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External Sovereign Debt Restructurings and Borrowing Costs, 1822-1980: Exploring the Reputational Costs of Sovereign Default

Jaakko Ikonen (42133)

Abstract

This paper attempts to shed light to one of the oldest puzzles in the sovereign debt literature: do sovereigns face higher borrowing costs after sovereign default? By utilising a newly released dataset by Meyer et al. (2022), this paper brings forth new evidence by examining all hard-currency sovereign bonds that were quoted on the London and New York stock exchanges between 1822 and 1980. When accounting for countries' macroeconomic indicators, as well as the size and timing of the restructuring that precedes the sovereigns market re-entrance, I find little evidence in favour of the hypothesis that sovereign default is consistently associated with higher future borrowing costs for sovereigns. There is some weak evidence for the hypothesis that time spent in a defaulted state is associated with higher post-restructuring spreads for individual bonds. However, as several methodological and data concerns posed problems to identification, further empirical evidence is needed to address this question conclusively.

Keywords: sovereign default, economic history, sovereign spreads, government reputation

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Discussants: Jiadi He & Zhe Wang
Examiner: Magnus Johannesson

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1 Introduction

The costs of sovereign default to the debtor sovereign can take many shapes and forms, but they can be roughly categorised into the following groups: credit market exclusion, changes borrowing rates, sanctions, and domestic costs arising from the lack of credit (Panizza et al. 2009). The first two - credit market exclusion and changes to borrowing rates - can be seen as *the reputational costs* associated with a sovereign default as they arise from the changes in perceived creditworthiness of the sovereign in the eyes of external creditors. This paper will explore the reputational costs of sovereign default, with explicit emphasis paid to the impact of default on borrowing rates. To this end, I will utilise the remarkable, newly-released Meyer et al. (2022) dataset of monthly price observations for more than 800 sovereign bonds that were quoted on the London and New York Stock Exchanges between 1822 and 1980. This data is combined with data on all default and restructuring episodes associated with these bonds.

In early theoretical literature, it has generally been held that sovereign default involve substantial costs for the debtor country in the form of higher borrowing costs in the future.[1] However, most empirical studies have found mixed evidence in favour of this theoretical belief. Typically, sovereigns are able to re-access credit markets relatively quickly and with surprisingly low rates (Panizza et al. 2009). Even when a sovereign credit premium is demanded after a restructuring, it is often short-lived (Bolton et al. 2023; Mitchener and Trebesch 2023). When first raising capital, countries with underdeveloped capital markets and lackluster institutions are often forced to issue debt in a foreign currency. Often dubbed as "the original sin", this phenomena has persisted for 200 years (Flandreau and Sussman 2004). It is well-documented how this type of borrowing is prone to sovereign default later down the road. Regardless of this apparent irrationality, investors continue to support sovereign issuers with questionable credit records to this day. Considering the countless of instances of sovereign default in the past two centuries, it appears that reputational theories of sovereign debt are ill-suited to explain why credit markets remain open to many hard-currency issuances.

If sovereigns are not penalised for forfeiting their debt commitments, why should they pay back their debts at all? In the corporate world, firms cannot simply repudiate as debt contracts are enforced by courts. If they were to do so, an appropriate portion of their assets would be liquidated by a court ruling and distributed to the creditors. However, such an enforcement mechanism is absent in sovereign credit markets due to the principle of "sovereign impunity", which states that a sovereign cannot be sued in foreign courts without their consent (Sinyagina-Woodruff 2003; Panizza et al. 2009). In this sense, sovereign debtors de facto retain the option to renounce their debt obligations at their will. As noted by Panizza et al. (2009), this principle generally applied even to commercial transactions between foreign states and private individuals from another state before the Second World War. While reputational theory of sovereign debt, a broad group of theories that highlight the importance of upholding ones obligations in a perpetual game if it wished to borrow more

[1] See, for example, Eaton and Gersovitz (1981) and Cole et al. (1995).

on good terms in the future, has been advanced by many academics over the past decades, empirical literature on the topic is sparse, particularly in the context of the nineteenth century and the first half of the twentieth century (Sinyagina-Woodruff 2003). This paper hopes to contribute to fill this gap in the literature.

The turn of nineteenth century, in particular, was characterised by rapid growth of the external sovereign debt market, aided by the adaptation of new technologies that made communication between regional debt exchanges quicker and less error prone (Michie 2001). Investment opportunities into assets that were previously out-of-bounds for the median investor surfaced rapidly. As we will come to see, new opportunities were often shrouded in misinformation as next to no reliable financial records existed for new issuers. In fact, the first century of the external sovereign debt market is an excellent case study into how information dispersion happens in a new marketplace; and how failure is penalised. As with many new asset classes that have been born out of financial innovation, the external sovereign debt market is just one of them. Examining this topic in 2020s might be more topical than in a long time. History shows us that the neoliberalist ideas of capital mobility and globalisation, which have come under increasing pressure by the ever more tense geopolitical climate and protectionist rhetoric, have failed investors before. I believe it is vital for investors and academics alike to examine the historical context and trends of an asset class in order to fully grasp how it may evolve in the future.

This paper finds little support for the notion that sovereign default consistently leads to elevated future borrowing costs for sovereigns. These findings appear to be resilient to the macroeconomic fundamentals of countries, the size of investor losses incurred in restructurings, the length of default periods, and post-1945 shifts in international finance. Nevertheless, the robustness of these results is called into question by several methodological and data-related concerns. The lack of suitable and credible data sources poses considerable challenges to identifying the causal impact of sovereign default on sovereign spreads. Considering these limitations to the causal inference strategy, this paper's aims must be modest. Rather than interpreting the results of this study as the true causal relationship, these results should primarily act as a foundation for more robust empirical work in the future. Nevertheless, to my knowledge, this is the most comprehensive study to date on the empirical relationship between sovereign default and spreads before 1980s, as most comparable papers have tended to focus on later time periods.

2 Literature review

Eaton and Gersovitz (1981) first formalised a theoretical model in which international lending is sustained by the fact that sovereign defaults are associated with a permanent exclusion from the global financial markets. This theoretical view was subsequently questioned by the empirical evidence of Bulow and Rogoff (1989), who found that defaulting countries do not seem experience a significant increase in borrowing costs after a debt settlement event; it appeared as if "debts which are forgiven will be forgotten". Since the seminal paper of Eaton and Gersovitz (1981), theoretical papers have experimented with rationalising this seemingly time-inconsistent investor behaviour. To this end, elements have been integrated to quantitative models in order to mimic the empirically observed "stylised facts" that appear to govern sovereign debt markets (Mitchener and Trebesch 2023). One of the most significant innovations in this new strain of theoretical models was the relaxation of the permanent capital market exclusion assumption. For instance, Aguiar and Gopinath (2006) and Arellano (2008) both proposed debt and default models for a small open economy, in which default entails a temporary exclusion for international capital markets as well as domestic output costs.[2]

Drawing from anecdotal evidence, Tomz (2007) suggests that reputation of a sovereign has played a significant role in how credit markets treated issuers over the past two centuries. In one example, he notes that countries' credit history played a pivotal role as an information factor during the 1820-1840 Latin American sovereign credit boom. Compared to the debt of more established European borrowers, the sovereign debt of the newly independent Latin American countries traded at a deep discount. This may have reflected the perceived risk associated with dealing with a potential lemon.[3] According to reputational theory of sovereign debt, countries are judged not only on their default history, but also on the reasonableness of debt settlements to creditors and on the primary reasons for their default spell. Tomz (2007) suggests that defaulted Latin American countries could not issue new bonds if they failed to offer the bondholders of the defaulted bonds an acceptable settlement. This appears to indicate that the threat of exclusion from the credit markets was upheld. Indeed, while the threat of credit market exclusion *after* a sovereign restructuring appears to be poorly upheld, there is a widespread consensus that defaulted sovereigns are only able to regain access after reaching a settlement on defaulted debts (Mitchener and Trebesch 2023).

A general consensus has arisen that credit markets discriminate by lending at higher rate to countries with a default history.[4] However, there is little consensus in the literature on whether these discriminatory effects are short-term or long-lasting. Moreover, recent literature has extensively focused on examining default spells after the syndicated bank loan

[2]The theoretical literature dedicated to models of endogenous sovereign default is rather rich. Some of the other papers dedicated to the matter include Mendoza and Yue (2012), Egorov and Fabinger (2016), Dovis (2019), and Amador and Phelan (2021).

[3]As an interesting side-note, it may be worth mentioning the loan to the fictitious Central American country of Poyais, a historical mishap that strengthens the idea of asymmetric information in the sovereign credit markets of the early nineteenth century. As noted by Tomz (2007), Gregor MacGregor, a Scottish adventurer, allegedly managed to convince a Mosquito king on grant him a concession of 8 million acres in modern day Nicaragua and Honduras in 1820. Upon his return to Europe, he sought to collect funds to establish a country called Poyais on the land concession. To this end, a book called "Sketch of the Mosquito Shore, Including the Territory of Poyais" was published in 1822 (Strangeways 1822). In a remarkable feat, MacGregor managed to raise £200,000 at 6 % interest and the Poyais loan started trading on the London Stock Exchange in October 1822. Most interestingly, the loan traded very close to the sovereign bonds of Columbia, Chile and Peru for around 12 months before the fraud was uncovered in late 1823 with the arrival of first settlers to the fictitious country.

[4]Some early evidence suggested that future long-run borrowing costs were not associated with sovereign default history (Lindert and Morton 1989, Chowdhry 1991). This evidence was largely focused on examining the reputational implications of past defaults multiple decades ahead by utilising very simple regression models.

era of the 1980s and early 1990s.[5] Earlier time periods have gained more limited attention by academics, mostly due to the lack of adequate data.

With a sample of sovereign defaults from 1820 to 1968, Ozler (1993) finds that defaults can have a long-run effect on a sovereigns' borrowing costs. For instance, the effect of a default in the 1930s is small but statistically significant for explaining interest rates in the 1970s.[6] Flandreau and Zumer (2004) finds that sovereign spreads are 90 basis points higher during a year immediately after a debt restructuring that ends a default spell. For a data set stretching from 1997-2004 for 31 countries, a random effects model of Borensztein and Panizza (2009) show that defaults have a sizable statistically significant effect on spreads in the first two years directly after the event. However, the memory of credit markets appears to be short as this effect becomes negligible in the subsequent years. When accounting for changes in credit ratings, these reputational effects appear to become even more short-lived. As noted by the authors, this may suggest that reputational risks are at least partly reflected by rating downgrades post-default. In an analysis of emerging market debt restructuring episodes from 1998-2005, Sturzenegger and Zettelmeyer (2007) find that a default history of sovereign is associated with higher spreads when controlling for macroeconomic fundamentals, but only shortly after default episodes. Moreover, the fact that front-loaded repayment schedules were underutilised in subsequent post-default debt issuances may suggest that sovereigns did not enjoy the same access to international credit markets. Some studies, by utilising credit ratings as indirect measures of borrowing costs, find that a history of default is positively associated with lower credit ratings (Cantor and Packer 1996; Borensztein and Panizza 2009). Certain contemporary sources also appear to suggest reputational costs of default were real and sizable. Fortune (1833) notes how particularly Danish debt securities "always enjoyed great favor with the public" on account of the "punctuality and straightforwardness which the government ... has hitherto observed in all its financial dealings." Likewise, Brazil's relatively low spread relative to its Latin American peers was attributed to good credit track-record (Fenn and Nash 1869).

A subset of more recent empirical literature has established a positive relationship between past default episodes and subsequent spreads. Cruces and Trebesch (2013) find that higher haircuts (i.e. the extent of creditor losses suffered in a restructuring) are associated with higher post-restructuring spreads and longer duration of exclusion from capital markets for a panel of 83 countries and 179 restructuring/default episodes between 1978-2010. Using the same data set as Cruces and Trebesch (2013), Asonuma (2016) found that emerging countries that have a history of sovereign default face a higher probability future default when controlling for debt-to-GDP. In their analysis of all sovereign default episodes from the end of the Napoleonic Wars in 1815 to 2010, Meyer et al. (2022) finds that higher haircuts are associated with more sluggish bond price changes in the years after the default event. While interesting, these results of Meyer et al. (2022) can not be credibly identified as the pure

[5]Sovereigns were extensively reliant on syndicated bank loans (rather than bonds) for their foreign-currency denominated funding needs starting from the 1970s all the way until the early 1990s (Meyer et al. 2022). The market for EM sovereign bonds only became more liquid in the 1990s under the Brady Plan.

[6]As noted by Borensztein and Panizza (2009), results of Ozler (1993) do not seem to cluster standard errors, which would reduce the significance of the coefficients substantially.

'reputational effect'. In the absence of macroeconomic control variables, their results serve a more illustrative purpose rather than suggesting a causal relationship between defaults and subsequent bond returns.

Lindert and Morton (1989) suggests that a majority of countries that defaulted during global crises "escaped" punishment, and only countries that defaulted in isolation before 1918 were punished on the credit markets. There appears to be little evidence of credit discrimination for defaulted countries besides small-scale punitive trade restrictions and occasional incidents of gun-boat diplomacy (Lindert and Morton 1989). Perhaps the most serious effort at credit discrimination was undertaken in the form of the April 1934 U.S. Johnson Act, which prohibited private loans to foreign governments in default. However, the impact of this legislation was short-lived, as exemptions from the Johnson Act were granted to all governments affiliated with the IMF and the IBRD starting from 1945 (Lewis 1948). Nevertheless, as early as 1936, new loans were extended to "good neighbors," many of which were Latin American countries defaulting on dollar bonds (Felix 1987)." By analysing the sovereigns' macroeconomic standing immediately prior an adverse credit event from 1970-2001, Reinhart et al. (2003) suggests that serial defaulters are characterised pervasive "external debt intolerance". That is, serial defaulters tend to default at generally low levels of debt-to-output, generally with a debt-to-GNP ratio of under 50%.

Evidence from the syndicated sovereign bank loan market indicates that sovereign default history is a determinant of borrowing costs. Using a dataset of 4,000 sovereign loans from 1991 to 1997, Eichengreen and Mody (2000) identifies a significant positive relationship between countries' debt rescheduling records and bank loan spreads. By analysing bond and public syndicated loan issuances between 1980 and 2000, Gelos et al. (2011) finds that a sovereign's default frequency or the shiftiness by which the default is resolved does not significantly affect the sovereign access to credit. Benczúr and Ilut (2016), on the other hand, notes that recent repayment history is significant determinant of the spreads of syndicated bank loans that where taken by developing countries between 1973 and 1981.

Sanction-based theories offer an alternative view on what may deter against neglecting interest and principle payments to creditors. The compellingness of such theories have decreased since the World Wars, but "gunboat diplomacy" could be said to have enjoyed its heyday in the Victorian period with little modern evidence of such behaviour (Panizza et al. 2009). Mitchener and Weidenmier (2010) finds a dozen cases of "supersanctions" that a third party creditor countries forcefully imposed on the defaulting country to force repayment from 1870-1913. For instance, the US Marines were periodically utilised as "a debt collection force" to re-enforce repayment (Aliber 2002). Interestingly, Mitchener and Weidenmier (2010) find that these "supersanctions", involving military or political pressure, significantly decreased ex-ante default probabilities on new issues by defaulted sovereigns. While sanction-based theories fall outside of the scope of this paper, it is nevertheless important to be aware of their existence and the challanges they might pose to causal inference. As noted by Mitchener and Weidenmier (2010), creditors often took direct control of customs or tax revenues to finance debt repayment as a part of "supersanctions"; actions which may have changed the sovereign's capacity to serve its debts and its standing in international credit markets.

3 Data and Methodology

This paper attempts to identify the extent to which sovereign default affects a sovereign's future borrowing costs. While the methods employed in this study largely mirror those used in prior literature, its primary contribution lies in the utilization of a novel comprehensive bond price dataset by Meyer et al. (2022), which spans from the Congress of Vienna to the the popularisation of syndicated bank loans as tools of sovereign financing in the 1970s-80s. The research question is formulated as follows: *"To what extent, if at all, did external sovereign default history determine future borrowing cost of sovereigns from 1822 to 1980?"*

In this paper *external sovereign debt* is defined by a bond's currency of issuance rather than by jurisdiction of issuance or domicile of the ultimate owner. The reasons for excluding *domestic* sovereign debt (i.e. sovereign debt denominated in the domestic currency) from this analysis are twofold. Firstly, there is a lack of comprehensive data for local currency denominated sovereign debt. For an extended period of time, a theoretical assumption that countries do not default on local currency debt but instead choose to "inflate the problem away" persisted in the academic community (Amstad et al. 2020). The likes of Reinhart and Rogoff (2011a) have since disproved this view by showing that sovereign defaults on local currency denominated debt are not a new phenomenon. In addition to the academic community's lack of initial interest in the subject, it is possible that the nature of regional debt markets have contributed to the lack of quality data today. Unlike external debt, domestic debt was held almost exclusively by domestic residents (Reinhart and Rogoff 2011a) and traded in local exchanges (Michie 2001). Beers and Mavalwalla (2018) suggest that a high degree of ownership concentration in domestic hands can make it hard for domestic investors to pursue compensation during and after times of distress. Secondly, as shown by Rousseau and Sylla (2001), only a select few non-periphery countries have been able to borrow in their local currency over the past two centuries.

Focusing purely on bonds quoted on the London and New York stock exchanges is unlikely to exclude many external bond issuances, as these two were the primary exchanges of the day. In 1853 the market value of sovereign debt securities (excluding debt issued by colonial authorities, foreign public bodies and foreign railways) traded on the London Stock Exchange amounted to £69.7 million, which accounted for 5.7% of all assets traded on the exchange. By 1913 this had ballooned to £1,421.9 million, or 14.9% of the entire market capitalisation of the exchange. A further £1,712.0 million in sovereign debt assets was quoted on the London Stock Exchange by 1913(Michie 2001).[7] As anecdotal evidence of London's dominance before the 1918 as the world's premier hub for sovereign borrowing, roughly 18% of all of the Russia's total debts of 1.5 billion silver rubles (about £242 million pounds) were domiciled in London in 1863 according to official Russian statistics (Fenn and Nash 1869). Starting from the interwar period, New York gradually dethroned London as the largest marketplace for foreign sovereign securities (Michie 2001).

[7]Michie (2001) notes that the general willingness by London to grant a quotation to foreign government securities was not shared by all exchanges. For instance, Paris restricted the quotation of foreign government bonds to such an extant that much of the investment activity in sovereign debt undertaken by French nationals occurred in London.

3.1 Data

The primary dataset utilized in this paper is from Meyer et al. (2022). The data consists of monthly price observations for 857 Pound sterling and US dollar denominated bonds, issued by 54 different countries, that were catered to foreign creditors from 1822 to 1980. Hence, the period covered stretches from the (arguable) inception of the modern sovereign bond market to the rise of syndicated bank loans as the primary tool of sovereign finance. Unlike past literature that has relied on country level data, dataset of Meyer et al. (2022) allows us to distinguish between different bonds by the same issuing sovereign. A sovereign default usually pushes multiple bonds of the same issuer to default. The dataset excludes bonds issued by subnational regions within sovereign states and colonial governments.

The sample includes 693 default events (see Figure 2 for a graphical illustration).[8] As many sovereigns were serial defaulters during the time period of the sample, many bonds defaulted multiple times.[9] The most extreme case of this were the Mexican bonds of 1824/1825, which were ultimately restructured eight times over 100 years. Out of the 857 bonds, 87 have their last price recorded in a defaulted state. Nevertheless, only several of these cases result in debt repudiation. In vast majority of these cases, a debt restructuring takes place and the sovereign continues repayment of the debt but under a different debt instrument. The dataset claims that 28 debt instruments are in default at issuance. Rather than being examples of new debt issuances, such securities are generally resultant from the restructuring of old debt that was already in default.

The country coverage (the share of independent countries represented by the full sample) ranges from around 90% in the 1920s to under 10% in 1979-1980. It's important to note that this ratio can be below 100%, not due to missing data, but because some countries didn't have outstanding external debt in certain years. Decline in share of countries in the sample after the Second World War can be largely attributed to rise of syndicated bank loans, which largely replaced bonds as a source of external financing for developing countries in the 1980s (see Figure 9 in subsection A.1).

Figure 2 highlights an important characteristic of the global external borrowing market: cyclicality of defaults. Instances of default often occur in clusters and typically ensue after lending booms (Sturzenegger and Zettelmeyer 2007). The first observable cluster in 1820-1840 was largely associated with increased lending to newly independent countries of Latin America, with Latin American counties accounting for approximately 70% of all defaults during the period. After twenty years of relative tranquility, the period from 1861 to 1920 was characterized by 58 default episodes, mostly again in Latin America. The 1930s Great Depression brought on another wave of defaults, mostly centered in the Americas and Europe. The collapse of the international lending market due to World War Two led to a relatively low

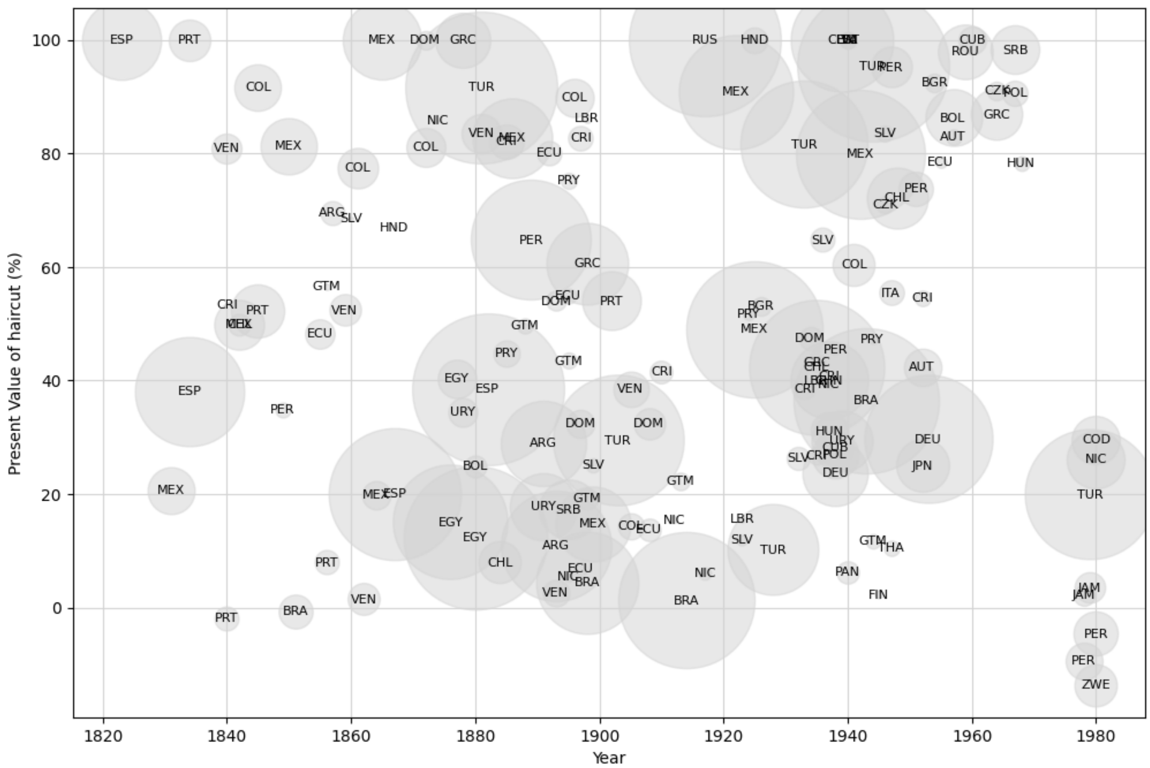
[8]Although Meyer et al. (2022) also provides haircut data for all default events spanning from 1981 to 2016, I couldn't incorporate it into this paper due to the proprietary nature of the post-1981 bond price data.

[9]Out of the 857 bonds, 224 were restructured twice, 32 were restructured three times, 13 were restructured four times, and the remaining 17 bonds were restructured five times or more (Online Appendix of Meyer et al. (2022)).

Figure 1: Countries: time spent in sample and in default

The light grey color indicates that at least one bond issued by the sovereign in question has monthly price data in the dataset of Meyer et al. (2022). The dark grey color indicates that the sovereign is in default, but that at least one of the sovereign's bonds continue trading in ether London or New York.

Figure 2: Sovereign defaults and associated real investor losses ("haircuts")



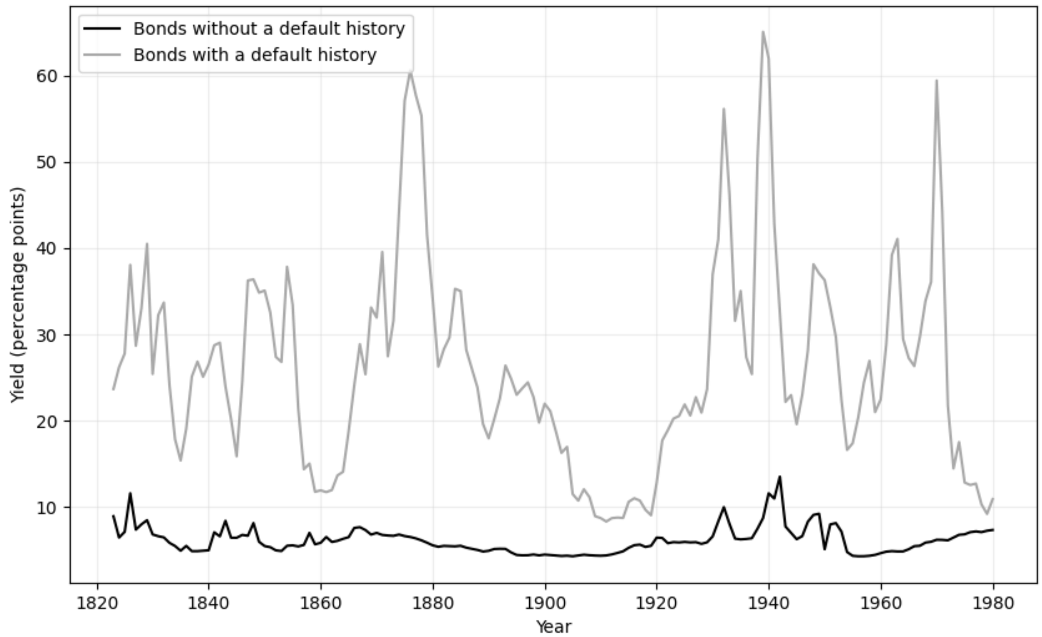
The size of the circle implies the real amount of debt restructured in the base currency. Note: three largest circles were scaled down due to the sheer size of the restructurings. These restructuring events were (i) the repudiation of the debt of Tsarist Russia by the Bolsheviks in 1917, (ii) the restructuring of Ottoman debt in 1881 after the aftermath of the 1875 default, and (iii) the restructuring of Spanish external debts in 1882.

number of defaults in the postwar era until 1980 (Borensztein and Panizza 2009). In several default episodes, a sovereign continues to serve its debt but only partially. In such cases, coupon payments are often slashed to a fraction what is specified in the debt contract.[10] As can be noted from Figure 3, yields on bonds that have default history are significantly higher than those without one.

In multiple cases a debt restructuring was carried out by issuing a new bond. The old, defaulted debt could be converted to this new instrument at a predetermined conversion rate. As with other types of debt restructurings, this process often implies a nominal loss to the investor. In several cases this bond restructuring cycle occurred multiple times before the final bond iteration of the initial instrument was redeemed. As an example of these kind of cases, Figure 4 showcases the spread evolution for a group of Spanish sovereign bonds denominated in Pound Sterling. Following Spain's default in 1823, the 5% bonds of 1821 were converted into the newly issued 5% bonds of 1834 at a 1:1 conversion ratio. In 1852,

[10]For instance, Bulgaria continued to make partial coupon payments, ranging from 14-50% of the contractually agreed coupon rate, on its debt for eight years after its sovereign default of 1932.

Figure 3: Bond yields and default history



several different debt securities were conglomerated into one single bond.

3.1.1 Dependent variable

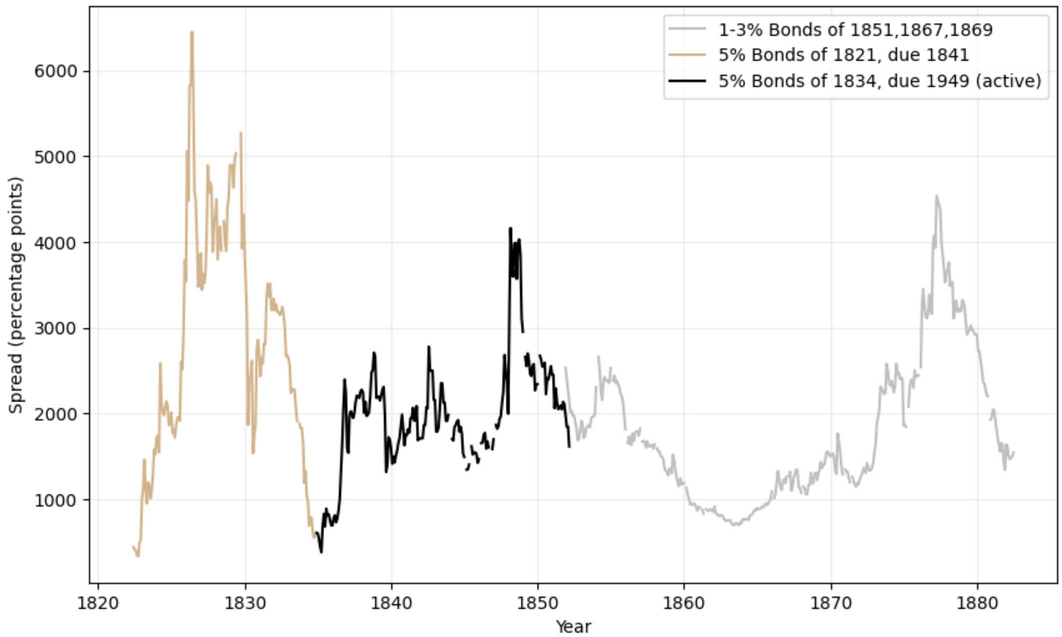
To compute the dependent variable (i.e., the spread of a given bond), time series' of relevant long-run risk free rates are needed. Ideally, the spread on bond should calculated with respect to a safe benchmark asset with a similar maturity profile. This study uses two benchmark rates: the yield on 3% UK perpetual Consols is used for GBP-denominated assets and the yield on US High Grade Municipal Bonds is used for USD-denominated assets.[11] The US dollar only became prevalent in hard-currency sovereign issuances after turn of the twentieth century, particularly in the Americas.[12] It should be noted that in an ideal world, this study should be using long-term borrowing rates as the benchmark rate. Indeed, this is the approach that has been taken in the literature (Cruces and Trebesch 2013, Meyer et al. 2022). However, as discussed earlier, proprietary restrictions posed constraints on data access in this study.

Unfortunately, the dateset of Meyer et al. (2022) did not contain information on the

[11]Siegel (1992) suggests that municipal bonds, for a significant portion of the nineteenth and early twentieth centuries, were considered lower risk than corresponding US federal government bonds. In the early nineteenth century, specific municipal bonds, such as those issued by the Commonwealth of Massachusetts and the City of Boston, were often deemed of superior quality compared to their federal counterparts. Moreover, unlike the 'bimetallic' repayment option of federal bonds, many municipal bonds promised to pay interest and principle exclusively in gold.

[12]In the data set of Meyer et al. (2022), the first USD-denominated bond was issued by Cuba in 1904.

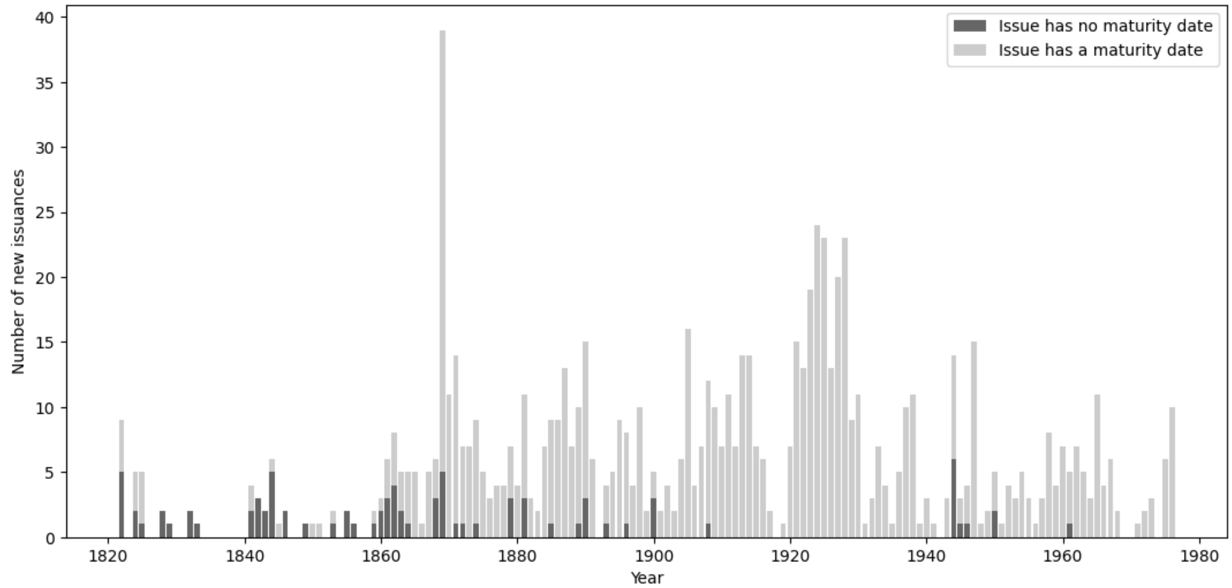
Figure 4: A restructuring cycle: Spread evolution of Spanish bonds of 1821



coupon periodicity and payment dates. Prior to 1980s, sovereign bonds would typically make semi-annual coupon payments and the dates of these coupon payments varied by bond, but quarterly coupon payments were also commonplace before the first half of the nineteenth century (Carey 1825; Fenn and Nash 1869; Porter 1927; Eitrheim et al. 2004). Given this shortfall in the data, I shall pursue the same methodology as Meyer et al. (2022); namely, I assume coupon income is equally distributed throughout the twelve months of the year, with an identical payment being made each month. Naturally, this gives rise to potential measurement bias as prices are quoted in dirty prices. However, it is not clear whether this will pose a significant concern given the relatively illiquidity of bond markets and the relative difficulty in arbitraging minuscule price differentials in the nineteenth and early twentieth century. It must also be noted that even if there existed a complete payment schedule for all bonds, it may be difficult to determine which coupon payments had been paid as noted by Meyer et al. (2022).

The amortization schemes of sovereign bonds issued throughout nineteenth century and for much of the twentieth century differed greatly from modern-day bonds. While modern bullet bonds have one or several redemption dates that are public information at issuance, a vast majority of sovereign bonds issued before the 1990s did not have a predetermined call schedule (Eitrheim et al. 2004). Rather, amortization was handled through sinking funds that typically prolonged the principle repayment period over multiple years or even decades. Under these sinking fund arrangements, bonds were generally redeemable at par by sinking funds that were accumulated by the sovereign after the bond's issuance. Usually, after a predetermined period, bonds became eligible for redemption from these funds. These

Figure 5: Sovereign bond issuances by year

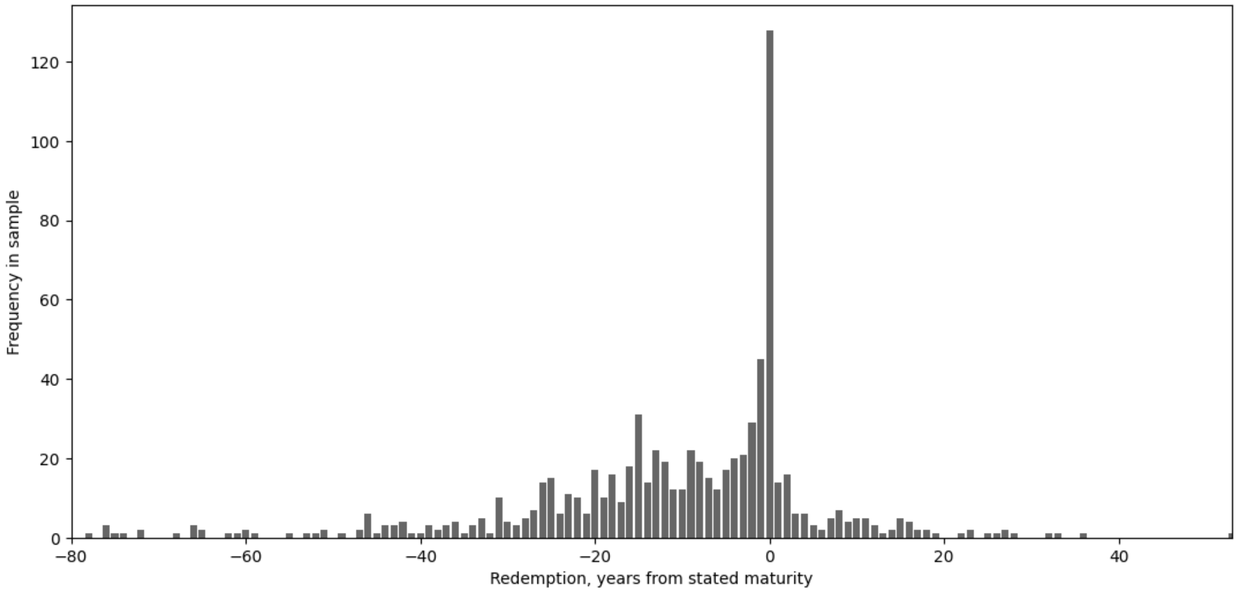


redemptions would be carried out on a quarterly, semi-annual or annual basis through lottery. Payments and redemptions were generally administered by a third-party payment agent that was contracted by the sovereign to carry out the bond issuance in the foreign jurisdiction.

Under such sinking fund arrangements, the sovereign maintained a degree of flexibility in redemption timing. Typically a bond contract obligated the sovereign to finalise all redemptions by a certain date, but consequently it allowed some leeway on the exact dates on which redemption could happen. For instance, Colombian government reserved the right to redeem Colombian bonds of 1863 at par at any point before maturity. Danish bonds of 1822 did not have a set maturity date, but they were redeemable at the wish of the Danish monarch. On the other hand, biannual redemptions of the Moroccan loan of 1862 and the El Salvadorian bonds of 1863 began directly after issuance (Fenn and Nash 1869). The Ecuadorian 6% loan of 1913 was redeemable at par at any time before its maturity in 1946. The Hungarian 7% gold loan of 1926 was redeemable on any interest date on three months' notice after 1929 (Porter 1927). Figure 5 shows the distribution of bonds by issuance year based on whether a maturity date was stated by Meyer et al. (2022). Out of the 857 bonds in the sample, 83 bonds did not have a prescribed maturity date. In turn, 774 bonds had a maturity date associated with them, implying an upper bound on the life of these bonds. It appears that by late nineteenth century, a vast majority of bonds had a maturity date; albeit I am unable to determine of how many of these issuances were reliant on atypical amortization schemes.

The practical implication of sinking fund structures is that traditional yield-to-maturity calculations are impractical as the maturity date only implies an upper bound on the life of a performing bond. Eitrheim et al. (2004) approached this problem by arguing that the average expected life of a bond is more meaningful metric for yield calculations than the

Figure 6: Ultimate redemption versus maturity date at issuance



maturity date. To this end, they defined bond's average life as the average of the sinking fund redemption dates weighted by the repayments. Unfortunately, Meyer et al. (2022) does not provide bond-specific redemption schedules. As such, I devise an alternative average remaining life measure that assumes that a bond will be redeemed at maturity date. For each month that a bond spends in a defaulted state, the expected maturity date will be pushed into the future by one month. When no maturity date is available for a bond, an ex-post yield is computed. Essentially, this yield metric takes the last month on which there is price data and sets it as the maturity date of the bonds, under the assumption that the final batch of bonds is redeemed at this time. This metric is ex-post in the sense that the ultimate redemption date is unknown at issuance. Computing an ex-post yield, rather than an ex-ante yield, will undoubtedly introduce bias into estimates. The degree to which this will bias estimates is unclear. Theoretically, the degree to which a sovereign is willing and able to communicate its future actions to investors ought to be a significant determinant of the extent of this bias. Figure 6 showcases the difference between the year of the last price observation of a bond and the bond's maturity date at issuance. Assuming that a bond's last price observation coincides with its ultimate redemption, 232 bonds (30% of all bonds with a maturity date) were redeemed within three years of the maturity date. 44% of bonds recorded their last trading price 10 or more years before their maturity date.[13]

[13]See Figure 11 in subsection B.1.

3.1.2 Independent variables

A commonplace metric for estimating the extent of investor losses in a sovereign debt restructuring was formulated by Sturzenegger and Zettelmeyer (2007) and Sturzenegger and Zettelmeyer (2008). Under this approach, the net present value of investor losses ("the haircut") is defined as "the percentage difference between the market value of new instruments received by the investor ... and the net present value of the remaining contractual payments on the old instruments", both of which are discounted by the yield of the new instruments, r^* (Sturzenegger and Zettelmeyer 2008, pg. 782).

$$H_{\text{NPV}} \equiv 1 - \frac{NPV(new, r^*)}{NPV(old, r^*)} \tag{1}$$

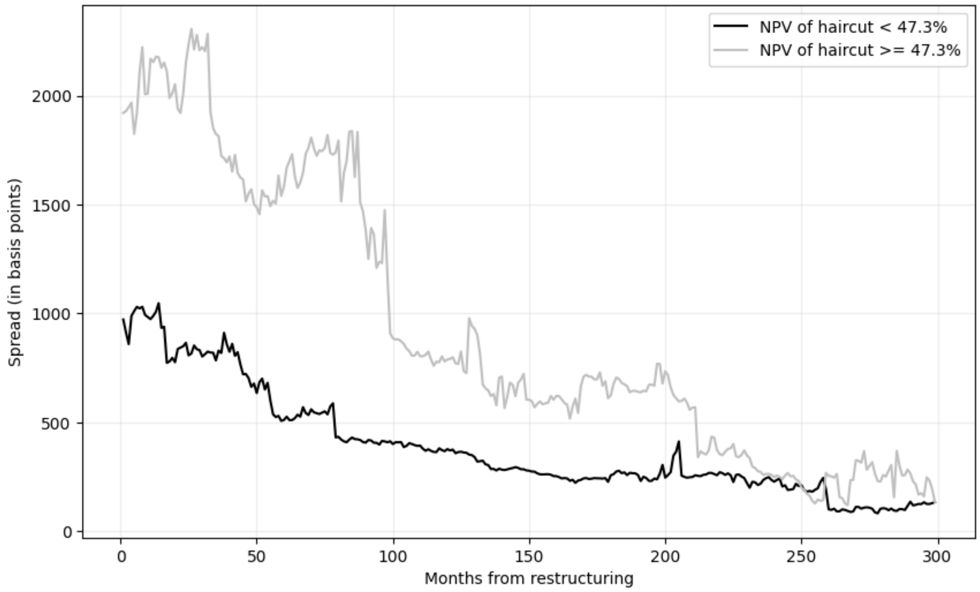
This measure is attractive due to the fact that it compares the hypothetical cash flows of the old debt had it not defaulted to those of the newly restructured debt using the same discount rate, r^*. This measure, widely dubbed as the "SZ-haircut", has made numerous appearances in subsequent literature, including Cruces and Trebesch (2013). The real haircut data series utilised in this paper was obtained from the calculations of Meyer et al. (2022), which directly mirror the approach of Sturzenegger and Zettelmeyer (2008). Like Sturzenegger and Zettelmeyer (2008), Meyer et al. (2022) used the secondary market yield of the new bonds that started trading after the restructuring as the discount rate, r^*. The mean and median haircut in the sample are 49.7% and 47.3%, respectively. Figure 7 showcases how bond spreads change, on average, after a sovereign restructuring. We can note that bonds that experienced a higher than median net present value haircut saw 300-1200 bps higher spreads up to 10 years after the restructuring event.

It is sensible to believe that sovereign defaults are, first and foremost, instigated by perceived economic foes faced by the sovereign. In the absence of political motivations, defaults cannot be viewed as exogenous events independent of the underlying macroeconomic conditions. Hence, causal inference of the reputational effect is reliant on establishing a credible set of macroeconomic controls that accounts for the presence of confounding factors. Due to the historical time period in question, data availability posed a barrier to obtaining comprehensive data for this study.[14] Obtaining relevant and reliable data for the time period is problematic for a number of reasons. Much of the time period in question predates the widespread adoption of systematic data collection and standardized statistical methods. Many countries did not have established statistical agencies or comprehensive record-keeping systems. Secondly, many macroeconomic indicators that are commonplace today were either absent or early in their theoretical development during the time period in question.[15] Indeed,

[14]Global Financial Data (GFD) has been frequently cited as a data source by multiple papers in the literature. Despite its potential relevance to the research, the prohibitive cost of the license prevented inclusion of data from this source.

[15]While first serious attempts at national income accounting can be attributed to William Petty in eighteenth century Great Britain, the field was in its infancy well into the first half of the twentieth century. Gross Domestic Product (GDP), in its modern form, was pioneered by work of Colin Clark and Simon Kuznets in the 1930s (Lepenies 2016). The first attempt to measure national unemployment was in the US 1880

Figure 7: Average spread, months from restructuring



as an contemporary author, Baxter (1871) notes that national income accounting exercises were "of a nature more conjectural than scientific", continuing that "general adoption of such a method had to be left for an age of more complete statistical knowledge". Many of the datasets utilised in this study have arisen retrospectively through community efforts of countless academics across the world. The patchiness and incompleteness of data was taken into account in the analysis whenever possible. Table 1 gives a descriptive list of all variables that were utilised in this study. A significant portion of the macroeconomic covariates utilised in this analysis were sourced from Reinhart and Rogoff (2011b), a comprehensive collection of data from various primary and secondary sources. The rest of this section will consists of discussion on the challenges faced throughout the data preparation process.

The nature of sovereign bond investment analysis before the World Wars was in many ways different to what investors and academics are used to currently. In their discussion of contemporary sources, Flandreau and Zumer (2004) find that contemporaries' agreed (unsurprisingly) that the magnitude of debts mattered for debt sustainability, but there was less of a consensus on how and to what extent debt accumulation translated into a higher default probability. For instance, Baxter (1871) placed emphasis on various interest service indicators, such as annual interest payment to gross income of the population, which can be seem as an early predecessor of debt service to GDP indicators. Essentially, many contemporaries saw other fiscal and monetary variables as of secondary importance. They only mattered to the extent to which they could influence the credibility of the sovereign's interest-service commitment (Flandreau and Zumer 2004). Secondary sources also suggest

Census. However, it was only in the wake of the 1930s Great Depression that systematic efforts to track unemployment were started in the US (Card 2011).

that contemporaries were highly aware of certain countries posing significant investor risks due to the absence of credible data. For instance, in an official report from the British Secretary of Legation dated 1858, it is exclaimed that "in Portugal, it is almost impossible to arrive at the real truth on any subject, particularly when statistical information is required" (Fenn and Nash 1869). In similar fashion In many secondary sources that were catered to nineteenth century British investors, the quality of analysis for many time issuers remained poor (Fortune 1833; Fenn and Nash 1869).

A relevant covariate should be able to explain some portion of a sovereign bond's spread *besides* the effect of a past default episode. Past literature has suggested a range of variables that can affect sovereign spreads. One clear determinant of spread developments could be the macroeconomic fundamentals of a country. To this end, past papers have utilised various variables as proxies for a country's ability to serve its external debts, including ratio of interest service to tax revenues (Flandreau and Zumer 2004), ratio of nominal debt to nominal output (Ozler 1993; Obstfeld and Taylor 2003; Cruces and Trebesch 2013) and ratio of reserves to nominal output (Ozler 1993). Some papers have utilised real output per capita as a measure of the level of socio-institutional development in a country (Ozler 1993; Obstfeld and Taylor 2003). Flandreau and Zumer (2004) employed estimates for the portion of enfranchised population as a proxy for a country's institutional development. Metrics of political instability have been used by some papers to account for the possibility of government change (Ozler 1993; Sturzenegger and Zettelmeyer 2008). Various terms of trade variables have commonly been used as proxies for a country's capacity to generate foreign exchange to meet debt obligations (Ozler 1993; Obstfeld and Taylor 2003; Borensztein and Panizza 2010). An addition of a terms of trade covariate could be further motivated by Reinhart and Rogoff (2009), who demonstrated that during the period spanning 1800 to 1940, increases in commodity prices are consistently succeeded by waves of sovereign defaults, particularly in the periphery. On a different front, Obstfeld and Taylor (2003) opted to include the lagged inflation rate as a covariate on the grounds that high inflation could be interpreted as slippage from gold standard commitments in an era when governments extensively operated within the bounds of fractional gold banking. Contemporaries appeared to share this view. In a discussion of "increase in paper currency" in Russia, Fenn and Nash (1869) note how "the premium on coin [over bank notes] has varied very much from time to time" from the highs of the Napoleonic Wars to the relative lows of 1860s. Likewise, in relation to finances of the Austrian Empire in the 1830s, Fortune (1833) note how it is "impossible to give account of the national debt of the empire without entering into

[16] The yield time-series is a combination of two indices, both sourced from the National Bureau of Economic Research: (i) Index of Yields of High Grade Municipal Bonds for United States (M13023USM156NNBR) between 1900-1952, (ii) Bond Buyer Go 20-Bond Municipal Bond Index from January (WSLB20) 1953 onwards.

[17] The exact variables used to compute the ratio vary widely based on data availability and quality. Consult Carmen M. Reinhart's website for case specific details.

[18] Certain bonds traded substantially above par value (100). This was the case especially for restructured Japanese and German bonds after the Second World War as creditors were retrospectively entitled to the interest income they forwent during the conflict. See Figure 12 for a graphical representation of the temporal distribution of price outliers.

Table 1: List of variables

Variable	Description	Frequency and time-period	Source
Bond prices and default periods	Bond prices, coupon rates, missed payments, defaults dates, restructuring dates, nominal and real values of debt issuances, etc.	Monthly, 1822-1980	Meyer et al. (2022)
Haircut measures	Face value and present value haircut estimates	Per episode, 1822-1980	Meyer et al. (2022)
UK Consol yields	Yields on UK government perpetual 3% Consols	Monthly, 1822-1980	Thomas and Dimsdale (2017)
US High Grade Municipal yields	Yields on an index of US high-grade municipal bonds [16]	Monthly, 1900-1980	National Bureau of Economic Research (2024), National Bureau of Economic Research (2024)
Gold standard	A dummy variable indicating the participation of a country in the Gold Standard	Annual, 1822-1939	Reinhart and Rogoff (2011b)
Inflation	Annual change in prices by country	Annual, 1822-1980 (depending on the country)	Reinhart and Rogoff (2011b)
Debt-to-GDP	Total gross central government debt divided by gross domestic product[17]	Annual, 1822-1980 (depending on the country)	Reinhart and Rogoff (2011b)
Real GDP per capita	Real gross domestic product per capita in 2011 US dollars	Annual, 1822-1980 (depending on the country)	Bolt and van Zanden (2020)
Exports-Imports	Exports and imports by country, reported in constant prices (millions of 1913 US dollars)	Annual, 1822-1938 (depending on the country)	Federico and Tena Junguito (2018b)
Trade openness	Ratio of exports to GDP by country	Annual, 1830-1938 & 1950-1980	Federico and Tena Junguito (2018a)
BCDI index	A composite index of banking, currency, sovereign default, and inflation crises	Annual, 1822-1980	Reinhart and Rogoff (2013)

Table 2: Descriptive statistics on annualised data [18]

Variable	**Obs.**	**Min**	**Max**	**SD**	**First Year**	**Last Year**	**Countries**
Price	19,475	0.6	430.0	32.9	1822	1980	54
Yield	19,475	0.4	880.1	31.3	1822	1980	54
Gold standard	20,462	0.0	1.0	0.5	1822	1980	54
Inflation	17,537	-200.0	3000.0	1081.8	1822	1980	43
Debt-to-output	17,311	-11.1	1993.0	200.4	1823	1980	47
GDP-per-capita	19,058	349.0	23632.0	3928.9	1822	1980	52
Trade openness	14,393	0.0	4.5	0.2	1830	1980	39
Exports/imports ratio	14,651	0.0	5.2	0.5	1822	1939	54

particulars of the bank notes." As highlighted by Reinhart and Rogoff (2009), a government that opts to default on its debts can hardly be trusted to maintain the stability of its currency, and hence sovereign default is invariably linked to inflation.

Metallism, in some shape or form, heavily influenced the global monetary system throughout the nineteenth century and well into the twentieth century. While literature offers different explanations for why countries moved away from the silver and bimetallic standards during the nineteenth century, it is nevertheless clear that gold standard had become the international standard by the end of the century (Bordo and Schwartz 1997). The traditional assumption in twentieth century monetary thinking was that the compression of interest rate spreads after 1870 was driven by the wider adaptation of the gold standard (Obstfeld and Taylor 2003; Flandreau and Zumer 2004). Bordo and Rockoff (1996) note that the primary economic rationale for adopting the gold standard was to enhance access to international credit markets. Maintaining a reliable record of gold convertibility was perceived as a 'good housekeeping seal of approval'. Indeed, the paper finds that adherence to the gold standard reduced sovereigns' marginal borrowing rates by around 50 basis points. Flandreau and Zumer (2004) find a larger statistically significant result: adherence to the gold standard reduced borrowing rates 150 basis points. Therefore, adherence to the gold standard could be seen as a proxy for fiscal and monetary discipline. Moreover, Schumpeter (1954) suggests that maintaining gold convertibility was as much about national prestige as about monetary prudence. In accordance with Schumpeter's view, Yeager (1984) argues that Austria-Hungary's adaptation of the gold standard was to a significant extent driven by non-monetary factors, such as the perceived inferiority of paper fiat money and the challenges that this posed to national prestige. Nevertheless, even if adherence to the gold standard was held in high regard by contemporaries, the potential intrinsic value of this fact to an investor was not particularly well-developed by contemporary academics prior to 1914 (Flandreau and Zumer 2004).

It is not clear cut to determine when a country is said to have maintained gold convertibility. For a country to credibly maintain convertibility, it was necessary for it attain a sufficiently favourable current account balance as well as accumulate sufficient gold and foreign exchange reserves to maintain exchange rate stability (Jevtic 2022). Moreover, de facto stabilisation can be achieve before de jure, as was the case with Yugoslavia. Albeit the kingdom was one of the last European countries to join the gold standard in June 1931, it had already achieved de facto market stabilisation of the Yugoslav dinar by 1925 (Pisha et al. 2015). Likewise, countries like Spain and Italy spent a prolonged period of time 'shadowing' the gold standard, by pegging their exchange rates to a gold standard currency, but not themselves offering gold convertibility (Reinhart and Rogoff 2009). In accordance with Reinhart and Rogoff (2011b), all periods during which a country achieved de facto exchange rate stabilisation shall be counted towards periods of gold standard adherence.

3.2 Methodology

A baseline regression is formulated in accordance with Cruces and Trebesch (2013), which built on earlier regression methods of Dell'Ariccia et al. (2006) and Panizza et al. (2009) by estimating a continuous measure of investor outcomes (see subsection A.2 for more detail).

$$\mathrm{S}_{b,i,t} = \alpha + [\boldsymbol{\phi_\tau}\mathbf{C_\tau}(\mathbf{b},\mathbf{i},\mathbf{t})] * \mathrm{H_{b,i,t}} + [\boldsymbol{\xi_\tau}\mathbf{C_\tau}(\mathbf{b},\mathbf{i},\mathbf{t})] * \mathrm{R_{b,i}} + \boldsymbol{\beta}^{'}\mathbf{X_{it}} + \xi_b + \nu_t + \epsilon_{b,i,t} \quad (2)$$

where $S_{b,i,t}$ is a spread of bond b in month t in country i. $C_\tau(b,i,t)$ is a binary variable that takes the value of 1 if a bond b in month t exited default τ year(s) ago (where $\tau = 1, 2, 3, 4-5, 6-7$). $X_{i,t}$ is a vector of macroeconomic control variables. ξ_b is a bond fixed effect. ν_t is a time fixed effect. $H_{b,i,t}$ is the most recent SZ-haircut experienced during the life of bond b. $R_{b,i}$ is a time invariant binary variable that takes the value of 1 if bond b defaulted at any point in time. This method allows us to differentiate between the spread changes attributable to default itself (occurrence) and those attributable to the size of the haircut at restructuring (magnitude). Methodologically, inclusion of the $R_{b,i}$ term will allow the intercept of defaulting bonds to differ from that of non-defaulting bonds. The key parameters of interest are the coefficients of the haircut variables ($\boldsymbol{\phi}_\tau$) and the default variables ($\boldsymbol{\xi}_\tau$). In addition to terms and conditions of bond documentation, bond fixed effects will capture time-invariant economic, cultural, institutional and geographic characteristics of a country during the life of the bond. Time fixed effects will control for time variant heterogeneity that is country invariant.

Coefficients of the haircut variables, ϕ_τ, will give us the average effect for sovereign default τ years after the end of the default spell. Obviously, should the treatment group (i.e., the bonds that happened to default) be systematically different to the control group (i.e., the bonds that did not default) our estimators will be biased. It is not too difficult to believe that there is selection into these groups as sovereign defaults are not exogenous events. Endogeneity concerns can be alleviated by including a vector of relevant control variables. However, the problem will likely persist as it is likely that there exists a relevant omitted variable in the error term that is correlated with sovereign default. An instrumental variable could be used to bypass this issue. However, as far as I am aware, a variable that is correlated with sovereign default but independent of the error term has not been found. As such, regression results should not be interpreted with caution.

4 Results

This section will provide the results of regression analysis.[19] As all control variables are measured on an annual basis, most regressions will exploit yearly variation in the data. A simple arithmetic mean of monthly bond level observations is taken to compute annual variables. An average life measure, as developed in section 3, is used to capture differences in remaining bond life in all models. All models also allow serial defaulters to have a different intercept from other countries.[20] For macroeconomic variables, I will predominately use one-year lagged variable to avoid simultaneity bias.[21]

4.1 Full sample

Table 3 showcases the results from running different variations of Equation 2 on the full sample from 1822 to 1981. Model (1) uses a rudimentary version of the baseline equation in which spread (measured in basis points) is regressed on lagged restructuring dummies. The coefficients estimated by the model differ substantially, both in sign and magnitude, from those obtained in past literature. The coefficients indicate that the bonds experienced 505 bps *lower* spreads within the first year after of the restructuring. Coefficients of more distant restructuring lags remain negative and statistically significant until 6-7 years after the restructuring event. Model (3) estimates the same regression with annual data instead of monthly data. Coefficients estimated by this model are similar to those estimated by Model (1), albeit the second and third restructuring lags are not statistically significant. By first noting the strongly statistically significant positive coefficient of the intercept, these findings appear to contrast the graphical evidence portrayed in Figure 7. [22] These results are also in conflict with the descriptive evidence of Tomz (2007), who made explicit reference to sovereign debt before Second World War: yields were high for recently restructured bonds, but they were to expected to fall gradually over time with "years of faithful service". Moreover, using an identical approach for post-1970 data, Borensztein and Panizza (2009) and Cruces and Trebesch (2013) find statistically significant positive coefficients on the restructuring dummies, implying that sovereign spreads tend to *decrease* over time after restructurings, rather than increase. However, it should be recognised that Models (1) and (3) are very simple models without macroeconomic controls. Indeed, when including vector of covariates in Model (3), the coefficients of the restructuring dummies become largely positive and

[19] Occasional missing data observations were addressed whenever possible. See subsection B.3 for detailed notes on all the actions taken to address missing data.

[20] A country is defined as a serial defaulter if it has defaulted more than once on its payment obligations within the last 20 years.

[21] While the use of lagged variables to address simultaneity bias is frowned upon (Reed 2015), I believe that it is the best possible course of action in this case due to the lack of alternatives.

[22] Assuming a constant remaining life of 5 years, regression results of Model (1) would suggest that, on average, bonds exhibit a spread of 624 basis points (bps) within the first year of a debt restructuring, 719 bps between 12-24 months after the event, and 798 bps from 24-32 months following the restructuring.

Table 3: Regression results, full sample

	(1)	(2)	(3)	(4)	(5)	(6)
Data frequency	Monthly	Monthly	Annual	Annual	Annual	Annual
Time period	1822-1980	1822-1980	1822-1980	1822-1980	1822-1980	1822-1980
Observations	217,247	217,247	19,475	19,475	15,844	12,154
# of countries	54	54	54	54	44	34
# of bonds	857	857	857	857	763	646
Dep. Variable	spread	spread	spread	spread	spread	spread
Estimator	PanelOLS	PanelOLS	PanelOLS	PanelOLS	PanelOLS	PanelOLS
R-squared	0.0029	0.0037	0.0033	0.0041	0.0097	0.0216
F-statistic	90.184	66.586	8.6448	6.3759	9.7569	12.563
Intercept	1116.4***	1123.1***	1106.2***	1113.3***	750.07***	1248.1**
	(45.269)	(47.753)	(46.231)	(49.176)	(203.05)	(537.24)
R[τ=1]	-477.79***	-51.059	-464.84**	-88.397	-360.54	-26.442
	(163.89)	(206.48)	(220.64)	(235.16)	(229.19)	(172.19)
R[τ=2]	-383.14**	-139.19	-332.02	-63.290	-439.28*	-165.16
	(174.22)	(207.48)	(278.23)	(331.11)	(242.20)	(190.38)
R[τ=3]	-303.89*	-202.36	-333.79	-207.97	-451.67*	-242.53
	(170.00)	(225.57)	(294.55)	(330.94)	(240.14)	(192.39)
R[τ=4-5]	-499.47***	-23.296	-530.05***	-24.739	-174.19	-39.530
	(130.43)	(167.71)	(186.52)	(201.57)	(143.38)	(113.70)
R[τ=6-7]	-349.33**	29.963	-379.01**	51.493	-109.72	2.4523
	(140.73)	(161.62)	(189.25)	(175.96)	(125.19)	(147.38)
Expected	-0.2393**	-0.2544**	-0.2498**	-0.2671**	0.0356	0.0645
bond life	(0.1041)	(0.1071)	(0.1084)	(0.1128)	(0.0643)	(0.0567)
Serial defaulter	-40.310	-64.920	-48.555	-69.896	120.32	137.46
	(205.85)	(218.29)	(256.25)	(267.00)	(255.77)	(235.11)
H[τ=1]		-9.4308*		-8.3543	-1.3750	3.2520
		(4.9599)		(5.5736)	(7.1824)	(10.218)
H[τ=2]		-5.3521		-5.9384	3.5103	23.392
		(5.8639)		(7.9393)	(9.7357)	(16.262)
H[τ=3]		-2.1987		-2.7490	7.0491	20.948
		(6.3349)		(8.5142)	(10.475)	(14.524)
H[τ=4_5]		-10.867**		-11.249*	-4.3261	4.9138
		(5.2470)		(6.2477)	(4.6160)	(4.4273)
H[τ=6_7]		-8.6011		-9.7198	-2.7536	4.7432
		(5.7139)		(6.4953)	(4.2954)	(5.6528)
GDP per Capita					-0.0320	-0.0061
					(0.0339)	(0.0221)
Gold standard					-297.17***	-222.32**
					(94.115)	(95.226)
Debt-to-output					0.8934***	0.7002**
lagged					(0.3170)	(0.2993)
Inflation lagged						0.0025
						(0.0019)
Yield UK						-199.84*
						(111.42)
Trade openness						379.66
						(429.72)
Trade openness :						-40.680
Yield UK						(70.481)
Trade openness :						-4.4217
BCDI						(3.3354)
Bond FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: All models use robust, bond-year clustered standard errors.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

statistically insignificant (see Table 6 in Appendix C).[23]

Models (1) and (3) have essentially estimated the costs of sovereign defaults on borrowing costs when the extent of investor losses is not accounted for. Model (2) introduces haircut variables to the regression equation, $H[\tau = t]$, as per Equation 2. In practice, these variables are interaction terms between lagged restructuring dummies and net present value haircuts associated with the most recent sovereign debt restructuring. These variables allow us to distinguish between the effect of the magnitude of net present value loss imposed on investors from the occurrence of the restructuring event per se. In Model (2) the coefficients of lagged dummies for restructurings, denoted as $R[\tau = t]$, are larger and no longer statistically significant. The first coefficient of the continuous lagged haircut variables, $H[\tau = 1]$, suggests that a one percentage point increase in the net present value loss experienced by investors is associated with the 9.4 bps *lower* bond spreads within one year of the restructuring event. The coefficients of are all negative in sign, but only two of them are significant at the 10% significance level. This result is surprising in the sense that Cruces and Trebesch (2013) found an opposite result: higher the net present value loss experienced by investors, higher the spread after the restructuring event. However, it must be noted that Model (2) does not utilise a vector of macroeconomic covariates. Repeating this exercise for annual data in Model (4) yields predictable results: coefficients of haircut variables remain negative, but statistically insignificant, potentially due to less variation in the data. In general, the significance of restructuring dummies in Models (1) and (3) appears to have been driven by the magnitude of net present value of losses experienced by investors (or other unobservables that may be correlated with the extent of investor losses).

Models (5) and (6) proceed to add covariates to the regression models. In Model (5), the addition of controls makes the lagged restructuring dummies more negative and, in the case of the second and third dummies, statistically significant. Interestingly, the de facto adherence to the gold standard appears to be associated with significantly lower spreads. Model (6) introduces two interaction terms, along with their constituent elements, as controls to the regression equation: (i) an interaction between the yield on the 3% UK Consul and the trade openness variable, and (ii) an interaction between BCDI index and the trade openness variable.[24] The addition of these interaction terms to the regression equation is motivated by the believe that changes in global variables impact different countries to different extents, based on their integration to the global economy. If global crises and credit conditions were believed to affect all countries to the same extent, there would be no reason to include interaction terms as year fixed effects would capture all variation in the data. Changes in the base rate, as well as the absolute level of the base rate, can be seen as a financial risk indicator. In a high/rising rate environment, sovereigns facing tight fiscal constraints may struggle to refinance or restructure their debts in a way that would be compelling for

[23] To examine whether the restructuring dummies capture heterogeneous temporal effects of sovereign default on spreads, Model (3) was also ran for two subsamples: one from 1820-1939 and another one from 1950-1980. While the coefficients of restructuring dummies appear to be generally larger for the 1820-1939 sample, most coefficients are statistically insignificant. See Table 6 in Appendix C.

[24] Constituent terms of each interaction are added to the regression equation as it is deemed best practice (Brambor et al. 2006).

investors. In the absence of information asymmetries, the lack of fiscal maneuvering room should feed into credit spreads. It is also compelling to believe that a country's degree of global trade integration should govern a country's sensitivity to global credit crunches.[25] The rationale for including an interaction term with the BCDI index follows a similar line of reasoning: it is sensible to believe that a degree of uncertainty in the global economy - as proxied by the prevalence and severity of banking, currency, default and inflation shocks - could have heterogeneous impacts on the sovereign spreads of different countries based on their integration to the global economy.

The introduction of the interaction terms, along with lagged inflation variable, to Model (6) makes the haircut variables more positive, albeit standard errors also increase substantially. The relatively large positive coefficients for the haircut variables, especially in Model (6), is somewhat surprising as it implies that these regression results fall largely in line with those of Cruces and Trebesch (2013). However, it's essential to highlight that coefficients of the variables of primary interest (i.e., the restructuring dummies,$R[\tau = t]$, and the haircut variables, $H[\tau = t]$), are far from statistical significance. Moreover, it should be noted that the number of observations decreases when going from Model (4) to Model (6) as bond observations are dropped with the successive addition control variables.[26] When adding additional covariates, it could be that the change in coefficients could simply reflect the changes in the underlying sample rather than the additional variables themselves. To examine whether this is the case, Model (4) was run on the smaller samples of Model (5) and (6). We find that while coefficients change somewhat in magnitude, the changes remain largely insignificant.[27]

It should be noted that R^2 of even the fully specified models is very low. In contrast, Cruces and Trebesch (2013) attained R^2 figures ranging from 0.4 to 0.5. High R^2 coefficients in Cruces and Trebesch (2013) are potentially partially driven by the inclusion of US low-grade corporate yield as a control variable, which has been relatively strongly correlated with emerging market sovereign bond yields since the 1990s. In my sample, external rates appear to have remarkably little predictive power in explaining variation in spreads, and when UK 3% Consul yield is used as an additional covariate in Model (6), the R^2 coefficient only increases marginally.

4.2 Heterogeneous effects of sovereign default

The patchiness of certain control variable time-series compelled me to subdivide the sample into two subsamples: 1822-1938 and 1950-1980. The unbalanced nature of several covariate times-series, most notably the trade openness variable, results in many observations being

[25] In this specific case, the yield on UK Consuls is assumed to closely mirror the base rate for all bonds. As noted in subsection B.2, I believe that this is a reasonable assumption to make given how small the tracking error is for UK and US rate time-series from 1900 to 1980.

[26] Obviously, this may give rise to selection bias. It is reasonable to believe that ease of access to reliable national accounts by contemporaries may have contributed to their ability to make educated investment decisions. A rational investor ought to have punished countries with poor or unreliable national accounting standards by demanding a higher rate of return on invested capital (Tomz 2007).

[27] See Table 7 in Appendix C for regression results.

dropped from the sample in Table 3. This is not ideal as some variation in data is lost. Likewise, the lagged exports-to-imports ratio could not be used effectively as a covariate with the full sample without sacrificing a substantial amount of variation as the variable only encompasses the years from 1822 to 1938.

In addition to this econometric reasoning, the subdivision of the sample will allow us to examine whether the results in Table 3 hide some heterogeneous effects of sovereign default in different time periods. Examining sovereign debt dynamics before and after the Second World war separately may be reasonable, given that the dynamics in global sovereign lending landscape had changed significantly post-1945 with the creation of new supranational institutions/groupings, like the Paris Club. As noted by Trebesch et al. (2012), the portion of official debt restructurings by the Paris Club increased sharply in the second half of the twentieth century in contrast to private debt restructurings by private financial institutions and other bondholder groups. This trend coincided with low investment into sovereign debt by private creditors in the 1950-60s (Trebesch et al. 2012).

The average (treatment) effect of sovereign default on spreads that is captured in Table 3 is simply the average of the individual (treatment) effects of sovereign default on the spreads of individual bonds. In this section, I have attempted to address the question of if heterogeneous outcomes of sovereign default on spreads may be driving the main results in Table 3.

Figure 8 shows the coefficients obtained for the variables of primary interest from running a fully specified regression with three different subsamples. The gray area around each coefficient is the 10% confidence interval. The first two graphs on the first row showcase the results obtained from Model (6) in Table 3. The second and third row of graphs feature the coefficients obtained from the two smaller subsamples (see Table 8 in Appendix C for complete regression results). Generally, the results from the two subsamples mirror those from the full sample: coefficients of restructuring dummies are (mostly) negative, while those of the haircut variables are positive. Nevertheless, in both samples, some coefficients appear to edge closer to statistical significance. In fact, the first and third lags of haircut variable are highly statistically significant in the postwar sample. However, the lack of a clear channel explaining how defaults lead to higher spreads, combined with the inconsistency in the statistical significance of coefficients (for instance, the coefficient of the second lag does not significantly differ from zero), raises doubts about the robustness of these findings.

It is possible that the time spent in default is a determinant of future borrowing costs. To examine whether this is the case, Equation 2 is amended with a time-in-default variable, $TID_{b,i,t}$, which is the length of the latest default spell (in months) for bond b in country i at time t.[28] Models (7) to (9) in Table 4 showcase the results of including lagged time-in-default variables in the regression equation with a full set of covariates. For the most part coefficients of lagged time-in-default variables are positive and statistically insignificant, no matter whether restructuring dummies or lagged haircuts variables are included. However,

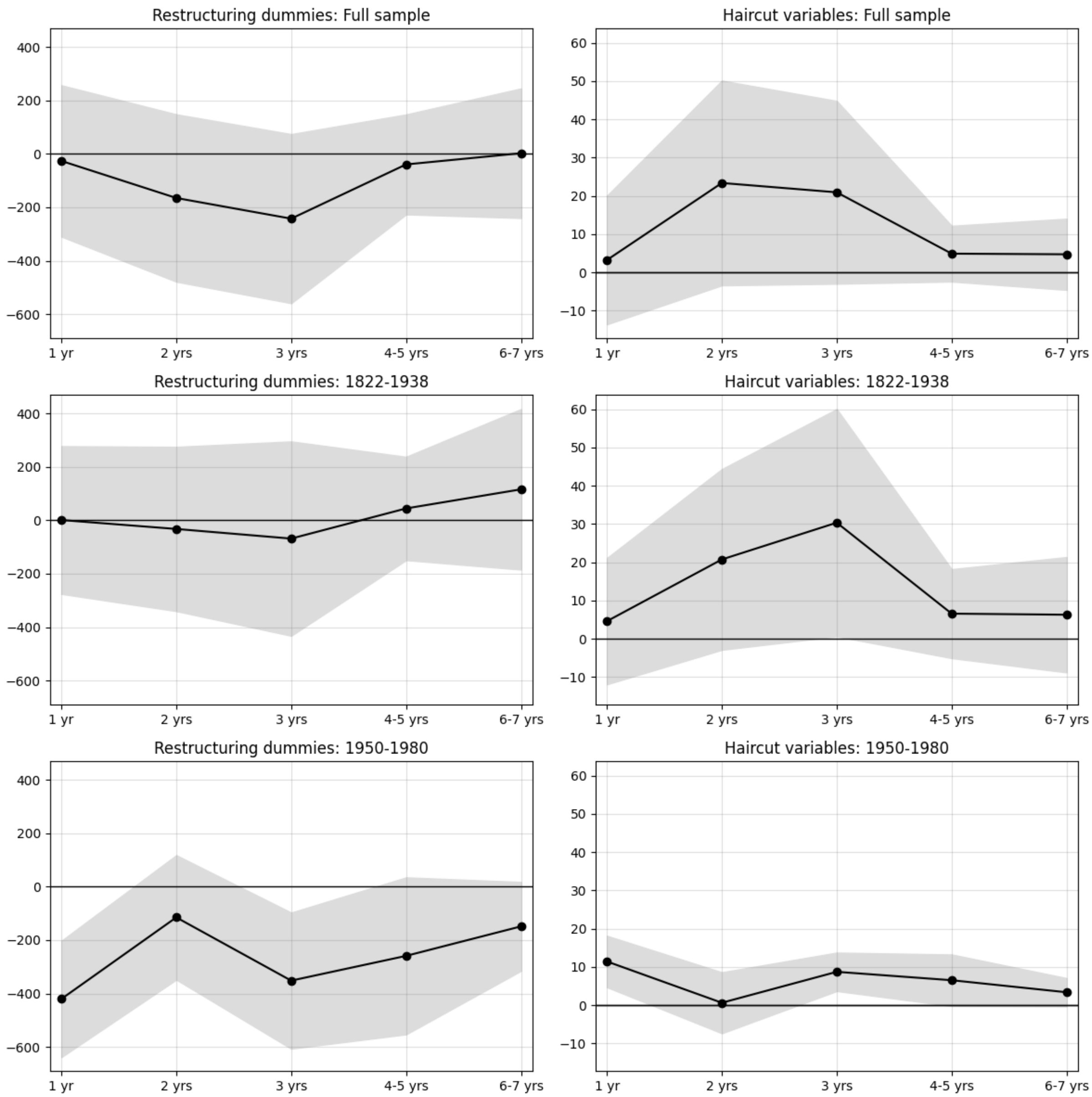
[28]The time-in-default variable is constructed in the same way as the haircut variables, $H[\tau = t]$, introduced earlier. As such, the variable can be interpreted as an interaction between the restructuring dummies, $R[\tau = t]$, and a variable that tracks the cumulative sum of months that a bond has spent in default.

Table 4: Regression results, time spent in default

	(7)	(8)	(9)
Data frequency	Annual	Annual	Annual
Time period	1950-1980	1950-1980	1950-1980
Observations	12,496	12,496	12,496
# of countries	35	35	35
# of bonds	645	645	645
Dep. Variable	spread	spread	spread
Estimator	PanelOLS	PanelOLS	PanelOLS
Cov. Est.	Clustered	Clustered	Clustered
R-squared	0.0236	0.0248	0.0278
F-statistic	20.239	15.629	13.911
Intercept	1143.5**	1146.9**	1122.7**
	(521.25)	(523.24)	(516.25)
TID[τ=1]	2.9876	4.0735	4.2250*
	(2.1575)	(2.6706)	(2.5613)
TID[τ=2]	6.0083	6.5404	4.8368
	(4.1708)	(4.9075)	(4.1490)
TID[τ=3]	4.3373	4.2214	1.5399
	(4.0591)	(4.8333)	(4.1942)
TID[τ=4_5]	0.1772	-0.4815	-1.3922
	(0.9502)	(1.1180)	(1.4840)
TID[τ=6_7]	-0.1025	-1.1680	-2.1960
	(1.1484)	(1.3431)	(1.6897)
R[τ=1]		-242.11	-227.85
		(272.15)	(262.73)
R[τ=2]		-123.54	-335.71
		(217.85)	(304.24)
R[τ=3]		28.816	-249.54
		(244.73)	(284.68)
R[τ=4-5]		143.16	40.979
		(126.54)	(104.75)
R[τ=6-7]		222.52	102.40
		(149.10)	(144.01)
H[τ=1]			-1.1807
			(8.4479)
H[τ=2]			12.304
			(8.4566)
H[τ=3]			16.608*
			(9.9510)
H[τ=4_5]			5.7992
			(5.2521)
H[τ=6_7]			6.6999
			(6.2206)
Bond FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Covariates	Yes	Yes	Yes

Note: All models use robust, bond-year clustered standard errors. All models use a vector of covariates consisting of expected bond life, serial defaulter dummy, GDP per capita, debt-to-output, gold standard, trade openness, UK Consul yield, Trade openness:BCDI, and Trade openness:UK yield.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 8: Coefficients of variables of primary interest



the first lag of the time-in-default variable in Model (9) is statistically significant at the 10% level, implying that each month that a bond spends in default increases post-restructuring spreads. The impact of the time spent in default appears to be short-lasting as the subsequent variables become more insignificant.

5 Discussion & Limitations

There are multiple concerns with the methodology that should be addressed. While not an exhaustive list, this section attempts to highlight some of the most important concerns that could challenge the robustness of my results.

The general issue with my methodology is that sovereign defaults cannot be considered to be exogenous events. In a sense, sovereign default can be considered to be a "treatment" of sorts. If this treatment were randomly allocated and if we were to build a credible set of control variables, causal inference would be theoretically possible. Unfortunately, neither of these are likely to be true in this paper's empirical setting. Due to the low explanatory power of the models in section 4, it is likely that spread changes are associated with unobservables and that my models are miss-specified. For instance, it is clear that the magnitude of haircuts is endogenous, subject to the macroeconomic fundamentals of the country, among many other factors.

It is clear that not all bonds are created equal. Bond characteristics (covenants, collateral, etc.) that may have affected market pricing of bonds are not accounted for in my regression analysis. For instance, there are multiple cases in history where third-party sovereigns offered a guarantee on the debt obligations of another country, sometimes for a set number of years (Tomz 2007). Not controlling for changes in the underlying seniority or collateral of an instrument is likely to lead to biases. To my benefit, it would be fair to claim that covenants of the time period's bonds were relatively simple and infrequent, and mainly related to the obligations of the debtor if payment difficulties were to arise (Fenn and Nash 1869). Obviously, exceptions to this rule are bound to exist.

As with bonds, not every sovereign restructuring should be inspected through the same lenses. While I am able to distinguish between restructurings based on their severity by utilising a haircut measure, I am not able to differentiate between restructurings based on other characteristics. For instance, in many cases, sovereigns have attempted to restructure their debt preemptively, and debt restructuring can be viewed as a preemptive political choice with respect to an impending default. Asonuma and Trebesch (2016) finds that preemptive debt restructurings from 1978-2010 were frequent in nature (38% of all deals), had shorter negotiation periods and lower haircuts. However, it is not possible to distinguish between "normal" and "preemptive" restructurings in my sample. Likewise, it plausible that some "preemptive restructurings" do not show up in my sample at all, as they did not involve a sovereign default in the first place. As anecdotal evidence, Caselli et al. (2021) found that while Colombia was the only major Latin American country not to default on its debts in the 1980s, it nevertheless went through a debt restructuring in largely similar fashion to its Latin American peers. Colombia achieved this through ample assistance from US authorities, who, among other motivations, viewed Colombia as an important ally on its war on illegal drug trafficking. Without archival research into the topic, it is unclear how many similar cases there are throughout history. In the opposite end of the spectrum, some sovereign restructurings involve a short-term fix that enables the sovereign to continue to serve its debt in the short-run, but fails to restructure the debt in a fashion that would allow long-run debt sustainability (Reinhart and Trebesch 2016). For instance, by utilising a sample of 90 defaults by 73 countries from 1989-2006, Benjamin and Wright (2009) showed that sovereign restructurings do not result in a uniform reduction in the debtor's debt overhang. In fact, the median country exited default with a debt-to-GDP ratio 5 percentage points higher than before they entered default. Neither can I account for any possible debt relief that may have taken place[29] or structural subordination of certain bonds over others.[30]

As another challenge for identification, it is possible that the spread development of countries are not independent from one another. Financial contagion, in which a weakness or default of one sovereign may invite crises to other countries as investors become more anxious, is possible, particularly in the aftermath of lending booms. If the stable unit treatment value assumption (SUTVA) is violated, it would imply that sovereign default episodes are linked. This mechanic is not too distant from what was seen during the 1840s Latin America in the wake of the region's lending boom or, more recently, during the Euro crisis and 1990s Asian Financial crisis. One solution would be to control for inter-linkages between countries. As these linkages between entities are unlikely to be constant in time, they are not captured by entity fixed effects. During the research process, I did not find a database which would have allowed me to credibly establish extensiveness of bilateral links between countries.

As noted by Meyer et al. (2022) the process of compiling the bond price dataset was a laborious and manual process due to the non-digitalised nature of the secondary data. During the writing process, I was able to uncover several probable errors in the data.[31]

5.1 Avenues for further research

There are several weaknesses to this paper that future research could address.

Credit market access after a sovereign default. In this paper, I have attempted to answer the question of how sovereign default affects the subsequent borrowing costs of the sovereign. However, an equally important question is whether the sovereign was excluded from credit markets altogether as a result of the default. À la Cruces and Trebesch (2013), future research could attempt to answer this question in a more comprehensive fashion for the time period preceding 1980.

Redemption schedules. The inability to obtain bond-specific redemption schedules constitutes as this papers most significant weakness. As discussed earlier, a significant amount of sovereign bonds in the nineteenth and early twentieth century did not have a predetermined maturity date. Rather, bonds were called periodically through sinking fund

[29]While debt relief was mostly utilised in relation to sovereign-to-sovereign war debts (Reinhart and Trebesch 2016), it is nevertheless possible that different forms of debt relief may have directly affected the credit risk associated with the bonds in the dataset of Meyer et al. (2022).

[30]While seniority should be respected in a restructuring, there are multiple cases in history in which this has not been the case (Bolton et al. 2023). However, literature on structural subordination in sovereign bonds before the 1980s in generally under-developed and it is not clear to me how much emphasis should be based on it.

[31]In several cases, restructured bonds were assigned to different countries than the bond in default. See source code for further details.

arrangements. To the best of my knowledge, a comprehensive dataset containing redemption dates for nineteenth and early twentieth century sovereign bonds has not been published. Creating such a dataset would necessitate a manual review of historical archives, a meticulous effort comparable to the one undertaken by Meyer et al. (2022). Within the time constraints a postgraduate thesis, this would not have been practical or feasible. The construction of bond-specific redemption schedules would have allowed the creation of a more sophisticated duration measure for bonds. In this paper, I was forced to settle on a less-than-ideal average life measure (see subsubsection 3.1.1), which is likely to overstate the maturity of a bond. A more refined approach to that of this paper would be to estimate the entire term structure of interest rates for each country, as done by Eitrheim et al. (2004) for Norway 1819-2003. While more data-intensive, this method would have also allowed me to address the problem of duration mismatch when calculating spreads.[32]

Accounting for domestic sovereign debt. Reinhart and Rogoff (2011a) showed that domestic sovereign debts were significant in absolute terms (accounting for two thirds of all sovereign debt issued) for a sample of 64 countries between 1900-2010.[33] Large domestic sovereign debt loads also appeared to suggest why many countries defaulted on seemingly small external sovereign debt amounts. Evidence of Reinhart and Rogoff (2011a) appears to suggest that local currency denominated debt should not be neglected when estimating the impact of sovereign default on post-event spreads. Yet, in most papers, the question of domestic debt is often simply bypassed, if it is mentioned at all in the first place. The most pressing hurdle in the way of incorporating domestic sovereign debt into the literature discussion has been poor data availability. The topic remains important nevertheless.

Effect of bond characteristics of investor sentiment. Bond prospectuses of the era discussed how the proceeds of the bonds would be used and how bond interest payments were financed. In many cases a certain percentage of a particular taxes' revenue was allotted for debt repayment (Fenn and Nash 1869). An interesting avenue for further research could be to examine how, if at all, credit characteristics affected bond spreads and sovereign debt sustainability.

6 Conclusion

In the wider literature, few attempts have been made to quantify the reputational costs of sovereign default before 1980. To my knowledge, this paper is the first to comprehensively study the empirical relationship between sovereign spreads and default before the 1980s. When controlling for country's macroeconomic fundamentals, I find limited evidence that higher net present value losses for investors in restructurings ("haircuts") are associated with

[32]In this paper, spreads were calculated by comparing bond yields to an appropriate base rate *irrespective* of bond maturity. This method is far from appropriate, but a necessary simplification considering the time limitations that I faced.

[33]In the context of Reinhart and Rogoff (2011a), domestic sovereign debt is defined simply as sovereign debt issued under the home jurisdiction, regardless of the currency of denomination or the debt holder. Nevertheless, the authors note that the vast majority of such debt was in fact denominated in the local currency and held by domestic investors.

higher sovereign spreads for once-defaulted bonds. While the size of the haircut appears to be associated with elevated sovereign spreads up to 36 months after a sovereign restructuring, these results are not statistically significant at any commonly used significance level. I also find that the amount of time that a bond spends in default is associated with higher spreads. However, the robustness of this result is also questionable. This paper is unable to suggest a channel through which these effects may operate. As made abundantly clear in the earlier sections, these results should not be interpreted as the causal effect as there are concerns over misspecification and weak statistical power of even fully specified models.

A Appendix: Supplementary material

A.1 Supplementary graphs

Figure 9: Number of countries in the sample

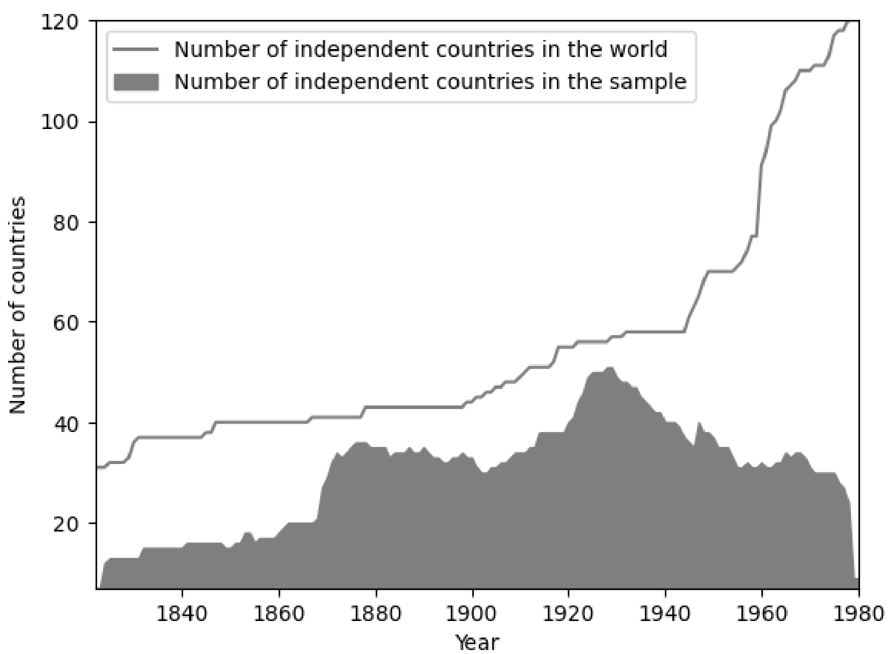
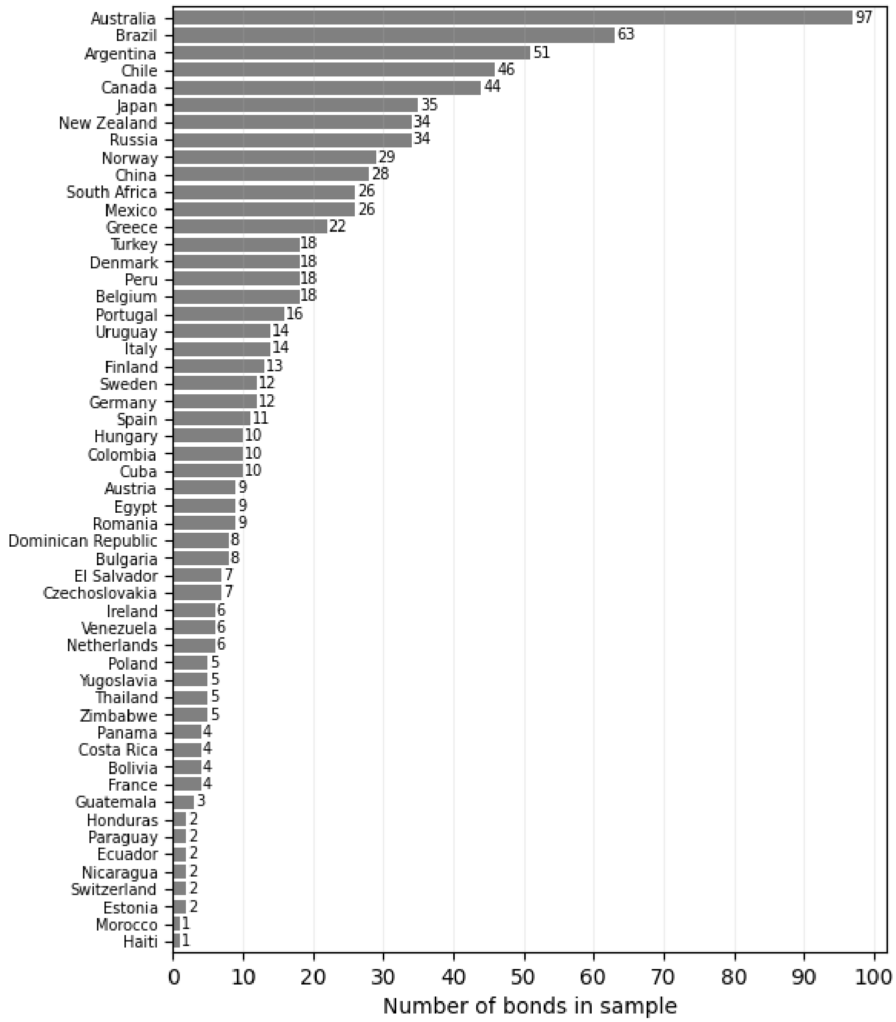


Figure 10: Number of bonds in the full sample by country



A.2 Regression equations

Equation 3 corresponds to the rudimentary version of Equation 2, including only the lagged restructuring dummies along with various covariates. This regression is used in Models (1) and (3) in Table 3.

$$\begin{aligned}\mathrm{S}_{b,i,t} = \alpha \quad & + [\phi_1 \mathrm{C}_1(\mathrm{b,i,t}) + \phi_2 \mathrm{C}_2(\mathrm{b,i,t}) + \phi_3 \mathrm{C}_3(\mathrm{b,i,t}) + \phi_{4-5} \mathrm{C}_{4-5}(\mathrm{b,i,t}) \\ & + \phi_{6-7} \mathrm{C}_{6-7}(\mathrm{b,i,t})] * \mathrm{H}_{\mathrm{b,i,t}} \\ & + \boldsymbol{\beta}_k \mathbf{X_{it}} + \xi_b + \nu_t + \epsilon_{b,i,t}\end{aligned} \tag{3}$$

To distinguish between the effect of the size of the haircut (magnitude) from the effect of very existence of a default episode (occurrence), a vector of haircut variables can be added to to previous regression as seen in Equation 4. This regression is analogous to Equation 2 in subsection 3.2. This is also the primary regression equation used by Cruces and Trebesch (2013).

$$\begin{aligned}
\mathrm{S}_{b,i,t} = \alpha \quad & + [\phi_1 \mathrm{C}_1(\mathrm{b,i,t}) + \phi_2 \mathrm{C}_2(\mathrm{b,i,t}) + \phi_3 \mathrm{C}_3(\mathrm{b,i,t}) + \phi_{4-5} \mathrm{C}_{4-5}(\mathrm{b,i,t}) \\
& + \phi_{6-7} \mathrm{C}_{6-7}(\mathrm{b,i,t})] * \mathrm{H_{b,i,t}} + \\
& + [\xi_1 \mathrm{C}_1(\mathrm{b,i,t}) + \xi_2 \mathrm{C}_2(\mathrm{b,i,t}) \xi_3 \mathrm{C}_3(\mathrm{b,i,t}) + \xi_{4-5} \mathrm{C}_{4-5}(\mathrm{b,i,t}) \\
& + \xi_{6-7} \mathrm{C}_{6-7}(\mathrm{b,i,t})] * \mathrm{R_{b,i}} \\
& + \boldsymbol{\beta}_k \mathbf{X_{it}} + \xi_b + \nu_t + \epsilon_{b,i,t}
\end{aligned} \tag{4}$$

B Appendix: Data Manipulation

B.1 Bond pricing

The dataset of Meyer et al. (2022) does not indicate the price at which a bond was redeemed. The approach taken in this paper was to generally assume that the final trading price of the bond was indicative of the ultimate redemption value. Figure 11 gives the final trading monthly prices of bonds (excl. to-be-restructured bonds) in the sample. When a bond traded below or slightly above par value in the final price observation, the bond was assumed to be redeemable at par. When bond traded above 105 in the final price observation, the bond was assumed to be redeemable at the last trading price, rounded to the nearest integer.

Figure 11: Final trading prices of bonds

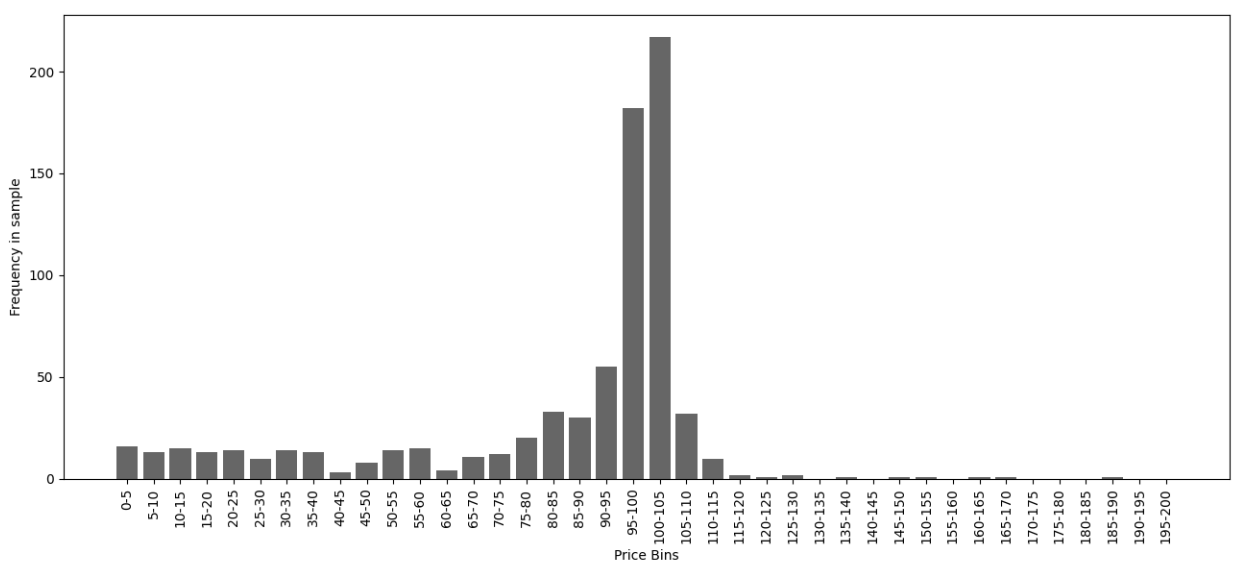
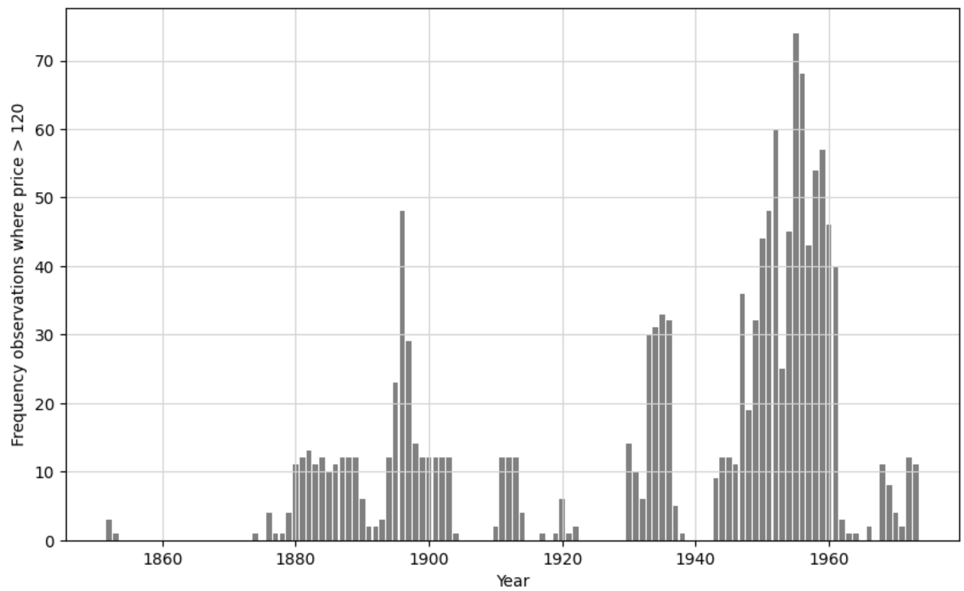


Figure 12 shows that only 0.1% of price observations have a price above 120. While these price observations are outliers, they will not pose a problem to yield calculations as we assume that these bonds were redeemed above par.

Figure 12: Frequency of price observations with a price above 120



B.2 Risk-free rates

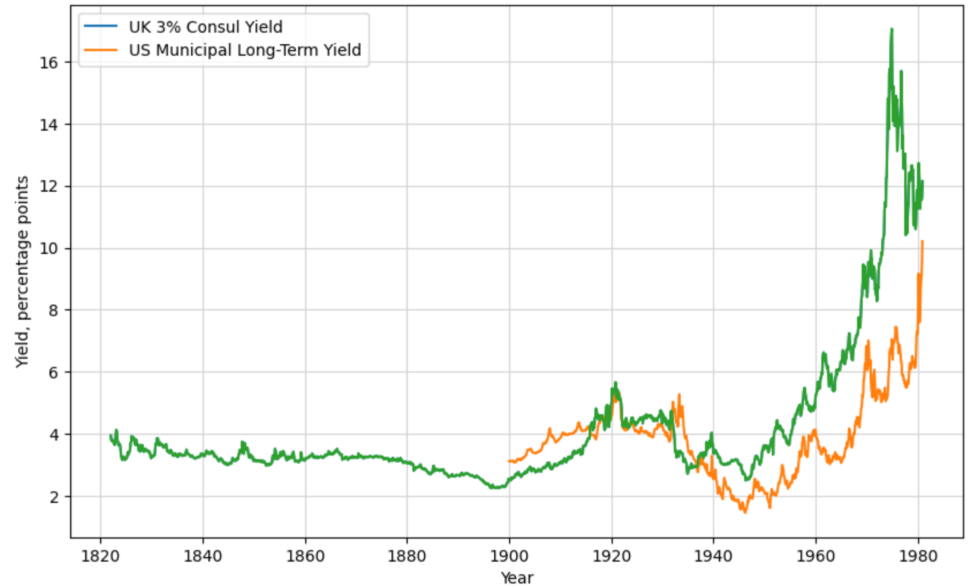
As observed in Figure 13, the twentieth century was characterised by substantial rate volatility when compared to the nineteenth century. Particularly the UK 3% perpetual Consul yields surged after Second World War.

A peculiar stickiness in the prices of certain bonds is observed in the latter half of the sample. For instance, as showcased in Figure 14, the yield development of the Chilean bonds of 1911, denominated in Pound Sterling, exhibits no clear positive correlation with the UK risk-free base rate, which is likely mirrored by the 3% Consul yields. In fact the bond appears to trade near par for an extended period of time before the bond's final redemption in the 1980s.

It is predominantly bonds like this that give rise to negative spread observations. There are several explanations for this type of behaviour. Firstly, duration mismatch between the risk-free rate and instrument is likely to cause measuring error, especially when the instrument is on the verge of being redeemed. Indeed, this type of yield developments of may arise purely due to the very nature of the instruments themselves, as the instrument is gradually redeemed by the sovereign by successive lotteries. Other possible explanations for sticky yields can be the illiquidity of secondary markets and the unwillingness or inability of market actors to exit a position.

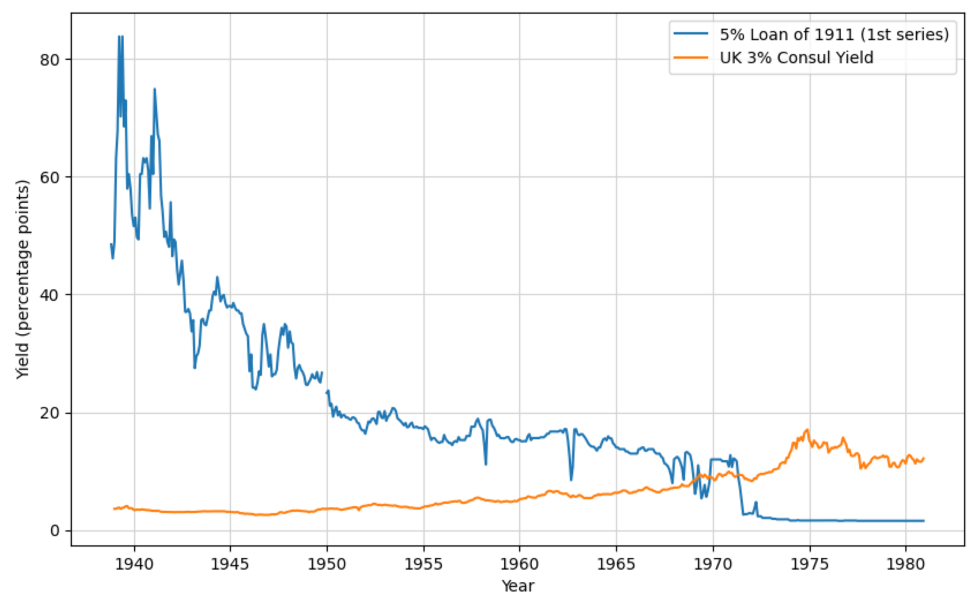
As alluded to in the Online Appendix of Meyer et al. (2022), it is possible that bonds like the Chilean 5% loan of 1911 are subject to covenants that de facto make them tied to an alternative medium of exchange, such as gold or USD, rather than the British Pound Sterling. As noted by Flandreau and Sussman (2004), many sovereigns made use of exchange clauses that made payment possible in multiple mediums of exchange particularly in the

Figure 13: Risk-free rates over time



nineteenth century. Encouragingly, if we opt to drop outlier bonds with multiple negative spread observations, like the Chilean 5% loan of 1911, the results do not change to significant extent.

Figure 14: Chilean Bonds of 1911



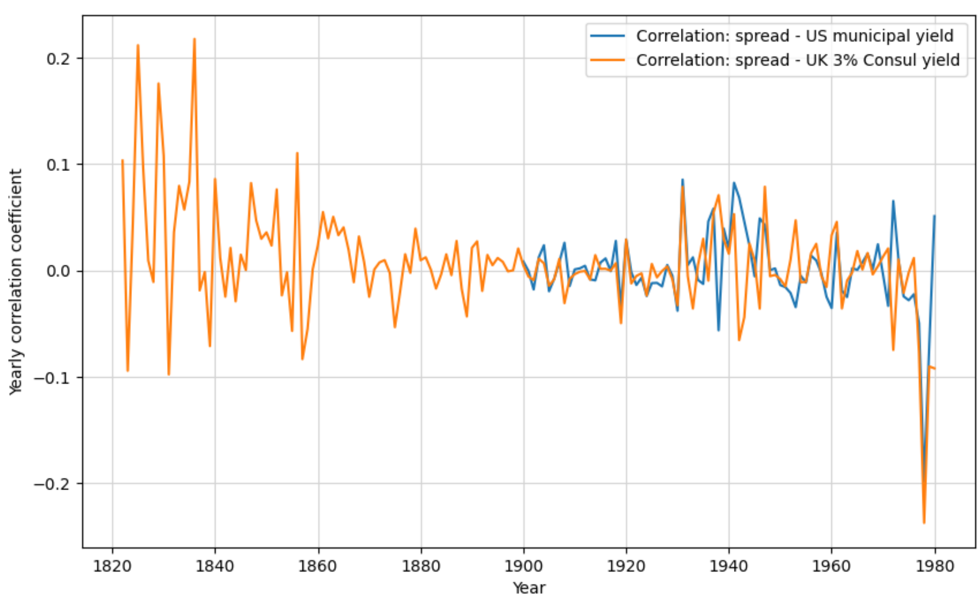
The correlation coefficient between external rates and spreads varies substantially over time in the sample as demonstrated by Figure 15. During several periods, most notably 1820-

Table 5: Average correlation coefficients of external rates and sovereign spreads

Variable pair	Time period	Corr. coefficient
Spread - UK Consul yield	1822-1980	-0.0314
	1822-1939	-0.0464
	1939-1980	-0.1207
Spread - US Municipal yield	1900-1980	-0.1257
	1900-1939	-0.0487
	1939-1980	-0.1057

1870 and 1970-1980, were characterised by increased volatility in the correlation coefficient. When comparing at the average correlation coefficient for both of the external rate time series before and after 1939, the results are markedly similar, as suggested by Table 5. Traditionally, a correlation between spreads and external rates is seen as a proxy for financial distress (Gilchrist et al. 2022). A more open country more may be more susceptible to changes in global funding conditions. If we were believed that all countries were to be impacted by global financial distress to the same extent, the effect would be captured by time fixed effects. If this is not the case, coefficients may be biased. I propose that a country's trade openness, as defined by Federico and Tena Junguito (2018a), can act as a proxy to the extent to which global event may affect a country.

Figure 15: Correlation coefficients of external rates and sovereign spreads



B.3 Addressing missing data

The general challenge with addressing missing data is that the probability that a given data point is missing for a country is likely not random, but correlated with internal and global developments. For instance, data is often missing for conflict-ridden countries during periods of global unrest, like for many European countries during the two World Wars. By and large, this reduces the usefulness of data imputation methods addressing the issue. Below I have listed all the efforts taken to address missing data observations.

- **GDP-per-capita**. Multivariate imputation was used to extend the GDP-per-capita time series for several countries. The purpose of this covariate is to act as a proxy for country's general level of social, economic and institutional development. Assuming that the temporal variance in the levels of these variables is relatively low, I deemed multivariate quadratic imputation an appropriate tool in this instance.

- **Debt-to-exports/output**. Multivariate imputation was used to extend the debt-to-exports/output time series for several countries when the measure was deemed appropriate. For many countries, debt-to-exports/output figures were missing for years in which the country was engaged in a military conflict, particularly during and after the two World Wars. In the following cases a direct utilisation of multivariate imputation was deemed inappropriate: France (1932-1948), Germany (1939-1950), Greece (1914-1918 and 1940-1949) and Japan (1940-1951). In each of these cases, I will assume that the last available debt ratio for each country is equal to the country's ratio at the end of the conflict (either 1918 or 1945). The remaining missing values were derived using quadratic multivariate imputation.

- **Balance of trade data**. Data sourced from Federico and Tena Junguito (2018b) had several missing data points for Austria-Hungary (Austria), Belgium, Serbia (Yugoslavia), Romania and the Ottoman Empire (Turkey) between 1914-1922. All these missing data points therefore concern European countries that were militarily major participants of the First World War. By looking at development of the ratio of exports to imports for those European countries that also partook in the conflict but for which we have data, we can develop an understanding of how the ratio may have developed for the countries with missing data. On average, Germany, France, Italy, Russia, and United Kingdom saw their exports-to-imports ratio change by -0.2% in 1914, -42.5% in 1916, -19.9% in 1917 and +10.6% in 1918. I will use these figures to estimate the missing levels of the exports-to-imports ratio.

- **Gold standard**. Reinhart and Rogoff (2011b) did not provide gold standard data for six countries: Bulgaria, Cuba, Czechoslovakia, Estonia, Haiti, and Serbia/Yugoslavia. Data for these countries was gathered using other sources. Estonia was part of the gold standard from 1927 to 1933 (Sutt et al. 2002). Bulgaria maintained gold convertibility between 1906-1912 and again from 1928-1931 (Pisha et al. 2015). Yugoslavia de facto maintained gold convertibility from 1925 to 1931 (Pisha et al. 2015).Czechoslovakia maintained gold convertibility between 1926-1931 (Wandschneider 2008). I could not find any information on whether Haiti or Cuba followed the gold standard.

C Appendix: Robustness checks

Table 6 gives regression results for running a rudimentary version of Equation 2 without haircut terms. Model (C1) is identical to Model (3) in Table 3 of section 4. Models (C2) to (C3) introduce covariates to the regression equation. To examine whether results vary by time period, Models (C4) and (C5) are ran on temporal subsamples of the main sample.

Table 7 gives regression results for running Equation 2 using continuously smaller samples. Model (C6) utilises a full sample, just like Model (4) in Table 3. The sample of Model (C7) accounts for the observations dropped due to the addition of GDP-per-capita as a covariate, corresponding to that of Model (5). The sample of Model (C8) accounts for the observations dropped due to the addition of debt-to-output and GDP-per-capita as covariates. The sample of Model (C9) accounts for the observations dropped due to the addition of debt-to-output, inflation and GDP-per-capita as covariates. The sample of Model (C9) corresponds to that of that of Model (6) in Table 3. The sample of Model (C10) accounts of the observations dropped due to the addition of trade openness to the model.

Table 8 shows the results for running various variations of the baseline equation with smaller subsamples of the full sample.

Table 6: Robustness check: rudimentary model

	(C1)	(C2)	(C3)	(C4)	(C5)
Data frequency	Annual	Annual	Annual	Annual	Annual
Sample years	1822-1980	1822-1980	1822-1980	1822-1938	1950-1980
No. Observations	19,475	18,120	12,496	9,701	2,795
# of countries	54	52	35	35	24
# of bonds	857	827	645	507	220
Dep. Variable	spread	spread	spread	spread	spread
Estimator	PanelOLS	PanelOLS	PanelOLS	PanelOLS	PanelOLS
Cov. Est.	Clustered	Clustered	Clustered	Clustered	Clustered
R-squared	0.0033	0.0147	0.0154	0.0294	0.0851
F-statistic	8.6448	28.478	14.116	19.645	19.624
Intercept	1106.2***	2509.4***	1100.6**	1540.9	1815.2*
	(46.231)	(658.43)	(558.07)	(5208.4)	(957.52)
R[τ=1]	-464.84**	-477.32*	104.47	157.84	-23.416
	(220.64)	(246.04)	(226.86)	(227.36)	(115.39)
R[τ=2]	-332.02	-314.97	438.01	559.87	-69.552
	(278.23)	(313.91)	(306.42)	(341.73)	(103.44)
R[τ=3]	-333.79	-329.90	450.26	692.40	93.774
	(294.55)	(332.21)	(349.11)	(469.82)	(94.175)
R[τ=4-5]	-530.05***	-509.07***	96.898	195.94	74.978
	(186.52)	(191.77)	(103.68)	(144.41)	(84.123)
R[τ=6-7]	-379.01**	-325.48*	100.01	266.26*	12.734
	(189.25)	(182.19)	(111.82)	(161.04)	(49.198)
Expected bond life	-0.2498**	-0.3424**	0.0484	-0.0616	0.1398***
	(0.1084)	(0.1593)	(0.0559)	(0.0891)	(0.0527)
Serial defaulter	-48.555	-278.73	88.841	231.61	-169.91**
	(256.25)	(305.28)	(217.12)	(431.02)	(74.443)
GDP per Capita		-0.2645**	-0.0042	-0.2475***	0.0240
		(0.1292)	(0.0216)	(0.0919)	(0.0173)
Gold standard		-543.19***	-156.56**	-180.93**	
		(130.09)	(79.652)	(84.192)	
Debt-to-output, lagged			0.9273***	0.9069**	-0.0518
			(0.3539)	(0.3949)	(0.2999)
Yield UK			-166.74	20.522	-237.41*
			(117.87)	(1482.2)	(122.11)
Trade openness			403.15	1255.3	-2311.6***
			(490.42)	(796.55)	(865.37)
Trade openness: Yield UK			-142.73	-425.86*	88.667**
			(89.829)	(224.29)	(44.820)
Exports/Imports lagged				-86.509	
				(162.51)	
Bond FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Note: All models use robust, bond-year clustered standard errors.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: Robustness check: effect of sample size on regression coefficients

	(C6)	(C7)	(C8)	(C9)	(C10)
Data frequency	Annual	Annual	Annual	Annual	Annual
Sample years	1822-1980	1822-1980	1822-1980	1822-1980	1822-1980
Observations	19,475	18,120	15,844	15,134	12,154
# of countries	54	52	44	40	34
# of bonds	857	827	763	736	646
Dep. Variable	spread	spread	spread	spread	spread
Estimator	PanelOLS	PanelOLS	PanelOLS	PanelOLS	PanelOLS
Cov. Est.	Clustered	Clustered	Clustered	Clustered	Clustered
R-squared	0.0042	0.0041	0.0034	0.0029	0.0020
F-statistic	6.4349	5.9049	4.2754	3.4062	2.1616
Intercept	1113.6***	1032.6***	613.21***	578.44***	550.76***
	(49.136)	(53.183)	(27.117)	(27.656)	(29.797)
R[τ=1]	-89.839	12.149	-263.81	-102.93	-84.083
	(235.06)	(253.63)	(238.42)	(248.10)	(263.08)
R[τ=2]	-62.668	31.814	-388.67	-281.33	-269.66
	(330.82)	(349.01)	(248.37)	(263.29)	(287.41)
R[τ=3]	-206.61	-144.42	-356.69	-312.99	-318.62
	(330.58)	(357.54)	(240.69)	(267.37)	(301.59)
R[τ=4-5]	-22.918	85.198	-50.172	-51.175	-17.400
	(201.47)	(213.30)	(147.72)	(152.63)	(169.42)
R[τ=6-7]	52.237	95.313	-31.028	-38.670	-51.168
	(175.90)	(174.09)	(125.34)	(128.55)	(138.94)
Expected	-0.2674**	-0.2660**	0.0384	0.0631	0.0525
bond life	(0.1127)	(0.1220)	(0.0629)	(0.0645)	(0.0659)
Serial defaulter	-67.351	-164.18	181.00	-41.561	-75.785
	(266.95)	(296.43)	(246.90)	(247.62)	(246.71)
H[τ=1]	-8.3430	-10.006*	-3.1883	-6.2188	-6.7569
	(5.5718)	(6.0621)	(7.1003)	(7.3478)	(9.5258)
H[τ=2]	-6.0454	-7.4714	2.0722	-0.2288	1.3232
	(7.9269)	(8.5237)	(9.5417)	(10.314)	(13.242)
H[τ=3]	-2.8971	-3.7098	5.3171	4.3859	6.3400
	(8.5007)	(9.1850)	(10.179)	(10.737)	(13.856)
H[τ=4-5]	-11.378*	-12.963**	-6.5193	-6.2951	-5.9244
	(6.2472)	(6.3179)	(4.6047)	(4.9660)	(6.5228)
H[τ=6-7]	-9.7737	-10.221*	-4.4262	-4.1050	-1.8838
	(6.4889)	(6.1870)	(4.1096)	(4.4785)	(5.6309)
Bond FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Note: All models use robust, bond-year clustered standard errors.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8: Robustness check: subsamples

	(C11)	(C12)	(C13)	(C14)	(C15)	(C16)
Data frequency	Annual	Annual	Annual	Annual	Annual	Annual
Time period	1822-1938	1822-1938	1822-1938	1950-1980	1950-1980	1950-1980
Observations	12,891	11,381	9,701	3,511	2,988	2,795
# of countries	50	44	35	39	27	24
# of bonds	670	615	507	260	233	220
Dep. Variable	spread	spread	spread	spread	spread	spread
Estimator	PanelOLS	PanelOLS	PanelOLS	PanelOLS	PanelOLS	PanelOLS
Cov. Est.	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered
R-squared	0.0256	0.0286	0.0391	0.0198	0.0623	0.1027
F-statistic	22.651	20.877	18.469	4.9856	12.874	16.077
Intercept	1810.2***	1098.7***	1728.2	3034.1***	309.22	2012.0**
	(345.80)	(305.37)	(5095.3)	(1155.1)	(259.25)	(959.62)
R[τ=1]	-82.828	-79.824	0.8858	1515.3***	-75.456	-420.38***
	(228.60)	(172.25)	(168.09)	(534.65)	(172.05)	(132.37)
R[τ=2]	205.96	-132.76	-32.552	1954.5***	-8.0749	-115.05
	(293.29)	(204.43)	(186.89)	(546.45)	(209.86)	(141.54)
R[τ=3]	-30.349	-211.05	-68.608	1892.3***	49.150	-351.77**
	(188.69)	(218.13)	(221.11)	(589.83)	(225.30)	(154.81)
R[τ=4-5]	29.206	27.558	44.266	1147.3*	54.654	-259.11
	(159.91)	(117.46)	(117.56)	(675.67)	(213.00)	(178.56)
R[τ=6-7]	77.413	74.641	115.83	1119.9*	171.81	-148.70
	(157.87)	(137.01)	(182.59)	(658.82)	(194.91)	(101.09)
Serial defaulter	399.49	682.33	280.30	-601.43	-159.67	-142.56*
	(286.69)	(416.28)	(455.18)	(405.60)	(106.67)	(73.398)
Expected	-0.0420	0.0290	-0.0448	-0.1763	0.1565***	0.1484***
bond life	(0.0596)	(0.0647)	(0.0925)	(0.2171)	(0.0466)	(0.0528)
H[τ=1]	1.3750	2.9870	4.6236	-35.692***	-4.6820	11.442***
	(6.6793)	(8.5294)	(10.032)	(11.224)	(4.3230)	(4.0811)
H[τ=2]	9.1210	13.492	20.756	-42.994***	-4.9222	0.6481
	(10.420)	(13.085)	(14.358)	(13.380)	(5.8342)	(4.8430)
H[τ=3]	16.937	23.671	30.371*	-38.712***	-4.1511	8.7388***
	(12.000)	(15.896)	(18.047)	(12.480)	(5.2082)	(3.0510)
H[τ=4_5]	1.3913	2.2454	6.5891	-26.603**	-6.0846	6.5437
	(5.8388)	(6.3380)	(7.0666)	(11.687)	(4.3818)	(4.0895)
H[τ=6_7]	0.6945	1.3529	6.3052	-20.124*	-7.3683	3.3945
	(6.2849)	(7.7814)	(9.1604)	(11.932)	(4.7588)	(2.2488)
GDP per Capita	-0.2687***	-0.1592**	-0.2314***	-0.1835	-0.0049	0.0242
	(0.0882)	(0.0696)	(0.0889)	(0.1141)	(0.0231)	(0.0172)
Gold standard	-530.18***	-247.68***	-214.51**			
	(123.77)	(82.353)	(91.812)			
Debt-to-output		0.5357	1.0099***		0.2775	-0.0235
lagged		(0.3344)	(0.3705)		(0.5221)	(0.3043)
Yield UK			-49.803			-261.93**
			(1446.5)			(121.05)
Trade openness			980.75			-2612.9***
			(735.75)			(961.40)
Exports/Imports			-101.74			
lagged			(161.57)			
Trade openness:			-308.84			172.17**
Yield UK			(233.88)			(78.870)
Trade openness:			-2.2862			-20.305*
BCDI			(4.0491)			(11.794)
Bond FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: All models use robust, bond-year clustered standard errors.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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