective legislature* in specification 1. Once again, the size of the effects is considerable: specification 5, our baseline model, suggests that *true democracies* faced 5.7% higher spreads than *true autocracies*. This is the equivalent of 35.4% of the penalty imposed on defaulters, which, from the results in specification 5, increases spreads by 16.3%. The effect with *constitutional monarchy* is even higher: 7.9%. *Presidential democracy* is not significant.

The coefficients of the control variables have the expected sign and do not show any correlation with our polity and political stability variables. Particularly notable is the variable *years of domestic peace*, which has a negative and significant coefficient, suggesting that creditors were averse to political violence, as one would expect. We control for *market spread* and for time fixed effects separately, since both variables capture time-specific conditions common to all countries. Results hold irrespectively. The same applies to the regional fixed effects, whose introduction increases the polity variables' coefficients.

5.2. Robustness checks

Robustness checks confirm the baseline results shown above. Table 2 introduces different definitions of polity and peace variables. In specifications 1 to 3, we use alternative definitions of polity variables from the Polity IV project (*executive constraint, executive recruitment*, and the *Polity index*). In contrast with the polity variables in our baseline regressions, these indexes rely on modern data and methods, and thus may not reflect how contemporaries perceived political systems. Another difference is that they are continuous rather than dummy variables. We have normalized these variables so that they take values from 1 to 3, with higher values indicating a more democratic regime type. *Executive recruitment* captures how political leaders come to occupy their positions. It combines three indicators of the degree to which the method of choosing heads of states was institutionalized, competitive, and open to society as a whole. Free and fair elections, in which any party may run, receive the highest score, while autocratic transfers of power between leaders, such as the inheritance of the throne in absolutist monarchies, receive the lowest. *Executive constraint* captures how much the legislature can limit the power of the executive. Strong parliaments are associated with high scores, in opposition to polities with no legislative power at all. Countries with weak parliaments are between these extremes. The variable *Polity index* is a combination of the above with a third variable that captures civil liberties (Marshall et al., 2017).

The Polity IV variables have positive and significant coefficients, which supports our conclusion that democracies were associated with higher spreads. Results only differ for elections: while we have not found any results on this in our baseline models, in this robustness check *executive recruitment* is positive and significant. Hence, the conclusion that democracies borrowed more expensively

Table 2
OLS regressions with different polity and peace variables.

Variables	(1)	(2)	(3)	(4)	(5)
Polity index	0.099***				
	(0.038)				
Executive constraint		0.051***			
		(0.025)			
Executive recruitment			0.081***		
			(0.037)		
True democracy				0.125***	0.125***
				(0.070)	(0.070)
Presidential democracy				0.006	0.006
				(0.076)	(0.076)
Constitutional monarchy				0.156***	0.156***
				(0.063)	(0.063)
Years of domestic peace	−0.035*	−0.058***	−0.055***		
	(0.001)	(0.001)	(0.001)		
Years of external peace					−0.002
					(0.001)
Observations	971	938	938	974	974
R-squared	0.732	0.708	0.702	0.741	0.741
Controls	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES

Note: Dependent variable is log values of market-weighted bond spreads at annual frequency (mean = 1.30). Standardized beta coefficients. Robust standard errors in parentheses. Controls include years on default, years on gold, debt/tax, exports/population, share of exports to UK. Constants are significant at *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: Appendix 1.

than autocracies is stronger when we use Polity IV's modern indexes instead of our main variables, which have been constructed from contemporary sources.

We next consider variations of the political instability variable that, as explained in Section 4.3, may be endogenous to the polity variables. In specification 4, we run our baseline model (from Table 1, column 5) without the variable *years of domestic peace*. Overall, results remain the same, with the variables *true democracy* and *constitutional monarchy* positive and significant. According to this new specification, *true democracies* had spreads 5.5% higher than *true autocracies*. This effect is almost the same as the figure we found in our baseline model (5.7%). This exercise does not allow us to rule out the hypothesis that there is endogeneity between our polity and peace variables. However, it strongly suggests that endogeneity is not driving our main conclusions: results hold with and without *years of domestic peace*.

In model 5, we introduce a different peace variable, *years of external peace*, which captures the effect of external conflicts. This variable is constructed in a similar manner to *years of domestic peace*, but it takes into account interstate rather than domestic civil wars. It takes the value of 1 in the year *t* when country *i* wages wars with other states, 2 in the first peaceful year after such conflict, and so on. The coefficient is very small and not significant, which makes sense because conflicts in a foreign land were less harmful to economic activity than civil wars, while victorious countries could profit from the economic exploitation of conquered territories and war reparations, both common practices at the time.[20]

The most important conclusion of specification 5 is that our variables of interest *true democracy* and *constitutional monarchy* remain significant. This is another indication that endogeneity is not driving our main results, since *years of external peace* is arguably less endogenous than *years of domestic peace*. The decision of going to war abroad depended not only on the domestic but also on foreign governments. Hence authoritarian belligerent leaders were more likely to use civil than interstate wars to manipulate the political systems at home.

Table 3 shows a second round of robustness tests that include additional variables and a different estimation procedure. In specification 1, we take financial intermediaries into account by introducing two dummy variables that control for countries under international financial controls and whose debt was underwritten by Rothschilds. International financial controls emerged following defaults, when bondholders' representatives or their governments took charge of economic policy in indebted countries (Mitchener and Weidenmier 2010; Tunçer 2015). In our sample, that was the case of Bulgaria, China, Greece, Guatemala, Nicaragua, the Ottoman Empire, Peru, Serbia, and Venezuela in different years. Rothschilds was the world's most prestigious debt underwriter and did business with a relatively small number of governments: from our sample, these include Austria-Hungary, Brazil, Chile, Italy, the Ottoman Empire, Russia, and Spain.[21] Apart from Chile, which joined the club relatively late, the Rothschilds club was composed only of

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[20] Japan is a notorious case of a belligerent nation that profited from military victories (Suzuki, 1994).
[21] London House of Rothschild (1905).

Table 3
Robustness checks: additional controls and model.

Variables	(1)	(2)	(3)	(4)
True democracy	0.110***	0.094***	0.130***	0.130***
	(0.066)	(0.068)	(0.069)	(0.071)
Presidential democracy	−0.040	−0.023	0.0003	0.001
	(0.075)	(0.074)	(0.075)	(0.080)
Constitutional monarchy	0.147***	0.123***	0.166***	0.170***
	(0.058)	(0.066)	(0.063)	(0.062)
Years of domestic peace	−0.032*	−0.070***	−0.080***	−0.081***
	(0.001)	(0.001)	(0.001)	(0.001)
Rothschilds	−0.177***			
	(0.067)			
International financial control	0.005			
	(0.092)			
Log GDP		−0.129***		
		(0.017)		
Log GDP per capita			−0.016	
			(0.058)	
Observations	974	974	974	974
R-squared	0.761	0.750	0.744	0.744
Model	OLS	OLS	OLS	PCSE
Controls	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES

Note: Dependent variable is log values of market-weighted bond spreads at annual frequency (mean = 1.30). Standardized beta coefficients. Robust standard errors in parentheses. Controls include years on default, years on gold, debt/tax, exports/population, share of exports to UK. Constants are significant at *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Panel-corrected standard errors (PCSE) model (4) retains OLS parameter estimators, but replaces OLS standard errors for PCSEs, which takes into account the groupwise heteroscedasticity for 27 groups (countries).
Source: Appendix 1.

monarchies in the 1870s.[22] These controls do not substantially change the coefficient of the variables of interest. The coefficient of the Rothschilds dummy is negative and significant, as one would expect from the literature.[23] International financial control is not significant. This may be because its positive impact on bond spreads was conditional upon the domestic politics of the host country.[24]

In specifications 2 and 3, we control our baseline result for the logarithms of GDP and GDP per capita, both of which are measured in PPP dollars of 2011. Even though these variables are ahistorical, they are useful to account for variation in the size of the economies (say Russia in opposition to Costa Rica) and living standards (rich Argentina and poor China, for example) in our sample. The main results hold. In specification 2, which controls for log GDP, the effect of *true democracy* is 9.7%. The introduction of GDP per capita, in specification 3, does not alter the polity variables' coefficients. Finally, in column 4, we run our baseline specification with panel-corrected standard errors (PCSE).[25] The results remain the same.

5.3. Event study

So far our analysis has focused on average correlations between bond spreads and political regime types. However, the dynamics of regime changes may also be informative. In this section, we run a panel event study drawing on our baseline model.[26] Our goal in this exercise is to estimate the impact of regime changes that took place in different countries of our sample between 1870 and 1913. We define events as *democratization*, which happened when countries switched to a more democratic polity, as opposed to the

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[22] Ferguson (1998) claims that the bank's owners preferred monarchies to republics, which may explain why they picked monarchic Brazil as their only Latin American client in the 1820s.

[23] Flandreau and Flores (2009) propose that Rothschilds guided bondholders by associating its premier default-free "brand" only with creditworthy borrowers.

[24] In the case of the Middle East and the Balkans, Tunçer (2015) shows that authoritarian Ottoman Empire was able to implement the reforms proposed by the international financial control more easily than democratic Greece. A similar point can be made for Peru, where the autocratic regime could cede the control of key natural resources to British bondholders with limited political challenge (Vizcarra 2009; Sicotte et al. 2010).

[25] The PCSE model retains OLS parameter estimators, but replaces OLS standard errors for PCSEs, which takes into account the contemporaneous correlation of the errors and groupwise heteroscedasticity. For examples of its use in economic history research, see Karaman and Pamuk (2013), Meissner (2005), and Ferguson and Schularick (2012).

[26] Panel event studies are widespread in policy research, and in contrast with those of finance, which usually rely on high-frequency data, it helps to analyze the events that may not create an immediate impact in time series in a cross-sectional context (Clarke and Schythe, 2020).
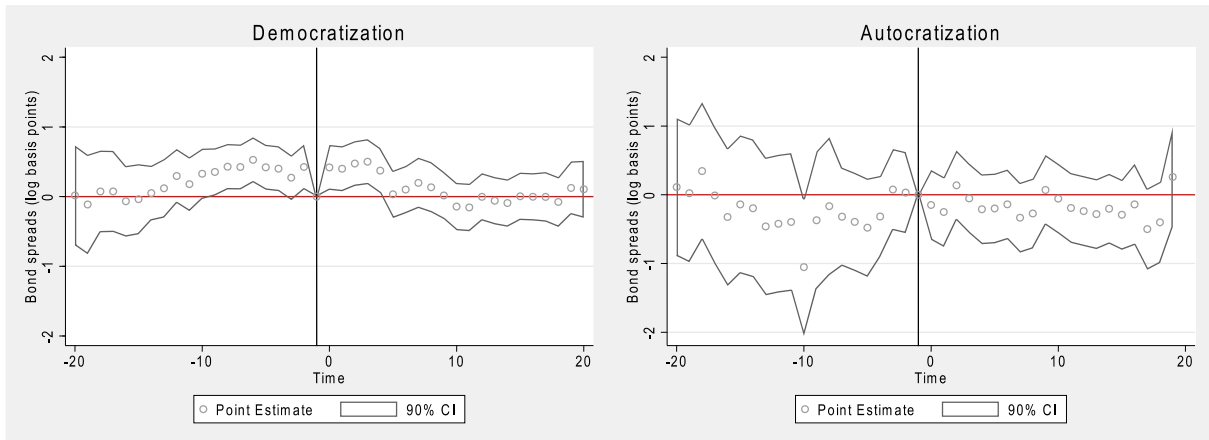
Fig. 4. Panel event study: the impact of regime changes on bond spreads. *Note*: Events are defined as changes in polity type, which happen in year 0. The treatment group are countries in which new polity types remain stable for at least ten years after the regime change. Democratization happens when polities become more democratic after year 0, and the inverse applies to autocratization. The order of polities, from less to more democratic, is: true autocracy, presidential democracy, constitutional monarchy, and true democracy. Full regression results are reported in Appendix 5. *Source*: Appendix 1.

event of *autocratization*.[27] The treatment countries are defined as those in which polity changed and remained the same until 1913.[28] Therefore, we do not include countries that continuously switched across polity groups. For example, Colombia had the most frequent polity changes in our sample and thus is not included in the event study.[29] We also exclude events that happened after 1903, in the final 10 years of our period. We do not apply such stringent conditions in the years that preceded the events, since regimes often changed several times before they stabilized as a single polity.[30] In each event, the control group consists of the countries where no regime change took place. We have a relatively larger number of *democratization* events because that was the historical trend during our period of analysis.[31] As for *autocratization*, we have a more limited treatment group, consisting of only four countries.[32]

After we identify the events, the treatment, and the control groups, we then calculate the lags and leads of the events. We use the panel event study as the extension of our baseline OLS regression, as outlined in Table 1, column 5. This is the most historically consistent specification since it includes all our controls, as well as year and region fixed effects. Finally, we include a "post-event" indicator for all periods after the regime change in the treated countries. The estimated model is the following:

$$\log R_{it} = \sum_{l=2}^{L} \beta_l (Lag\ l)_{it} + \sum_{k=2}^{K} \gamma_k \left(Lead\ k\right)_{it} + \chi_{it} + \alpha_{regions} + \beta_t + \varepsilon_{it} \tag{3}$$

Besides the lags and leads terms, all variables are defined as in Eq. (1). Lags and leads are binary variables indicating that country *i* was a given number of years away (respectively before and after) from the event of *democratization* or *autocratization*. *l* and *k* are respectively lags and leads. A single lag or lead variable is omitted to capture the baseline difference between areas where the regime change does and does not occur.[33]

We report regression results in Appendix 5. Fig. 4 shows point estimates along with their 90% confidence intervals. The baseline (omitted) period is one year prior to the regime change and is indicated by a solid vertical line. In the case of autocratization, leads and lags variables are not statistically significant. Democratization, however, is associated with significantly higher spreads nine years before and five years after the transition. This suggests that creditors punished countries that went through processes of democratization, which often started prior to the transition due to political uncertainty and lasted until the polity got stabilized.

This is consistent with our findings so far. However, these results should be interpreted with caution because of limitations in our sample. Our treatment and control groups are not perfectly balanced, and the number of countries is small. Moreover, some

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[27] As explained in the previous section, we characterize the polity group according to the following order, from least to most democratic: (1) *true autocracy*, (2) *presidential democracy*, (3) *constitutional monarchy*, and (4) *true democracy*. Hence, *democratization* happened whenever countries moved from 1 to 4, even when they switched to neighboring groups (say, from *presidential democracy* to *constitutional monarchy*), while *autocratization* happened when the transition was from 4 to 1.

[28] The only exception was Nicaragua, which had another polity change in 1912.

[29] See Fig. 2.

[30] If we were to introduce countries that had only one regime change (for example, Japan in 1890), there would be three events to run this exercise.

[31] Brazil (1894), Costa Rica (1882), Japan (1890), Norway (1884), Peru (1886), Sweden (1883), Uruguay (1887), and Venezuela (1888). Event years are in parentheses.

[32] Bulgaria (1894), Guatemala (1898), Nicaragua (1894), and Spain (1874). Event years are in parentheses.

[33] We conduct this analysis with the eventdd command in STATA, as outlined by Clarke and Schythe (2020). Our specification is also based on the general specification by Clarke and Schythe (2020: 4).

of our countries switched across polity groups before time 0 but maintained the same polity after that. In other words, our sample includes countries which showed polity variation before the event but not after. This is particularly a concern in the event study on democratization because most countries that democratized had frequent polity changes. In the short run, bondholders reacted negatively to these changes, and the event study on democratization is likely to be capturing this reaction several years *before* time 0.[34] Nevertheless, this data-driven bias does not apply to the period *after* the breaks. Thus, the event study suggests that democratization led to significantly higher spreads for another five years, when polities were durable.

6. Conclusion

This article shows that autocracies were associated with lower borrowing costs in London than democracies during the first financial globalization between 1870 and 1913. Even though it does not claim causality, the exercise suggests strong association between low credit risk and unelected governments that ruled without parliamentary tutelage. Following the language of contemporaries, we call such polities "true autocracies". In association with primary qualitative evidence, these results suggest that creditors preferred autocratic regimes, since they were perceived as more capable of promoting political stability than democratic ones.

This result is at odds with the influential democratic advantage hypothesis, according to which democratic institutions lead to lower borrowing costs. The notion of democratic advantage has been widely discussed for early modern Europe, with the British parliamentary system being presented as a model. There are two main differences between early modern Europe and the countries and period we analyze. Firstly, eighteenth-century Britain could be characterized as a constitutional monarchy because it lacked free and fair elections. In contrast, by the turn of the twentieth century, elections were a common institution across the world, including in some of the countries that make up our sample. Hence, we should be cautious when comparing these two periods. The evidence presented above suggests that there was no democratic advantage for capital-importing countries during the first financial globalization, based on what contemporaries understood by "true democracies" at the time.

A second contrast between our period and early modern Britain may explain why that was so: in an era of globalization, the countries we analyze borrowed extensively abroad, whereas pre-modern European sovereigns did so mainly at home. The empowering of voters and legislators may increase the power of domestic creditors, as proposed by the democratic advantage hypothesis. Yet one may assume that democratization *reduces* the capacity of foreign creditors to influence the actions of governments, since those governments become more accountable to domestic taxpayers. Foreign bondholders do not vote and are not represented in the Parliaments of the indebted countries.

In this paper, we do not discuss the long-term consequences of accessing relatively lower costs of borrowing for the autocratic regimes, especially with regards to the development of domestic financial institutions and fiscal capacity. Arriving at a precise understanding of the political economy of sovereign debt during the first financial globalization – involving foreign creditors, legislators, policymakers, and taxpayers – requires case studies that are beyond the scope of this article, but the results presented here make the case for future research along such lines.

Acknowledgments

Appendix 1. Data sources

Spreads: Investor's Monthly Manual (1870–1913).
Polity variables: Statesman's Yearbook (1870–1913), Banks and Wilson (2020), Polity IV project.
Years of domestic peace, years of external peace: Sarkees and Wayman (2010), Banks and Wilson (2020), Clodfelter (2002).
Years on default: Corporation of Foreign Bondholders (1870–1913).
Years on gold: Reinhart and Rogoff (2009).
International trade: Federico and Tena-Junguito (2019).
State finances and tax revenue: Statesman's Yearbook (1870–1913), Flandreau and Zumer (2004), Tunçer (2015), Banks and Wilson (2020).
International financial control: Mitchener and Weidenmier (2010), Tunçer (2015).
Rothschilds: Investor's Monthly Manual (1870–1913), London House of Rothschilds (1905).
GDP per capita: Bolt and van Zanden, (2020).

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[34] We do not rule out the possibility of a pre-trend driven by another variable.

Appendix 2. Summary statistics

Table A
Summary Statistics, annual frequency.

Variable	Obs	Mean	Std. dev.	Min	Max
Bond spreads (weighted average)	999	7.17	11.55	0.26	99.17
log Bond spreads (weighted average)	999	1.30	1.07	-1.33	4.59
Effective legislature	1,170	0.66	0.47	0.00	1.00
Elected executive	1,170	0.44	0.50	0.00	1.00
True autocracy	1,188	0.20	0.40	0.00	1.00
True democracy	1,188	0.30	0.46	0.00	1.00
Presidential democracy	1,188	0.13	0.34	0.00	1.00
Constitutional monarchy	1,188	0.35	0.48	0.00	1.00
Polity IV	1,176	1.81	0.53	1.00	3.00
Executive constraint	1,140	1.80	0.74	1.00	3.00
Executive recruitment	1,142	1.86	0.75	1.00	3.00
Years without coup or civil war	1,182	16.80	18.37	1.00	99.00
Years without interstate war	1,182	31.21	27.51	1.00	105.00
Rothschilds	1,188	0.15	0.36	0.00	1.00
International financial control	1,172	0.14	0.35	0.00	1.00
GDP per capita (in 2011 USD)	1,172	1,931.53	1,181.88	424.00	6,687.00
GDP (in 2011 USD)	1,172	40,174.73	80,947.37	339.53	482,538.30
log GDP (in 2011 USD)	1,172	9.07	1.77	5.83	13.09
log GDP per capita (in 2011 USD)	1,172	7.39	0.60	6.05	8.81
Years on default	1,180	0.20	0.40	0.00	1.00
Years on gold	1,180	0.26	0.44	0.00	1.00
Debt/tax revenue	1,123	3.68	2.88	0.00	21.05
Exports/population (£)	1,170	2.41	2.45	0.05	14.62
Share of exports to Britain (3-years-ma)	1,119	0.32	0.28	0.01	1.82
Market weighted spread	1,188	0.02	0.01	0.01	0.06

Table B
Summary statistics, monthly frequency with weighted averages of bond spreads.

Variable	Obs	Mean	Std. dev.	Min	Max
Bond spreads (weighted average)	11,855	6.44	10.01	0.00	115.31
log Bond spreads (weighted average)	11,721	1.26	1.02	-2.85	4.75
Effective legislature	11,855	0.62	0.49	0.00	1.00
Elected executive	11,855	0.44	0.50	0.00	1.00
True autocracy	11,855	0.23	0.42	0.00	1.00
True democracy	11,855	0.29	0.45	0.00	1.00
Presidential democracy	11,855	0.15	0.36	0.00	1.00
Constitutional monarchy	11,855	0.33	0.47	0.00	1.00
Polity IV	11,820	1.81	0.54	1.00	3.00
Executive constraint	11,402	1.83	0.75	1.00	3.00
Executive recruitment	11,417	1.84	0.74	1.00	3.00
Years without coup or civil war	11,855	17.75	18.93	1.00	99.00
Years without interstate war	11,855	31.27	28.28	1.00	105.00
Rothschilds	11,855	0.18	0.38	0.00	1.00
International financial control	11,855	0.13	0.34	0.00	1.00
GDP per capita (in 2011 USD)	11,855	2,035.68	1,206.18	424.00	6,687.00
GDP (in 2011 USD)	11,855	44,158.08	82,963.65	339.53	482,538.30
log GDP (in 2011 USD)	11,855	9.26	1.77	5.83	13.09
log GDP per capita (in 2011 USD)	11,855	7.45	0.59	6.05	8.81
Years on default	11,855	0.20	0.40	0.00	1.00
Years on gold	11,855	0.27	0.44	0.00	1.00
Debt/tax revenue	11,801	3.69	2.61	0.00	15.77
Exports/population (£)	11,855	2.53	2.56	0.05	14.62
Share of exports to Britain (3-years-ma)	11,617	0.29	0.25	0.01	1.77

Table C
Summary statistics, monthly frequency without weighted averages of bond spreads.

Variable	Obs	Mean	Std. dev.	Min	Max
Bond spreads	33,345	6.19	10.73	0.00	136.88
log Bond spreads	33,345	1.14	1.03	-5.49	4.92
Effective legislature	33,345	0.65	0.48	0.00	1.00
Elected executive	33,345	0.43	0.49	0.00	1.00
True autocracy	33,345	0.29	0.45	0.00	1.00
True democracy	33,345	0.35	0.48	0.00	1.00
Presidential democracy	33,345	0.07	0.25	0.00	1.00
Constitutional monarchy	33,345	0.29	0.46	0.00	1.00
Polity IV	33,288	1.80	0.58	1.00	3.00
Executive constraint	32,375	1.81	0.78	1.00	3.00
Executive recruitment	32,296	1.76	0.76	1.00	3.00
Years without coup or civil war	33,345	18.04	18.93	1.00	99.00
Years without interstate war	33,345	26.44	23.96	1.00	105.00
Rothschilds	33,345	0.30	0.46	0.00	1.00
International financial control	33,345	0.18	0.38	0.00	1.00
GDP per capita (in 2011 USD)	33,345	2,225.92	1,385.68	424.00	6,687.00
GDP (in 2011 USD)	33,345	57,840.88	95,218.79	339.53	482,538.30
log GDP (in 2011 USD)	33,345	9.73	1.68	5.83	13.09
log GDP per capita (in 2011 USD)	33,345	7.52	0.61	6.05	8.81
Years on default	33,345	0.17	0.38	0.00	1.00
Years on gold	33,345	0.34	0.47	0.00	1.00
Debt/tax revenue	33,250	3.91	2.82	0.00	15.77
Exports/population (£)	33,345	2.86	2.95	0.05	14.62
Share of exports to Britain (3-years-ma)	33,085	0.29	0.20	0.01	1.77

Appendix 3. List of bonds used in the monthly analysis

Argentina 3.5% External Loan of 1889	Hungary 3% State Loan of 1895
Argentina 4.5% Sterling Bonds	Hungary 4% Gold Rentes
Argentina 4% Bonds of 1897 (Law 3378 and 3783)	Hungary 5% Bond of 1871
Argentina 4% Bonds of 1897 (Law 3378)	Hungary 5% Bond of 1873
Argentina 4% Bonds of 1897 (Law 3655 and 3750)	Hungary 6% Gold Rentes
Argentina 4% Bonds of 1898	Italian Irrigation 6% Loan
Argentina 4% Bonds of 1898 Redeemable 1901	Italy 3.5% Rentes
Argentina 5% Bonds of 1884	Italy 5% Rentes of 1861
Argentina 5% Bonds of 1886–1887	Italy 5% State Domain Bonds of 1865
Argentina 5% Treasury Consolidation Bonds	Italy 6% Tobacco Loan of 1868
Argentina 6% Bonds of 1866–1868	Japan 4.5% Sterling Bonds of 1905 1st Series
Argentina 6% Funding Loan	Japan 4.5% Sterling Bonds of 1905 2nd Series
Austria 4% Gold Rentes	Japan 4% bonds of 1905
Brazil 4.5% Bonds of 1852–1858	Japan 4% Sterling Bonds of 1899
Brazil 4.5% Bonds of 1860	Japan 7% Bond of 1873
Brazil 4.5% Bonds of 1863	Japan 9% Customs Loan of 1870
Brazil 4.5% Bonds of 1883	Mexico 3% Bond of 1851
Brazil 4.5% Bonds of 1888	Mexico 3% Bond of 1864 (issued in arrears)
Brazil 4.5% Gold Loan of 1879	Mexico 5% Consolidated External Loan of 1899
Brazil 4% Bonds of 1889	Mexico 6% Consolidated Bonds
Brazil 5% Funding Bonds of 1898	Mexico 6% External Bonds of 1890
Brazil 5% Loan of 1859	Nicaragua 6% Bonds reduced to 4%
Brazil 5% Loan of 1865	Nicaragua 6% Sterling Loan of 1909
Brazil 5% Loan of 1871	Norway 3.5% Assenting Bonds of 1886
Brazil 5% Loan of 1875	Norway 3.5% Bonds English Scrip
Brazil 5% Loan of 1903	Norway 3% Bonds of 1888
Bulgaria 4.5% Bonds	Norway 4.5% Bonds of 1876
Bulgaria 5% Gold Loan of 1902	Norway 4.5% Bonds of 1878
Bulgaria 6% Bonds of 1888	Norway 4% Bonds of 1880
Bulgaria 6% Sterling Mortgage Bonds	Peru 5.5% Salt Loan
Chile 4.5% Bond of 1858	Peru 5% Bonds of 1865
Chile 4.5% Bond of 1885	Peru 5% Consolidated Loan of 1872
Chile 4.5% Bond of 1893	Portugal 3% Irredeemable Loan
Chile 4.5% Gold Loan of 1906	Portugal 3% Irredeemable Loan 1st Series Stamped
Chile 4.5% Loan of 1886	Portugal 3% Irredeemable New Loan of 1869
Chile 4.5% Loan of 1887	Portugal 3% Irredeemable New Loan of 1874
Chile 4.5% Loan of 1889	Russia 3.5% Bond
Chile 4.5% Loan of 1895	Russia 3% Bonds of 1859

Chile 5% Loan of 1870 (2% Sinking Fund First 5 Yrs)	Russia 4.5% Bonds of 1850
Chile 5% Loan of 1873	Russia 4.5% Bonds of 1860
Chile 5% Loan of 1875	Russia 4.5% Bonds of 1875
Chile 5% Loan of 1892	Russia 4% Rentes
Chile 5% Loan of 1896	Russia 5% Bonds of 1822
Chile 5% Loan of 1905	Russia 5% Bonds of 1862
Chile 6% Bond of 1867	Serbia 4% Unified Bonds
Chile 7% Bond of 1866	Spain 2% Bonds (overdue coupons)
China 4.50% Gold Bonds of 1898	Spain 3% Deferred Bonds
China 5% Bond of 1896	Spain 3% External Bonds (1% in Cash)
China 5% Registered Bond of 1896	Spain 4% External Bonds of 1882
China 6% Bond of 1885 Series A	Spain 4% Sealed Bonds
China 6% Bond of 1885 Series B	Sweden 3.5% Bonds of 1900
China 6% Gold Bond of 1895	Sweden 3% Bonds of 1889
China 6% Gold Bond of 1895 Redeem 1901–1915	Sweden 4.5% Bonds of 1876
China 7% Bond Series A	Sweden 4.5% Government Loan of 1864
China 7% Bond Series B	Sweden 4% Bonds of 1878
China 7% Silver Bond	Sweden 4% Bonds of 1880 1st Issue
China 8% Bond of 1874–1876	Sweden 5% Bonds of 1868
China 8% Bond of 1877	Ottoman 3.5% Scrip of 1894
Colombia 1.50% to 3% External Loan of 1896	Ottoman 4% Loan of 1902
Colombia 3% External Loan	Ottoman 4% Priority Loan of 1890
Colombia 4.50% New Granada Debt Converted Bond	Ottoman 4% Unified Bonds
Colombia 4.75% Bond of 1873	Ottoman 5% Customs Loan
Costa Rica 4% Bond of 1911	Ottoman 5% General Debt of 1865
Costa Rica 5% A, 3% in 1898–1904	Ottoman 5% Ottoman Deferred Bonds of 1877
Costa Rica 5%B, 4% in 1887, 2.5% in1898–1904	Ottoman 5% Secured Egyptian Bond of 1854
Costa Rica 6% Bond of 1871 First Issue	Ottoman 6% Bonds of 1869
Costa Rica 6% Bond of 1871 Second Issue	Ottoman 6% Bonds of 1873
Costa Rica 7% Bond of 1872	Ottoman 6% Egyptian Trust Bonds of
Danubian Provinces 7% Loan of 1864	Ottoman 6% Loan of 1862
Danubian Provinces 8% Loan of 1867	Ottoman 6% Loan of 1863–1864
Denmark 3% Amortisable Bonds of 1897	Ottoman 6% Loan of 1868
Denmark 3% Gold Bond of 1894	Ottoman 9% Treasury Bonds of 1872 Series A
Denmark 3% Stock of 1825	Ottoman Converted Bonds – Series B 1 %
Denmark 4% Bond of 1850–1861	Ottoman Converted Bonds – Series C 1 %
Denmark 4% Stock of 1862	Ottoman Converted Bonds – Series D 1 %
Ecuador 1% New Consolidated Bond	Uruguay 3.5% Bonds
Ecuador 4.5% New External Bonds Increasing to 5%	Uruguay 5% Bonds of 1896
Greece 4% Monopoly Loan	Uruguay 5% Consolidated bonds of 1905
Greece 4% Rentes	Uruguay 5% Unified Bonds of 1883
Greece 5% Funding Loan of 1893	Uruguay 6% Bonds of 1871
Greece 5% Loan of 1881	Venezuela 6% Bond of 1862
Greece 5% Loan of 1884	Venezuela 6% Bond of 1862 (issued for arrears)
Greece 5% Loan of 1890	Venezuela 6% Bond of 1864
Guatemala 4% Bond	Venezuela 3% Bond
Guatemala 4% External Loan	Venezuela 3% Diplomatic Debt
Guatemala 5% Bond	Venezuela 4% New Consolidated Bonds
Guatemala 6% Loan of 1869	

Source: Investor's Monthly Manual (1870–1913)

Appendix 4. Results at monthly frequency

4.1. Market value weighted average of bond spreads

Although monthly frequency data is available for most bond prices in London during our period, all our regressors are defined per year, including the variables that capture polity. For this reason, in the main text, we have annualized the spreads by averaging monthly data and conducted our empirical exercise at an annual frequency. This has the disadvantage of reducing the variation in our dependent variable. In this appendix, we run our main specification at a monthly frequency to check if annualization is somehow affecting our main results. The model is the same as in equation 1, but now time *t* refers to months rather than years. Hence, α_{it} is the time fixed effects for 527 months. As shown in Table D below, that is not the case: results hold at monthly frequency. With higher frequency, all coefficients become smaller than in our annual exercise. The effect of *true democracy* on spreads is 2.8%, according to specification 3, our preferred model in Table D, compared with 5.7% at annual frequency (our baseline model in Table 1). This reduction is expected, given that we are now varying the dependent variable monthly and repeating all covariates every 12 months. This is the reason why all other coefficients are also smaller. The best way to compare this effect with that found in our baseline model is by normalizing it for other variables, such as default. Seen in this way, the results do not change considerably: the effect of *true democracy* on spreads is 18.9% of the effect of *default*. Another difference with our annual results is that *presidential democracy* is now significant and positive, although less robust than the other polity groups.

Table D
Market value weighted average of bond spreads by country, monthly frequency.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Effective legislature	0.046***					
	(0.036)					
Elected executive		0.022				
		(0.062)				
True democracy			0.062*	0.056	0.055	0.062***
			(0.081)	(0.085)	(0.086)	(0.022)
Presidential democracy			0.050*	0.055*	0.053*	0.050***
			(0.083)	(0.083)	(0.085)	(0.025)
Constitutional monarchy			0.068***	0.055**	0.059**	0.068***
			(0.053)	(0.057)	(0.058)	(0.018)
Years of domestic peace	-0.079***	-0.068***	-0.078***			-0.078***
	(0.001)	(0.001)	(0.001)			(0.000)
Years of external peace					-0.069***	
					(0.001)	
Years on default	0.347***	0.347***	0.349***	0.354***	0.358***	0.349***
	(0.075)	(0.074)	(0.074)	(0.077)	(0.079)	(0.016)
Years on gold	0.001	0.003	0.005	-0.001	-0.015	0.005
	(0.054)	(0.055)	(0.057)	(0.056)	(0.061)	(0.015)
Debt/tax	0.180***	0.173***	0.177***	0.179***	0.185***	0.177***
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.003)
Exports/population	0.041*	0.057***	0.052***	0.061***	0.070***	0.052***
	(0.008)	(0.007)	(0.008)	(0.009)	(0.008)	(0.003)
Share of exports to UK	0.112***	0.117***	0.119***	0.123***	0.127***	0.119***
	(0.102)	(0.101)	(0.096)	(0.100)	(0.098)	(0.029)
Observations	11,477	11,477	11,477	11,477	11,477	11,477
R-squared	0.713	0.712	0.715	0.711	0.714	0.715
Model	OLS	OLS	OLS	OLS	OLS	PCSE
Time FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES

Note: Dependent variable is log values of market-weighted bond spreads at monthly frequency (mean = 1.26). Standardized beta coefficients. Robust standard errors in parentheses. Constants are significant at *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are adjusted for 44 clusters in *year*. Panel-corrected standard errors (PCSE) model (6) retains OLS parameter estimators, but replaces OLS standard errors for PCSEs, which takes into account the groupwise heteroscedasticity for 27 groups (countries).
Source: Appendix 1.

4.2. Bond based spreads without averaging

We have followed Alquist and Chabot (2011) in defining our dependent variable as the weighted average of the spreads issued per country (see Eq. (1)). As explained in footnote 5, an alternative approach is to define the dependent variables without averaging the bonds, so that spreads are defined as $R_{jt} = r_{jt} - r_{UKt}$, where r_{jt} is the coupon yield of bond j in time t. To address the small-cluster problem and account for the repeated values in some of the control variables, regressions include double clustered standard errors on year and country (27 countries * 44 years). Table E shows that results hold when we run our main models with bond-based spreads at monthly frequency. The coefficient of *true democracy* is higher than in Table D. It suggests that *true democracies* are associated with a rise of 5.4% in spreads, the equivalent of 44.9% of the penalty paid by defaulters.

Table E
Bond based spreads, monthly frequency.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Effective legislature	0.101***					
	(0.061)					
Elected executive		0.017				
		(0.055)				
True democracy			0.112***	0.101***	0.098***	0.112***
			(0.075)	(0.079)	(0.080)	(0.014)
Presidential democracy			0.059***	0.068***	0.065***	0.059***
			(0.090)	(0.095)	(0.097)	(0.019)
Constitutional monarchy			0.124***	0.099***	0.104***	0.124***
			(0.070)	(0.072)	(0.073)	(0.011)
Years of domestic peace	-0.157***	-0.140***	-0.156***			-0.156***
	(0.001)	(0.001)	(0.001)			(0.000)
Years of external peace					-0.059***	
					(0.001)	
Years on default	0.300***	0.304***	0.305***	0.321***	0.328***	0.305***
	(0.070)	(0.069)	(0.069)	(0.072)	(0.073)	(0.012)
Years on gold	-0.045	-0.044	-0.041	-0.055*	-0.064**	-0.041***
	(0.060)	(0.062)	(0.060)	(0.062)	(0.063)	(0.009)
Debt/tax	0.232***	0.208***	0.235***	0.252***	0.256***	0.235***
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.002)
Exports/population	0.008	0.036	0.023	0.043	0.061**	0.023***
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.002)
Share of exports to UK	0.113***	0.108***	0.120***	0.118***	0.123***	0.120***
	(0.110)	(0.112)	(0.110)	(0.116)	(0.117)	(0.021)
Observations	32,990	32,990	32,990	32,990	32,990	32,990
R-squared	0.657	0.651	0.659	0.645	0.647	0.659
Model	OLS	OLS	OLS	OLS	OLS	PCSE
Time FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES

Note: Dependent variable is log values of individual bond spreads at monthly frequency (mean = 1.14). Standardized beta coefficients. Robust standard errors in parentheses. Constants are significant at *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are adjusted for clusters in year and country (27 countries * 44 years). Panel-corrected standard errors (PCSE) model (6) retains OLS parameter estimators, but replaces OLS standard errors for PCSEs, which takes into account the groupwise heteroscedasticity for 173 groups (bonds).
Source: Appendix 1.

Appendix 5. Event study: democratization vs autocratization

Variables	Democratization	Autocratization
Years of domestic peace	-0.036	-0.051**
	(0.001)	(0.001)
Years on default	0.390***	0.410***
	(0.057)	(0.059)
Years on gold	0.020	0.034
	(0.050)	(0.052)
Debt/tax	0.050**	0.065***
	(0.010)	(0.010)
Exports/population	0.214***	0.202***
	(0.011)	(0.011)
Share of exports to UK	0.131***	0.126***
	(0.097)	(0.096)
Lag_{10}	0.100	-0.321*
	(0.219)	(0.608)
Lag_9	0.108*	-0.113
	(0.206)	(0.608)
Lag_8	0.131**	-0.051
	(0.197)	(0.609)

Variables	Democratization	Autocratization
Lag_7	0.129**	-0.097
	(0.196)	(0.434)
Lag_6	0.161***	-0.120
	(0.196)	(0.434)
Lag_5	0.128**	-0.146
	(0.196)	(0.434)
Lag_4	0.122**	-0.097
	(0.195)	(0.359)
Lag_3	0.083	0.023
	(0.195)	(0.358)
Lag_2	0.130**	0.010
	(0.195)	(0.358)
$Lead_0$	0.128**	-0.045
	(0.196)	(0.309)
$Lead_1$	0.122**	-0.076
	(0.196)	(0.308)
$Lead_2$	0.145**	0.042
	(0.195)	(0.308)
$Lead_3$	0.153**	-0.016
	(0.196)	(0.308)
$Lead_4$	0.114*	-0.064
	(0.196)	(0.308)
$Lead_5$	0.010	-0.061
	(0.206)	(0.308)
$Lead_6$	0.031	-0.043
	(0.206)	(0.308)
$Lead_7$	0.060	-0.102
	(0.219)	(0.309)
$Lead_8$	0.041	-0.083
	(0.219)	(0.309)
$Lead_9$	0.005	0.021
	(0.206)	(0.309)
$Lead_{10}$	-0.044	-0.017
	(0.207)	(0.309)
Observations	974	974
R-squared	0.742	0.735
Time FE	YES	YES
Region FE	YES	YES

Note: Dependent variable is log values of market-weighted bond spreads at annual frequency (mean = 1.30). Standardized beta coefficients. Robust standard errors in parentheses. Constants are significant at *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Only 10 years of lags and leads are reported. Remaining lags and leads are not significant. Democratization sample: Brazil (1894), Costa Rica (1882), Japan (1890), Norway (1884), Peru (1886), Sweden (1883), Uruguay (1887), and Venezuela (1888). Autocratization sample: Bulgaria (1894), Guatemala (1898), Nicaragua (1894), and Spain (1874). Years of transition are in parantheses. Analysis is conducted by eventdd command in STATA (Clarke and Schythe, 2020).

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