Zombies at large? Corporate debt overhang and the macroeconomy*

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Abstract

Debt overhang is associated with higher financial fragility and slower recoveries from recessions. However, while household credit booms have been extensively documented to have this property, we find that corporate debt does not fit the same pattern. Newly collected data on non-financial business liabilities for 18 advanced economies over the past 150 years shows that, in the aggregate, greater frictions in corporate debt resolution make for slower recoveries—with weak investment and more persistent "zombie" firms—and that this is an important factor in explaining the difference in outcomes relative to household credit booms.

Keywords: corporate debt, bankruptcy, debt resolution costs, business cycles, local projections.

JEL classification codes: E44, G32, G33, N20.

1. Introduction

Corporate debt has risen markedly around the world in recent years, especially, but not only, in emerging market economies (Abraham, Cortina, and Schmukler, 2020).[1] In the decade after the global financial crisis, in a time of low interest rates, businesses in many countries have increased borrowing from banks and markets. In the U.S., debt levels of the non-financial business sector increased by about 20 percentage points in the past 30 years. The COVID-19 pandemic has aggravated this state of affairs. U.S. business debt now stands at a historical high of 130% of GDP (Board of Governors of the Federal Reserve System, 2020) as emergency lending facilities have pushed business debt ratios higher, potentially amplifying the risks of debt overhang.

Do the lessons from the Global Financial Crisis regarding the economic aftermath of credit booms apply to this business debt boom as well? After 2008, as in previous debt-boom episodes, stressed household balance sheets were key reasons for the crisis, recession, and the slow recovery. Households saddled with mortgage debt needed time to repair their balance sheets following the housing crash. This time around, many observers see potential risks from the corporate debt boom. While some previous studies suggests lower risks from this form of debt (Jordà, Schularick, and Taylor, 2013; Mian, Sufi, and Verner, 2017), we still lack a detailed understanding of corporate debt booms, their after-effects and what distinguishes them from household credit booms. Without a better understanding can we really be sure this time will be no different?

[1] Throughout this paper, we use "business debt" and "corporate debt" interchangeably to refer to total debt of the non-financial corporate and non-corporate sectors. We also refer to debt (the liability) and credit (the associated asset) interchangeably.

A new body of research suggests that, in assessing the role of household debt overhang at a macroeconomic level, it is important to pay attention to the role of debt renegotiation and restructuring frictions. As noted by Auclert, Dobbie, and Goldsmith-Pinkham (2019), individual banks can have little interest in restructuring household debt because such policies are beneficial only at the macro level; the problem is aggravated by the recourse nature of most household debt, especially mortgages. Consequently, household bankruptcy and debt restructuring have potentially large macroeconomic effects (Auclert, Dobbie, and Goldsmith-Pinkham, 2019).

Matters are different for business debt, where bankruptcy and restructuring are a routine process. When corporations cannot meet their obligations, debt holders can take over the firm's equity while limited liability provides little or no recourse. Liabilities are ultimately limited by firm assets. When the value of a firm drops below the market value of its assets, the difference will be erased upon liquidation. Assets will be freed up for other productive ends. Both owners and creditors gain from successful reorganization (see also, e.g. Fama, 1978; Aivazian and Callen, 1980) and have incentives to restructure the debt.

Could these differential frictions be a mechanism to explain the contrast between the effects of household and business debt overhang? To study this hypothesis, we build on Djankov, McLiesh, and Shleifer (2007) and Djankov, Hart, McLiesh, and Shleifer (2008) to quantify country-level institutional frictions to corporate debt reorganization or liquidation in corporate debt booms since the 1970s. Such frictions relate to coordination issues among dispersed creditors, weak contract enforcement, hold-out problems, asymmetric information, and other frictions that can make renegotiation difficult or even prevent it altogether (Gertner and Scharfstein, 1991; Philippon, 2010). The liquidation process too can be more or less efficient. Poor creditor rights protection, or costly legal procedures can discourage or delay liquidation.

Note that frictions can also affect the behavior of lenders, making them more likely to avoid the losses and keep insolvent "zombie" firms afloat. Inefficient liquidation increases the survival probability of zombie firms and their importance at the macro level (Becker and Ivashina, 2021). Research by Peek and Rosengren (2005) describes the "evergreening" of loans by banks (i.e., rolling over loans of unprofitable firms to avoid formal loss provisioning). Such evergreening may discourage the entry and growth of healthy competitors and eventually undermines productivity growth (Caballero, Hoshi, and Kashyap, 2008).[2] Recent papers also point to the two-way relationship between zombies and loose monetary conditions (Acharya, Crosignani, Eisert, and Eufinger, 2020; Hong, Igan, and Lee, 2021) and the role of national bankruptcy regimes in preventing zombification (Andrews and Petroulakis, 2019).

In this paper, we assess how reorganization frictions modulate the economic costs of corporate debt booms over the business cycle. We assemble a new long-run dataset on non-financial business sector liabilities (bank loans, corporate bonds, trade credit, and other liabilities) for 17 advanced economies since the 19th century. A contribution in its own right, the new dataset is an enhancement to previous data collected by us.[3] Data before WW2 mainly consist of business loans, though in some cases we were able to augment

[2] Note how the zombification channel differs from the debt overhang channel in two important respects. Zombification undermines growth through a deterioration of the *quality* of investments, as funds *flow* into unprofitable, highly leveraged firms that are unable to carry their debt burden otherwise. By contrast, debt overhang impairs growth through depressing the *quantity* of investment as highly leveraged firms are *denied access* to finance, and funds might flow elsewhere.

[3] The previously collected historical data are available at `www.macrohistory.net/database`. The new data will be added to this database and will be publicly available.

these with corporate bond market data.[4] Data after WW2 builds on bank lending data from the Macrohistory Database (Jordà, Schularick, and Taylor, 2017), hand-collected data from Müller (2018), as well as financial accounts collected by the Bank of International Settlements (BIS) that capture the growing importance of non-bank lending channels.

Naturally, countries can adapt legislation to their economic experience. Hence, in order to account for this adaptation, we turn to a local projection instrument variable strategy (Jordà, 2005; Jordà, Schularick, and Taylor, 2015). Specifically, we appeal to the exogenous variation of legal origins in the spirit of La Porta, López-de-Silanes, Shleifer, and Vishny (1997, 1998) and La Porta, López-De-Silanes, and Shleifer (2008). Using legal traditions as an instrument for debt renegotiation costs, we find that where institutions encourage efficient restructuring and liquidation, the drag from business debt booms is small, as previous results have suggested. However, and more interestingly, we find that in countries where frictions due to renegotiation costs are high, the recovery from a business debt overhang can be just sluggish as with household debt overhang.

How do our empirical findings fit with the existing literature? In a seminal departure from the classic Modigliani and Miller (1958) theorem on the irrelevance of the firm's capital structure, Myers (1977) showed that default risk undermines the incentives to invest for indebted firms. Some projects with positive net present value will not be realized as equity holders do not benefit in case of default. Debt overhang may depress any expenditure with delayed returns, such as hiring, training, advertising, or maintenance (Hennessey, 2004). Yet the possibility of underinvestment also provides an incentive for owners and creditors to restructure debt. Underinvestment pushes the value of the firm below its potential, so

[4] In most countries, the share of non-corporate business debt is a small fraction of corporate debt. See the data section below.

that both sides could in principle buy out the other party and gain from implementing the efficient investment policy (Fama, 1978). Bergman and Callen (1991) argue that "running down assets" through underinvestment constitutes a credible and effective threat to bring creditors to the negotiation table.

At the microeconomic level, a large empirical literature has mostly focused on documenting mechanisms linking corporate debt and firm-level investment decisions and outcomes. Several papers show the adverse investment effects of debt overhang at the firm level (e.g. Lang, Ofek, and Stulz, 1996; Hennessey, 2004; Kalemli-Özcan, Laeven, and Moreno, 2020; Albuquerque, 2021). These studies suggest that highly levered firms invest less and grow slower. This is especially true when firms are financially vulnerable and dependent on external creditors who perceive investment opportunities to be bleak. Recent studies with European firm-level data, such as Kalemli-Özcan, Laeven, and Moreno (2020) and Popov, Barbiero, and Wolski (2018) find conflicting evidence with respect to investment levels and efficiency effects of high corporate debt. Favara, Morellec, Schroth, and Valta (2017) examine the role of national bankruptcy regimes empirically. They show how differences in such regimes affect the investment behavior of firms near default, and highlight the role of frictions to debt renegotiation.

It is an open question, however, if any of these mechanisms matter quantitatively at the aggregate macro level. Firm-level estimates may overstate aggregate effects in general equilibrium. For example, financially constrained firms may leave room for competitors to pick up the slack. Moreover, existing micro evidence appears to be particularly strong for small- and medium-sized firms (Kuchler, 2020; Kalemli-Özcan, Laeven, and Moreno, 2020), which account for only a minor share of aggregate business debt.[5] Thus, via composition effects, the financing decisions of bigger corporations could greatly attenuate the aggregate effects of debt overhang. It remains unclear how these firm-level mechanisms aggregate into macroeconomic forces and shape business cycle fluctuations. Consequently, beyond individual firm-level behavior, we need a better understanding of the macroeconomic effects of corporate debt (Brunnermeier and Krishnamurthy, 2020).This is where our analysis of debt renegotiating and restructuring costs and their business cycle consequences helps clarify this muddy landscape of conflicting forces.

Our analysis brings empirical evidence to this question and carries a straight-forward policy implication. Frameworks that efficiently facilitate the restructuring or liquidation of debt reduce the macroeconomic fall-out of corporate debt booms. Conversely, legal and regulatory frictions will worsen debt overhang and corporate zombification, impairing productivity growth and slowing recoveries after recessions as emphasized by, among others, Caballero, Hoshi, and Kashyap (2008); McGowan, Andrews, Millot, and Beck (2018).

2. Data description

The basis for our analysis is a novel long-run dataset on business credit, including bond market debt and credit from non-bank intermediaries, covering 18 advanced economies since the nineteenth century.[6] Notably, we have been able to construct separate series for business debt for 9 countries in the pre-WW2 period. Data for the U.S. start in 1916 and

[5] For example, in the United States more than 80% of firms are—typically small—non-corporate businesses that jointly account for less than 25% of total non-financial sector leverage (Pomerleau, 2015; Board of Governors of the Federal Reserve System, 2020).

[6] The 18 advanced economies are the U.S., Japan, Germany, France, U.K., Italy, Canada, Netherlands, Ireland, Belgium, Sweden, Australia, Spain, Portugal, Denmark, Switzerland, Finland, Norway.

build on the business sector debt data calculated by James and Sylla (2006), from which we deduct debt obligations of financial institutions. For other countries, we calculate bank credit to the non-financial business sector based on the assets of specialized commercial banks that provide loans to business and other corporate financing. As an example, for Germany we sum loans and advances extended to non-banks by joint-stock industrial banks as well as commercial credit unions. We identify similar proxies for business credit in other countries, as detailed in the appendix. The new data enhance the long-run dataset in Jordà, Schularick, and Taylor (2017), from which we take data on household bank credit as well as a long list of macroeconomic controls, updated to 2019.

We rely on comprehensive business credit data provided by the financial accounts and the Total Credit Database assembled by the Bank for International Settlements for data after WW2.[7] These include secured and unsecured debt, of all maturities, and from all types of lenders, in addition to conventional bank lending contracts. Financial accounts data on non-financial business liabilities come from the OECD and Eurostat databases and individual publications, such as Bonci and Coletta (2012) on Italy and Roe (1971) on the U.K., for example. All postwar U.S. data are from the Fed's financial accounts (Board of Governors of the Federal Reserve System, 2020).

As noted earlier, we use the terms "business debt" and "corporate debt" interchangeably throughout the paper to refer to all debt liabilities of all firms, whether corporate or non-corporate. Whenever available, our series include the debt liabilities of non-corporate businesses as well. Historical sources do not always allow for a clean separation of the two. Corporate debt is the dominant component. In the U.S., non-corporate businesses account

[7] For details on its construction see Dembiermont, Drehmann, and Muksakunratana (2013).

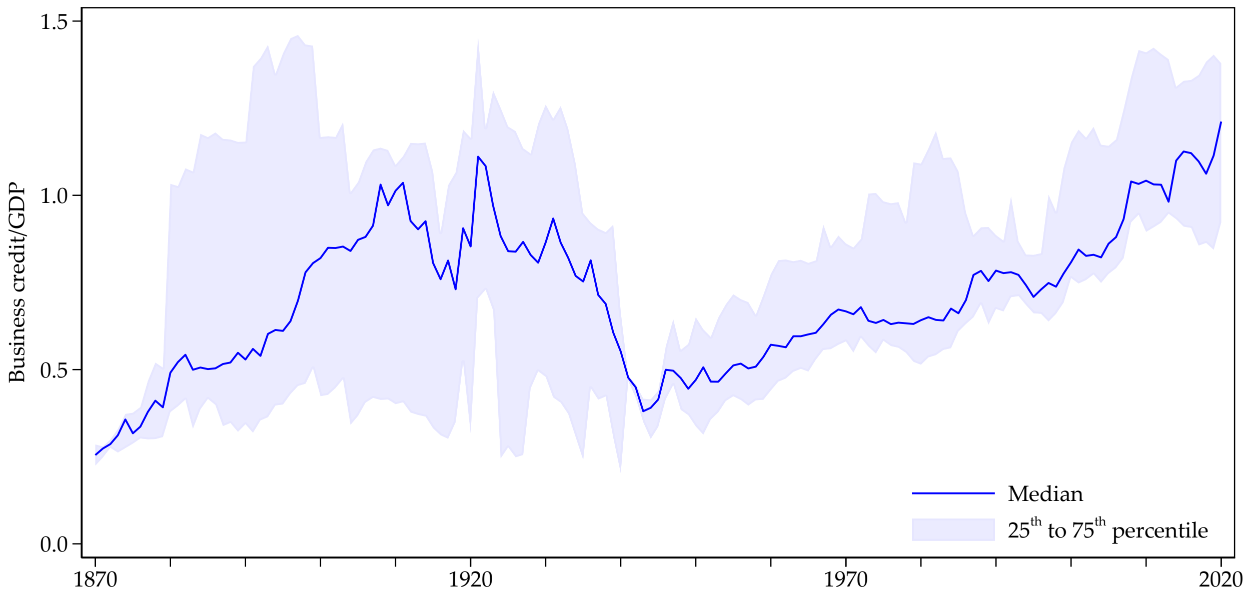
for only one third of total non-financial business debt outstanding.

In total, there are 1,717 country-year observations for business sector debt, 480 of which correspond to the previously less well-documented pre-WW2 period. The appendix describes details of the construction and underlying sources, including the materials kindly shared by Müller (2018). The results presented in the paper always use the entire dataset, excluding the wartime years of WW1 and WW2. All findings are qualitatively and quantitatively similar when restricting the data to the post-WW2 period, but, for brevity, we place those robustness results in the appendix.

In corporate finance, the term "leverage" often refers to the ratio of debt to equity. However, we instead focus on the ratio of corporate debt to GDP. The reason is that several episodes that we investigate involve sudden and dramatic repricing of assets. Thus, debt and equity could both be shifting at the same time, making the traditional definition of leverage harder to interpret. For the ratio of debt to GDP, one can think of it as a cash-flow based measure of *leverage*, hence we will often just denote this ratio as *leverage.*

Figure 1 shows the evolution of business debt over the full sample, which starts in 1870. The figure plots the cross-country median and the inter-quartile-range of business credit relative to GDP—our measure of leverage. Historically, business credit has ranged between 50% and 100% of GDP for most advanced countries. The series trends upwards in the lead-up to WW1 before entering a period of high volatility in the interwar years, followed by a sharp reduction during the 1930s and WW2. Since then, business credit has doubled from about 50% to 100% of GDP today. On this measure, several countries are currently at their highest level in the past 150 years, but not at dramatically higher levels than were seen circa 1900.

Figure 1: *Business credit/GDP since 1870*

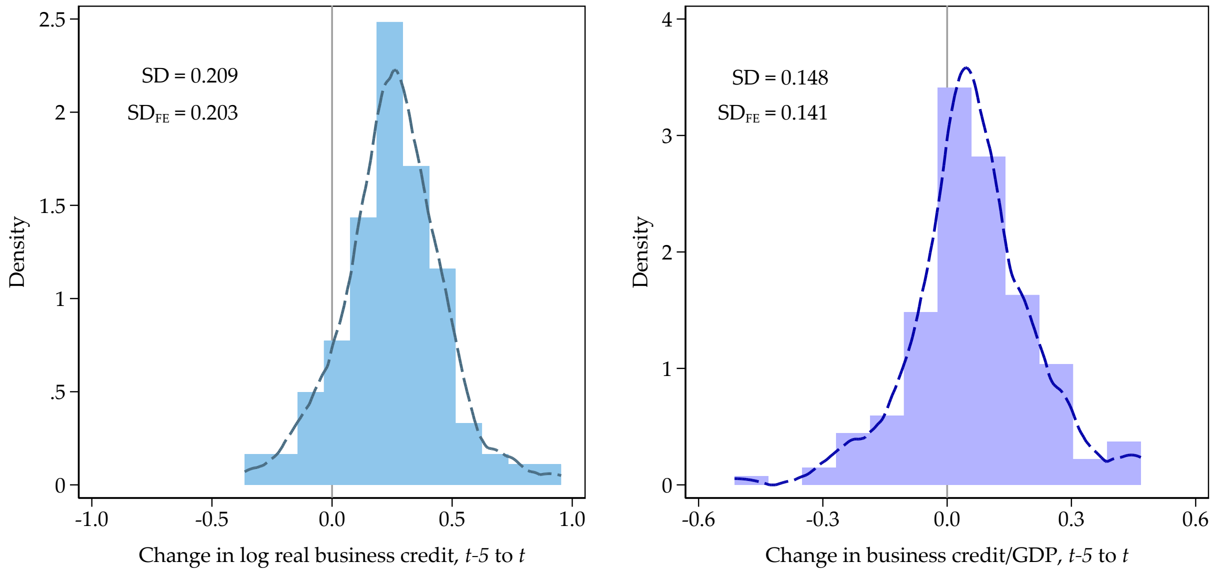


Notes: The figure shows non-financial business credit over GDP for our sample of 18 advanced economies. The range spanned by the first and third quartile shown as the shaded region. See text.

Another aspect of our empirical strategy requires that we identify business cycle turning points. Here we follow Jordà, Schularick, and Taylor (2013) and use the Bry and Boschan (1971) algorithm for all countries. At annual frequency and for the U.S., this algorithm reproduces almost exactly the NBER's dating. Briefly, the Bry and Boschan (1971) algorithm dates turning points as local maxima and minima of real GDP per capita data in levels. Minima are labeled as troughs and maxima as peaks. Recessions go from peak to trough, expansions from trough to peak.

We document large amplitudes in business credit cycles alongside a weak sychronisation with the boom-bust pattern in real activity. This provides rich statistical variation to be exploited in our analysis: Figure 2 displays the distribution of business credit booms in the run-up to recessions. It plots the change in the log real credit (left panel) and the credit-to-GDP ratio (right panel) over the five years preceding any given business cycle

Figure 2: *Business credit booms in the run-up to recessions, 1870-2020*



Notes: Histogram and kernel density estimates of the distribution of non-financial business credit booms unfolding over the five years prior to recessions (t denotes business cycel peaks). Empirical standard deviations before and after removing country fixed effects from the distribution in the top left corner.

peak of our sample. As the figure shows, there is a great deal of variation. Importantly, virtually all of this variation takes place *within* countries, as accounting for country fixed effects barely affects the standard deviation of these distributions.

Finally, we further separate recessions into two types. We will refer to *financial* recessions as those associated with a financial crisis in a ± 2 year window around a peak. The reason is that financial crises sometimes lead to recessions, sometimes recessions lead to financial crises. All recessions not associated with a financial crisis are denoted *normal* recessions. The financial crisis chronology itself is based on the latest version of the Jordà, Schularick, and Taylor (2017) Macrohistory Database (http://www.macrohistory.net/database).

3. The business cycle-business debt disconnect

The literature already highlights some differences between household and business credit booms (Mian and Sufi, 2010; Schularick and Taylor, 2012), so here we find it useful to first establish some comparable baseline empirical facts with our data and approach, from which we can build out to highlight our new findings. Our analysis provides added values along two dimensions. First, we test the macroeconomic role of corporate debt using new data, stretching further back in time and trying to construct as comprehensive a measure of business liabilities as possible. Second, the theory suggests that business debt becomes problematic when default risk is high. Therefore, it is natural to condition on recession events to obtain the most sensitive test of this proposition.

Consider next evaluating the path of the recession and recovery as a function of how much corporate debt grew leading up to the recession peak, conditional on a rich vector of macroeconomic variables. This can be easily done with local projections of the cumulative change in real GDP per capita from the recession peak year to h periods thereafter, conditional on controls. In particular, let y_{it} denote log real GDP per capita multiplied by 100, observed for country i at time t. Our interest on the trajectory of recessions/recoveries means that we will focus on those time periods associated with a peak in economic activity and denoted $p = 1, \ldots, P$ where the index is understood to be specific to each country and hence it is not expressly indicated to avoid cluttered notation. Hence, we denote by $t = t(p)$ the time period associated with the p^{th} recession peak.

Thus, our main outcome variable of interest, $y_{it(p)+h} - y_{it(p)}$, will measure the cumulative (log-form) percentage change in real GDP per capita, h horizons after the peak p, where we will display responses up to 5 years out. Using similar notation, $\Delta_5 x^j_{it(p)} \equiv x^j_{it(p)} - x^j_{it(p)-5}$, for $j = B, H$, denotes the 5-year change in Business, B, or Household, H, debt measured as a ratio to GDP in the years prior to the peak p. Hence, these debt variables are predetermined once the recession starts. In addition, the vector $\boldsymbol{w}_{it(p)}$ summarizes all other predetermined macroeconomic variables observed before the start of the recession. This vector includes the current plus two lagged values of real GDP growth, inflation, real investment growth and changes in the investment-to-GDP ratio.

With these variable definitions, we estimate the following local projections,

$$y_{it(p)+h} - y_{it(p)} = \alpha_h + \alpha_{hi} + \beta^B_h \Delta_5 x^B_{it(p)} + \beta^H_h \Delta_5 x^H_{it(p)} + \gamma_h \boldsymbol{w}_{it(p)} + \epsilon_{it(p)}, \quad \text{for } h = 1, \ldots, 5, \quad (1)$$

where α_{hi} are country-fixed effects normalized to sum to zero so that α_h is the average percentage change in real GDP per capita after a peak since we demean all regressors by their full-sample averages.[8] The coefficients of interest are β^j_h for $j = B, H$, each indicating how the expected future path of real GDP per capita varies with the behavior of credit (in the business and household sectors) during the expansion.

We do not interpret the coefficients β^j_h for $j = B, H$ causally since our interest is in comparing the typical trajectory in a recession/recovery given the behavior of business versus household debt in the preceding expansion. That said, the predetermined nature of our variables of interest and our rich set of controls reduce the chances that the differences could be explained by unobserved factors.

[8] This somewhat unusual specification is done to have a direct coefficient estimate of the desired parameter.

Baseline results Table 1 presents estimates of α_h, β_h^B, and β_h^H from our main specification.[9] Based upon these coefficient estimates, Figure 3 plots predicted trajectories or responses for the average recession as well as recessions preceded by a two-standard-deviation (above mean) change of business debt (about 14.4 percentage points). The peak year is normalized to zero and deviations in subsequent years are measured in log points times 100 (approximate percentage changes). For comparison, we also show the responses for a corresponding two-standard-deviation change of household debt (approximately 34 percentage points).

The table presents formal tests, but Figure 3 makes it unequivocal that the effects of past corporate credit booms (especially once controlling for other macroeconomic aggregates) are negligible—in the economic and statistical senses—as compared to household credit booms. Recessions preceded by household debt expansions are not only deeper, but are followed by significantly slower recoveries. These findings are very much consistent with the existing evidence in Jordà, Schularick, and Taylor (2013); Reinhart and Rogoff (2014); Bordo and Haubrich (2017), for example. We cluster standard errors at the country level to allow for non-parametric error-term dynamics. As a robustness check, we can also allow for spatially (and auto-) correlated residuals using the procedure of Driscoll and Kraay (1998) in a version that accommodates unbalanced panels and time series gaps (Hoechle, 2007). Standard error estimates remain very similar to the baseline and are shown in Appendix G.

To provide some context, a 10 percentage points (pps) increase in the business credit/GDP ratio in the expansion—a considerable rate of growth by historical standards—is not associated with a slower recovery. After 5 years from the start of the recession, GDP per

[9]Results omitting controls are shown in appendix Table A.15. They are qualitatively similar.

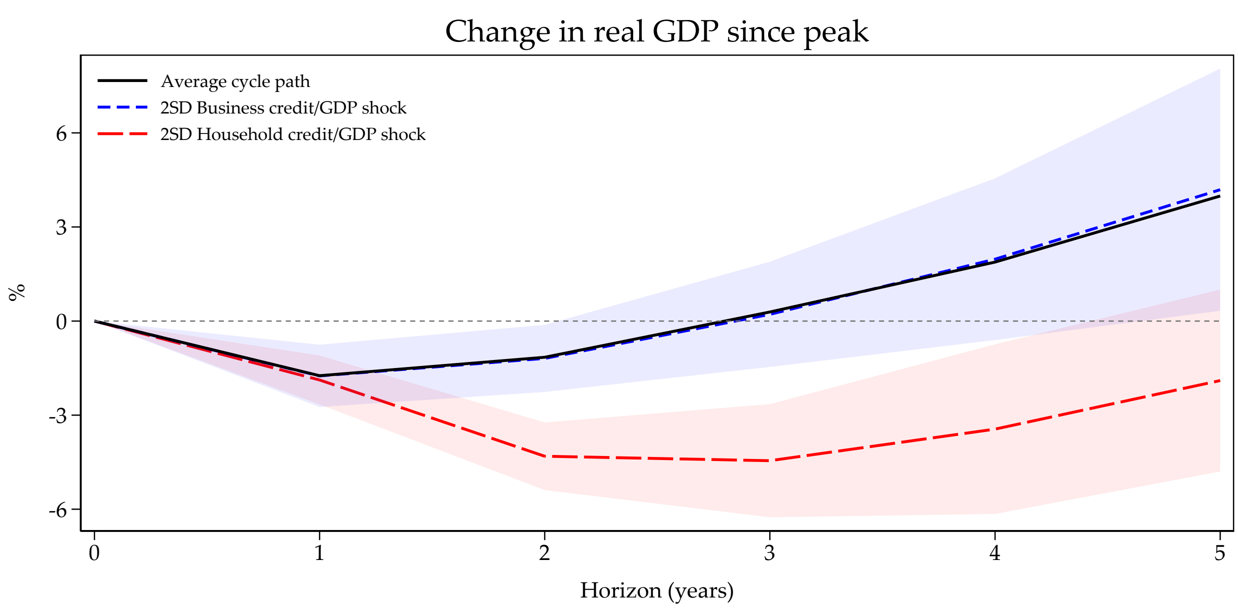
Table 1: *Credit booms and business cycle responses: local projections, with macro controls*

Dependent variable: Change in real GDP per capita since peak (log ×100)

	(1) $h=1$	(2) $h=2$	(3) $h=3$	(4) $h=4$	(5) $h=5$
Average cycle, α_h	-1.66^{**}	-1.10^{**}	0.35	1.85^{**}	3.89^{**}
	(0.08)	(0.14)	(0.17)	(0.24)	(0.33)
Business credit/GDP change, $\Delta_5 x^B_{it(p)}$	−0.32	2.49	0.70	1.22	4.01
	(1.29)	(1.41)	(1.62)	(3.17)	(3.57)
Household credit/GDP change, $\Delta_5 x^H_{it(p)}$	−4.65	-22.15^{**}	-33.09^{**}	-44.44^{**}	-42.10^{**}
	(3.65)	(4.77)	(5.36)	(9.00)	(9.26)
Macro controls, $\boldsymbol{w}_{it(p)}$	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.247	0.000	0.000	0.000	0.000
R^2	0.15	0.35	0.41	0.44	0.47
Number of cycles	150	150	150	150	149

Notes: Within-estimator, standard errors clustered on countries in parentheses. ** $p < 0.01$, * $p < 0.05$. Debt change denotes past 5-year change in credit/GDP ratio.

Figure 3: *Business and household debt, average and +2SD responses*



Notes: Figures show the predictive effects on growth of a two-SD debt expansion in the five years preceding the recession for business debt booms (left panels) and household credit booms (right panels). Estimates based on all business cycles in 18 advanced economies since 1870. Standard errors are clustered at the country level. Shaded areas denote the 95% confidence interval. See text.

capita is 4.2% higher compared with 4.0% observed in more normal times. In contrast, a 10 pps increase in household debt in the expansion is associated with dire consequences. The economy barely recovers (–0.1%) the level it had at the start of the recession 5 years later. Formal Wald tests confirm that the coefficients on business and household debt are significantly different from one another starting in year two.

These results are robust to introducing linear and quadratic time trends as well as to the exclusion of all recessions after 2007, that is, excluding data after the Global Financial Crisis. Moreover, we examine alternative definitions of our measure of credit booms. In particular, we looked at 3- and 10-year changes in credit/GDP (instead of 5-year changes), 3- and 5-year growth of real debt, and the measure proposed by Jordà, Schularick, and Taylor (2013). The results are robust and the corresponding estimates are tabulated in Appendix G.

To go further, we also consider that a firm's default probability increases in the *level* of liabilities relative to cash flow. Extrapolating to the macroeconomy, one may suspect that the level of debt could modulate the aggregate risk confronted by an economy that sees a rapid increase in borrowing. We test this hypothesis by interacting credit booms with the *level* of credit/GDP. This is shown in Figure A.8 in Appendix G. Interestingly, we do not find evidence that debt levels play an important role. At business credit/GDP levels one standard deviation above the country's historical standards, business credit booms predict recession outcomes, which are statistically indistinguishable from both booms at low levels, as well as from average recessions.

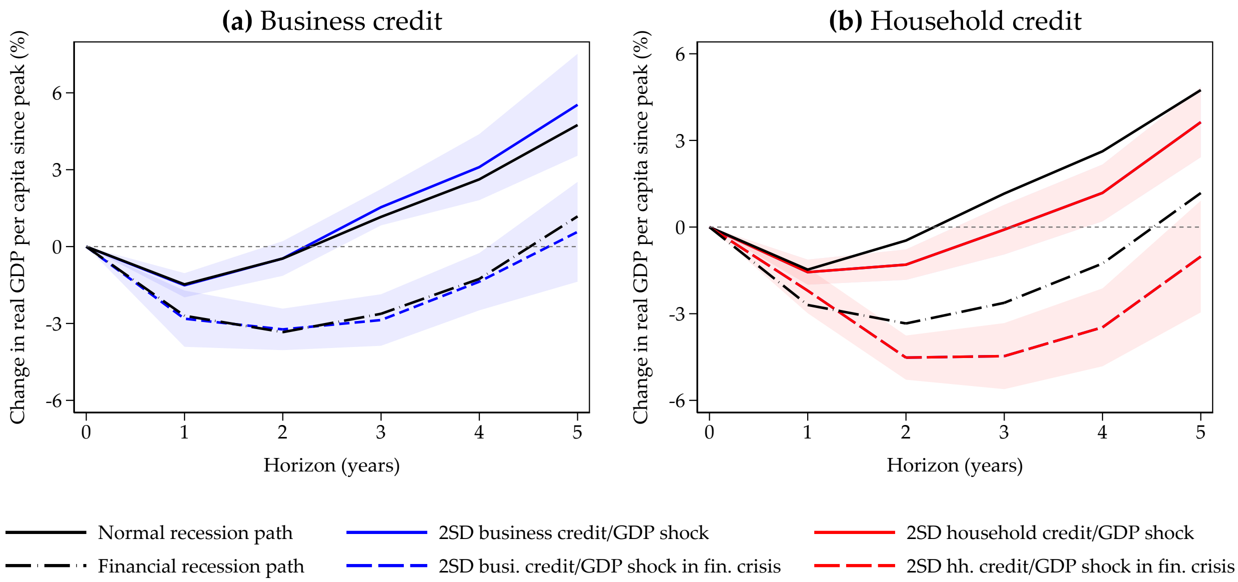
Corporate debt in financial crises The baseline business cycle effects reported in previous sections could simply reflect the fact that there is a greater propensity to experience a financial crisis after a household credit boom (Jordà, Schularick, and Taylor, 2013). Would the differences between household and business credit booms survive differentiating recessions by whether they are associated with a financial crisis?

We investigate this issue by stratifying recessions into two bins: financial recessions as defined earlier, and all other recessions, labelled as normal. We then examine how each type of debt build-up, business versus household, affects the subsequent response path in each type of recession.

Figure 4 presents the results. Panel (a) shows the results for business debt, and panel (b) for household debt. In both cases, as is well-known, financial recessions are deeper and last longer than normal recessions. The effect of a business credit boom (characterized by debt growth two standard deviations above the mean, as before) is essentially zero, economically and statistically speaking. The trajectories do not change one way or the other. In contrast, the effects are very sizable when we look at household debt. A credit boom during the expansion (again, measured by debt growing two standard deviations above the mean) makes either type of recession much more severe economically speaking. The effects may even be somewhat larger in a financial crisis, although the uncertainty bands are large enough to prevent any forceful conclusions on this issue.

Additional tests We examine a broader set of left-hand side variables and potential differences in tail risk using quantile local projection methods (see, e.g. Adrian et al., 2019). First, Appendix C shows recession paths for various other macro-financial variables. These are consistent with the general equilibrium effects that would lead to the GDP trajectories shown earlier. Next, in Appendix D, we set up quantile local projections to estimate the

Figure 4: *Business and household debt, normal versus financial recessions, average and +2SD responses*



Notes: Local projections stratified by type of recession. Left panel displays average and two SD business debt growth paths, right panel displays the same figure but for household debt. All regressions include the full set of macro controls and country fixed effects. Standard errors clustered by country. Shaded areas denote the 95% confidence interval. See text.

predictive effect of business debt booms on the lower tails of GDP outcomes. We find that business debt booms do not appear to increase the fragility of the economy.

4. The role of bankruptcy institutions

The striking dichotomy between the cyclical implications of household and business credit warrants caution, however, as it hints at the role of legal factors. Many countries have developed a robust legal scaffolding to buttress corporate debt renegotiation and restructuring out of a desire to facilitate the continuation of businesses in bankruptcy that can become profitable again in the future (Brouwer, 2006). By contrast, household debt exemptions and protections tend to be flimsy (Niemi-Kiesiläinen, 1997; Mitman, 2016). In fact, Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2016) and Auclert, Dobbie, and Goldsmith-Pinkham (2019), among others, have highlighted the potential importance of household debt relief as a tool for aggregate demand management.

Household debt is owed by individuals to smooth consumption whereas businesses lever up as abstract legal entities for the sake of profits (Jensen and Meckling, 1976). As a consequence, the incentives to renegotiate and restructure debt are very different. When default risk rises, the mere possibility of underinvestment incents creditors to renegotiate and restructure debt to preserve the full value of the business as a going concern (e.g., Bergman and Callen, 1991). At the aggregate level, such behaviour will tend to attenuate ancillary damage to the economy from lower investment—unless bankruptcy frictions impede corporate debt resolution.

Instead, poorly structured debt renegotiation procedures can aggravate hold-out problems. In such settings, creditors will rush to seize and liquidate collateral assets with the concomitant downward spiral on asset prices: a negative feedback doom loop. Moreover, legally burdensome procedures and/or poor creditor rights that depress expected recovery rates are likely to discourage creditors from initiating liquidation procedures in situations where they would be advisable. For example, Andrews and Petroulakis (2019) find that legal frictions in bankruptcy frameworks stall the liquidation of zombie firms.

These are substantively firm-level arguments, however. It is hard to know whether they translate one-for-one on a macro scale to quantitatively large aggregate fluctuations. Our goal in this section is to determine empirically whether the data are better characterized by the Myers (1977) perspective based on the high liquidation costs of debt, or the frictionless Modigliani and Miller (1958) benchmark view, where debt overhang and zombie firms would not arise. We put these ideas to work by assessing how measures of debt restructuring and liquidation frictions moderate the impulse responses of business credit booms. That is, for a recession preceded by corporate debt built-up, do frictions impeding business debt restructuring and liquidation deepen the recession or slow down the recovery?

4.1. Measuring legal bankruptcy frictions

We draw on two indicators to measure the characteristics of bankruptcy procedures. We use the creditor rights index of La Porta, López-de-Silanes, Shleifer, and Vishny (1997), later expanded by Djankov, McLiesh, and Shleifer (2007), covering the years 1978–2003. This measure codes creditors' protections in case of bankruptcy. We further extend the sample by using World Bank survey data collected using the methodology of Djankov, Hart, McLiesh, and Shleifer (2008) for the more recent 2004–2019 period. The survey is administered to attorneys and judges practicing bankruptcy law. More specifically, we use the *recovery rate* measure, which determines the share of debt paid to creditors in the event of default. A high recovery rate reflects low frictions in both renegotiations and liquidations.[10,11]

In order to merge data from Djankov, McLiesh, and Shleifer (2007) and Djankov, Hart, McLiesh, and Shleifer (2008), we proceed as follows. First, we determine an indicator-specific cut-off value (specifically, the sample median) to separate "low-friction," $L_{it} = 0$, from "high-friction," $L_{it} = 1$, observations. The resulting binary variable, L_{it}, covers from 1978 to 2019, totaling 65 business cycles from all 18 countries.[12,13]

[10] The results are very similar when using other indicators provided by the survey: bankruptcy procedure time, bankruptcy procedure cost, or the "resolution score" summary measure.

[11] In the model of Corbae and D'Erasmo (2021), the recovery rate constitutes an economic summary statistic of how various bankruptcy frictions distort behavior of creditors and lenders. Empirically, the European Banking Authority (2020) identified the recovery rate as a prime indicator for non-performing loans on bank balance sheets across the European Union. Using bank-loan level data, the report documents a positive and statistically significant link between loan-level recovery rates and strength of creditor rights encoded in bankruptcy regime across borrower types.

[12] Our results are robust to alternative threshold choices as documented in subsection G.2. They are also robust when mapping indicators into quintiles, deciles or their sample rank values to obtain harmonized ranges and interpretable step-sizes.

[13] Appendix E details the coding of L_{it} for each country-year.

4.2. Assessing our measure of debt restructuring frictions

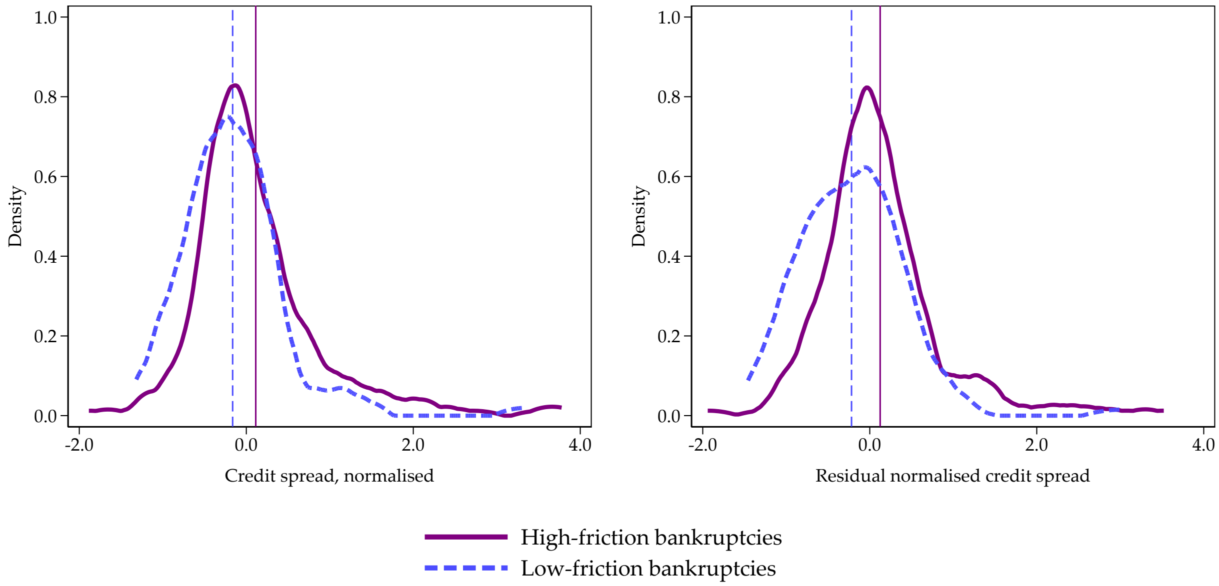
Figure A.5 in Appendix E displays the series of our newly constructed measure of bankruptcy frictions, L_{it}, for each country. Overall, the classification of countries featuring either high or low-friction bankruptcy regimes is pretty stable over time. On average, countries switch once over the four decades we observe. We note that group of South-European countries as well as France and Switzerland stand out for scoring high on the friction index consistently across the entire sample period.[14] The index behaviour is more mixed for other countries such as the Scandinavian and Central-European countries. Anglo-Saxon law systems are mixed too, but tend to be characterised by lower frictions. The UK, the Netherlands and Japan stand out for scoring low for most, if not the entire sample period 1978 to 2019.

In order to assess the plausibility of our newly constructed measure as well as potential identification concerns, we check whether countries with different bankruptcy frictions differ systematically in terms of business credit outcomes. First, one could expect that bankruptcy institutions affect the *quantity* of business debt, i.e., that the propensity to experience business credit booms and/or the size of such booms to be larger in low-friction regimes. Appendix F plots the distribution of business credit booms—measured by the change in the business credit/GDP ratio over the past five years—differentiated by bankruptcy regime. However, we find no relevant differences in first or second moments, neither for the full set of country years, nor for our sample of business cycle peaks.

In addition to the level of debt, we consider what happens to the *price* of debt depending

[14] Countries with little (or no) variation in bankruptcy regime do not affect our empirical strategy since we allow for country fixed effects and our emphasis is on the *interaction* with business credit booms.

Figure 5: *Bankruptcy frictions and credit spreads*



Notes: Kernel density estimates of normalized credit spreads between high-yield (corporate) and low-yield (government) bonds (Krishnamurty and Muir, 2020), by friction intensity of legal bankruptcy regime (constructed based upon Djankov, McLiesh, and Shleifer, 2007; Djankov, Hart, McLiesh, and Shleifer, 2008, see main text). The right panel shows distributions of residuals from a panel regression of credit spreads on country fixed effects and the familiar set of macroeconomic controls. The sample overlap consists of 11 countries, totaling 261 country-years. Vertical lines mark sample averages.

on bankruptcy institutions. Figure 5 uses data on credit spreads between sovereign and corporate bond yields from Krishnamurty and Muir (2020).[15] The figure shows that credit spreads above 2 percentage points almost exclusively occur in country-year observations with high legal frictions for bankruptcy procedures.

Mean differences become even starker when estimating panel regressions of credit spreads on legal regime, controlling for country fixed effects, business cycle phase, and current and past macroeconomic covariates. A switch to a high-friction bankruptcy regime significantly increases the spread by about 1.1 percentage points.That is, markets seem to price in higher bankruptcy costs, however without leaving a systematic imprint on the business credit cycle.

[15]We thank the authors for sharing the data with us.

4.3. Legal frictions and the aftermath of business credit booms

We now revisit the original analysis of how recessions evolve following a business credit boom while allowing for the legal regime to play a role. Relative to the original specification in Equation 1, we now allow an interaction of legal frictions L_{it} with business debt change $\Delta_5 x^B_{it}$, denoted $x^{BL}_{it} = \Delta_5 x^B_{it} \times L_{it}$. Using this new variable, the resulting local projection is

$$\Delta_h y_{it(p)+h} = \alpha_h + \alpha_{hi} + \beta^{BL}_h x^{BL}_{it(p)} + \beta^H_h \Delta_5 x^H_{it(p)} + \beta^B_h \Delta_5 x^B_{it(p)} + \gamma_h \boldsymbol{w}_{it(p)} + e_{it(p)} \,, \qquad (2)$$

including the same set of macroeconomic control variables used earlier.

Before discussing the results, a word on identification. Can we interpret estimates of Equation 2 causally? One could certainly make the case, though the next sections explore this issue in greater detail. First, note that by focusing on recessions and in particular, the build-up of debt during the expansion, we are using the arrow of time to remove potential contamination of our analysis from contemporaneous responses of agents' debt choices to current economic conditions. Second, we condition on an extensive set of macroeconomic controls and their lags so as to ensure that any variation on the shape of the recession and recovery is not explained by well-known economic drivers. Third, legislation and attitudes toward bankruptcy are not fast moving variables. One can certainly argue that these variables might respond to previous economic outcomes. But changes often follow the slow political cycle more than the fast economic one. Finally, variation across countries may hide the fact that legal frictions simply measure other features characterizing the economic setup of these countries. While certainly a concern, we note that our regressions contain country-fixed effects to sterilize such variation. Moreover, by conditioning on other macroeconomic controls, we greatly reduce (or even eliminate) that possibility. In sum, we follow a *selection-on-observables* identification strategy no different from what is commonly done in the vector autoregression (VAR) literature. But we don't rest here and the next section probes our results further in a variety of ways, including an instrumental variable approach.

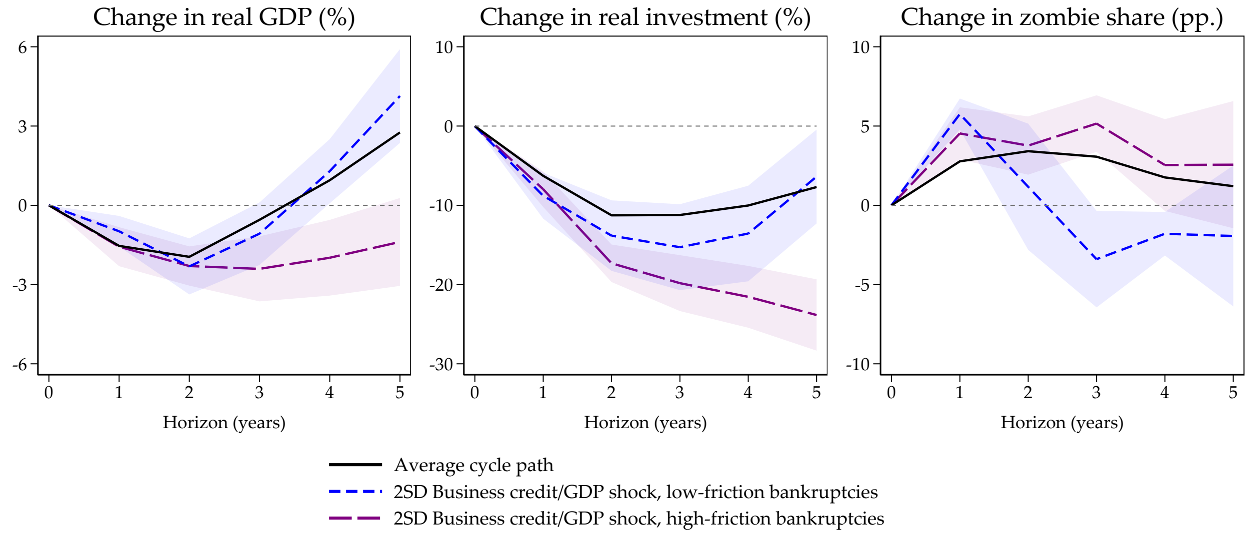
The core results are presented in Figure 6, which is organized into three panels. The first panel reports the response of real GDP; the second panel the response of real investment; and the third panel the share of zombie businesses (to be discussed momentarily). These estimates are for the full 1978–2019 sample using OLS. Within each panel, we display two paths, depending on whether legal bankruptcy frictions are high ($L_{it} = 1$), or low ($L_{it} = 0$). Recall that the setting evaluates the recession path when corporate debt during the expansion grows at a rate that is two standard deviations above the historical mean.

Real GDP and investment are from the JST dataset that we used earlier. The share of zombie businesses is based on data from Banerjee and Hofmann (2018), itself based on WorldScope financial statements from listed firms in 14 advanced economies from 1985 to 2017. Corporate zombies are characterized by having insufficient earnings to cover debt service costs and sub-par stock-market valuation, an indication that expectations of future profitability are low.[16]

In the figure, as we suspected, the more frictions there are, the deeper and longer is the recession following a business credit boom. In fact, countries with inefficient bankruptcy

[16] The zombie share regressions are augmented by controls for the zombie share at business cycle peak and the two preceding years. Due to the reduced sample coverage in these LPs, we cut the lag-length of the auxiliary macro controls to contemporaneous values and one lag only.

Figure 6: *The aftermath of business credit booms, by legal regime*



Notes: Path of the recession predicted when business debt grows at 2 SD above the historical mean in the five years preceding a peak. Long-dash purple line corresponds to high legal bankruptcy friction regimes ($L_{it} = 1$). Short-dash, blue line corresponds to the low-friction regime instead ($L_{it} = 0$). All other covariates in Equation 2 evaluated at their country-specific sample averages. Estimates for zombie shares further condition on the level and the annual change of the zombie share at business cycle peak. Standard errors are clustered at the country level. Shaded areas denote the 90% confidence interval. See text.

regimes experience recoveries that resemble those generated when the economy experiences a household credit boom during the expansion. When frictions are low, the recession bottoms out in the second year, and by the fourth year the economy is above the level it had entering the recession. When frictions are high, the recession takes an additional year to bottom out, and the recovery of the previous level is not visible in the 5 years displayed. Institutional frictions in tandem with business credit booms can therefore have substantial macroeconomic effects.[17]

Investment follows an even more pronounced pattern, in line with the findings of the business cycle literature (see, e.g., Zarnowitz, 1992). In addition, when frictions are high, investment declines over the entire 5-year period displayed in the middle panel of Figure 6. By year 5 investment is about 25% lower than it was entering the recession whereas in a

[17] These results are qualitatively unchanged when restricting the sample to 2003, i.e., using variation in creditor rights only and excluding the Global Financial Crisis. Restricting to the post-2003 sample however does not yield enough business cycle observations to estimate all parameters.

low friction environment, investment has already begun to recover and is only about 5% below the peak.

Finally, we turn to examine the role that frictions play in preventing liquidation of bad businesses and therefore expanding the corporate zombie share. As, for example, Caballero, Hoshi, and Kashyap (2008) and Andrews and Petroulakis (2019) argue, a large population of nearly defunct businesses depresses industry dynamism and investment—which, as we have seen, takes a big hit when frictions to renegotiation and restructuring are present. As the right-hand panel of Figure 6 shows, during the first year of the recession, the zombie share jumps by about 5% regardless of whether legal frictions are high or low. However, whereas this share tends to remain elevated for over 5 years when frictions are high, the share drops quickly after the second year when frictions are low, and remains mostly below the value at the peak.

5. Threats to identification

The aggregate results reported in Figure 6 very much accord with several micro-level mechanisms discussed in the literature. Moreover, the nexus between considerable declines in investment and increases in the share of nearly defunct business kept alive by frictions to restructuring of these businesses is consistent with the literature. However, beyond our *selection-on-observables* identification strategy, one could imagine some potential threats to identification that we address in this section.

5.1. Robustness to confounders of bankruptcy institutions

Figure 6 shows that the recession and recovery trajectories following business credit booms have been very heterogeneous. Economies where bankruptcy is inefficient have markedly slower recoveries after equally deep recessions. On average, these episodes were also accompanied by an investment slump and an elevated share of corporate zombies among listed companies.

However, conclusions that legal bankruptcy reforms could thus alleviate problems of debt overhang and zombification after business credit booms need to be stress-tested. Variation of bankruptcy frictions across countries and time is, after all, never quite random as legal systems evolve alongside society and respond to a country's economic experience. Certain features of bankruptcy laws will occur in tandem with other characteristics of economic institutions such as labour market regulations or tax codes. These institutional confounders, in turn, will also influence recession trajectories. Their confounding effects might not be fully captured by country fixed effects and our extensive control set.

The first in a series of checks on the robustness of our results consists in evaluating whether our legal friction indicator has any predictive power on the recession trajectories themselves. Moreover, we introduce additional controls to rule out alternative explanations. First, La Porta, López-de-Silanes, Shleifer, and Vishny (1997) and Djankov, McLiesh, and Shleifer (2007) document broader and deeper capital markets—measured as private debt relative to GDP—in places with better creditor rights protections. Better developed capital might moderate the aftermath of business debt booms for various reasons, e.g., providing well-functioning secondary markets for toxic assets which restore intermediation capacity.

Second, bankruptcy frictions might more often than not go along with poor financial regulation, with the latter being the true reason why corporate debt—that is, non-performing loans—can slow down recovery from recessions. Reassuringly, we find the interaction effects of bankruptcy frictions and corporate debt to be robust in all those specifications, which we report in Appendix G.[18]

5.2. Instrumenting bankruptcy frictions by legal origins

In thinking about a possible instrumental variable, we follow an established literature that distinguishes traditions of *civil law* and *common law* and which has demonstrated their relation to bankruptcy frictions, notably investor rights protection (La Porta, López-de-Silanes, Shleifer, and Vishny, 1997, 1998; La Porta, López-De-Silanes, and Shleifer, 2008).

[18] We expanded the baseline specification in Equation 2 with the frictions indicator directly in the regression to account for a possible connection between credit booms and the legal framework as follows,

$$\Delta_h y_{it(p)+h} = \alpha_h + \alpha_{hi} + \beta_h^L L_{it} + \beta_h^{BL} x_{it(p)}^{BL} + \beta_h^H \Delta_5 x_{it(p)}^H + \beta_h^B \Delta_5 x_{it(p)}^B + \gamma_h w_{it(p)} + e_{it(p)} , \quad (3)$$

and this did not affect our conclusions. Figure A.10 in Appendix G plots trajectories for each legal regime, with and without credit booms.

Similarly, the direct predictive value of our friction indicator on investment and zombie shares is small compared to the business credit boom scenario as shown in Figure A.10 in Appendix G. Hence, in order for latent factors to invalidate causal interpretations of the role played by bankruptcy institutions, they would have to *interact* with the extent of business leveraging during expansions.

Such interactions are not impossible but seem unlikely in view of the literature. In fact, using the same coding of creditor rights underlying our bankruptcy index, La Porta, López-de-Silanes, Shleifer, and Vishny (1997) and Djankov, McLiesh, and Shleifer (2007) document broader and deeper capital markets—measured as private debt relative to GDP—in places where outside investors are legally better protected against inside management. In addition, bankruptcy frictions might occur in tandem with poor financial regulation and well-governed financial systems are presumably more resilient and capable to deal with a business credit boom turning into a pile of non-performing loans.

We check these caveats by introducing additional controls proxying for the depth of private debt markets—the eleven-year centered moving average of total bank lending to the non-financial private sector relative to GDP, mapped into within-year cross-sectional percentiles to obtain a stationary variable—and an index of banking supervision quality obtained from Abiad, Detragiache, and Tressel (2010). We introduce each variable in levels and as interactions with our measure of business credit booms. Reassuringly, estimates for predicted trajectories by legal regime remain qualitatively unchanged, as shown in Appendix G, Figure A.11.

Finally, confounders might be dynamic in nature. For instance, large-scale legal reforms might comprise changes to bankruptcy law alongside shifts in economic institutions that also influence the broadening and deepening of business debt markets. To rule out such dynamic interactions more generally, we resort to the instrumental variable strategy described in the next subsection.

Civil law traditions originate in Roman law, but they then morphed into different European varieties under the influence of gradual or drastic changes such as the French Revolution. By contrast, the British common law tradition is associated with legal principles of private dispute resolution with less public control (La Porta, López-de-Silanes, Shleifer, and Vishny, 1998; Glaeser and Shleifer, 2002). The adoption of either civil or common law dates back to the 17th and 18th centuries, when revolution, colonization, and Napoleonic conquest laid the foundations of legal principles across Europe and America. In Japan, a variety of German civil law was adopted during extensive reforms under Emperor Meiji at the end of the 19th century (Djankov, McLiesh, and Shleifer, 2007).

Most importantly for us, civil and common law traditions differ markedly in their handling of bankruptcy. La Porta, López-de-Silanes, Shleifer, and Vishny (1998) were the first to show empirically that (French) civil law traditions were associated with much weaker legal protection of investors' interests, including creditor rights in the case of default. Djankov, McLiesh, and Shleifer (2007) and Gamboa-Cavazos and Schneider. (2007) document that civil law systems rely on frequent interlocutory appeals, leading to bankruptcy procedures that are more intricate and costly than those under common law.

Based on the coding of legal origins in Djankov, McLiesh, and Shleifer (2007), we instrument the variable x_{it}^{BL} with the interaction of the business debt build-up variable, $\Delta_5 x_{it}^{B}$ and the legal origin dummy, d_{it}^{LO}, which is defined to be $d_{it}^{LO} = 1$ for common law economies, and is $d_{it}^{LO} = 0$ otherwise. Hence, the instrument is for $x_{it}^{BL} = \Delta_5 x_{it}^{B} \times L_{it}$ is constructed similarly as $z_{it} = \Delta_5 x_{it}^{B} \times d_{it}^{LO}$.

The first stage regression of $x_{it(p)}^{BL}$ on $z_{it(p)}$ and the controls previously used for our sample of business cycle peaks is shown in Table 2. The results presented in the table

Table 2: *Explaining legal frictions to debt resolution by legal origin*

Dependent variable: Interaction term $x^{BL}_{it(p)}$

	$x^{BL}_{it(p)}$
Instrument, $z_{it(p)}$	0.76**
	(0.23)
Business credit/GDP change, $\Delta_5 x^{B}_{it(p)}$	−0.20
	(0.21)
Household credit/GDP change, $\Delta_5 x^{H}_{it(p)}$	−0.18
	(0.13)
Macro controls, $\boldsymbol{w}_{it(p)}$	Yes
F-statistic	10.45
R^2	0.75
Number of cycles	65

Notes: First stage results for LP-IV estimation. Within-estimator, standard errors clustered on countries in parentheses. ** $p < 0.01$, * $p < 0.05$. Debt change denotes past 5-year change in credit/GDP ratio. Macro controls include contemporaneous and two lags of GDP per capita growth and inflation.

suggest that our instrument is strong, with an F-statistic above 10. The sign of the coefficient is significant and consistent with our hypothesis, that is, that common law countries tend to have more efficient systems today for the resolution of corporate financial distress.

As noted in the literature, a country's legal origin likely affects various other legal aspects, or it might be correlated with cultural factors that have (La Porta, López-de-Silanes, Shleifer, and Vishny, 1998; Stulz and Williamson, 2003). Such channels pose a threat to the instrument's exclusion restriction. Since we have just-identification, obviously we cannot formally test for violations of the exclusion restriction. However, we can characterize the sign of the potential bias that may exist.

Simple instrumental variables algebra suggests that:

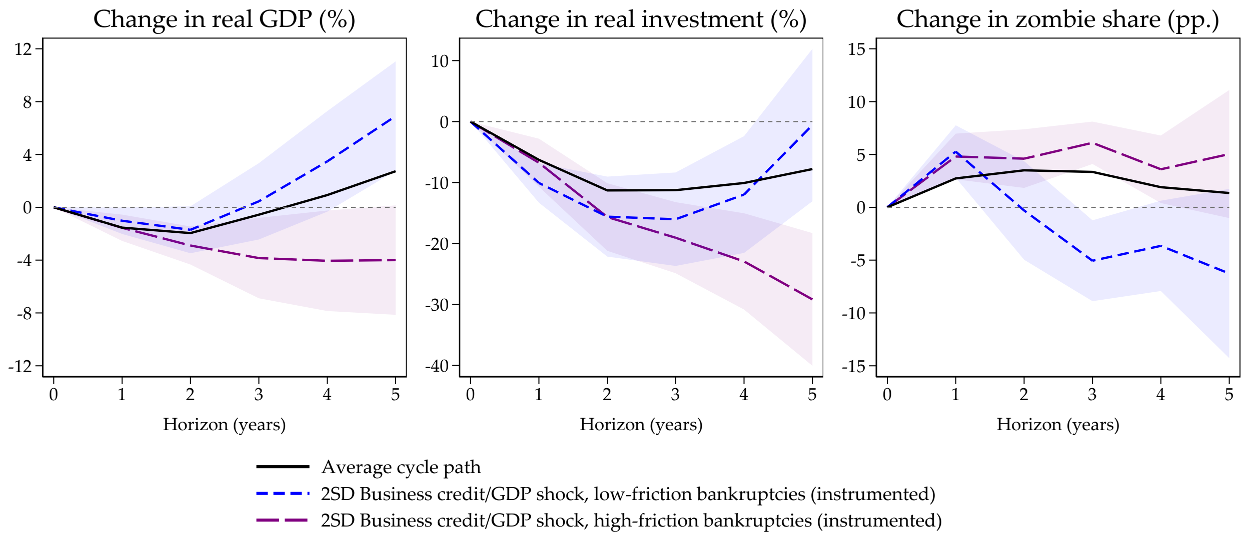
$$\hat{\beta}_h^{BL} \xrightarrow{p} \beta_h^{BL} + \theta_h \frac{E(z_{it}^2)}{E(\Delta_5 x_{it}^B z_{it})}, \tag{4}$$

where the coefficient θ_h captures the direct effect of the instrument on the outcome. Note that the right-hand side fraction is positive since the numerator refers to a squared variable and the denominator, by the first-stage regression in Table 2, is also positive.

Next, we can speculate about the sign of θ_h. Most common law countries (the U.S. and the U.K. being the two clearest examples) did better during the recent financial crisis than civil law economies (such as Spain, for example). This would suggest that if there are spillovers, θ_h is probably positive. Since the β_h are presumably negative (because they refer to the effect on recession and its recovery), potential spillovers from the legal origin instrument would then tend to attenuate our estimates. In fact, our IV results below compared to the selection-on-observables results reported earlier are quite similar, so, if anything, potential spillovers would reinforce our findings.

While definite proof is elusive, extensive robustness checks documented in subsection 5.1 and Appendix G make us reasonably confident that major confounding baseline channels running through any auxiliary institutions, as well as interaction effects stemming from financial development or banking regulation, are not sufficiently correlated with bankruptcy frictions to be latent drivers behind the predictions presented earlier. However, financial institutions might still be a determinant of recession trajectories while being influenced by legal origins. Hence, for the subsequent analysis we add the familiar control of private debt market depth, measured at business cycle peak.

Figure 7: *Instrumenting frictions to debt resolution by legal origin*



Notes: Predicted recession paths from the peak stratified by whether bankruptcy frictions are low, $L_{it} = 1$, or high, $L_{it} = 0$ and instrumented using the legal origin instrument described in the text. We assume a 2 SD corporate credit/GDP boom in the 5 years leading up to the recession. All estimates are conditional on the same set of controls detailed in the text. Estimates for zombie shares further condition on the level and the annual change of the zombie share at business cycle peak. The estimates include country-fixed effects. Standard errors are clustered at the country level for non-parametrically adjustment for residual serial correlation. Shaded areas denote the 90% confidence interval. See text.

The results based on estimating Equation 2 with local projection instrumental variables (LP-IV) are shown in Figure 7. These confirm the findings from Figure 6; although the estimates are slightly less precise, the path differences are even starker. As before, frictions impeding restructuring and liquidation aggravate the effects of corporate financial distress to the point of making the recession trajectory resemble the typically much more adverse path associated with a household credit boom seen earlier. In contrast, efficient institutions are associated with recession trajectories that resemble normal recession trajectories, even when there is a preceding large build-up of corporate debt. The differences between the "high-friction" and "low-friction" trajectories are statistically significant at the 5% level.

Summing up, we think that history has important lessons to offer. Institutional factors have a profound impact on how efficiently the financial aftermath from business credit booms can be resolved. In line with theory, debt overhang becomes costly when frictions impede a quick resolution and reallocation of resources. It makes one wonder if similarly frictionless debt resolution procedures were available to individuals, whether household credit booms would also then be associated with milder recessions.

6. Conclusion

Debt overhang can lead to under-investment by firms. Following Myers (1977), a large theoretical literature has explored the idea that investment shrinks for such indebted firms because the existing debt holders, not new investors, would be the main beneficiaries from new investment. Similarly, the risk of zombie lending increases in the exposure of bank balance sheets to corporate debt. Ever-greening of loans keep unprofitable firms in the market, likely undermining future productivity growth. In practice, the strength of these effects depends on departures from the canonical Modigliani and Miller (1958) theorem. How large these departures are in practice and how strong their the macroeconomics repercussions, is largely an empirical question.

At the aggregate level, our results show that neither corporate debt overhang nor zombie lending have played an economically or statistically significant role historically on average. However, the after-effects of business credit booms become more problematic when debt restructuring and liquidation become more costly, as the data also show. In this situation, zombie firms are more likely to emerge and persist, as high costs of liquidation increase incentives for banks to "extend and pretend" instead of liquidating.

We used institutional proxies for the costs of balance sheet reorganization to delineate different institutional environments that make debt reorganization more or less efficient. In those places and times where reorganization and restructuring is inefficient and costly, corporate debt overhang is an important macroeconomic force that has measurably negative effects at business cycle frequency.

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ONLINE APPENDIX

A. Business cycle peaks

As described in the text, we date business cycle peaks using the algorithm of Bry and Boschan (1971). Moreover, we characterize the ensuing recession to be "financial" when the peak falls into a ±2-year window around a financial crisis dated by Jordà, Schularick, and Taylor (2017). Table A.1 shows business cycle peaks followed by normal recession and not falling into any war episode. Table A.2 shows business cycle peaks followed by financial recession and not falling into any war episode. Both types of peaks are also visualized in Figure A.1.

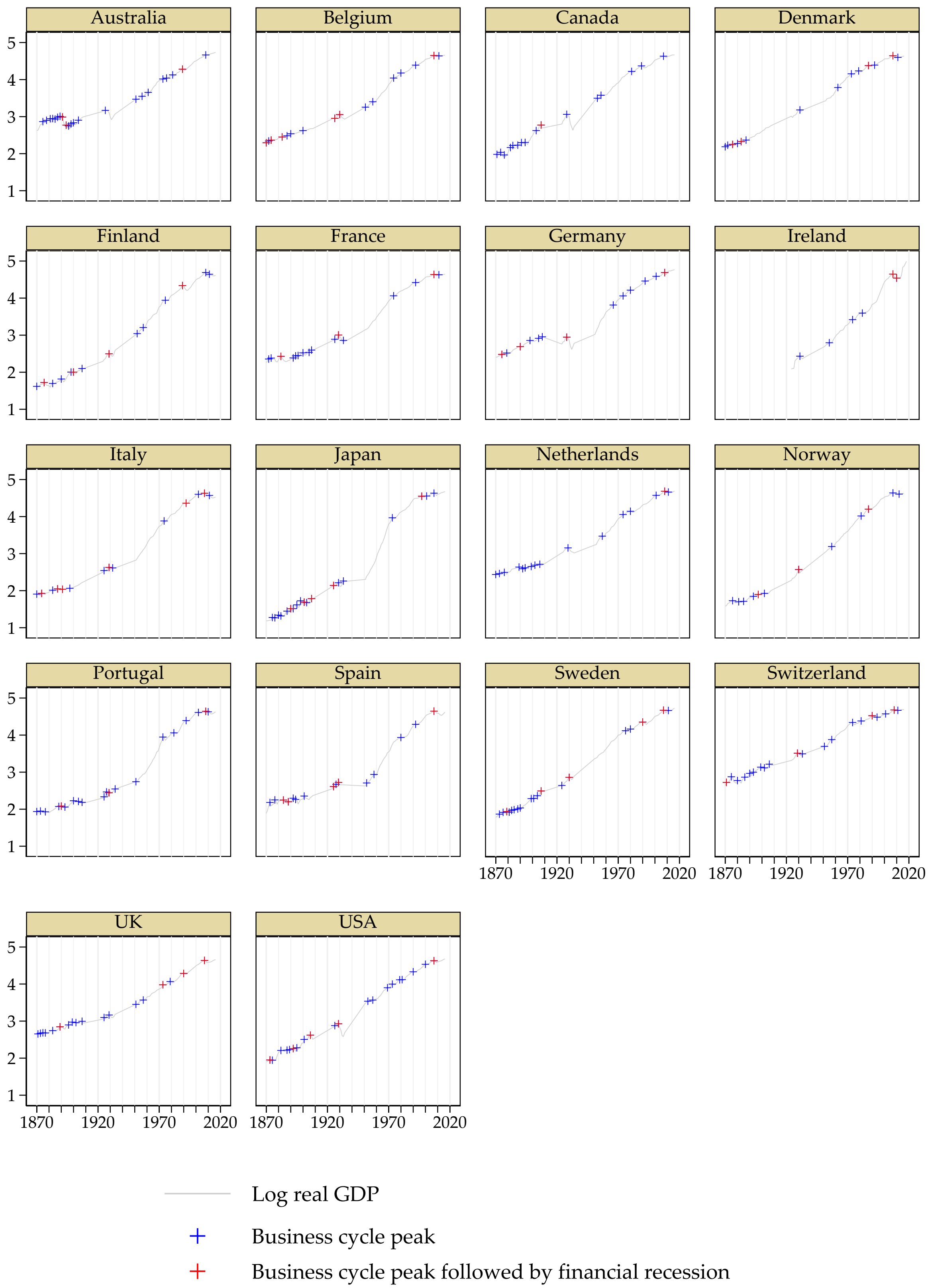
Table A.1: *List of business cycle peaks followed by normal recessions*

Australia	1961, 1973, 1976, 1981, 2008
Belgium	1957, 1974, 1980, 1992, 2011
Canada	1891, 1894, 1903, 1928, 1953, 1956, 1981, 1989, 2007
Denmark	1880, 1887, 1931, 1962, 1973, 1979, 1992, 2011
Finland	1957, 1975, 2008, 2011
France	1905, 1907, 1926, 1933, 1974, 1992, 2011
Germany	1898, 1905, 1908, 1966, 1974, 1980, 1992, 2001
Ireland	1955, 1974, 1982
Italy	1974, 2002, 2011
Japan	1973, 2001, 2007
Netherlands	1957, 1974, 1980, 2001, 2011
Norway	1876, 1881, 1885, 1893, 1902, 1957, 1981, 2007, 2012
Portugal	1973, 1982, 1992, 2002, 2010
Spain	1927, 1952, 1958, 1980, 1992
Sweden	1876, 1881, 1883, 1885, 1888, 1890, 1899, 1901, 1904, 1924, 1980, 2011
Switzerland	1875, 1880, 1886, 1890, 1893, 1899, 1902, 1906, 1933, 1951, 1957, 1974, 1981, 1994, 2001, 2011
UK	1896, 1899, 1902, 1907, 1925, 1929, 1951, 1957, 1979
USA	1926, 1953, 1957, 1969, 1973, 1979, 1981, 1990, 2000

Table A.2: *List of business cycle peaks followed by financial recessions*

Australia	1989
Belgium	2007
Canada	1907
Denmark	1883, 1987, 2007
Finland	1989
France	1929, 2007
Germany	1890, 2008
Ireland	2007, 2010
Italy	1992, 2007
Japan	1997
Netherlands	2008
Norway	1897, 1930, 1987
Portugal	2008
Spain	1925, 1929, 2007
Sweden	1879, 1907, 1930, 1990, 2007
Switzerland	1929, 1990, 2008
UK	1889, 1973, 1990, 2007
USA	1929, 2007

Figure A.1: *Business cycle peaks followed by financial and normal recessions*



B. Corporate debt over the business cycle

Table A.3 decomposes real business debt growth by business cycle phase and time period. Business debt grew strongly during the years of reconstruction after WW2. Across all periods, average real debt growth has been positive even during recessions. These averages, however, mask large swings in debt growth. The stock of outstanding business debt declined in real terms at several instances in our sample when the economy was in expansion and vice-versa. This indicates that cycles in GDP and business debt have not always been synchronized.

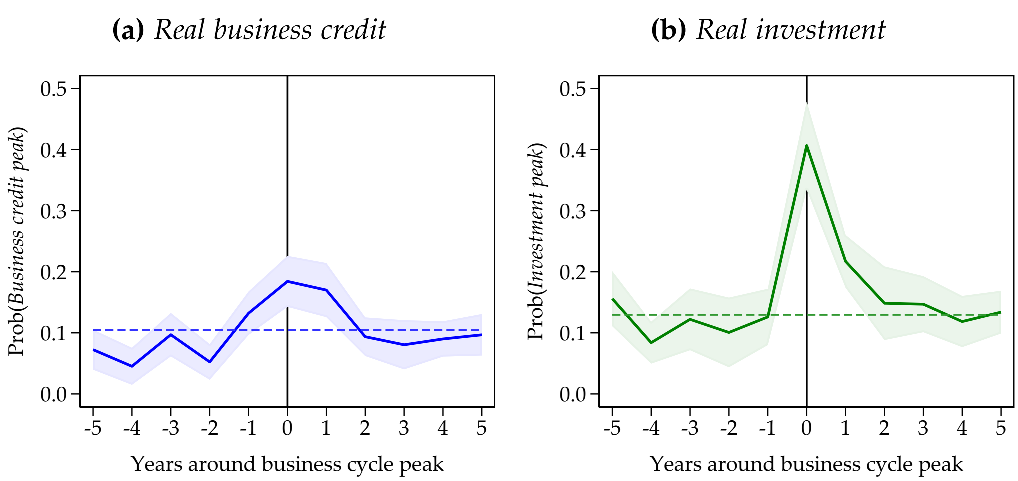
Next, using the same Bry and Boschan (1971) algorithm, we date business debt periods of expansion and recession. We find that only around a fifth of all business cycle peaks coincided with peaks in real business debt, as shown in Figure A.2. Contrast this lack of synchronicity with the right-hand panel of the figure. This panel shows shows estimates of the coincidence of real GDP and real investment peaks, documenting a much stronger cyclical link. Also on an annual basis, growth rates in real GDP and investment are much stronger correlated (ρ = 0.60) and than real GDP and real business credit (ρ = 0.23). There is a weak offset pattern for business credit, though: the probability of real business credit peaking at the year before or after the business cycle is notably higher than at other horizons off the GDP peak. By contrast, the peak probability shift is much sharper for investment.

Table A.3: *Real business credit growth over the business cycle, 1870-2020*

	Full sample	Sub-samples		
		Pre-WW2	Bretton Woods	Modern Globalisation
Full cycle	4.61 (8.94)	4.51 (11.06)	7.76 (8.29)	3.05 (7.12)
Boom years	5.29 (8.61)	5.04 (10.84)	8.28 (8.06)	3.71 (6.97)
Recession years	1.56 (9.64)	3.09 (11.54)	0.77 (8.25)	0.03 (7.06)

Notes: Subsample averages of annual changes in log real business credit, standard deviations thereof in parentheses. Excluding World War I and II and the Spanish civil war.

Figure A.2: *Cycle synchronisation of business credit and real GDP is relatively weak*



Notes: Event study estimates using dummies for peaks in real GDP, real business credit and real investment. Estimates are purged of country fixed effects; standard errors are clustered on the country level. The dashed lines mark unconditional baseline probabilities.

C. Other macroeconomic aggregates

We confirmed that business credit booms have on average no predictive power for the recession path of GDP. Here, we investigate a variety of other macro-financial variables. This should give us a better sense of the underlying channels.

Using a similar approach to Figure 3, in Figure A.3 we display the responses of other key indicators, as follows. Panels (a) and (b) of Figure A.3 show the responses of real consumption and real investment per capita, two important components of GDP. Panels (c) and (d) display the responses of unemployment rate and inflation, respectively, two key variables in any analysis of monetary policy. Panels (e) and (f) display the responses of real household and business debt to get a sense of frictions that may impede the recovery and hence justify the dynamics that we observe for GDP. Finally, panels (g) and (h) show the responses of asset prices.[19]

Generally speaking, business credit booms do not depress aggregate demand—whether consumption, or more interestingly, investment. This is in stark contrast to a household credit boom, whose effects are particularly visible in investment. A possible explanation as to why investment is relatively insensitive to a corporate boom is that firms may shift to other internal sources of financing, i.e., equity instead of debt. Covas and Den Haan (2011) document that for large firms, equity issuance is countercyclical while debt take-up is procyclical. Another alternative explanation is that, although business debt declines (as shown in panel (e)), lower business debt may simply reflect debt restructurings and haircuts since our data are aggregated.

The effects on the unemployment rate and inflation are consistent with the behavior of consumption and investment, though they are measured more imprecisely. Nevertheless, a household credit boom generally results in higher unemployment and lower inflation than in the average recession, though, of the two, the inflation response is less clear cut. Thus, a recession that follows a boom in household debt appears to require stronger monetary support. These same features are not apparent in business credit booms.

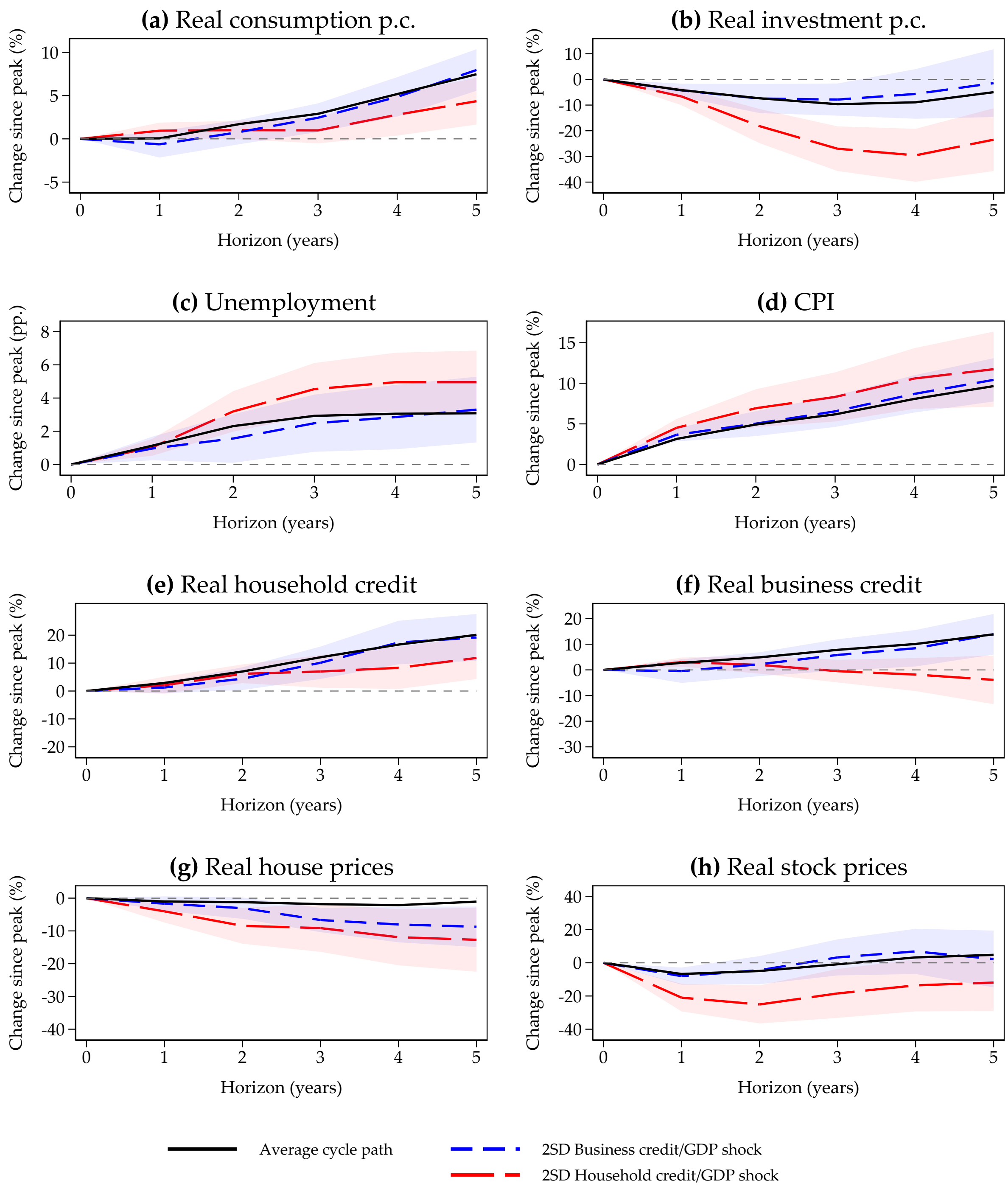
Panels (e) and (f) of Figure A.3 show the aftermath in debt markets. Echoing Jordà, Schularick, and Taylor (2013), household credit booms are followed by a long period of household deleveraging, which in turn is consistent with depressed aggregate demand, as panels (a) to (d) indicate. Business debt also grows significantly slower after business credit booms, requiring a similar period of financial repair. And this is true even if the boom takes place in the household sector. The same cannot be said as strongly for household debt. A business credit boom has much less effect on it, and it recovers more rapidly.

Finally, we investigated asset price behavior, reported in panels (g) and (h). We find that both house and stock prices are more negatively affected after household credit booms as compared to business credit booms. Declining housing wealth and falling residential investment seem to have been an important catalyst for the toxic general equilibrium spiral of household debt reported by Mian and Sufi (2010) for the U.S. after 2008.

We also show regression tables for the plots above.

[19] All local projections underlying the figures are presented in tabular form in the appendix. They include the full set of macro economic controls. The left-hand side variable, as before, refers to the cumulative change since the cycle peak. All experiments refer to a credit boom 2 standard deviations above the historical mean.

Figure A.3: *Business and household debt, responses of various macro-financial variables*



Notes: The figure shows responses to a two-SD debt expansion in the five years preceding the recession for business credit booms on the left and household credit booms on the right of each panel. The sample includes all business cycles in 18 advanced economies since 1870. Standard errors clustered at the country level. Shaded areas denote the 95% confidence interval. See text.

Table A.4: *Change in log real consumption*

	(1) $h=1$	(2) $h=2$	(3) $h=3$	(4) $h=4$	(5) $h=5$
Average cycle	-0.01	1.52^{**}	2.78^{**}	5.24^{**}	7.47^{**}
	(0.18)	(0.15)	(0.19)	(0.29)	(0.29)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	-2.79	1.22	-0.97	-1.94	-2.02
	(1.93)	(2.00)	(2.62)	(2.84)	(3.88)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	-1.14	-13.77^{*}	-22.22^{**}	-35.88^{**}	-42.96^{**}
	(3.93)	(5.64)	(4.95)	(7.81)	(7.43)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.725	0.024	0.001	0.002	0.000
R^2	0.23	0.29	0.42	0.38	0.36
Cycles	150	150	150	150	149

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.5: *Change in log real investment*

	(1) $h=1$	(2) $h=2$	(3) $h=3$	(4) $h=4$	(5) $h=5$
Average cycle	-4.09^{**}	-6.66^{**}	-8.45^{**}	-7.11^{**}	-3.15^{*}
	(0.36)	(0.71)	(1.08)	(1.24)	(1.48)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	3.24	5.36	9.59	10.12	12.46
	(5.28)	(7.97)	(11.02)	(13.21)	(14.14)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	-16.33	-75.14^{*}	-122.73^{**}	-144.57^{**}	-133.24^{**}
	(12.12)	(26.25)	(37.97)	(35.32)	(33.35)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.133	0.011	0.006	0.002	0.002
R^2	0.28	0.37	0.35	0.43	0.46
Cycles	150	150	150	150	149

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.6: *Change in unemployment rate*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	1.21^{**}	2.23^{**}	2.88^{**}	2.87^{**}	2.94^{**}
	(0.28)	(0.41)	(0.51)	(0.69)	(0.88)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	0.28	-0.40	-0.80	-0.86	-1.04
	(0.99)	(2.50)	(3.40)	(3.98)	(4.22)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	-0.42	5.74	12.50	16.17	14.47
	(3.06)	(6.44)	(7.98)	(8.36)	(8.47)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.837	0.410	0.177	0.126	0.141
R^2	0.39	0.23	0.35	0.35	0.34
Cycles	112	113	113	113	112

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.7: *Change in log real household credit*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	3.47^{**}	8.19^{**}	13.67^{**}	18.41^{**}	22.23^{**}
	(0.23)	(0.45)	(0.52)	(0.69)	(0.97)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	-13.96^{**}	-32.54^{**}	-43.08^{**}	-38.77^{*}	-36.26^{*}
	(3.68)	(9.53)	(11.00)	(16.44)	(15.67)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	-15.40	-32.50	-83.22	-137.02^{*}	-166.99^{**}
	(15.63)	(34.43)	(40.81)	(49.08)	(43.73)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.938	0.999	0.420	0.109	0.023
R^2	0.28	0.29	0.37	0.33	0.40
Cycles	149	149	149	149	146

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.8: *Change in log real business credit*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	3.06**	5.47**	7.73**	10.30**	15.06**
	(0.28)	(0.46)	(0.58)	(0.78)	(1.04)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	-17.69**	-24.80*	-24.26	-34.27	-43.87
	(4.30)	(11.17)	(18.74)	(23.86)	(31.79)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	7.18	-22.71	-73.91*	-117.26**	-185.43**
	(10.81)	(19.12)	(27.49)	(36.57)	(42.91)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.055	0.930	0.173	0.093	0.026
R^2	0.39	0.35	0.27	0.28	0.31
Cycles	149	149	149	149	148

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.9: *Change in log CPI*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	3.00**	4.70**	5.92**	7.77**	9.33**
	(0.10)	(0.22)	(0.31)	(0.39)	(0.50)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	0.15	-0.31	1.76	4.60	4.73
	(2.03)	(4.27)	(6.19)	(7.16)	(7.67)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	0.47	2.99	1.79	-9.30	-22.31
	(4.33)	(10.97)	(13.84)	(16.99)	(19.22)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.941	0.749	0.999	0.454	0.209
R^2	0.75	0.71	0.68	0.66	0.67
Cycles	150	150	150	150	150

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

D. Tail risks: quantile local projections

The possibility remains, that corporate debt has no visible mean effects, while carrying considerable tail. Recent research by Adrian, Boyarchenko, and Giannone (2019) make a similar point for household debt, whereas and Jordà, Schularick, and Taylor (2020) suggest that the economy exhibits fat-tailed behavior, that is, the lower quantiles of the GDP growth distribution contain potentially extreme losses. Thus, to investigate whether corporate debt makes the worst recessions have very extreme declines, we estimate quantile local projections (see, e.g., Linnemann and Winkler, 2016; Adrian, Boyarchenko, and Giannone, 2019; Stolbov and Shchepeleva, 2020). Specifically, we examine how corporate debt affects the distribution of GDP per capita growth *conditional* on observables.

Denote $\Delta_h y_{it(p)+h} = y_{it(p)+h} - y_{it(p)}$, that is, the approximate cumulative growth rate of GDP per capita using the same notation of the previous section. Let $\boldsymbol{X}_{it(p)}$ collect the debt growth variables defined earlier ($\Delta_5 x^j_{it(p)}$, $j = B, H$), as well as the vector of macro controls, $\boldsymbol{w}_{it(p)}$, the constant, and the fixed effects. Given this setup, quantile local projections can be estimated based on

$$\hat{\boldsymbol{\theta}}_{h,\tau} = \underset{\boldsymbol{\theta}_{h,\tau}}{\operatorname{argmin}} \sum_{1}^{t(P)} \Big(\tau\, \mathbb{1}(\Delta_h y_{it(p)+h} \geq \boldsymbol{X}_{it(p)}\boldsymbol{\theta}_{h,\tau}) |\Delta_h y_{it(p)+h} - \boldsymbol{X}_{it(p)}\boldsymbol{\theta}_{h,\tau}| + (1-\tau)\, \mathbb{1}(\Delta_h y_{it(p)+h} < \boldsymbol{X}_{it(p)}\boldsymbol{\theta}_{h,\tau}) |\Delta_h y_{it(p)+h} - \boldsymbol{X}_{it(p)}\boldsymbol{\theta}_{h,\tau}| \Big), \tag{5}$$

where $\mathbb{1}(\,.\,)$ denotes the indicator function and $\tau \in (0,1)$ indicates the τ^{th} quantile. The quantile of $\Delta_h y_{it(p)+h}$ conditional on $\boldsymbol{X}_{it(p)}$ is then given by

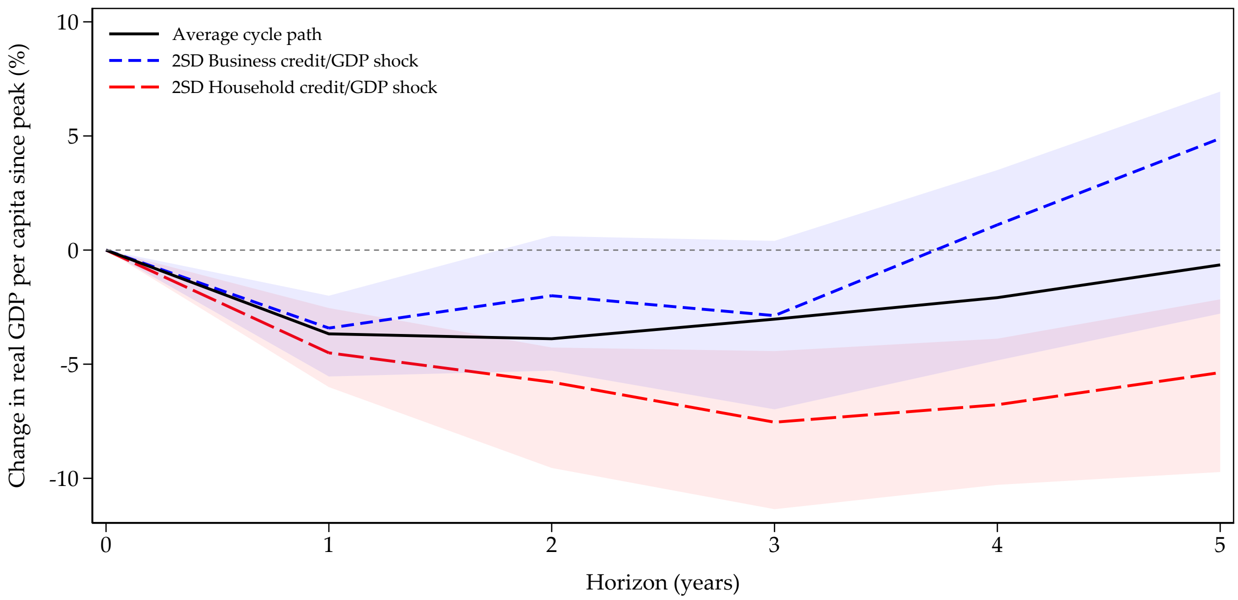
$$Q\left(\Delta_h y_{it(p)+h} | \boldsymbol{X}_{it(p)}\right) = \boldsymbol{X}_{it(p)}\boldsymbol{\theta}_{h,\tau} \equiv q^h_{\tau,t}\,. \tag{6}$$

The coefficients $\boldsymbol{\theta}_{h,\tau}$ measure the effect of the right-hand side variables on the τ quantile of the conditional distribution of $\Delta_h y_{it(p)+h}$. Specifically, using notation analogous to that in Equation 1, the coefficient $\beta^B_{h,\tau}$ will measure the effect of a business credit boom on the conditional distribution of $\Delta_h y_{it(p)+h}$, and similarly for household debt with the coefficient $\beta^H_{h,\tau}$. Hence, note that these coefficients will vary depending on the quantile τ selected.

Our approach to calculating quantile local projections is completely parallel to the way one usually computes local projections at the mean, as we did in Equation 1. The only difference is that we are now dealing with a nonlinear model so the marginal effect of a change in corporate (household) debt has to be evaluated accordingly. However, this simple setup admits these nonlinear effects in an unspecified, flexible manner.

Figure A.4 shows how we apply these methods to our data. In particular, we focus on the 20^{th} percentile of the conditional distribution of GDP per capita growth to investigate tail events. We did not choose a smaller quantile so as to have a reasonable data sample size for estimation. The figure displays quantile local projections alongside typical local projections evaluated at the mean. We display two cases, one for corporate debt, and one for household credit booms. These are defined as before, comparing the debt growth at the

Figure A.4: *Business and household debt, responses at 20th percentile of real GDP per capita growth*



Notes: Figures show the predictive effects on growth of a two-SD business/household debt buildup in the five years preceding the recession based on a LP series of quantile regressions. Business credit booms shown in the left-hand side panel and household debt booms shown in the right-hand side panel. Shaded areas denote the 95% confidence interval based on bootstrap replications. See text.

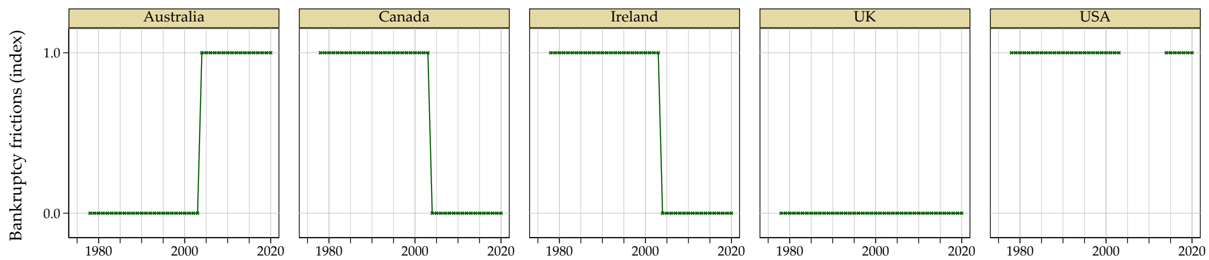
historical mean against growth at a rate two standard deviations above the historical mean. Consider first the figure associated with a business credit boom. The marginal effect of a business credit boom on the recovery path is the same whether considering the average growth path or the path of the 20$^{\text{th}}$ percentile worst recessions. In contrast, a household credit boom of a similar magnitude affects the worst 20$^{\text{th}}$ percentile recession paths very differently than the mean path: household credit booms increase the risk of experiencing a bad recession. These results therefore align well with Adrian, Boyarchenko, and Giannone (2019).

E. Legal index of bankruptcy frictions

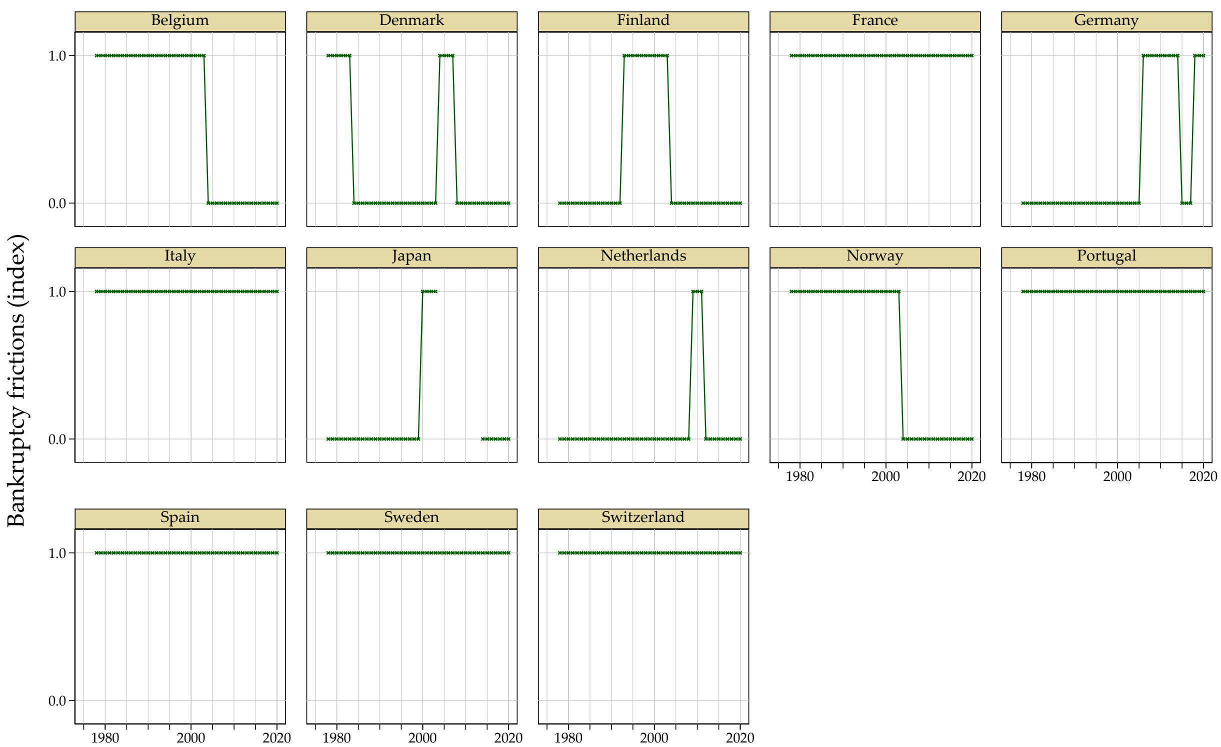
Figure A.5 plots the index of legal frictions to bankruptcy reform constructed based on the work of Djankov et al. (2007) and Djankov et al. (2008). After inverting each underlying index to interpret the resulting measure in terms of *frictions*, we split each index at its median to separate country-year observations into those with "high" and "low-"friction bankruptcy regimes. By construction, swings in the creditor rights index running until 2003 (Djankov et al., 2007) are induced through legal reforms. By contrast, the latter part – measuring the recovery rate as obtained from expert surveys (Djankov et al., 2008) – may also be moved by more subtle institutional changes.

Figure A.5: *Legal index of bankruptcy frictions for all counties*

(a) Common law countries



(b) Civil law countries

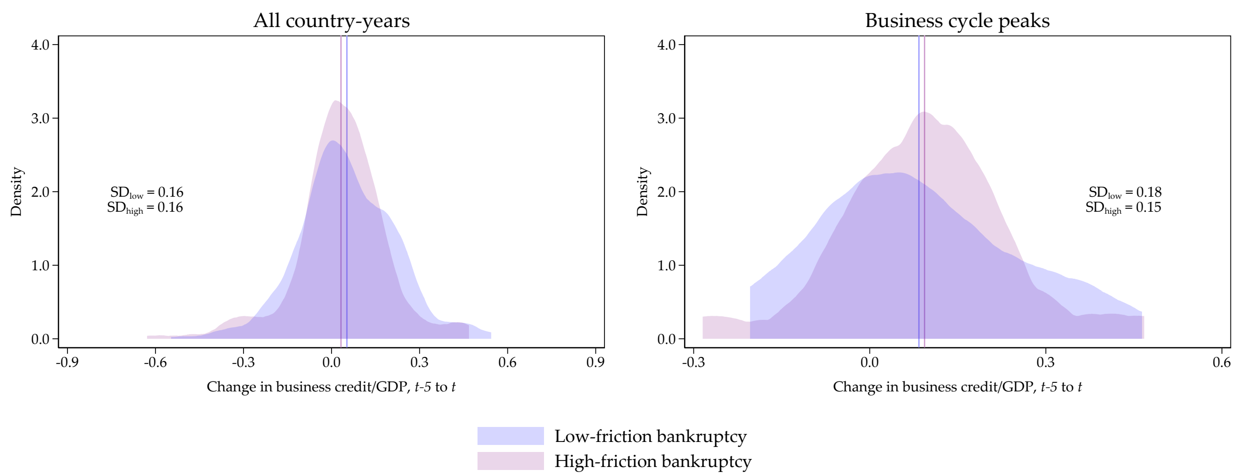


Notes: Time series of the legal index for frictions to liquidation and restructuring in business insolvency cases.

F. Bankruptcy frictions and business credit booms

We found indicative evidence that bankruptcy frictions are associated with differences in credit spreads across countries and time. Do they beyond that also affect the amplitude or likelihood of credit booms? Figure A.6 shows various distributions of business credit boom measures, distinguished by legal bankruptcy regime. Overall, we find limited evidence that bankruptcy regimes affect credit booms *quantitatively*. On average, business credit does not grow notably stronger during booms when frictions are low, nor does the business credit cycle exhibit different amplitudes over the whole business cycle (e.g. as measured by the full-sample standard deviation of five-year changes) under high-friction bankruptcy regimes.

Figure A.6: *Business credit booms and legal frictions*



Notes: Kernel density estimates of the change in business credit/GDP from $t-5$ to t, by bankruptcy regimes. Straight coloured lines indicate mean values.

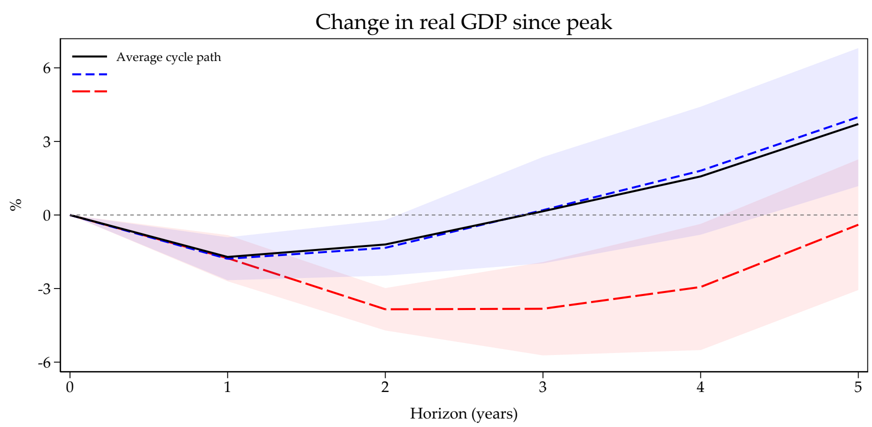
G. Robustness

We test different specifications for our main model and expose it to a battery of robustness checks. Results are documented below. Across all variants, our main conclusions indeed remain unchanged.

G.1. Robustness checks for baseline result

Figure A.7 shows our baseline specification with Driscoll-Kraay standard errors in a version that accommodates unbalanced panels and gaps in time series (Driscoll and Kraay, 1998; Hoechle, 2007). Moreover we introduce linear and quadratic time trends (Table A.10), exclude all recessions from 2007 onwards (Table A.11) and test alternative measures of debt overhang (Table A.12, Table A.13, Table A.14). We also report unconditional estimates in Table A.15. Finally, we test whether the effects of business credit expansions show up only at particularly high or low levels of business debt levels. We introduce an interaction term of 5-year changes in business credit/GDP with the level of business credit/GDP at peak and condition on the usual set of macro controls, country fixed effects and a linear and quadratic time trend. Figure A.8 plots the effects of credit expansion interacted with credit/GDP level.

Figure A.7: *Driscoll-Kraay Standard Errors*



Notes: The figure shows the effects of a two-SD credit expansion in the five years preceding the recession for household credit booms (lhs) and business credit booms (rhs). Shaded areas mark 95% CIs based on standard errors which are robust to autocorrelated and spatially correlated residuals. The estimations rely on all business cycles in 18 advanced economies since WW2.

G.2. Robustness checks of the role of bankruptcy institutions

For our results in the main text, we defined *high-friction bankruptcy* observations using the median of the underlying indices as cut-off. Figure A.9 presents result for predicted trajectories when choosing the 33th, 40th, 60th or the 67th percentile instead. Results are qualitatively robust in all specifications. When cutting at the 67th percentile, estimates shift more notably in quantitative terms, but still remain qualitatively unchanged.

Figure A.10 compares predictions by based on legal regimes *and* by business credit boom. These predictions are based on re-estimating Equation 2 augmented by a level control of the bankruptcy friction index. In fact, legal frictions to bankruptcy barely affect predictions in the absence of business credit booms.

Table A.10: *Introducing linear and quadratic time trends*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	-1.60**	-0.85**	0.57	1.85**	3.58**
	(0.13)	(0.23)	(0.35)	(0.41)	(0.50)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	0.96	0.69	-0.35	-0.63	-2.37
	(1.12)	(1.47)	(1.90)	(2.80)	(3.31)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	-1.00	-13.59**	-19.76**	-21.27*	-23.45*
	(3.29)	(4.01)	(5.75)	(9.65)	(11.03)
Time trend, linear	-0.71	1.86*	2.78*	5.10*	6.06*
	(0.72)	(0.79)	(1.29)	(2.14)	(2.59)
Time trend, quadratic	0.00	-0.00*	-0.00*	-0.00*	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Macro controls	Yes	Yes	Yes	Yes	Yes
Time trends	No	No	No	No	No
$\beta^B_h = \beta^H_h$ (p-value)	0.563	0.008	0.008	0.068	0.089
R^2	0.11	0.35	0.42	0.43	0.40
Cycles	150	150	150	150	149

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Figure A.11 re-estimates Equation 2 augmented by controls for (a) private credit market development and (b) quality of financial sector regulation. In each specification, the variable enters both as level and as interaction with business credit boom. Referring to Djankov, McLiesh, and Shleifer (2007), private credit sector depth is measured as the centered five-year moving average of total bank lending relative to GDP. To measure financial sector regulation quality, we use the index of bank supervision quality constructed by Abiad, Detragiache, and Tressel (2010). Note that the latter covers data until 2003, reducing degrees of freedom ($N = 38$) and making estimation of the zombie share regression impossible altogether ($N = 21$). LPs underpinning the zombie share predictions in the right panel of (b) thus omit country fixed effects and macroeconomic controls.

Table A.11: *Omitting recessions post 2006*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	-1.65**	-0.50**	1.01**	2.66**	4.76**
	(0.08)	(0.16)	(0.24)	(0.30)	(0.40)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	1.77	1.20	-0.05	0.48	-0.12
	(1.25)	(1.50)	(2.15)	(3.40)	(3.78)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	0.60	-9.53*	-20.34**	-22.50	-24.23
	(3.04)	(3.36)	(6.84)	(11.44)	(11.87)
Macro controls	Yes	Yes	Yes	Yes	Yes
$\beta^B_h = \beta^H_h$ (p-value)	0.739	0.017	0.014	0.086	0.085
R^2	0.16	0.30	0.37	0.37	0.35
Cycles	121	121	121	121	121

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.12: *Expansion measured by 3-year change in credit/GDP*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	-1.77**	-0.40	1.33**	3.04**	5.70**
	(0.13)	(0.25)	(0.31)	(0.43)	(0.60)
Business credit/GDP expansion $\Delta_3 x^B_{it(p)}$	0.53	-2.36	-4.19	-7.45	-14.59*
	(2.26)	(2.67)	(3.38)	(4.84)	(6.47)
Household credit/GDP expansion $\Delta_3 x^H_{it(p)}$	2.66	-20.19**	-28.88**	-30.10*	-36.14*
	(3.62)	(5.43)	(8.62)	(13.30)	(16.31)
Macro controls	Yes	Yes	Yes	Yes	Yes
R^2	0.104	0.295	0.380	0.375	0.389
Cycles	155	155	155	155	154

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.13: *Expansion measured by 10-year change in credit/GDP*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	-1.81**	0.01	2.04**	3.40**	5.76**
	(0.20)	(0.41)	(0.46)	(0.55)	(0.81)
Business credit/GDP expansion $\Delta_{10}x^{B}_{it(p)}$	1.13	-0.53	-1.48	-1.99	-4.23
	(1.08)	(1.25)	(1.32)	(1.87)	(2.80)
Household credit/GDP expansion $\Delta_{10}x^{H}_{it(p)}$	-0.94	-11.40**	-16.41**	-17.80**	-18.83**
	(1.63)	(2.41)	(2.97)	(4.88)	(5.39)
Macro controls	Yes	Yes	Yes	Yes	Yes
R^2	0.127	0.244	0.341	0.344	0.356
Cycles	133	133	133	133	132

Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.14: *Expansion measured by real credit growth*

	(1) $h = 1$	(2) $h = 2$	(3) $h = 3$	(4) $h = 4$	(5) $h = 5$
Average cycle	-2.01**	-0.40	1.31	2.79*	5.54**
	(0.39)	(0.54)	(0.72)	(1.13)	(1.39)
Business credit expansion $\Delta_5 log(credit^{B}_{it(p)})$	0.01	-0.01	-0.02	-0.03	-0.04
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
Household credit expansion $\Delta_5 log(credit^{H}_{it(p)})$	0.00	-0.01	-0.01	-0.00	-0.01
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
Macro controls	Yes	Yes	Yes	Yes	Yes
R^2	0.109	0.229	0.306	0.302	0.283
Cycles	150	150	150	150	149

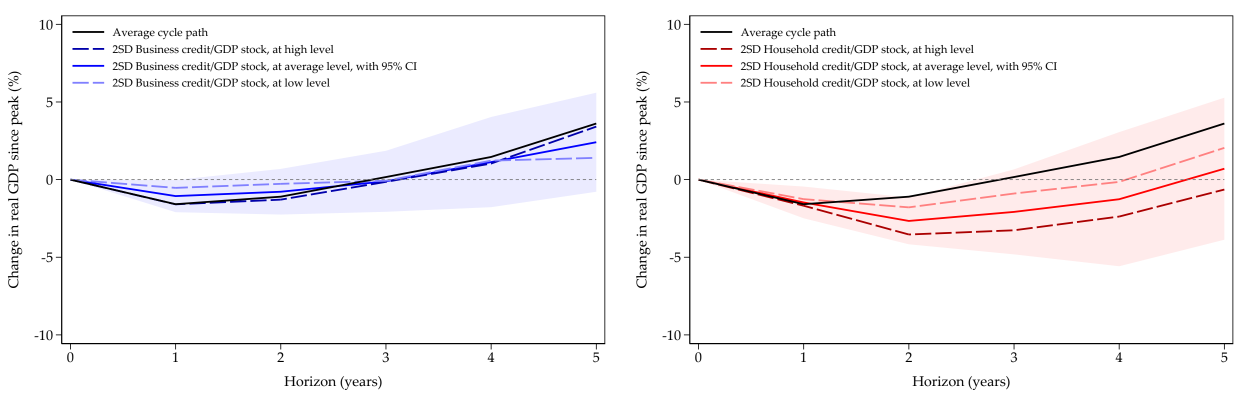
Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Table A.15: *Unconditional*

	(1)	(2)	(3)	(4)	(5)
	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
Average cycle	-1.70**	-0.91**	0.80**	2.55**	4.40**
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Business credit/GDP expansion $\Delta_5 x^B_{it(p)}$	0.13	-1.31	-2.85	-2.27	-1.48
	(1.65)	(1.68)	(2.58)	(3.55)	(5.87)
Household credit/GDP expansion $\Delta_5 x^H_{it(p)}$	0.43	-18.04**	-26.57**	-29.97**	-35.96**
	(2.54)	(3.80)	(5.32)	(8.13)	(8.78)
$\beta^B_h = \beta^H_h$ (p-value)	0.929	0.001	0.003	0.012	0.009
R^2	0.00	0.11	0.11	0.09	0.10
Cycles	158	158	158	158	157

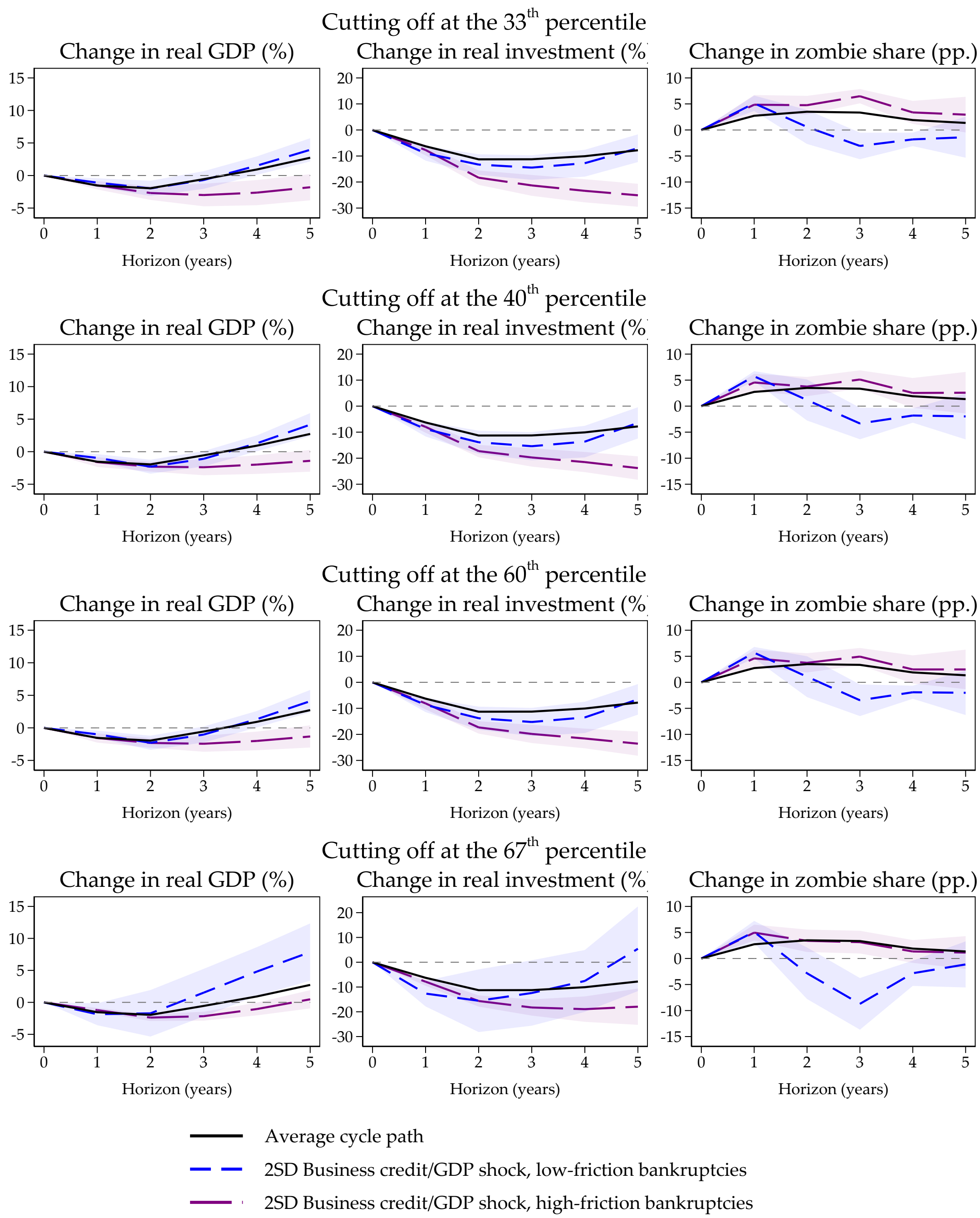
Within-estimator, standard errors clustered on countries in parentheses.
** $p < 0.01$, * $p < 0.05$.

Figure A.8: *Interacting expansions and levels*



Notes: The figure visualizes the effects of credit expansions when interacted with credit-to-GDP levels. It plots predictive effects on growth of a two-SD credit expansion in the five years preceding the recession when at the business cycle peak credit-to-GDP levels stand at i) country-specific historical averages, ii) one standard deviation above country-specific averages or iii) one standard deviation below country-specific averages. The usual set of controls are included. To make sure that credit level trends do not spuriously drive estimates, we include a linear time trend for the pre-WW2 period and—to account for the structural break on credit-to-GDP series—a dummy and separate time trend for the post-WW2 period. Estimates based on all business cycles in 18 advanced economies since 1870. Standard errors are clustered at the country level.

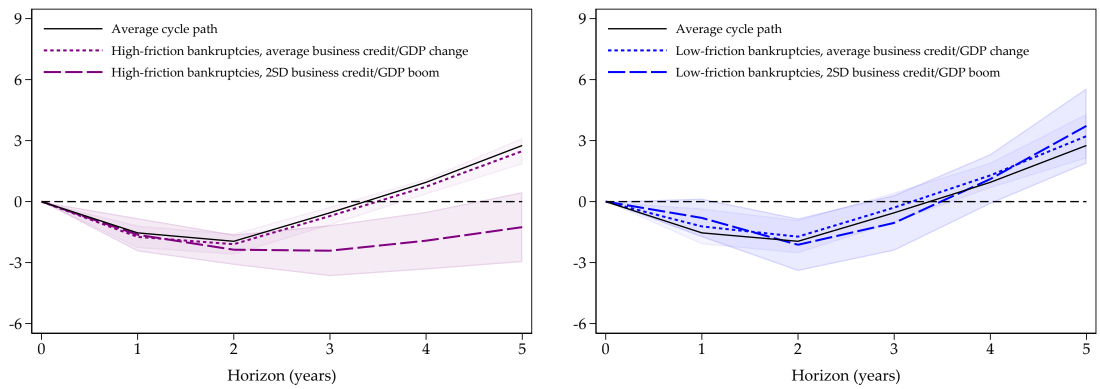
Figure A.9: *Using alternative threshold percentiles to define "high-friction" observations*



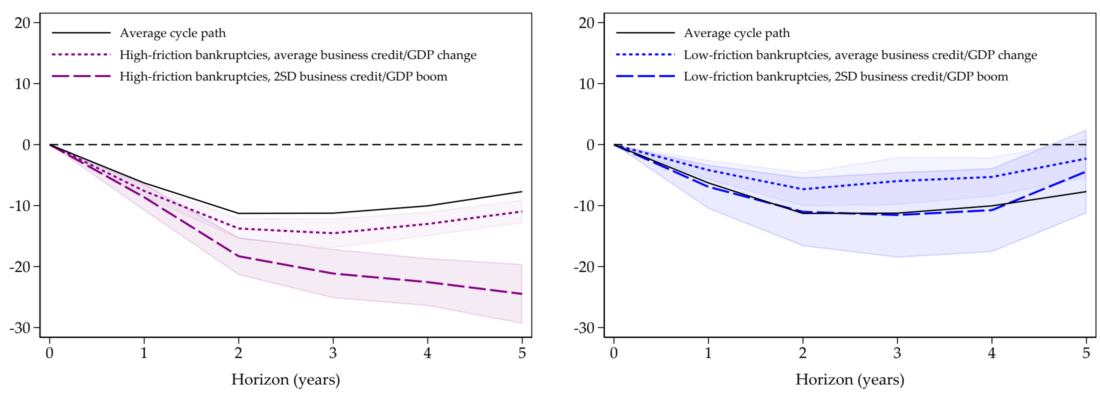
Notes: Predictions from a 2-SD expansion in business credit/GDP in the five years preceding a recession under legal regimes posing many frictions to bankruptcy and restructuring procedures ($L_{it} = 0$) and low-friction regimes ($L_{it} = 1$), respectively. All other covariates are at their country-specific sample averages. All estimates are conditional on the same set of controls detailed in the text. Estimates for zombie shares further condition on the level and the annual change of the zombie share at business cycle peak. Standard errors are clustered at the country level. Shaded areas denote the 90% confidence interval.

Figure A.10: *Effects of legal frictions with and without business credit booms*

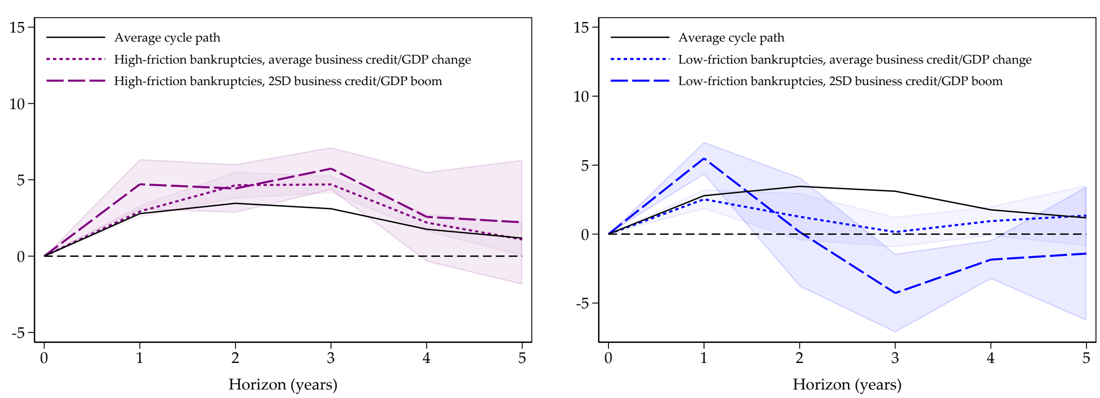
(a) *Change in real GDP (%)*



(b) *Change in real investment (%)*



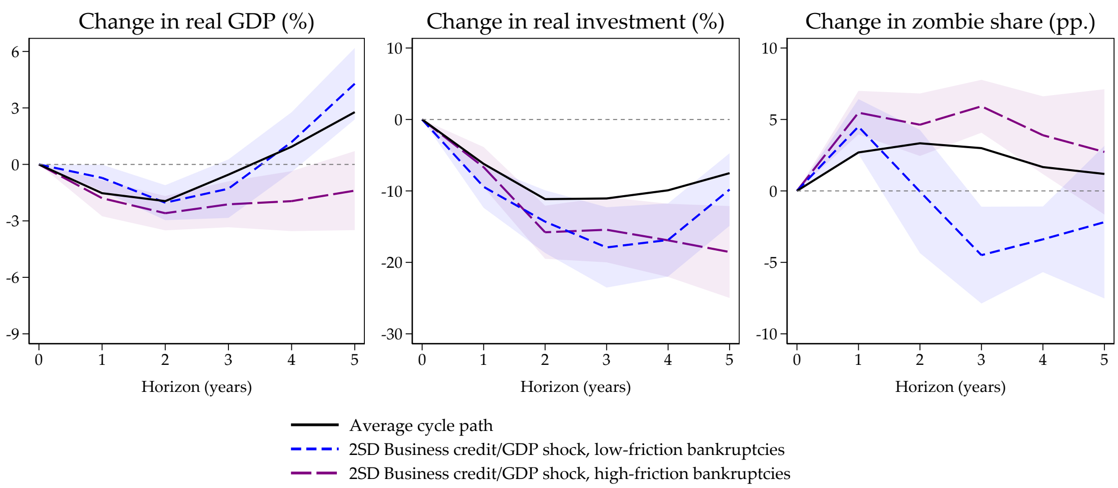
(c) *Change in the zombie share (pp.)*



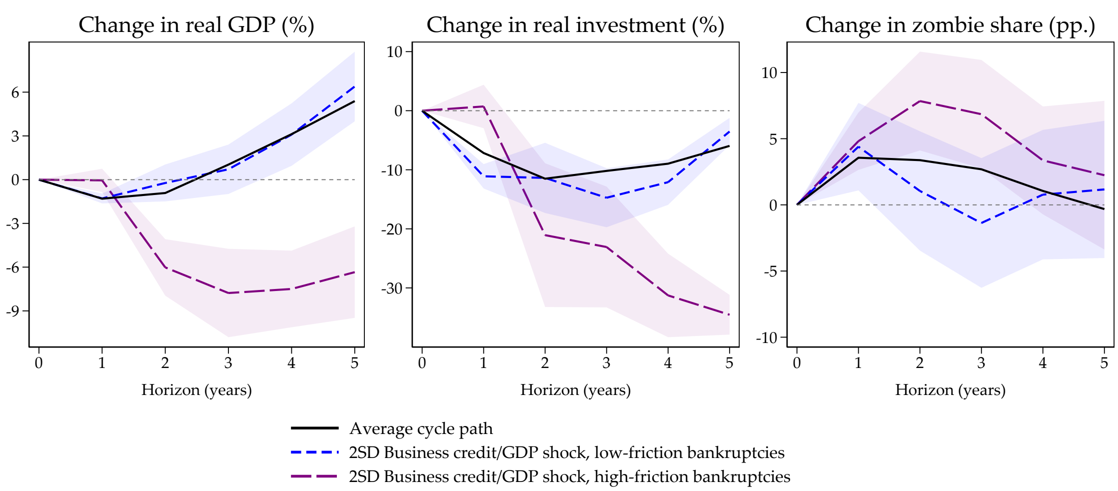
Notes: The figure shows predictions for real GDP, real Investment and the aggregate zombie share (from Banerjee and Hofmann, 2018) under high and low legal frictions, with and without business credit/GDP booms. Conditional country fixed effects and on the same set of controls as detailed in the main text. 90% CIs shaded based on standard errors clustered on countries.

Figure A.11: *Controlling for possible confounders of bankruptcy frictions*

(a) *Controlling for levels and interactions of private credit market development*



(b) *Controlling for levels and interactions of quality of financial sector regulation*



Notes: The figure shows predictions for real GDP, real Investment and the aggregate zombie share (from Banerjee and Hofmann, 2018) under high and low legal frictions, adding controls in levels and as interaction with business credit booms. Conditional country fixed effects and on the same set of controls as detailed in the main text, except a very small sample ($N = 21$) do not allow predictions in the right panel of (b) to include country fixed effects and macroeconomic controls. 90% CIs shaded based on standard errors clustered on countries.

H. Business credit data

For parts of the post-WW2 sample, we can draw on financial accounts data of the OECD and Eurostat databases and individual publications such as Bonci and Coletta (2012) for Italy, Roe (1971) and Office for National Statistics (2016) for U.K. data, Deutsche Bundesbank (1983) and Deutsche Bundesbank (1994) for German data. All postwar U.S. data are from the Fed's Flow of Funds.

In addition, we rely on comprehensive measures of business credit provided by the "Total credit database" assembled by the Bank of International Settlements (BIS). These include secured and unsecured debt obligations of all maturities and from all types of lenders in addition to conventional bank lending contracts. For methodological details see Dembiermont, Drehmann, and Muksakunratana (2013).

For earlier years, we proxy credit growth using data on bank lending to the non-financial business sector. In addition, we extend the business lending series of Jordà, Schularick, and Taylor (2017) to obtain data for the 19^{th} and the first half of the 20^{th}. We fill post-WW2 gaps with data kindly provided by Müller (2018).

For the pre-WW2 period, we calculate bank credit to the non-financial business sector based on the assets of specialized commercial banks, providing loans to business and other corporate financing. For example in the case of Germany, we sum credit extended to non-banks by joint-stock industrial banks as well as commercial credit unions. Where the banking sector is more diversified, we exploit that the bulk of pre-WW2 household loans were mortgages and obtain business credit as the residual to total private credit. Here, we can rely on Jordà, Schularick, and Taylor (2017) for necessary data on residential mortgages and total credit. We list all sources in detail below.

Table A.16 presents summary statistics of business credit and of household credit for comparison.

Table A.16: *Summary statistics for full sample of annual data*

	N	Mean	SD	SD resid.	Min	P10	P90	Max
Business debt/GDP, 5-year change	1286	0.04	0.16	0.15	-0.63	-0.15	0.22	0.62
Household credit/GDP, 5-year change	1218	0.04	0.08	0.07	-0.35	-0.03	0.13	0.44
Business debt/GDP	1373	0.86	0.37	0.19	0.12	0.44	1.40	2.14
Household credit/GDP	1313	0.35	0.26	0.11	0.00	0.03	0.72	1.21

Notes: SD resid denotes residual standard deviation after controlling for country fixed effects and country-specific linear time trends.

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