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# Aggregate debt servicing and the limit on private credit[1]

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## Abstract

This paper reviews the debt service ratio (DSR) as a theoretically well-grounded indicator of systemic risk. The DSR has the desirable feature that it fluctuates around a stable level which makes its early warning signals easy to understand and communicate. In contrast, current early warning indicators (EWIs) based on credit-developments lack clear economic interpretations and require statistical detrending, which can reduce their accuracy and usefulness for macroprudential policymakers. The review of the literature shows that the DSR provides highly accurate early warning signals for crises and future economic slowdowns, outperforming traditional credit-based indicators. By extending the measurement of the DSR back to the 1920s – a novel contribution in this paper – we demonstrate its EWI effectiveness across different historical periods and show that the DSR acts as an upper limit on benign financial deepening. The paper also outlines questions for future research.

Keywords: Macroprudential policy, early warning indicators, financial crises, debt service ratio, financial deepening, economic history

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# 1. Introduction

Effective macroprudential policy making hinges on the ability to identify the build-up of systemic risk in real time. In the absence of a single well-defined measure of systemic risk, policymakers utilise a broad set of early warning indicators (EWIs).[2] The core indicators among this set capture developments in private sector credit aggregates, such as the credit-to-GDP ratio, based on their outstanding early warning properties.[3] Deviations of the credit-to-GDP ratio from its estimated trend even factor into regulation, as the Basel Committee suggests it as a starting point for setting countercyclical capital buffer levels (Basel Committee (2010a)).

The use of credit-based EWIs comes with well-known conceptual and empirical challenges. For one, these measures lack clear theoretical foundations and are typically viewed as proxies for economic concepts like leverage, net-worth, or debt service capacity. This lack of clarity can hinder their applicability in practice, as policymakers typically prioritise interpretability over accuracy (Onkal et al (2002)). Theoretically well-grounded indicators also facilitate clearer communication, which is crucial for policy effectiveness (CGFS (2012)). Moreover, complications arise from the need to detrend these measures in the presence of the long-term growth pattern often referred to as the financial hockey stick (Schularick and Taylor (2016)). For instance, without detrending, the credit-to-GDP ratio would have misleadingly indicated a continual increase in systemic risk over decades. Therefore, it is common practice to use statistical transformations such as taking medium-term growth rates or deviations from some filtered trend estimate. However, these techniques are prone to error when the unknown underlying trend changes direction, which often is when policymakers most need accurate signals.

These complications ultimately reduce the ability to distinguish between "good" and "bad" credit booms (Gorton and Ordonez (2020)), which raises the potential for costly mistakes for macroprudential policymakers. For example, standard EWIs implicitly interpret heightened (or above trend) credit-to-GDP growth as "bad", reflecting increased leverage or risk taking. But it may sometimes be "good" if it supports broad-based productivity enhancing investment, for instance. Steady credit growth is, in turn, implicitly seen as benign and essentially without limit in the spirit of the early growth and finance literature (eg King and Levine (1993) or Rousseau and Wachtel (1998)). This may be true for countries that start off from very low levels of financialisation. But the scope for beneficial financial deepening might disappear at some point when credit to GDP is high and a sufficiently large share of the population has access to, and holds, credit. Later findings in the growth and finance literature suggest as much.[4] Taken together, these problems of credit-based EWIs can lead to two types of costly mistakes for macroprudential policymakers: (i) being too

[2] See eg IMF (2014a) or the ESRB risk dashboard (Risk Dashboard).

[3] There is a large literature showing the useful early warning properties of credit-based indicators, eg Kaminsky and Reinhart (1999), Borio and Lowe (2002), Reinhart and Rogoff (2009), Jorda et al (2013), or Greenwood et al (2022).

[4] See eg Rousseau and Wachtel (2011) and Richter et al (2021). Practitioners are also aware of possible tensions between financial deepening and measuring the build-up of systemic risk with credit aggregates (see eg IMF (2014b)).

restrictive during "good" credit booms, and (ii) being too lax when the scope for longer-term expansions is exhausted.

Given the aforementioned difficulties, we argue that it is paramount to look for theoretically well-grounded indicators of systemic risk rather than expanding the current arsenal of high-performing but difficult to interpret EWIs. Doing so holds the promise of bringing more clarity on the nature of the risks. For instance, is systemic risk more related to the quality distribution of borrowers or aggregate feedback dynamics associated with various externalities at the macro level? And in the latter case, at which point do such aggregate effects kick in? Data limitations are, however, the main obstacle to obtain such measures.

In this paper, we highlight the debt service to income ratio (DSR) as an example of how an alternative, more economically meaningful, indicator can enhance our understanding of systemic risk.[5] The DSR measures how much borrowers pay in aggregate to cover all debt related expenses – that is interest payments and amortisations – relative to their income. It therefore has a clear economic foundation corresponding to the debt service payments that directly enter the budget constraint of the consumer segment that borrows.

The DSR has several desirable statistical features in addition to its theoretical linkages. Most importantly, it lacks the credit-to-GDP-ratio's "financial hockey stick" pattern and provides highly accurate EWI signals without the need for statistical detrending. As we will explain in more detail below, this suggests that not only the level of credit, but also the maturity and the interest rate at which it is provided, matter for the financial burden that it imposes on borrowers.

We structure the discussion around three broader contributions.

First, we review what is known about the DSR. We start by discussing its measurement. We then discuss the extant evidence using post 1980s data. This shows that the DSR provides highly accurate early warning signals of financial crises and is an accurate predictor of future economic slowdowns even during normal periods. And the DSR does not require *prior detrending* as the trends in the credit and interest expense components of the DSR offset each other since the 1980s.[6] This reflects the fact that lower interest rates allow borrowers to take on more debt with the same risk (and vice-versa). But despite its medium-to-long term stability in this sample, the DSR tends to rise before financial crises and declines thereafter, which underpins its excellent early warning properties.

Second, we extend the measurement of the DSR back to the 1920s to capture the boom-bust cycle of the 1920s/30s as well as the decades of low financialisation and financial repression after World War II. We do this for 10 advanced economies. The long time series provide several insights into the evolution of debt capacity over time. For one, DSRs reached equally high levels around the Great Depression (1920-1938) and after the 1980s. In contrast, the credit-to-GDP ratio grew by 41% on average in the panel over the same period. This discrepancy between the two measures is mainly due to shorter loan maturities in the 1920s and 1930s, which

5 DSTI is another common abbreviation for the debt service to income ratio.

6 Only country level demeaning is required as absolute levels are not always comparable across countries due to measurement and institutional features (for details see Section 2.1).

increased debt servicing costs at lower credit levels. Interestingly, no financial crisis occurred during the financial repression period up to the early 1970s, when the DSR was very low even if credit grew at times rapidly. While the credit-to-GDP ratio rose significantly in the 1970s due to financial liberalisation, the DSRs increased even faster as nominal interest rates rose at the same time to counter inflationary pressures. The DSR then peaked in the early 1980s, after which gradually increasing loan maturities and declining interest rates kept it fluctuating around a constant level.

Third, our historical data capture periods of both low and high DSRs, which allows us to separate out when credit expansions promote financial deepening versus financial crisis risk. The credit-to-GDP ratio does not delineate good versus bad credit growth in the same way. This suggests that the DSR acts as an upper limit on the amount of credit that the private sector can carry relative to income, with the limit being determined by the credit terms, ie the interest rate and the maturity. Though the raw time series support this conjecture across a range of credit term regimes, we corroborate it formally in several ways. First, we show that the DSR continues to perform well as an EWI over the full historical sample. In fact, it strongly outperforms the credit-to-GDP ratio and is marginally better than the 3-year growth rate of credit to GDP. This is remarkable as there is no need to detrend the DSR, even over this very long timespan. Second, we show that 3-year credit-to-GDP growth has a positive effect on future output growth when the DSR is below a certain threshold – its 20$^{th}$ percentile in our sample. However, the growth effects turn negative once the DSR rises above the threshold.

The rest of the paper is as follows: the next section introduces the DSR and reviews existing evidence on it. Section 3 extends the results to a longer historical sample for a limited number of countries and makes use of the historical time series to show that the DSR puts a limit on the scope for benign financial deepening. Section 4 concludes and raises open issues for policymakers and researchers.

## 2. The DSR as a measure of financial vulnerabilities

To set the scene, we first look at the development in credit-to-GDP ratios. This ratio underpins many of the credit-based EWIs that accurately signal financial crises years in advance. We look at the credit-to-GDP ratio since the onset of financial liberalisation in the 1980s. We select the United States, the United Kingdom and Sweden as classical examples of advanced economies that encountered financial crises in the last 40 years. The example of Thailand underscores that the discussed themes are not merely an advanced economy phenomenon.

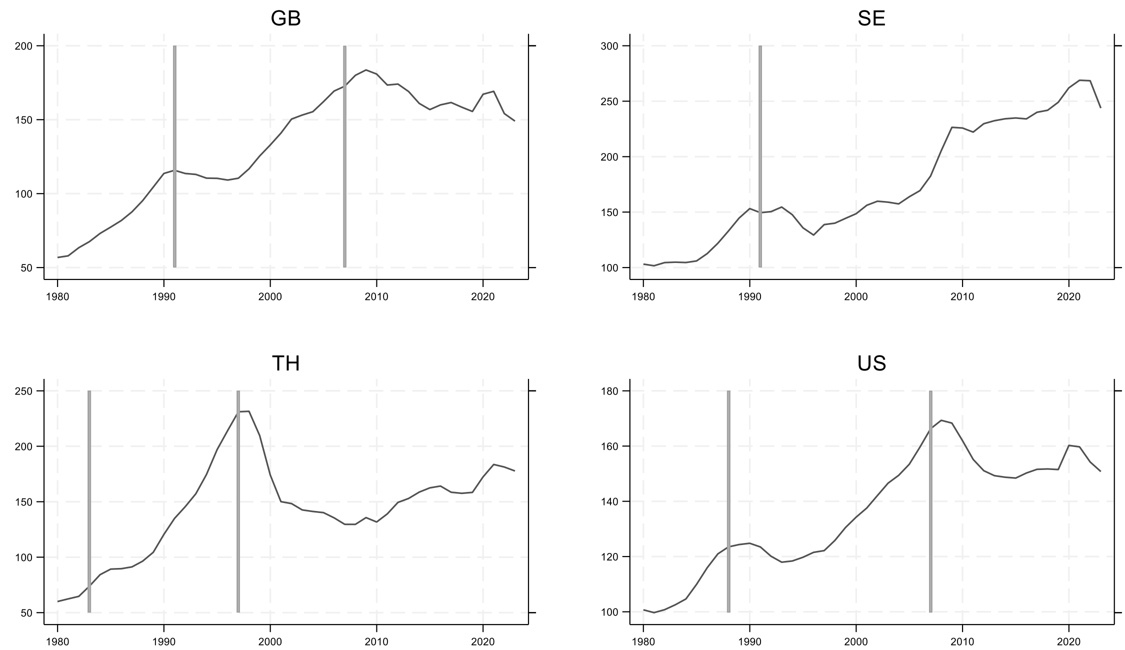
Graph 1 shows the well-known surge of the credit-to-GDP ratio over time and why the ratio in itself is not a good EWI for crises (indicated by the vertical lines).[7] The trend growth in credit to GDP over this period implies, for example, that this ratio has been much higher in the last 15 years than in the run-up to the crises in the 1980s and early 1990s. Hence, if used directly as an EWI, the credit-to-GDP ratio would (in

[7] For crisis dating, we rely on the European Systemic Risk Board crisis data set (Lo Duca et al (2017)) for European countries and Laeven and Valencia (2018) for the others. We do not consider the Swedish crisis in 2008 as this was imported from abroad (see Lo Duca et al (2017)).

sample) either generate many false positives if the threshold is set low or many false negatives if the threshold is set high. Hence, the level of the ratio has limited informational value for crisis prediction.

The credit-to-GDP ratio over time

Graph 1




Note: vertical lines indicate the start of financial crises based on Lo Duca et al (2017) for European countries and Laeven and Valencia (2018) for the others. We do not consider the Swedish crisis in 2008 as this was imported from abroad (see Lo Duca et al (2017)).

Given the pronounced trend in the credit-to-GDP ratio, the consensus approach is to detrend the measure before it is used as an EWI. Many possible de-trending methods have been suggested. The seminal work by Borio and Lowe (2002) proposes the credit-to-GDP gap measured by the deviations of the credit-to-GDP ratio from a one-sided Hodrick-Prescott (HP) filter with a smoothing parameter of 400,000.[8] Building on their work, Drehmann et al (2012) underpin the choice of the Basel Committee for Banking Supervision to single out the so derived credit-to-GDP gap as a useful guide for setting countercyclical capital buffers (BCBS (2010a)). Following the work of Jordà et al (2011), the academic literature has mainly relied on medium-term growth rates in credit to GDP, such as 3- or 5-year growth rates (for a recent contribution see eg Greenwood et al (2022)). After Hamilton (2018), linear projections

8 Starting with Edge and Meisenzahl (2012), there is by now a large econometric literature criticizing the use of the HP filter to derive credit to GDP gaps. While these criticisms may be valid, they miss the broader issue: any derived gap measure, no matter how sophisticated the method, is nothing more than an indicator in the absence of clear theoretical foundations (see Drehmann and Yetman (2021)).

to derive the trend and thus the gap have also become popular (eg Richter et al (2017)).

Most, if not all, of the proposed indicators that are based on slightly different statistical detrending of the credit-to-GDP ratio have outstanding EWI properties – hence their use in policy making or in academic studies. But the relative differences between them is marginal and there is no indicator that outperforms the others in all contexts. The EWIs' differences have therefore little practical relevance for macroprudential policy making (Drehmann and Yetman (2021)).

Independent of the specific (and largely arbitrary) detrending approach used, credit-based EWIs suffer from a lack of theoretical underpinnings. At best, such indicators are seen as crude proxies for economic concepts like leverage, net-worth, or borrowing constraints that drive macro-financial linkages in theoretical models. But this lack of clarity is an additional source of uncertainty for policy making. Should we implement a potentially costly policy measure in response to an EWI signal without a clear root cause?

As we argued in the introduction, the way forward is to focus on indicators with clear economic interpretation, such as the DSR. The main obstacle to doing so is that such indicators are often hard to measure.

## 2.1 Measurement

The DSR is defined as interest payments plus amortisations relative to income. While at the individual level, households and firms know their debt service costs, systematically collected microdata often lack sufficient detail to calculate the DSR. Even with individual credit data, like credit registries, the aggregate DSR can be hard to measure. One key reason for this is that contractual maturities do very often not correspond to repayment periods.[9] Luckett (1980) and Dynan et al (2003) propose an approach to measure aggregate DSRs. The core assumption is that debt service costs on the aggregate debt stock are paid in equal portions over the maturity of the loan (instalment loans). This is based on the idea that variations in individual loan repayment structures average out at the aggregate level. Using some simulations, Drehmann et al (2015) show that this is indeed the case.

Debt service for debt categories $j$ at time $t$ can be calculated with the standard formula for the per-period cost of an instalment loan:

$$debt\ service_{j,t} = \frac{i_{j,t}}{\left(1-(1+i_{j,t})^{-m_{j,t}}\right)} * D_{j,t} \qquad (1)$$

[9] Consider an example of a household that buys a house. The initial mortgage has a 10-year maturity but amortisations are based on the premise that the borrower repay the mortgage over a 25-year period. But this information is typically not recorded in the microdata. Moreover, observing recorded repayment flows may also not reflect actual amortisations. For example, after 10 years the household may change its mortgage provider. Hence, it would repay the full amount that is outstanding by simultaneously taking up a new mortgage, even though the net amortisations are very small or even zero, which would not be evident in the microdata.

where $D_{j,t}$ denotes the total stock of debt of category $j$, $i_{j,t}$ the average (nominal) interest rate on the stock of debt $D_{j,t}$ and $m_{j,t}$ denotes the average remaining maturity across the stock of debt $D_{j,t}$.

The aggregate debt service (to income) ratio for sector $i$ is then sum of debt service of the different categories divided by income of sector $i$ ($Y_i$)

$$DSR_{i,t} = \frac{\sum_j debt\ service_{j,t}}{Y_{i,t}} \tag{2}$$

Given equations (1) and (2), the DSR rises with increasing debt to income, higher interest rates and lower maturities, but in a non-linear fashion.

The non-linearities embedded in the DSR formula generate an approximation error when aggregate data are used, primarily impacting the level of the DSR rather than the changes. Drehmann et al (2015) show that this can lead to a mis-estimation of the level of the DSR but has less impact on its evolution over time. Elvery et al (2020) assess it empirically. They aggregate microdata on debt payments to build aggregate debt service to income ratios for the United States. They find a level difference between the micro-based and the aggregate debt service ratios based on Dynan et al (2003)). But the correlation between the two debt service measures is very high (eg 0.98 for the total household debt service ratio.[10]

The approximation errors have practical implications for the usage of the DSR. Most importantly, the absolute level of the DSR is inaccurately measured and difficult to compare across countries. Limited cross-country comparability is further amplified by structural differences across countries such as whether interest payments are tax-deductible or not. While one could in principle adjust the DSR estimates for these aspects, it is difficult to do so in practice due to limited data availability. It is therefore not possible to determine a "global" threshold for the DSR above which bad outcomes are likely. Hence, in our analysis below we always demean (not de-trend!) the DSR at the country level either before the analysis or by adding country fixed effects to regression specifications.

## 2.2 Data requirements

Different researchers have been more or less granular in the different debt categories they consider. The published BIS series, introduced by Drehmann et all (2015), cover the private non-financial sector.[11] For some countries they differentiate between the non-financial corporate and the household sector. The series published by the Fed (Dynan et al (2003)) for the household sector in the United States differentiate between several debt categories, such as mortgage and consumer debt.[12] Statistics

[10] Similarly, Federal Reserve Board (2013) found that having a consistent approach with aggregate data captures significant changes in debt service burdens despite the approximation error.

[11] The BIS series are published on the BIS website: Debt service ratios - overview | BIS Data Portal

[12] Data are published as "Household Debt Service and Financial Obligations Ratios" by the Federal Reserve Board at www.federalreserve.gov/releases/housedebt/default.htm.

Canada follows the US approach.[13] Drehmann et al (2023) consider 6 debt categories for the household sector. While these refinements improve the measurement of debt service, the correlation between their more refined series and the BIS series in a panel of 16 countries from 1980 to 2020 is 90%.

The granularity considered depends on the available data. In some countries, sectoral credit data has only become available since the mid-1990s or even later. That said, Muller and Verner (2024) recently closed important gaps in this area. The interest rate on the stock of debt is also hard to obtain as it needs to reflect the mix of new and old loans with different fixed and floating nominal interest rates attached to them. The BIS uses national accounts to derive it for the household and non-financial corporate sector.[14] If not available, interest rates on the stock of debt can be proxied by linking them to lags of interest rates of new loans and/or different market interest rates.

The main empirical problem in estimating aggregate debt service is capturing the average remaining maturity of the stock of debt. Very few countries, such as the US, have good information on this. The BIS data therefore assumes that average remaining maturities are fixed. For the household sector, the FED (Dynan et al (2003)) accounts for time-varying maturities in the United States for the different credit categories. Drehmann et al (2023) build on this and survey a wide range of information to obtain time-varying maturities, especially for mortgages across countries. For cases when only information on contractual maturities for new loans are available, they derive a formula that accounts for assumed roll-over and derives the average remaining maturity on the stock of debt.

## 2.3 The DSR and time trends

The time-series of the DSR for the total private non-financial sector in our four example countries highlights two important points (Graph 2).[15]

First, the DSR increases rapidly ahead of crises and peaks closely to their start. This provides a strong indication that the DSR is helpful in identifying the build-up of vulnerabilities, as we next show more formally. The exception is Thailand in the early 1980s before financial markets were liberalised.[16]

Second, the DSRs has long cyclical fluctuations but does not trend over time, in comparison to the credit-to-GDP ratio or interest payments. Simple regressions of the DSR on a linear time trend confirm this (Table 1): the coefficient on the time trend

[13] Data by Statistics Canada is published as "Debt service indicators of households, national balance sheet accounts" at www150.statcan.gc.ca/t1/tbl1/en/tv.action?pid=1110006501.

[14] The BIS computes the average interest rate on the stock of debt at the sectoral level by dividing gross interest payments plus financial intermediation services indirectly measured (FISIM) by the stock of debt.

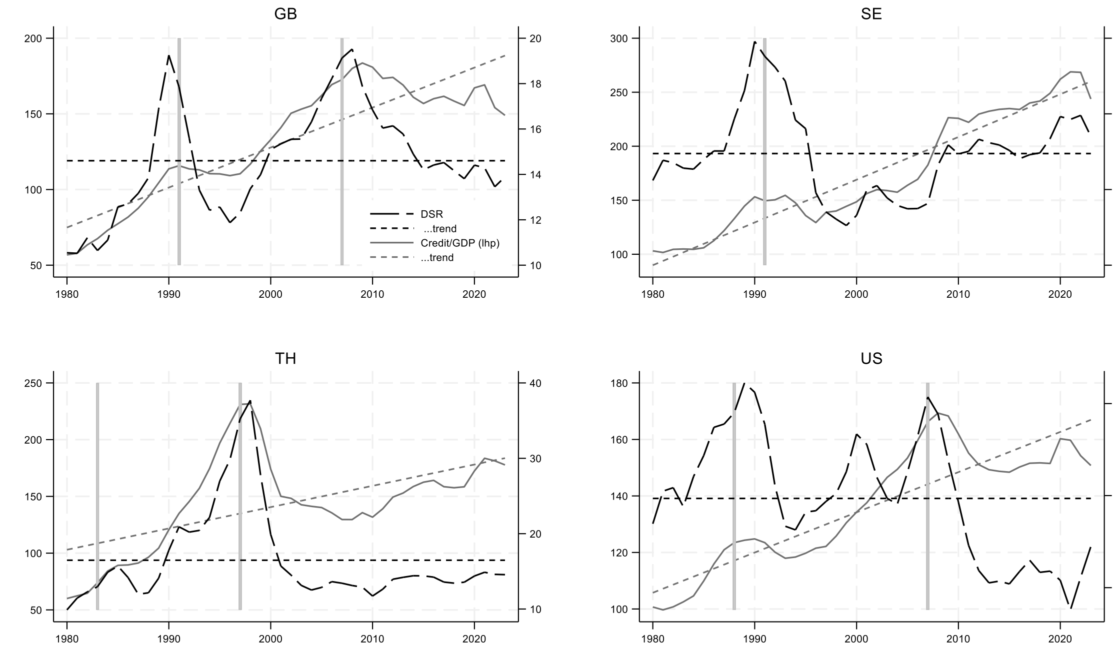
[15] The analysis is this section is based on BIS long-run DSR series from 1980s onwards which are available on request.

[16] Financial liberalisation started only in the early 1990s in Thailand (Watanagase (2001)). The underlying problem of the early 1980s crisis was not so much related to the quantity of credit rather than to the poor managerial practices at banks, accompanied by inadequate regulations and supervision (Johnston (1991)).

is very small and not significantly different from zero, independent on whether we do this in the sample of our four countries or for a sample of 32 countries where we have at least 20 years of DSR data available. The same exercise for credit to GDP, respectively interest payments to GDP, identify very strong and significant time trends (see also Graph 2).

## The DSR and credit-to-GDP over time

Graph 2




DSR trend: Country specific long-run averages of the DSR. Credit-to-GDP trend: a country specific regression on a time-trend. vertical lines indicate the start of financial crises based on Lo Duca et al (2017) for European countries and Laeven and Valencia (2018) for the others. We do not consider the Swedish crisis in 2008 as this was imported from abroad (see Lo Duca et al (2017)).

## Estimated time trends for the DSR and the credit to GDP ratio

Mean group estimates

Table 1

| | **DSR** | **DSR** | **Credit to GDP** | **Credit to GDP** | **Interest payments** | **Interest payments** |
|---|---|---|---|---|---|---|
| | 32 countries | GB, SE, TH, US | 32 countries | GB, SE, TH, US | 32 countries | GB, SE, TH, US |
| Time trend | 0.005 | -0.017 | 2.386*** | 2.475*** | -0.194*** | -0.243*** |
| N | 1265 | 176 | 1265 | 176 | 1265 | 176 |

The 32 countries are: AU, BE, BR, CA, CH, CN, CZ, DE, DK, ES, FI, FR, GB, HK, HU, ID, IN, IT, JP, KR, MX, MY, NL, NO, PL, PT, RU, SE, TH, TR, US, ZA. Interest payments are the average interest rate on the stock of debt times credit to GDP. *** significance at the 1% level.

The lack of a time trend of the DSR is useful from a policy perspective. First, it avoids the need for arbitrary statistical detrending and all the associated problems we discussed above in the context of the credit-to-GDP ratio. Second, it implies that the (country-specific) level of the DSR rather than changes or deviations from trend matter for macroprudential policy. This in turn matters significantly when thinking about financial deepening as explored in depth in Section 4.

## 2.4 EWI qualities of the DSR

Ideally, an EWI signal is both interpretable and balances type 1 errors (no signal but a crisis occurs) and type 2 errors (false alarm). Both kinds of errors have costs that are hard to estimate. That said, type 1 errors in financial crisis detection are more costly than type 2 errors, as the costs of a financial crisis can easily exceed 100% of annual GDP (eg Basel Committee (2010b)). The financial crisis prediction literature has proposed several ways to assess these trade-offs: minimising the noise-to-signal ratio (eg Kaminskiy and Reinhart (1999)), minimising the noise-to-signal ratio subject to capturing more than a minimum fraction of crises (eg Borio and Lowe (2002), specifying a loss function (eg Alessi and Detken (2011) or considering the whole range of possible type 1 and type 2 error combinations as encapsulated by the Area Under the Receiver Operating Characteristic Curve (AUC) (eg Jordá et al (2011)).[17]

In addition, an ideal EWI does not simply detect a crisis, it also flags it early and without the signal changing substantially period-to-period (Drehmann and Juselius (2013)). The appropriate timing is crucial for EWIs that underpin macroprudential policy making. On the one hand, macroprudential policies need time before they become effective. For instance, the rules of the countercyclical capital buffer give banks one year to build up the buffer (Basel Committee (2010a)). On the other hand, signals which arrive at very early stages can also be problematic as policy measures are costly. This can undermine the support for adopted measures if they are implemented too early (eg Caruana (2010)) and confronts policymakers with the "crying wolf" problem.[18] EWIs that provide stable and persistent signals are important, as policy makers tend to base decisions on trends (eg Bernanke (2004)) and gradual implementation allow to affect market expectations more efficiently and deal with uncertainties in transmission (CGFS (2012)).

Drehmann and Juselius (2012, 2014) are the first to evaluate the EWI performance of the DSR. In their 2014 work, they consider the DSR and nine other variables in a sample of 26 economies and data starting in 1980. Timing is assessed by requiring signals to occur between six quarters and five years prior to a crisis. The AUC measures forecast performance. Signals are also required to be stable and persistent.

Graph 3, based on Drehmann and Juselius (2014), compares the DSR with the credit-to-GDP gap as specified by Borio and Lowe (2002). It illustrates that both indicators issue very accurate EWI signals but with different timing. The DSR performs

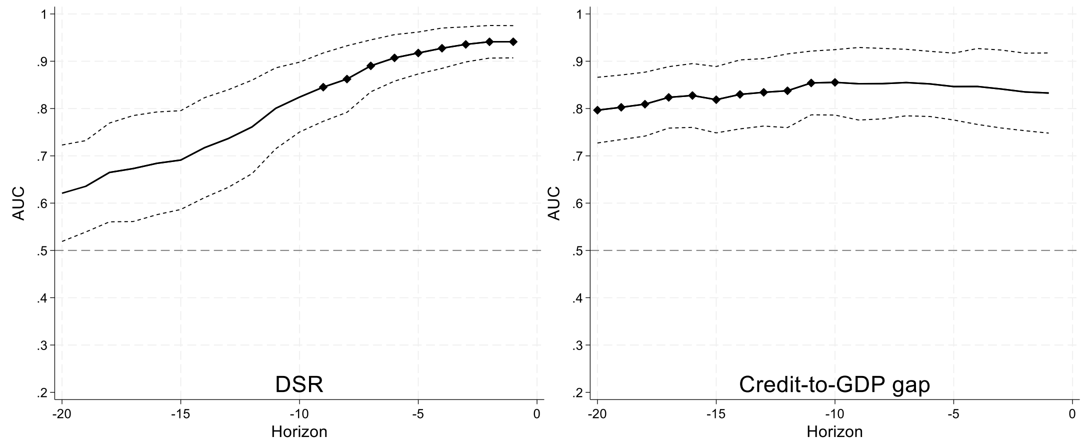
[17] The AUC is the area under the receiver operating characteristic curve (ROC). This is the mapping between the false positive rate (type 2 errors) and true positive rate (the complement of type 1 errors) across all possible signalling thresholds. A high AUC implies good EWI qualities. It does, however, not help policy makers to ascertain a threshold after which vulnerabilities have become so large that this would warrant activating macroprudential tools.

[18] For instance, after Spain introduced dynamic provisions in 2000, the provisioning system was weakened in 2004, because of pressures from banks and uncertainties by the authorities over the correct calibration (Fernández de Lis and Garcia-Herrero (2012)).

best in the run-up to crises. In the last four quarters before crises, the DSR is even a nearly perfect indicator: its AUCs are around 0.94% with the upper confidence intervals close to 100%. However, this may be too late for countervailing policy actions. But AUCs for horizons -6 to -10 quarters are still impressive with values between 0.82 and 0.91.[19] The credit-to-GDP gap exhibits a markedly different pattern. Its AUCs are stable, fluctuating between 0.80 and 0.85 over the full 5-year forecast horizon, indicating that the credit-to-GDP gap offers reliable signals years ahead of systemic banking crises.

EWIs and policy requirements – AUCs over time

Graph 3




Note: Horizon: quarters before crises. Dashed lines: confidence intervals. Diamonds – highest AUC amongst all indicators assessed by Drehmann and Juselius (2013).

Source: Drehmann and Juselius (2014)

The DSR also performs well when combined with other indicators. For example, Antunes et al (2018) find that the DSR adds valuable information in a dynamic probit model for banking crises prediction that also includes lags of the credit-to-GDP gap, house prices, and equity prices. Alessi and Detken (2018) use machine learning algorithms. This allows them to assess the predictive ability of a large range of variables and their transformations (eg levels, ratios, gaps). They find that the debt service ratio, bank credit developments and house prices are among the selected predictors by the model.

Researchers have also explored the information from micro-level DSRs. A common thrust in this strand of the literature is that the most vulnerable households or firms as measured by the upper quantiles of the DSR distribution matter from a financial stability perspective (eg Nier et al (2019), Banerjee et al (2022)). But the tails of the DSR distribution can evolve differently from aggregate measures (Banerjee et al (2022)). This raises questions about the relation between the aggregate DSR and more micro based measures – an open issue that deserves further research as we discuss more in the last section of this paper.

19 AUC values of 0.85 are high relative to other empirical findings. For instance, Jordá (2011) cites studies showing that a widely used prostate-specific antigen blood test has an AUC of around 0.8.

Given its forecast performance and intuitive properties as a measure of financial vulnerabilities, the DSRs of the household and corporate sectors are widely used by central banks for their financial stability assessments (eg Aikman et al (2017), Lee et al (2020), CGFS (2022)). In the same spirit, Juselius and Tarashev (2021) show that the DSR together with the credit-to-GDP gap can be used to obtain a forecast of the entire loss distribution of aggregate corporate loans. They also find that policy support measures kept debt service costs low during the Covid-19 pandemic, leading to low mean-loss forecasts. High indebtedness built up during the pandemic also increased tail risks.

The DSR also impacts on consumption and investment. There is ample micro-evidence showing that borrowers reduce consumption or investment when debt service burdens are high (eg Whited (1992), Olney (1999), Rauh (2006), Gan (2007), Johnson and Li (2010), Campello et al (2011), Dynan (2012), Kukk (2016)).[20] This also holds at the macro level (eg Drehmann et al (2017)). And as new borrowing generates highly predictable paths of future debt service payments with long-term debt contracts, this negative macro impact helps to explain why output tends to be depressed for a few years following a credit boom. The latter effects are well known. For example, Mian et al (2017) show that high three-year credit-to-GDP growth has a negative impact on future medium-term growth. When household debt service and new borrowing are added to the regressions, the credit-to-GDP ratio becomes insignificant (Drehmann et al (2023)). Instead, new borrowing has a significant positive effect and debt service a significant and strong negative effect. As new borrowing drives up debt service over time, this points to a propagation mechanism for how credit booms negatively affect output in the medium term.

## 3. The DSR since the 1920s

The post–1980 experience shows that a high DSR increases the likelihood of financial crises and depresses economic growth. Since there is no need to detrend the DSR, the evidence could be loosely summarised as "bad things happen once the DSR is high". This begs the question whether this is indeed the case if we look at a longer historical time series where there is more variation in the level of the credit-to-GDP ratio. And conversely, do we see more benign effects from credit expansions on output when the DSR is very far from its maximum levels? In this section, we therefore analyse long-run DSRs series based on historical data from the 1920s onwards.

### 3.1 Historical data

We derive historical DSR series for the private non-financial sector for 10 countries: Australia, Canada, Denmark, Finland, France, Germany, Norway, Sweden, the United Kingdom and the United States. Data are annual and start in 1920. The historical data are released alongside this paper at www.bis.org/publ/work1235.htm.

20 The DSR may also impact non-linearly on consumption, eg Choi and Son (2014) or Fasianos and Lydon (2022).

To calculate the long-run DSRs we need historical data for credit, GDP, the average interest rates on the stock of debt, and the average remaining maturity of the stock of debt. We generally start with long-run data from national accounts as collected by the BIS where possible and backdate them with historical data from previous studies or data that we collect specifically. This section summarises our approach, with detailed data sources in Annex B.

We compile historical series on the total credit to the private non-financial sector by combining series on total credit to the household and non-financial corporate sector. We backward extend the sectoral BIS series (Dembiermont et al (2013)) to 1940 using Muller and Verner (2024). For earlier years, we use banks' business and household loans from Jordá et al (2018). However, we include non-bank credit when historically available to be as consistent as possible with the modern data.

Our income series reflect BIS data as far back as possible. We supplement this with GDP data, as well as real GDP per capita estimates in the case of missing GDP data, harmonised by Jordá et al (2018).

To get the average interest rate on the stock of debt, we backdate the BIS interest rate series separately for household and non-financial corporate lending where possible, and then construct a debt-weighted total interest rate series at the country-year level. When we cannot identify sector-specific interest rates, we backdate them using country-specific correlations between the short-term interest rate (from Jordá et al (2018)) and existing sector-specific rates. At this juncture, these data represent a mixture of observations on the cost of new lending and the average cost of the stock of debt, due to the limited historical evidence on the latter.

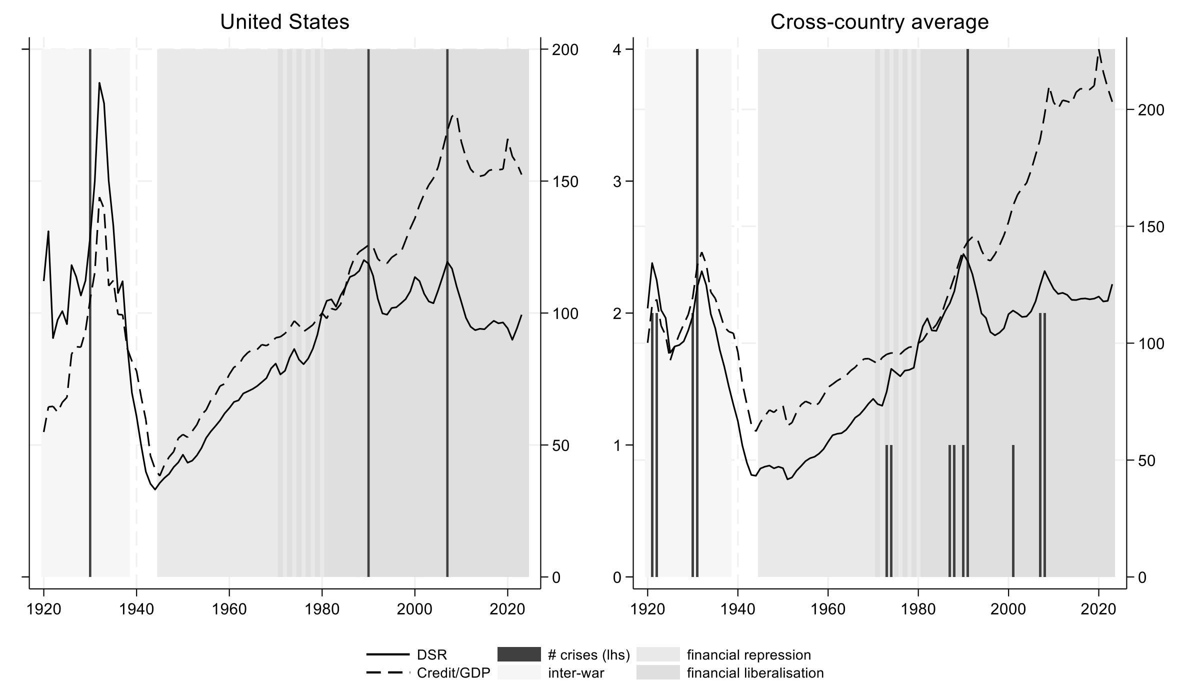
We create long-run data on average remaining maturities of the stock of debt by sector and country in several steps. We start from the BIS maturities from 1980 onwards. To backdate them, we use a range of archival sources to trace out the evolution of contractual maturities on new household debt over time for Denmark, Finland, Germany, and the United States. For countries where no information on household maturities is available, we use the average across these countries per year. We also manage to collect data on maturities of non-financial corporate debt for the United States, which we smooth in a five-year rolling average due to survey-based noise in the historical data. As this is the only available information for the corporate sector, we use this for all other countries. Third, we debt weight the two sectors to create the average remaining maturity on all non-financial private debt in each country in each year.

Our approach can only be seen as a first approximation of historical DSRs. The historical maturity data seem relatively robust when we have information at the country level. But using the cross-country average when no information is available is clearly a strong assumption. At best, we can corroborate this approach with country-specific histories, as for Great Britain and Canada. In other cases, such as in France, we have not found data that point in any direction. Getting better information on maturities is therefore one important open area to address going forward.

## 3.2 The historical evolution and EWI signals

The historical evolution of the DSR and the credit-to-GDP ratio is shown in Graph 4 for the United States and the cross-country average. Given the very different scales, we normalise the DSR and the credit-to-GDP ratio to 100 in 1980. The raw historical DSR series for all countries are shown in Graph A1 in Annex A.

The historical evolution of the DSR | Graph 4




Note: Series are indexed to 100 in 1980.

We separately highlight three different periods: the interwar period from 1920 to 1939, the period of financial repression starting after the war and the era after financial liberalisation. The demarcation between those last two periods is not clear cut. Depending on the dimension of liberalisation (eg banking sector, stock market, capital account) and country, financial liberalisation took place between the early 1970s and the early 1980s in the countries we consider (Kaminsky and Schmukler (2008)).

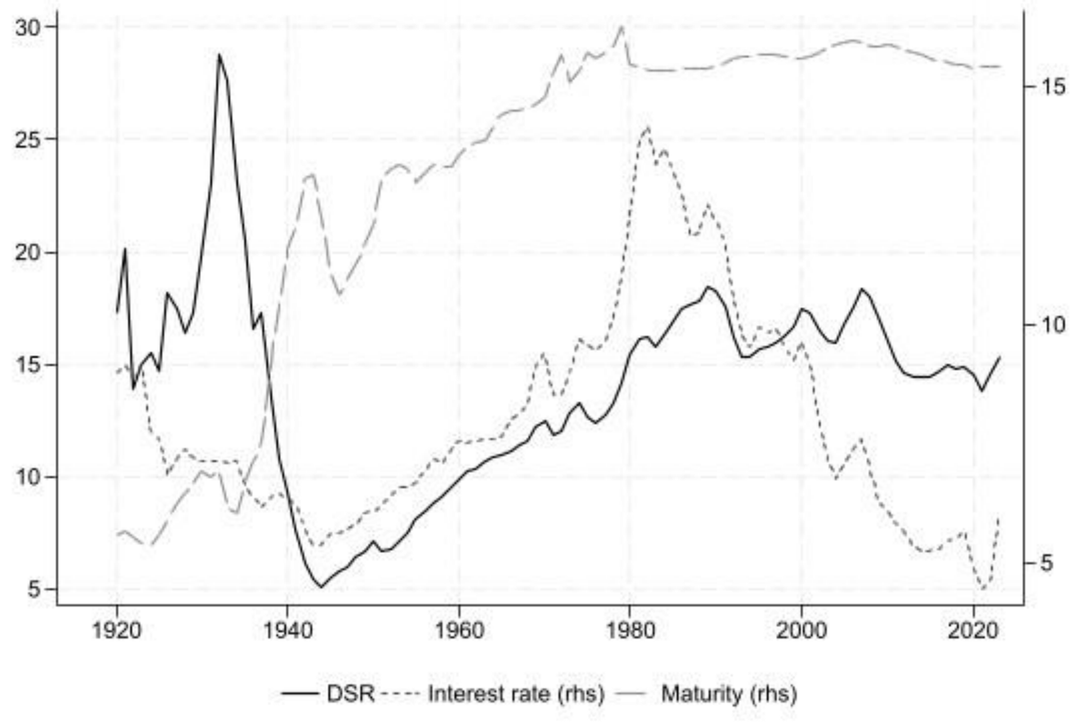
The graph highlights important commonalities and differences between the historical evolution of the DSR and that of the credit-to-GDP ratio. The main similarity is the strong growth of both series after WWII. After financial liberalisation, developments diverge. The DSR flattens out while growth of the credit-to-GDP ratio has essentially been unabated since the 1950s peaking only around the Global Financial Crisis (GFC) or the Covid-19 epidemic. The evolution before WWII is also different. The DSR fluctuates broadly around the same level in the interwar period than after financial liberalisation, compared to the credit-to-GDP ratio, which is much lower in the 1920s and 30s than after 1980.[21] For example, the DSR is on average

[21] The peak of the DSR and the credit-to-GDP ratio in the United States during the Great Depression is not a result of a credit boom but driven by the sharp drop in output.

across all countries equal to 18.2 in the interwar period and 19.1 after 1980, compared to 106 and 156 for the credit-to-GDP ratio (Annex Table A1).

The reason for the different evolution of the DSR compared to the credit-to-GDP ratio is that the DSR accounts for maturities of the stock of debt as well as interest payments. In the 1920s, the DSR was high despite low levels of credit to GDP because maturities were short, driving up amortisations (Graph 5 for the United States). After the Depression, the main channel of reducing debt service burdens was not lowering interest rates but extending loan maturities, as governments introduced standardized and insured mortgage programs (eg the United States and Canada).[22] Average remaining maturities doubled from around 6 years to 12 over the course of the next 10 years. Maturities increased further after WWII and then stabilised.[23] Since the 1970s, the key driver of the DSR has been the interest rate. Its rapid rise in the wake of inflation and the central bank tightening drove up debt service costs *ceteris paribus* and lead to a peak in the DSR around the late 80s. After this peak, the long downward trend in interest rates off-set the upward trend in the credit-to-GDP ratio so that the DSR has fluctuated around some constant level since then.

The evolution of the components of the DSR in the long run in the United States — Graph 5




[22] We focus on nominal rates here, though real interest rates fell starting in1933 (Romer (1992)).

[23] The stability of the average remaining maturity for the United States shown in the graph is somewhat an artefact of the BIS methodology that assumes constant maturities. In fact, maturities also changed over this period. For instance, the average remaining maturity of mortgage loans increased from 19 in the 1980s to above 23 years around the GFC, falling to 20 more recently.

| AUCs for crises prediction | | | Table 2 |
|---|---|---|---|
| | 1920-2023 | 1920-1938 | 1950-2023 |
| **1-year ahead** | | | |
| DSR | 0.79** | 0.67** | 0.80** |
| Credit-to-GDP ratio | 0.65** | 0.56 | 0.66** |
| 3-year credit-to-GDP growth | 0.69** | 0.65* | 0.71** |
| **2-years ahead** | | | |
| DSR | 0.77** | 0.67* | 0.77** |
| Credit-to-GDP ratio | 0.62** | 0.52 | 0.64** |
| 3-year credit-to-GDP growth | 0.69** | 0.70** | 0.67** |
| **3-years ahead** | | | |
| DSR | 0.75** | 0.65* | 0.73** |
| Credit-to-GDP ratio | 0.61** | 0.51 | 0.62** |
| 3-year credit-to-GDP growth | 0.68** | 0.73** | 0.65** |

**/* Significantly different from 0.5 – the AUC of an uninformative indicator – at the 5%/10% significance level.

The stability of the DSR over time suggests that the level of the DSR should have better EWI properties than the level of the credit-to-GDP ratio in the full historical sample. To test this more formally, we estimate the AUC for these two indicators in our sample of ten countries.[24] For comparison with measures used in the literature, we also detrend the credit-to-GDP ratio by taking the 3-year growth rate and calculate the associated AUC. In each case, we consider early warning signals issued one, two and three years ahead of crises, for the whole sample and various sub-periods.[25] As expected, the DSR outperforms the (level of the) credit-to-GDP ratio in all specifications and the difference in AUCs is statistically significant (Table 3). In contrast, the DSR and the 3-year credit-to-GDP growth perform similarly in the pre-WWII period (middle column). However, once the high growth period in the 50s and 60s is included in the estimation (first and third column) the AUC of the DSR also outperforms the AUC of the 3-year growth rate of the credit-to-GDP ratio. This result highlights the advantage of having a measure in absolute rather than relative terms.

To get a sense of the magnitudes, it is illustrative to translate the AUCs into EWIs (Annex Table A2). To do so, we minimise the noise-to-signal ratio subject of each indicator capturing more than two thirds of the crises. For DSR in the full sample, this gives a critical threshold 3.5 percentage points above the long run country specific average for a two-year forecast horizon. And given the stability of the DSR, the

[24] Historical crises dates are taken from Jorda et al (2017).

[25] We estimate the AUC by demeaning all the series by the full sample means. We drop two years after financial crises as there is a risk of bias if the post-crisis period is included in the analysis (Bussiere and Fratzscher (2006)). We also exclude crises that were imported from abroad based on Lo Duca et al (2017) as it cannot be expected that measures of domestic vulnerabilities can predict them.

threshold is essentially the same whether estimated on the interwar (3.3) or post-1980 (3.7) sample separately. In comparison, the critical threshold for the 3-year growth in the credit-to-GDP ratio is more than twice as high in the post-1980 period than before WW II (4.7 versus 2.2 percentage points). And thresholds for the credit-to-GDP ratio itself, are 52 percentage points (interwar period) and 6 percentage points (post war period) below long run averages.[26]

## 3.3 The DSR and the limit on benign credit growth

The previous analysis shows that the DSR issues accurate early warning signals when it rises to sufficiently high (country-specific) *levels*, in contrast to other credit EWIs that need de-trending. Moreover, these have been stable over the last 100 years. But if the likelihood for financial crises and output slumps increases once the DSR reaches some critical level, this also imposes a limit on the credit-to-GDP ratio. And this limit depends on the maturity and the level of interest paid on the debt stock.

Does the economy have scope for benign credit expansion as long as the credit-to-GDP ratio is sufficiently below the limit imposed by the DSR? The historical time series offer a good laboratory for testing the idea as the data include the era of financial repression where credit was restricted (eg McKinnon (1973)) and credit growth was possibly beneficial.

To set the stage, we first investigate the relationship between credit growth and future output growth over history. Following Mian et al (2017), we run predictive regressions of the form

$$(y_{i,t+h} - y_{i,t})/y_{i,t} = \mu_i + \beta g_{i,t}^{3Ycr} + \gamma' x_{i,t} + \varepsilon_{i,t+h|t} \qquad (3)$$

where $y_{i,t}$ is real per capita GDP, $g_{i,t}^{3Ycr}$ is the 3-year real growth rate in credit-to-GDP, and $x_{i,t}$ is a vector of controls consisting of annual real per capita GDP growth, the real ex-post interest rate and the fiscal balance-to-GDP ratio.[27] We split the sample into the interwar period (1920-1938), the financial repression period (1950-1970) and the post 1980s period (1980-2023) to see how results vary by era.[28] We consider specifications which vary on two dimensions: (i) with and without controls and (ii) 3-year and 5-year GDP growth as the outcome.

[26] The low thresholds imply that essentially all crises are predicted but the noise-to-signal ratio is close to 1, ie it is an uninformative indicator.

[27] This specification resembles the ones used in the growth and finance literature. The key difference is that we do not use the credit-to-GDP ratio, but rather its growth rate, on the left-hand side. The performance gap between measures is less stark if one detrends the credit-to-GDP ratio before adding it to the regression or adds time fixed effects to the specification, but the credit-GDP ratio results are less stable and often insignificant in this case.

[28] We do not consider the 1970s as this was the period when liberalization started at different points for different sectors in different countries (Kaminsky and Schmukler (2008)).

| Impact of 3-year credit-to-GDP growth on future real GDP growth in different periods | | | | Table 3 |
|---|---|---|---|---|
| | **3Y growth** | **3Y growth** | **5Y growth** | **5Y growth** |
| Full-sample | -0.05** | -0.04** | -0.07** | -0.04 |
| 1920-1938 | -0.109** | -0.13*** | -0.13 | -0.20*** |
| 1950-1970 | -0.00 | 0.07** | -0.04 | 0.12* |
| 1980-2023 | -0.16*** | -0.17*** | -0.20*** | -0.21*** |
| Country effects | yes | yes | yes | yes |
| Controls | no | yes | no | yes |

The table shows the regression coefficient for the 3-year credit-to-GDP growth on medium term real GDP per capita growth (equation (3)). The controls are 1 year GDP growth, the ex-post real short-term interest rate, and the fiscal balance to GDP ratio. Significance at the 10%, 5% and 1% levels based on robust standard errors are indicated by *, **, ***, respectively.

The results show that high credit-to-GDP growth has an adverse effect on output in the medium run except during the era of financial repression. (Table 3). At that time, when there was scope for benign credit expansions medium-term credit-to-GDP growth has a significantly positive effect on future output growth. Even if we leave out the controls, medium-term credit-to-GDP growth has no significant negative impact in this period. In the other time periods, we replicate the negative relationship in the literature (Mian et al (2017)).

The episode where credit growth has a relatively benign effect on output growth coincides with the episode where the DSR takes historically low values. To investigate more formally whether the level of the DSR – or the level of the credit-to-GDP ratio for that matter – affects the scope for benign financial deepening, we modify equation (3). In particular, we allow for different effects of medium-term credit-to-GDP growth on output depending on the level of the DSR and/or the level of the credit-to-GDP ratio. Let $I_{n,dsr_i}$ be an indicator variable that takes the value 1 if the DSR is above its *n*th percentile in country $i$ and zero otherwise, and $I_{n,c2y_i}$ is similarly defined for the credit to GDP ratio. The modified regression is

$$(y_{i,t+h} - y_{i,t})/y_{i,t} = \mu_i + \beta g_{i,t}^{cr} + \beta_{dsr} g_{i,t}^{cr} I_{n,dsr_i} + \beta_{c2y} g_{i,t}^{cr} I_{n,c2y_i} + \gamma' x_{i,t} + \varepsilon_{i,t+h|t} \quad (4)$$

Table 4 provides a representative snapshot of the results for the cases $n = 20$ and $n = 80$ with 3-year GDP growth on the left-hand side, and controlling for annual real per capita GDP growth, the real ex-post interest rate and the fiscal balance-to-GDP ratio as above.

Regression results show that the level of the DSR helps to differentiate whether credit deepening increases output or not (Table 4). Credit-to-GDP growth turns out to have a significant and positive effect (0.10) when the DSR is below its 20$^{th}$ percentile (column 1). When it exceeds this threshold, the sign reverses and the overall effect (*0.10-0.18=-0.08)* becomes negative and significant. Similarly, at the 80$^{th}$ percentile, the DSR indicator becomes insignificant and credit-to-GDP growth significantly negatively affects output (column 4). Credit growth thus benefits GDP growth in the medium run but only when debt service is low.

The credit-to-GDP ratio does not allow for a clean separation of "good" and "bad" credit expansions. At the 20th percentile, the effects of the credit-to-GDP ratio are muddled and insignificant (column 2), and $R^2$ declines relative to the DSR specification in column 1. In fact, even credit growth is not significant in column 2. Using the 80th percentile of credit-to-GDP threshold splits the sample into the pre and post GFC sub-samples, so column 5 shows that credit growth is associated with much lower output growth post GFC, a period without financial crises but with relatively low growth.

Impact of 3-year credit-to-GDP growth on 3-year real GDP per capita depending on levels of the DSR or the credit-to-GDP ratio

Table 4

| | **20th percentile credit threshold** | | | **80th percentile credit threshold** | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $g_{i,t}^{3Ycr}$ | 0.10*** | -0.02 | 0.10*** | -0.05*** | -0.04** | -0.05*** |
| $g_{i,t}^{3Ycr} * I_{n,dsr}$ | -0.18*** | | -0.17*** | 0.08 | | 0.10 |
| $g_{i,t}^{3Ycr} * I_{n,c2y}$ | | -0.07 | -0.03 | | -0.16** | -0.19** |
| Observations | 817 | 817 | 817 | 817 | 817 | 817 |
| R-squared | 0.13 | 0.09 | 0.14 | 0.09 | 0.09 | 0.11 |

Estimated results for equation (4) for the full historical sample. $I_{n,z} = 1$ if $Z_t > \rho_n(z_t)$ with percentiles given in the column headings. $I_{n,dsr}/I_{n,c2y}$ indicator variable depending on the DSR and credit-to-GDP ratio respectively. All specifications include country fixed effects and three controls (1 year GDP growth, the ex-post real short-term interest rate, and the fiscal balance to GDP ratio). Significance at the 10%, 5% and 1% levels based on robust standard errors are indicated by *, **, ***, respectively.

The lack of a precise growth signal with credit-to-GDP is also apparent from the specification where we run a horse race between the two threshold variables (ie when both $\beta_{dsr}$ and $\beta_{c2y}$ are allowed to be non-zero) in columns 3 and 6. In the former case, credit-to-GDP growth provides no additional information, while the latter only demonstrates that GDP growth has been limited post-GFC as explained above.

The results are robust. They do not alter much if we for instance look at 5-year instead of 3-year GDP per-capita growth, remove the controls, or add time-fixed effects (Annex Table A3). It also does not matter whether we use real GDP growth rather than the growth of real GDP per capita. We also looked at percentiles $n = 5, 10, 15$ .... Results for lower percentiles are very similar to the ones in the table for $n = 20$. The results for higher percentiles resemble those with $n = 80$, while $\beta_{dsr}$ and $\beta_{c2y}$ are mostly insignificant for mid-range percentiles.

The DSR thresholds highlighted in Table 4 provide an intuitive link between several strands of the finance-growth literature. Although early empirical contributions established the benefits of credit expansion (eg King and Levine (1993)), as theorised by Schumpeter (1934), more recent work emphasises that these effects depend on the time period (Rousseau and Wachtel (2011)) and the level of the credit-to-GDP ratio (Arcand et al (2015)). We demonstrate that financial expansions do have a consistently positive impact on growth, but only when aggregate debt burdens are sufficiently low.

# 4. Conclusion and open issues for policymakers and researchers

The overarching theme of this paper has been the benefits to macroprudential policy-making from shifting focus from "statistical" EWIs based on eg credit aggregates to more theoretically well-grounded indicators.

The first step in achieving this goal is to construct measures that better reflect economically meaningful concepts such as debt servicing, leverage, net worth, lending standards and so on. This would clearly be beneficial even if these measures do not necessarily improve upon existing EWIs in terms of statistical accuracy. Indeed, from our perspective, the benefits of investing more work in different econometric detrending methods for the credit-to-GDP ratio is limited, especially when there is little understanding why detrending is meaningful in the first place.

To illustrate these ideas, we contrast the debt service ratio - a specific economic measure - with standard credit-based measures. Unlike typical credit-based EWIs which necessitate complex detrending processes and suffer from limited interpretability, the DSR does not require detrending and provides a theoretically sound measure directly linked to borrowers' financial constraints. It is also a highly reliable EWI, even if we look back an entire century.

Studying the DSR in a longer historical context – a novel contribution in this paper – suggests that the DSR acts as a limit on financial deepening. When the DSR is high, credit growth is no longer positively associated with higher future output; and financial crises occur around its peaks. But these negative effects disappear when the DSR is sufficiently low. This indicates that the information contained within it is likely related to aggregate externalities. A concrete example could be an aggregate-demand externality that kicks in when individual households reduce their consumption to meet debt service obligations, but this action is jointly undesirable at the aggregate level if many households do this at the same time. Further theoretical work would be useful to underpin this intuition.

While we have shown that the DSR has several highly desirable properties, it also has limitations. For example, as an aggregate measure it has little to say about risks that stem from heterogeneity in the loan pool. Clearly, even relatively low aggregate levels of credit can pose financial stability risks if a large fraction of it is held by risky debtors. It also has little to say about risk explicitly generated within the banking system due to various agency problems. This suggests that developing more accurate measures of eg, banking sector leverage, increased risk-taking or poor lending practices by financial institutions are likely to be beneficial avenues for future work. Indeed, much work has already been done along these lines. Also, with increasing access to micro-data, studying how vulnerabilities measured by the aggregate DSR relate to the tails of the distribution at the micro level would be highly beneficial.

More work specifically on measuring the aggregate DSR would also be useful. The key challenge is to obtain better data on amortisations. One avenue is to record data on loan maturities that then can be used together with some assumption on the loan repayment structure, such as the instalment loan assumption in Section 2.1, to derive the DSR. Another avenue would be to compute amortisations directly by subtracting the change in the credit stock and defaults from data on new borrowing (see eg Drehmann et al (2023)). Data on aggregate new borrowing are increasingly available based on microdata, as banks are often obliged to report the purpose of individual loans. Another challenge is to expand the coverage of the DSR to more EMEs. Such data would be important for EME policy makers. They could also be used to further test the conjecture that the DSR constitutes a limit on benign financial deepening as financial liberalisation started later than in advanced economies and is still not complete in some countries.

While we are strong advocates of using the DSR for macroprudential surveillance, we do not suggest ignoring standard credit-based EWIs - or even more complex AI-based EWIs. For one, credit-based EWIs have been embedded in processes so that there is experience in how to interpret developments in real time for practical policy making. Moreover, credit-based EWIs can still provide reliable signals with different timings. It seems also clear that AI or machine learning will more and more be used for financial stability monitoring. These are the ultimate statistical black boxes. But given their ability to analyse millions of data points and recognise complex data patterns, they do, and will, provide useful information. That said, the scarcity of crises events and the uniqueness of each crises limit the stand-alone use of data-intensive AI models (eg BIS (2024)). As such, we have a strong prior that AI can only ever be a complement to, rather than a substitute for, well-grounded financial stability analysis based on theoretically meaningful indicators.

The results discussed in this paper also raise important policy questions for the future. Despite some deleveraging after the GFC or the Covid-crisis, DSRs remain at historically high levels. Moreover, interest rates have been rising in recent years, slowly feeding through to the average interest rate on the stock of debt. Hence, the DSR may reach the limit beyond which further credit expansions would be detrimental, possibly leading to increasing financial fragility and depressed growth. This also raises the spectre of a debt trap if the effective lower bound prohibits decreasing the DSR further. In such an environment, EWIs based on detrending the credit-to-GDP ratio likely miss their mark. Indeed, the scope for further trend increase in the credit-to-GDP ratio seems limited as rates are close to the effective lower bound and maturities are nearing the average remaining life-expectancy of borrowers in most jurisdictions. The long period of steady credit-to-GDP growth may thus finally be at its end.

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# Annex A: Additional tables and graphs

**The average DSR and average credit-to-GDP ratio in different periods** Table A1

| | Raw data | | | | Demeaned by country specific long-run averages | | | |
|---|---|---|---|---|---|---|---|---|
| | Full sample | Interwar | 1950-1980 | After 1980 | Full sample | Interwar | 1950-1980 | After 1980 |
| **DSR** | 15.6 | 18.2 | 10.8 | 19.1 | 0 | 2.7 | -4.7 | 3.6 |
| **Credit to GDP** | 115.8 | 106.0 | 79.8 | 156.1 | 0 | -9.8 | -36.0 | 40.3 |

**Critical EWI thresholds in different periods** Table A2

| | Threshold | Fraction of crises predicted | Noise-to-signal ratio |
|---|---|---|---|
| **DSR** | | | |
| Full sample | 3.5 | 0.67 | 0.36 |
| Interwar | 3.3 | 0.67 | 0.54 |
| Post 1980 | 3.7 | 0.79 | 0.51 |
| **Credit-to-GDP ratio (level)** | | | |
| Full sample | -8.4 | 0.72 | 0.66 |
| Interwar | -52.2 | 1.00 | 0.92 |
| Post 1980 | -5.8 | 0.92 | 0.94 |
| **3-year growth of the credit-to-GDP ratio** | | | |
| Full sample | 2.0 | 0.70 | 0.55 |
| Interwar | 2.2 | 0.67 | 0.53 |
| Post 1980 | 4.7 | 0.67 | 0.45 |

Critical EWI thresholds are derived by minimising the noise-to-signal ratio subject to capturing at least two thirds of the crises. Thresholds are expressed as difference to country specific whole, sample averages.

## Impact of 3-year credit-to-GDP growth on 3-year real GDP per capita depending on levels of the DSR or the credit-to-GDP ratio: Robustness

Table A3

| | **Threshold based on DSR** | | | **Threshold based on credit-to-GDP** | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Description | Without controls | With time effects | 5y GDP growth | Without controls | With time effects | 5y GDP growth |
| $g_{i,t}^{cr}$ | 0.10*** | 0.08*** | 0.19*** | -0.03** | -0.01 | -0.03** |
| $g_{i,t}^{cr} * I_{dsr>\rho_{20}(dsr)}$ | -0.19*** | -0.11*** | -0.29*** | | | |
| $g_{i,t}^{cr} * I_{c2y>\rho_{20}(c2y)}$ | | | | -0.06 | -0.00 | 0.01 |
| Country effects | yes | yes | yes | yes | yes | yes |
| Time effects | no | yes | no | no | yes | no |
| Controls | no | yes | yes | no | yes | yes |
| Observations | 880 | 817 | 777 | 880 | 817 | 777 |
| R-squared | 0.07 | 0.56 | 0.11 | 0.03 | 0.55 | 0.04 |

Estimated results for equation (4) for the full historical sample and the 20th percentile. $I_{n,z} = 1$ if $Z_t > \rho_{20}(z_t)$. The dependent variable in (1), (2), (4) and (5) is 3-year real GDP per capita growth, where as (3) and (6) uses 5-year growth. The controls variables are: 1y GDP growth, the ex-post real short-term interest rate, and the fiscal balance to GDP ratio. Significance at the 10%, 5% and 1% levels based on robust standard errors are indicated by *, **, ***, respectively.

## Historical DSR series

Graph A1


Note: vertical lines indicate the start of financial crises.

# Annex B: Historical series – data documentation

The below data series detail the construction of the historical DSR series. From 1980s we use the DSRs from the BIS based on the methodology of Drehmann et al (2015).

## Credit

*Australia*

Total debt

1979--on: BIS series

1953--1979: Bank of Australia (1996) Table 3.2, total private nonfinancial credit

1920--1953: We add together two composite series one on mortgages and one on non-mortgages

Mortgages
1946--1952: average growth rates of Butlin et al (1971) Tables 7, 67, 68 Commonwealth mortgage department mortgages, state and trustee savings banks' mortgages and non-bank financial institution mortgages (which is from hand entered from series of Australian Bureau of Statistics (various years) (Trustee company + building societies + general insurance + life insurance mortgages)

1920--1946: sum of (Butlin et al (1971) Tables 8, 11, 53(ii)) and Australian Bureau of Statistics (various years) nonbanking institutions' mortgages (trusts, building societies, life insurance, and other insurance) break adjusted

Non-mortgages:

Butlin et al, 1971Australian Bureau of Statistics (various years)1946--1955: White 1973 RBA rural credit, trading bank loans, Commonwealth trading loans, Commonwealth savings loans (tables 6, 44, 55, 70) + Australia Yearbook NBFI nonmortgage loans. Where NBFI nonmortgage loans include loans on policies for general insurance, life insurance; other loans to insurance companies and trustee companies via Australian Bureau of Statistics (various years)

1920--1945: sum of Butlin et al, 1971 advances from all cheque banks (table 6) and NBFI nonmortgage loans (Australian Bureau of Statistics (various years))

Household debt (used to construct irTOT)

1977--on: BIS series

1975--1976: Reserve Bank of Australia (2024) Table D5December observations lending to persons: housing, owner-occupied

1966--1974: Reserve Bank of Australia (1996) on housing finance commitments to individuals' total loans for purchase and construction of new dwellings + established dwellings table 3.3

1953--1965: Reserve Bank of Australia (1996), on housing finance commitments to individuals' total loans for purchase and construction of new dwellings table 3.3

1946--1952: average growth rates of Butlin et al, 1971Tables 7, 67, 68 Commonwealth mortgage department mortgages, state and trustee savings banks' mortgages and non-bank financial institution mortgages (which is from hand entered from series of Australian Bureau of Statistics (various years) (Trustee company + building societies + general insurance + life insurance mortgages)

1920--1946: sum of (Butlin et al, 1971Tables 8, 11, 53(ii)) and Australian Bureau of Statistics (various years) nonbanking institutions' mortgages (trusts, building societies, life insurance, and other insurance) break adjusted

Non-financial corporate debt (used to construct irTOT)

1977--on: BIS series

1953--1977: total debt- household debt

Butlin et al, 1971Australian Bureau of Statistics (various years)1946--1953 White 1973 RBA rural credit, trading bank loans, Commonwealth trading loans, Commonwealth savings loans (tables6,44,55,70) + Australia Yearbook NBFI nonmortgage loans. Where NBFI nonmortgage loans include loans on policies for general insurance, life insurance; other loans to insurance companies and trustee companies via Australian Bureau of Statistics (various years)

1920--1945: Butlin et al, 1971 advances from all cheque banks (table 6) and NBFI nonmortgage loans (Australian Bureau of Statistics (various years))

*Canada*

Total debt

1953-- on: BIS

1920—1953: We add together two composite series one on mortgages and one on non-mortgages

Mortgages:

From Statistics Canada sum of series trust companies (H535), mortgage companies (H492), credit unions (H334), life insurance companies (H375+H389+H400), farm loan companies (H576), and Quebec savings banks (J308+J309+J310)

Non mortgages:

From Statistics Canada, sum of the following series: trust companies (H537), chartered banks (1920-22 J171-J167-J168, 1923-34 J147-J143-J144, 1935-44 J123-H119-J120, 1944-53 H94-H88-H89-H91), sales finance companies (1920-22 H466 scaled down by their 1960-77 ratio of non mortgages to total loans, defined using H386+H387+H388+H389, 1923-1953 H480 scaled down by their 1960-77 ratio of non mortgages to total loans, defined using H386+H387+H388+H389)

Household debt (used to construct irTOT)

1969--on: BIS

1920--1969: From Statistics Canada sum of series trust companies (H535), mortgage companies (H492), credit unions (H334), life insurance companies (H375+H389+H400), farm loan companies (H576), and Quebec savings banks (J308+J309+J310)

Non-financial corporate debt (used to construct irTOT)

1969--on: BIS

1920--1969: From Statistics Canada, sum of the following series: trust companies (H537), chartered banks (1920-22 J171-J167-J168, 1923-34 J147-J143-J144, 1935-44 J123-H119-J120, 1944-53 H94-H88-H89-H91), sales finance companies (1920-22 H466 scaled down by their 1960-77 ratio of non mortgages to total loans, defined using H386+H387+H388+H389, 1923-1953 H480 scaled down by their 1960-77 ratio of non mortgages to total loans, defined using H386+H387+H388+H389)

*Denmark*

Total debt

1951--on: BIS

1920--1951: Abildgren (2017): bank credit, non-bank credit issued by resident deposit banks, savings banks, and commercial banks.

Household debt (used to construct irTOT)

1994--on: BIS

1951--1994: Mueller and Verner (2024) household credit

1920--1949: adjusted using Jorda et al (2017) tloans

Non-financial corporate debt (used to construct irTOT)

1990--on: BIS

1949--1990: Mueller and Verner (2024) firm data

1920--1949: adjusted using Jordà et al (2017) tloans

*Finland*

Total debt

1970--on: BIS

1958--1970: Mueller and Verner (2024) total credit

1920--1958: Jordà et al (2017) tloans

Household debt (used to construct irTOT)

1970--on: BIS

1958--1970: Mueller and Verner (2024) household credit

1948--1958: Jordà et al (2017) thh

1920--1948: adjusted using Jordà et al (2017) tloans

Non-financial corporate debt (used to construct irTOT)

1970--on: BIS

1958--1970: Mueller and Verner (2024) firm credit

1948--1958: Jordà et al (2017) tbus

1920--1948: adjusted using Jordà et al (2017) tloans

*France*

Total debt

1969--on: BIS

1920--1969: Mueller and Verner (2024) total credit

Household debt (used to construct irTOT)

1977--on: BIS

1958--1977: Mueller and Verner (2024) household credit

1920--1958: adjusted using Jordà et al (2017) tloans

Nonfinancial corporate debt (used to construct irTOT)

1977--on: BIS

1920--1977: Mueller and Verner (2024) firm credit

*Germany*

Total debt

1948--on: BIS

1940--46: linear interpolation

1920--1940: Mueller and Verner (2024) total credit

Household debt (used to construct irTOT)

1970--on: BIS

1949--1970: Mueller and Verner (2024) household credit

1920--1949: Jordà et al (2017) thh

Nonfinancial corporate debt (used to construct irTOT)

1970--on: BIS

1949--1977: Mueller and Verner (2024) firm credit

1920--1949: Jordà et al (2017) tbus

*Norway*

Total debt

1953--on: BIS

1920--1953: Norges Bank Monetary aggregates in Norway, Total Credit, Table A4

Household debt (used to construct irTOT)

1975--on: BIS

1940--1975: Mueller and Verner (2024) household credit

Non-financial corporate debt (used to construct irTOT)

1975--on: BIS

1940--1975: Mueller and Verner (2024) firm credit

*Sweden*

Total debt

1961--on: BIS

1920--1961: Edvinsson, et al (2022)

Household debt (used to construct irTOT)

1980--on: BIS

1920--1980: Mueller and Verner (2024) household credit

Non-financial corporate debt (used to construct irTOT)\

1980--on: BIS

1920--1980: Mueller and Verner (2024) firm credit

*United Kingdom*

Total debt

1963--on: BIS

1920--1963: UK millennia of data series titled Composite Break-adjusted stock of lending series based on M4L/M4Lx/M4Lxex (see Table A15 of Version 2.3).

Household debt (used to construct irTOT)

1966--on: BIS

1920--1965: Composite series for total secured lending series in UK millennia of data (see Table A15 of Version 2.3).

Non-financial debt (used to construct irTOT)

1976--on: BIS

1920--1975: Mueller and Verner (2024) firm credit

*United States*

Total debt

1952--on: BIS

1920--1952: sum of household and non-financial corporate debt below

Household debt (used to construct irTOT)

1952--on: BIS

1920--1952: sum of Board of Governors of the Federal Reserve System (1976) Table 16.1 total outstanding consumer instalment loans before 1943 and FRED series TOTALNS from December 1943 onwards, and Grebler et al total Table L1 through 1952

Non-financial corporate debt (used to construct irTOT)

1952--on: BIS

1924—1952: sum of IRS Statistics of Income (various years) reported non-financial IRS corporate debt, accounts and notes payable total corporate sectors minus that for financial sector, adjusted for number filing businesses as in Evans Clark, *Internal Debts of the United States*, 1933.

1920--191924: Mueller and Verner (2024) firm credit

## Interest rates

*Australia*

Total interest rates: debt weighted average of household and non-financial corporate interest rates

Household interest rates

1960--on: BIS

1951—1959: Reserve Bank of Australia (1996), Table 3.21b, actual bank housing loan interest rate

1946—1950: White 1973 Table 121 annual average Commonwealth savings banks' mortgage lending rates

1920-1945: Butlin et al, 1971 Table 51 annual average Melbourne trading banks' lending rate on loans over 3 months

Non-financial corporate interest rates

1979--on: BIS

1957--1978: Reserve Bank of Australia (1996), table 3.21a average bank overdraft advance interest rate

1946--1956: rates White 1973 Table 120 annual average trading banks' overdraft lending

1920--1945: Butlin et al, 1971 Table 51 annual average Melbourne trading banks overdraft rates

*Canada*

Total interest rates: debt weighted average of household and non-financial corporate interest rates

Household interest rates

1951--on: BIS

1937--1950: linear interpolation

1929--1936: mortgage loan rate from Nixon et al (1937)

1920--1929: advanced rate on ordinary loans from Shearer and Clark (1984)

Non-financial corporate interest rates

1935—on: BIS

1929--1935: corporate bonds from Nixon et al (1937)

1920--1929: advanced rate on ordinary loans from Shearer and Clark (1984)

*Denmark*

Interest rates

1970-on: BIS

1960--1970: adjusted using NFC interest rate

1920--1960: adjusted using interest rates from Danmarks statistik (1968)

*Finland*

Interest rates

1978--on: BIS

1970--1978: adjusted using non-financial corporate interest rates from the BIS

1920--1970: adjusted using Jordà et al (2017) stir

*France*

Interest rates

1978--on: BIS

1969--1978: adjusted using non-financial corporate interest rates from the BIS

1920--1969: adjusted using Jordà et al (2017) stir

*Germany*

Interest rates

1975--on: BIS

1967--1975: adjusted using non-financial corporate interest rates from the BIS

1948--1967: Zinsen im Rahmen des Kreditvertrages, Bundesbank (1976)

1945--1947: linearly interpolated

1923—1944: Kosten für vereinbarte Kredite in laufender Rechnung, Bundesbank (1976)

1920--1923: adjusted using Jordà et al (2017) stir

*Norway*

Interest rates

1980--on: BIS

1920--1980: adjusted using Jordà et al (2017) stir

*Sweden*

Interest rates

1971--on: BIS

1920--1971: adjusted using Jordà et al (2017) stir

*United Kingdom*

Total interest rates: debt weighted average of household and non-financial corporate interest rates

Household interest rates

1970--on: BIS

1920--1970: "effective mortgage rate" series in UK millennia of data (see Table A19 of Version 2.3).

Non-financial corporate interest rates

1971--on: BIS

1922--1970: "rate on corporate borrowing" series in UK millennia of data (see Table A19 of Version 2.3).

1920--1922: "prime commercial bill/paper rate" series in UK millennia of data (see Table A19 of Version 2.3).

*United States*

Total interest rates: debt weighted average of household and non-financial corporate interest rates

Household interest rates

1972-on: BIS

1920—1971: debt weighted average of non mortgage and mortgage maturities where weights derive from the HH debt components described above through 1952, then are succeeded by Federal Reserve Board (2016) total 1-4 and multifamily mortgages outstanding after 1952.

Non-mortgage:

1963—1971: linearly interpolated

1920—1962: auto loan interest rates from Juster 1966 Table 5

Mortgage interest rates:

1962—1980: Federal Reserve System (various years) Mortgages (new homes) table contract rate on conventional first mortgages

1957—1961: Linearly interpolated

1948—1956: Klaman 1961 contract rates

1920—1947 Grebler et al contract rates across all lender types, debt weighted

Non-financial corporate interest rates

1953—on: BIS

1920-1952: nternal Revenue Service (various years) reported interest payments on debt for non-financial corporations divided by stock of debt

## Income

*Australia*

1960--on: BIS

1920--1960: Jordà et al (2017) GDP

*Canada*

1955--on: BIS

1920--1955: Jordà et al (2017) GDP

*Denmark*

1966--on: BIS

1920--1966: Jordà et al (2017) GDP (we linearly interpolate 1936)

*Finland*

1960--on: BIS

1920--1960: Jordà et al (2017) GDP

*France*

1950--on: BIS

1920-1950: Jordà et al (2017) GDP

*Germany*

1960--on: BIS

1920--1950: Jordà et al (2017) GDP (we use Barro-Ursua (2008) estimates inflated by CPI and population for missing years)

*Norway*

1960--on: BIS

1920--1960: Jordà et al (2017) GDP (we use Barro-Ursua (2008) estimates inflated by CPI and population for missing years)

*Sweden*

1960--on: BIS

1920--1960: Jordà et al (2017) GDP

*United Kingdom*

1955-on: BIS

1920--1950: Jordà et al (2017) GDP

*United States*

1947-on: BIS

1920--1947: Jordà et al (2017) GDP

## Maturities

Household maturities

*Denmark*

1980---on: BIS

1920--1980: Hvolbøl et al (2006)

*Finland*

1980--on: BIS

1920--1980: Finanssiaala (2021)

*Germany*

1980--on: BIS

1920--1980: We follow the historical rule by which amortisations were derived by adding one percentage point to the long-term interest rate on loans (see eg Dieckhöner, 1984).

*United States*

1980-on: BIS

1920--1980: debt weighted average of non mortgage and mortgage maturities where weights derive from the HH debt components described above.

Non-mortgage:

1971—1980: Federal Reserve series G19/TERMS/H0.DTCTLVNM_N.M

1963—1970: Linearly interpolated

1920—1962: auto loan maturities from Juster 1966

Mortgage:

1962—1980: FHFA MIRS Table 9 term to maturity

1948—1962: Linearly interpolated

1920—1947 Grebler et al contract maturities across all lender types, debt weighted

*All other countries*

1980--on: BIS

1920--1980: Average per year across maturities for Denmark, Finland, Germany, and the United States

Non-financial corporate maturities

*United States*

*1955-on: BIS*

*1944-1955: : debt weighted average of non-financial corporate mortgage maturities (Morton (1956) nonfarm income-producing properties' mortgages*

*1920-- 1944: debt weighted average of non-financial corporate mortgage maturities (Morton (1956) nonfarm income-producing properties' mortgages) and commercial bond borrowing (Hickman (1960) by maturity by year par offerings all industries using weighted averages of each maturity bin and a three-year backward moving average)*

*All other countries*

*1980-on: BIS*

*1920--1980: US corporate sector*

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