The Causal Effects of the Darker Side of Financial Development*

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Abstract: We study the causal implications of financial deepening for economic development and financial crises, adopting a heterogeneous difference-in-difference framework. Using data for the past six decades we demonstrate that very high levels of finance, proxied by credit/GDP, are neither associated with lower long-run growth nor with higher short-run propensity of banking crises due to 'credit booms gone bust' cycles or unfettered capital inflows. When we investigate 'too much finance' at intermediate levels of credit/GDP we find increased crisis propensity due to capital inflows and commodity price movements, but, again, no detrimental long-run growth effects for these (emerging) economies.

Keywords: financial development, economic growth, financial crises, difference-in-difference, interactive fixed effects, heterogeneous treatment effects

JEL codes: F43, G01, G21, O40

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1 Introduction

Following decades of concerns over identification and inference in the 'econometrics club' (e.g. Leamer 1983, Angrist & Pischke 2008) we are presently in the foothills of a *heterogeneity revolution*, which seeks to increase the policy-relevance of empirical insights by tying the analysis closer to subgroups of individuals, firms, or countries. This development is implicit in the debate surrounding heterogeneous treatment effects in difference-in-difference approaches for microeconomic analysis (De Chaisemartin & d'Haultfoeuille 2020, Athey & Imbens 2022, Goodman-Bacon 2022), while unsatisfactory policy-insights from 'pooled' models have already led to alternative approaches in diverse areas of international macro and political economy, including work on the trade gravity model (Baier et al. 2018), international migration (Bertoli & Moraga 2013), debt and growth (Eberhardt & Presbitero 2015), banking crises (Summers 2017), macro productivity evolution (De Visscher et al. 2020), and the democracy-growth nexus (Boese & Eberhardt 2021).

In this paper we gain valuable new insights by taking a *heterogeneous* treatment approach to the analysis of financial deepening, long-run economic growth and financial crises. We focus on the 'new consensus' in the literature of a more complex link between finance and growth which has given rise to findings of 'too much finance'. We model country-experience of 'high' levels of finance as an endogenous binary treatment and estimate heterogeneous treatment effects in a factor-augmented difference-in-difference model, which controls for selection into treatment and differential pre-treatment trends between treated and control countries. We present our results relative to the 'years of treatment', focusing on the *long-run* relationship. Motivated by descriptive analysis we apply this strategy to countries near the top of the credit/GDP distribution (henceforth 'advanced country sample'),[1] and separately to countries at intermediate levels (henceforth 'developing country sample')[2] to reveal whether 'too much finance' can apply at, broadly speaking, different

[1]We adopt two cut-offs, 92% and 119% of credit/GDP, equivalent to the 90th and the 95th percentile of the full sample distribution (all countries, all years) — in Appendix B we demonstrate the robustness of our main results for alternative cut-offs.

[2]We focus on countries which have crossed the 34% or 47% credit/GDP threshold (50th and 60th percentiles of the full sample distribution). These are primarily middle income economies while the countries at the top of the distribution are virtually all advanced economies.

levels of development. In order to speak to the most recent literature highlighting more granular components of credit we employ the same methodology to study 'too much household credit' and 'too much corporate credit' in a sample of advanced and emerging economies. We then adapt our methodology to an early warning system approach for banking crises: we test whether elevated levels of finance increase the *within-country* effect of what are widely regarded as the dominant *short-term* triggers for banking crises, namely 'credit booms gone bust', excessive capital inflows, and, in the developing country sample, aggregate commodity price movements.

Our analysis in advanced economies finds no evidence for a diminishing long-run effect of high levels of finance on income per capita or increased financial vulnerability. This result holds if we study household credit and corporate credit instead of aggregate credit, with the caveat of very modest sample sizes for treated countries. In contrast, developing countries are subject to an amplified effect of large capital inflows and aggregate commodity price movements on banking crisis propensity when experiencing high levels of finance. However, this increased vulnerability does not appear to undermine their long-run growth prospects.

The link between finance and growth has been studied extensively[3] and the various beneficial aspects of finance for development are well-known (e.g. Schumpeter 1912, Greenwood & Jovanovic 1990, Beck, Levine & Loayza 2000, Levine et al. 2000, Levine 2005), also for less-developed countries (Beck et al. 2004, Gambacorta et al. 2014), although there is no consensus whether these or advanced economies benefit more (Deidda 2006, Loayza et al. 2018). On the 'darker side' of financial development (Loayza et al. 2018), back in focus among academics and policy-makers following the 2007/8 Global Financial Crisis (GFC), it potentially crowds out productive activity (Rioja & Valev 2004, Law & Singh 2014, Arcand et al. 2015)[4] and/or increases susceptibility to financial crises (Demirgüç-Kunt & Detragiache 1998, Kaminsky & Reinhart 1999, Loayza & Rancière 2006, Rancière et al. 2006). Carré & L'Œillet (2018) speak of a 'paradigm shift' whereby a pre-GFC consensus

[3]Comprehensive surveys are available in Levine (2005), Carré & L'Œillet (2018), Loayza et al. (2018), and Popov (2018).

[4]'Excessive' financial deepening may advance services with lower growth potential (e.g. household rather than firm credit, see Beck et al. 2009, Jordà et al. 2015, Sufi & Taylor 2021, Müller & Verner 2021), and/or a human capital 'brain drain' to vacuous but highly-paid finance jobs away from the pursuit of real economy activity and/or its innovation (Popov 2018).

of a strictly positive and linear relationship between finance and growth has more recently been replaced by a new consensus of a more complex, likely concave relationship. The financial crisis literature has always recognised that asset price growth and credit expansion play a key role (Kindleberger 1978). The renewed interest following the GFC fostered the creation of long time series data, which have helped consolidate the primary significance of credit and asset price growth for financial crisis prediction (Bordo & Meissner 2016, Sufi & Taylor 2021): First-order factors predicting banking crises in advanced economies are 'credit booms gone bust' (Jordà et al. 2011, Schularick & Taylor 2012, Müller & Verner 2021) and 'excessive' capital inflows (Reinhart & Rogoff 2013, Ghosh et al. 2014, Caballero 2016); in low-income economies dominant triggers include aggregate commodity price movements (Eberhardt & Presbitero 2021).

Few studies investigate growth and vulnerability in an integrated approach (Arcand et al. 2015, Rancière et al. 2006),[5] given that they address very different *timings* of effects: the link between finance and development should be viewed over the long-term (Loayza & Rancière 2006), while the analysis of banking crises adopts specifications which investigate the *'trigger'* function of various phenomena (credit growth, capital inflows, or commodity terms of trade) in an 'early warning system' (EWS) approach focused on the short-run (e.g. Bussière & Fratzscher 2006).

Our empirical approach adopts a treatment effects framework following Rancière et al. (2006), but in contrast to these authors we do not focus on the *overall* effect of finance on economic performance but investigate growth and crisis vulnerability separately. There are at least two sound reasons for this separation: (i) we are able to employ factor-augmented heterogeneous difference-in-difference (growth) and EWS (crisis) models which allow us to get closer to a causal interpretation of the results; and (ii) our specifications can speak to the long-run vs short-run effect

[5] Arcand et al. (2015) adopt a 'reduced form' approach whereby their finance-growth model (levels and squared credit/GDP terms) is augmented with a crisis dummy (negative significant) alongside interaction terms (insignificant for both levels and squared credit/GDP). Rancière et al. (2006), whose analysis of course pre-dates the 'too much finance' debate, adopt a more 'structural' approach: in a first step they model financial crises in a probit model, while the second-step equation for per capita GDP growth incorporates financial liberalisation, a crisis dummy along with the estimated hazard rates from the first step equation. They show that finance is positive and significant in both equations, their decomposition however suggests that the former dominates substantially, by an order of between five-to-one and seven-to-one.

of 'too much finance' in the growth and crisis analysis, respectively.

Our paper makes a number of contributions to the literature: we investigate the potential nonlinearity of the finance-growth nexus in a *heterogeneous* parameter framework, where each country has its own equilibrium relationship. Although there is an earlier literature which employed time series (e.g. Arestis & Demetriades 1997) or panel time series (e.g. Christopoulos & Tsionas 2004) methods, these were carried out within the confines of a linear finance-growth nexus and furthermore relied on weaker concepts of causality. We adopt a difference-in-difference setup which allows us, under reasonable assumptions, to get closer to causal identification without resorting to internal or external instrumentation. The Chan & Kwok (2022) Principal Component Difference-in-Difference (PCDID) estimator is a recent contribution to the literature on treatment estimators adopting a multi-factor error structure (Gobillon & Magnac 2016, Xu 2017). The PCDID augments the estimation equation for each treated country with common factors estimated from the control group of countries which remained below the treatment cut-off. These factors enable us to account for both selection into treatment (endogeneity of 'high' financial development), as well as non-parallel trends between treated and untreated countries.[6] When focusing on potentially attenuated growth effects of 'too much finance' it is self-evidently important whether a country spent one or thirty years above some suitable threshold. Our approach enables us to model this length *in treatment* while by-passing the concerns debated in the microeconometric literature cited at the top of this paper. Finally, we extend the heterogeneous treatment approach to the study of banking crises in a simple but intuitive way. Our approach is novel because we are among the first to employ a heterogeneous crisis model (the only study we are aware of is Summers 2017) based on factor-augmented implementations for the generalised linear model (Boneva & Linton 2017) and combine this with the PCDID setup of our growth analysis.

[6]We also provide results based on restricted control groups: we decimate the control sample by requiring that countries at least have to have exceeded 20%, 26%, 34% or 47% credit/GDP. The intuition is that the economic implications of 'too much finance' in a highly (financially and economically) developed economy (e.g. Australia) should not be benchmarked against those in an economy with significantly underdeveloped financial institutions (e.g. Mali). Curtailing the control sample moves the counterfactual closer to the treated sample in terms of shared characteristics.

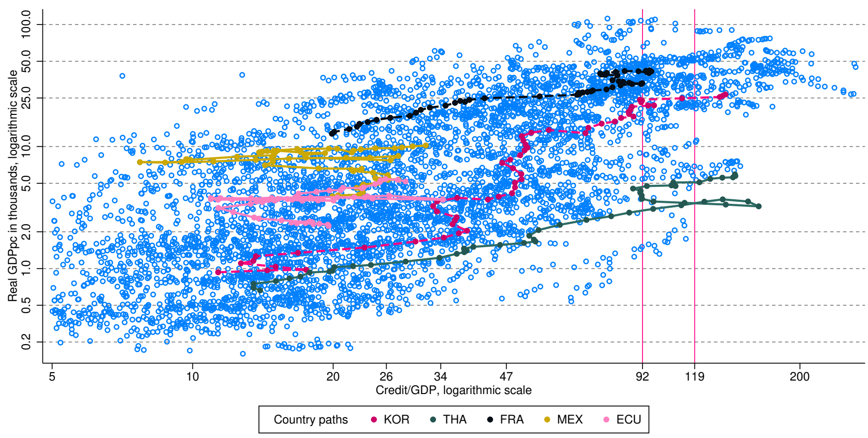
The remainder of this paper is structured as follows: in the next section we take a first look at the data, providing a motivation for the analysis of 'too much finance' not just at the top of the credit/GDP distribution, but also at intermediate levels. In Section 3 we study the finance-growth nexus, also in its implications for capital accumulation and TFP growth in developing countries, Section 4 turns to the investigation of banking crises. In both sections we first introduce the data and methods used and then present empirical findings. Section 5 concludes.

2 Stylised Facts

This section uses descriptive analysis to highlight the widely-discussed 'too much finance' nonlinearity for countries near the top of the credit/GDP distribution and an under-appreciated empirical fact: a similar pattern for countries at intermediate levels of the credit/GDP distribution.[7]

Figure 1 provides a simple scatter for real income per capita (in logs) and the credit/GDP ratio (in logs), which clearly shows a positive correlation, although this is not self-evident once we look at individual country experiences. Naturally, this correlation does not speak to the direction of causation. The vertical lines mark the 'thresholds' for 'too much finance' we adopt throughout our analysis: the 90th percentile of the credit/GDP distribution (92%) and the 95th percentile (119%).

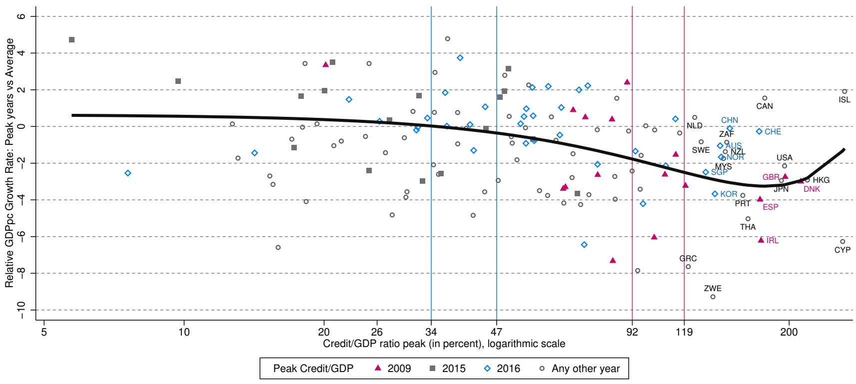
Figure 1: Financial Development and Economic Performance



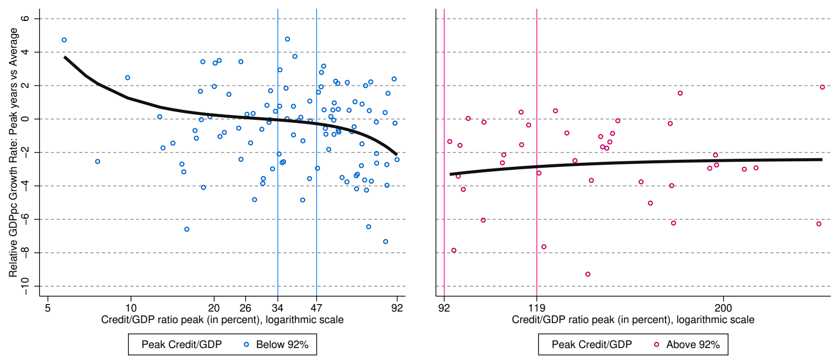
Notes: This is a scatter plot for log real GDP pc (in thousands of US$) and log of credit/GDP.

[7]Our sample for 152 countries has just under 6,000 observations — see Section 3.1 for details.

Figure 2: Peak Credit/GDP and Relative Growth Performance



(a) Country relative GDPpc growth rate and Peak Credit/GDP level



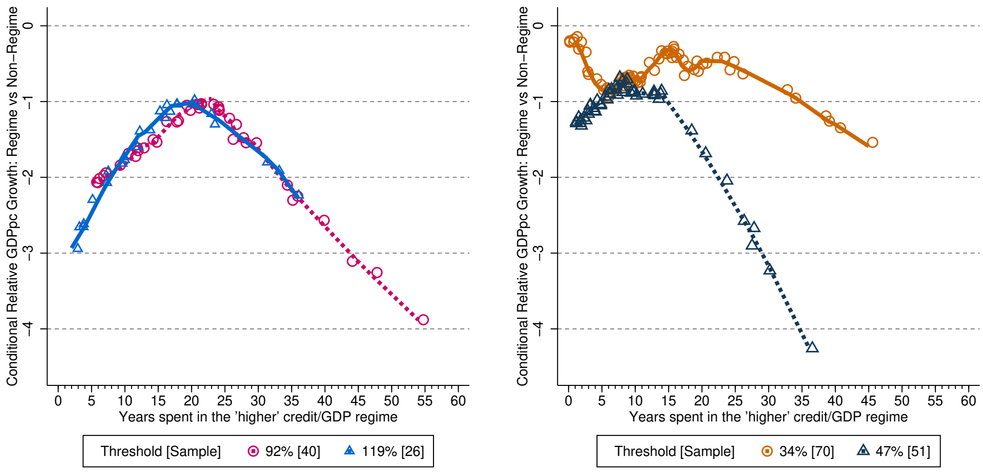
(b) Country relative GDPpc growth rate and Peak Credit/GDP level – differentiated

Notes: This plot shows the log of peak credit/GDP (x-axis) and 'relative' growth performance: average growth for the five years around the peak relative to the average in 'non-peak' years. The black lines are fitted fractional polynomial regression lines.

In the upper panel of Figure 2 we study the *relative* growth performance of countries in relation to their credit/GDP peak: we adopt the ratio of average per capita GDP growth in the five years around the peak to the average for all other years. Here the finance-growth nexus, in form of a fractional polynomial regression line, looks distinctly rotten: countries which peaked with credit/GDP ratios over 34% experienced *negative* relative growth, and those at the top-end of the distribution had on average *3% lower growth rates* than in non-peak times. There are of course many problems with this interpretation (e.g. secular growth slowdown in high-income, financially-developed economies) but we can use this descriptive analysis to pinpoint an important insight, highlighted in the lower panel of the figure, where we split the sample and regression lines into those countries below and above 92% credit/GDP:

if a simple plot like that in Figure 2 (a) is used to motivate the study of 'too much finance' *at the top of the credit/GDP distribution*, then the plot on the left in Figure 2 (b) suggests that we should also study this relationship *at intermediate levels of financial development* (credit/GDP thresholds around 34% or 47%).

Figure 3: Treatment Length and Relative Growth Performance



Notes: We present predictions from multivariate running line regressions of average GDPpc growth above the specified 'too much finance' threshold relative to the average below (y-variable) regressed on years spent above the threshold (x-variable), further conditioning on per capita GDP level in the year of crossing the threshold. The country-specific predictions are minimally perturbed to aid illustration (sample sizes in square brackets). Virtually all estimates are statistically significantly different from zero (not highlighted in the plots for ease of illustration).

A final set of graphs in Figure 3 focuses on the notion that if 'too much finance' affects growth *in the long-run*, then it should matter how many years a country spends in the 'danger zone': we should see a deterioration of the growth performance, the longer countries have spent above some 'too much finance' threshold. In these figures we subtract the average growth in those years not above the threshold from that during the above-threshold years and plot this difference against the number of years spent above the threshold — instead of a scatter we show the predictions from the multivariate running line regression which controls for per capita GDP level in the year the country crossed the threshold as well as a dummy for that year.

Once again, this analysis is simplistic as we cannot account for the passing of time or aspects of convergence, so we put more emphasis on the *shape* of the predicted regression lines: in the advanced country sample in panel (a) this points to an inverted-U shape, which suggests that adopting a 92% or 119% credit/GDP threshold countries first experience an improvement of their economic prospects, but eventually see their fortunes decline. In the developing country sample in panel (b) the 34% credit/GDP threshold portrays a less straightforward picture, whereas the 47% threshold repeats the inverted-U patterns of the advanced country sample.

We close with a brief analysis of the 'dominant narratives' for banking crisis prediction to demonstrate that these can be traced in the raw data. In Appendix Figure A-1 we present event analysis plots for per capita GDP growth, change in credit/GDP, change in the gross capital inflows/GDP, and change in gross fixed capital formation/GDP in the run-up and aftermath of banking crises. We produce these plots for the 'full sample' in Panel (a), for the sample of countries which exceeded the 92% credit/GDP threshold in Panel (b), and for those which had peak credit/GDP between 47% and 92% in Panel (c). Real GDP growth does not show any statistically significant patterns prior to the crisis date, although it drops over 3% below trend in the aftermath. The 'credit boom gone bust' narrative comes out clearly in all three sample. Investment share of GDP is slightly elevated two years prior to the crisis in the two 'restricted' samples but not in the full sample. Finally, the change in gross capital inflows/GDP shows elevated levels two years prior to the crisis onset in full and 'advanced country' samples, while in the 'developing country' sample there is an effect one year before the crisis. This points to a capital flow bonanza narrative, while only the 'advanced country' sample gives an indication of substantial decline in capital inflows after the crisis.

3 Financial Development and Growth

In this section we study the long-run implications of high levels of finance on economic prosperity. Our sample contains a mix of developing and developed economies, and spans 1960 to 2016. We employ a Difference-in-Difference (PCDID) method developed by Chan & Kwok (2022), modelling 'too much finance' as an endogenous treatment using alternative credit/GDP thresholds. Our results are presented with the aid of multivariate running line regressions, which allow us to plot the treatment effect of 'high' financial development against the years spent in this 'high' regime, while conditioning on country-specific data coverage (start year), minimum credit/GDP level, and 'regime dynamics' (number of times the country crosses the threshold). In the following, we describe our data and methodology (Section 3.1), present results for top percentiles (3.2) and intermediate levels of financial development (3.3). We conclude with an exploratory analysis of disaggregated credit data (households versus corporate) in a moderate sample of advanced and emerging economies (3.4).

3.1 Data and Methodology

Data and Transformations The literature studying the causal link between finance and growth (initiated by King & Levine 1993, Levine et al. 2000) adopts three main proxies for financial development: (i) private credit to GDP; (ii) liquid liabilities to GDP; and (iii) commercial bank assets relative to commercial bank plus central bank assets. Measures (i) and (ii) cover the activities of all financial intermediaries scaled by the size of the economy, while the third measure proxies the extent to which the government captures the financial activities in the economy relative to deposit taking institutions. Empirical research has stressed the growing importance of the non-bank financial intermediaries, particularly market financing (Levine & Zervos 1998) and measures (i) and (ii) relate to this growing segment. We follow Arcand et al. (2015) in adopting credit/GDP as our indicator for financial development, as it best captures financial activity as opposed to the size of the financial system (liquid liabilities) and furthermore provides the best data coverage.

We take 'private credit by deposit money banks and other financial institutions to GDP' from the July 2018 version of the *Financial Development and Structure Dataset* (FSFD; Beck, Demirgüç Kunt & Levine 2000, Beck et al. 2009). Our dependent variable, real GDP per capita in 2005 US$ values, as well as additional controls (inflation, average years of educational attainment in the population aged 25 and above, and trade as a share of GDP) are taken from the World Bank *World Development Indicators* — all of these are log-transformed and the income variable is multiplied by 100: our treatment estimates provide the percentage effect of 'high' finance (see below for definitions) on per capita income. The parsimonious choice of controls is selected on the basis of the existing literature (Beck, Demirgüç Kunt & Levine 2000, Arcand et al. 2015): openness to trade and inflation. In robustness checks we effectively estimate production functions augmented with a 'too much finance' dummy, using Penn World Table (Feenstra et al. 2015, PWT v. 10) data — see Appendix C. Following some restrictions on minimal number of observations[8] the full sample covers close to 6,000 observations in 152 countries. Appendix Table A-1 offers detailed information on our treatment and control sample make-up alongside descriptive information on income per capita and financial development.

In additional analysis we use quarterly data from the Bank of International Settlements (BIS) for 1991Q1 to 2018Q3 to disaggregate credit to the non-financial sector into 'household credit' and 'firm (non-financial corporation) credit'. These data are compiled for 44 countries, though the availability of per capita GDP and the inclusion of control variables reduce this to 41 countries. Our income variable is constructed from nominal GDP data, CPI data (benchmark year 2010) and local currency to US$ exchange rates averaged for the benchmark year (all quarterly) along with annual population data (interpolated to cover quarterly frequency) from the IMF IFS. Quarterly data on inflation (change in CPI) is from the same source, from the IMF Direction of Trade (DOT) dataset we obtain quarterly data for imports and exports to construct the export/trade control variable. Appendix Figure D-1 shows a simple scatterplot for the two credit ratios with per capita GDP, the sample makeup is reported in Appendix Table D-1.

Regime Thresholds For our main results we adopt the 90th and 95th percentiles of the credit/GDP variable in the full 152-country sample as 'thresholds' for a 'high' financial development regime. These cut-offs, equivalent to 92% and 119% of credit/GDP, are similar to the 88% threshold found by Law & Singh (2014) and the 100% found by Arcand et al. (2015). For these two threshold we observe 38 and 24 treated countries, respectively. We refer to these samples and related analysis as pertaining to 'advanced countries'.[9] In the analysis of intermediate levels

[8]We require each country to have at least 14 observations. This excludes 115 observations for 15 countries (such as Afghanistan, Equatorial Guinea, Iraq, Lao, Libya, and Zambia).

[9]Over 80% (79%) of observations in the treated sample using the 90th (95th) percentile cut-off are for high-income countries, seven (four) are middle-income countries, Zimbabwe (one observation above either threshold) is the sole low-income country. See Appendix Table A-1 for details.

of the credit/GDP variable we select the 60th (34% credit/GDP) and 70th (47%) percentiles of the full 152-country sample. We choose these cut-offs on the basis of our above discussion of Figure 2. As we want to exclude economies like Singapore, which evolved from 33% to 132% credit/GDP, from this 'intermediate-level' sample, we impose percentile *ranges* for our treated samples: 60th to 70th or 60th to 80th percentiles, alongside 70th to 80th or 70th to 90th percentiles — the narrower ranges capture 18 and 26 countries for the respective cut-offs, the wider ones 42 for the 34% and 47 countries for the 47% cut-off. We refer to these samples and related analysis as pertaining to 'developing countries'.[10] We detail the adopted regime thresholds for our analysis using quarterly data on household versus firm credit in Section 3.4.

Threshold PCDID We estimate country regressions for treated countries only, but augment each country-regression with common factors estimated from the residuals of the same regression model *in the control sample*. Using potential outcomes, the observed outcome of treatment D_{it} for panel unit i at time T_0 can be written as

$$y_{it} = D_{it}y_{it}(0) + (1 - D_{it})y_{it}(1) = \Delta_{it}\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_0\}} + y_{it}(0) \qquad (1)$$

$$\text{with} \qquad y_{it}(0) = \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \widetilde{\epsilon}_{it}, \qquad (2)$$

where the first and second indicator variables $\mathbf{1}_{\{.\}}$ are for the treated panel unit and time period, respectively, Δ_{it} is the time-varying heterogeneous treatment effect, x is a vector of observed control variables with associated country-specific parameters β_i,[11] $\mu_i' f_t$ represents a set of unobserved common factors f_t (which can be nonstationary) with country-specific factor loadings μ_i, and $\widetilde{\epsilon}_{it}$ is the error term.

The treatment effect is assumed to decompose into $\Delta_{it} = \overline{\Delta}_i + \widetilde{\Delta}_{it}$, with $E(\widetilde{\Delta}_{it}|t > T_0) = 0 \;\forall i \in E$ since $\widetilde{\Delta}_{it}$ is the demeaned, time-varying idiosyncratic component of Δ_{it}; we refer to $\overline{\Delta}_i$ as ITET, the treatment effect of unit i averaged over the treatment period — our parameter of interest. The reduced form model is

$$y_{it} = \overline{\Delta}_i\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_0\}} + \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \epsilon_{it}, \qquad (3)$$

[10] Over 80% (63%) of observations in the 60th (70th) percentile cut-offs are for middle-income economies, the remainder largely for former transition economies.

[11] We assume $\beta_i = \bar{\beta} + \tilde{\beta}_i$ with $E(\tilde{\beta}_i) = 0$ (Pesaran, 2006). x can be a function of f.

with $\epsilon_{it} = \widetilde{\epsilon}_{it} + \widetilde{\Delta}_{it}\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_0\}}$. Given the treatment effect decomposition ϵ_{it} has zero mean but may be heteroskedastic and/or weakly dependent.

The factor structure has a long tradition in the panel time series literature to capture strong cross-section dependence (Pesaran 2006, Bai 2009), a form of unobserved, time-varying heterogeneity. Strong correlation across panel members is distinct from weaker forms of dependence, such as spatial correlation, and if ignored can lead to serious (omitted variable) bias in the estimated coefficients on observable variables (Phillips & Sul 2003, Andrews 2005). Here, the combination of common factors and heterogeneous parameters also allows for potentially non-parallel trends across panel units, most importantly between treated and control units. The above setup can further accommodate endogeneity of treatment D_{it} in the form of *inter alia* correlation between treated units and factor loadings, the timing of treatment and factor loadings, or between observed covariates and timing or units of treatment.

The estimation of the country-specific treatment effect (ITET) $\overline{\Delta}_i$ proceeds in two steps: first, using Principal Component Analysis (PCA), we estimate proxies of the unobserved common factors from data in the control group (details below); second, country-specific least squares regressions of treated countries are augmented with these factor proxies as additional covariates. We experiment with the make-up of the control sample based on 'peak' credit/GDP values: countries for which financial development peaked close(r) to the 'high' threshold studied are more plausible counterfactual cases than countries with very low peak levels.

The main identifying assumptions are that all unobserved determinants of GDP per capita are captured by the factors, a standard assumption in the panel time series literature (Pesaran 2006, Bai 2009) and related causal panel models (Athey & Imbens 2022). It is further assumed that conditional on the estimated factors the control variable x are jointly insignificant predictors for the treatment.[12] Since the factors are estimated with error, there is potential correlation between the errors of treated and control countries, which will bias the treatment estimate. This bias can be removed if we require that $\sqrt{T}/N_c \rightarrow 0$, where T is the time series dimension and N_c the number of control countries.

[12]We carry out Wald tests for this assumption — see Appendix Tables B-1 to B-3.

The estimation equation for each treated country $i \in E$ is then:

$$y_{it} = b_{0i} + \delta_i \mathbf{1}_{\{t>T_0\}} + a_i' \hat{f}_t + b_{1i}' x_{it} + u_{it}, \tag{4}$$

where $\hat{f}$ are the estimated factors obtained by PCA on the residuals $\hat{e}$ from the heterogeneous regression of $y_{it} = b_{0i} + b_{1i}' x_{it} + e_{it}$ in the control group sample, and δ_i is the country-specific parameter of interest. We estimate (4) augmented with one to six common factors. The average treatment effect (ATET, $\hat{\delta}^{MG}$) is simply the average of the country estimates $\hat{\delta}_i$. We follow the practice in the literature and use the robust mean group estimate (Hamilton 1992) with the associated standard errors based on $\Sigma^{MG} = (N-1)^{-1} \sum_i (\hat{\delta}_i - \hat{\delta}^{MG})$ (Pesaran 2006).

Conditional Local Mean Results The standard approach in the treatment effects literature is to report the ATET, $\hat{\delta}^{MG}$. However, this ignores the length of time a country has spent in the higher regime — for some countries, e.g. Zimbabwe, only a single observation is above the threshold — and furthermore does not account for country data characteristics in an unbalanced panel or the 'regime dynamics'.

Below we follow the practice introduced in Boese & Eberhardt (2021) and adopt a multivariate smoothing procedure for the country estimates: running line regressions (Royston & Cox 2005), which are k nearest neighbour 'locally linear' regressions of the country treatment effect $\hat{\delta}_i$ on (i) the years in the higher regime, (ii) a dummy for the start year of the country series, (iii) the number of times the country crossed the threshold, and (iv) the country-specific minimum credit/GDP level. Our result plots present the evolution of the *predicted values* from this multivariate smoothing procedure[13] on the y-axis over the years in the higher regime on the x-axis. The associated standard errors are calculated based on the local weighted least squares fit and we highlight those local predictions for which the 90% confidence bound does (not) include zero with hollow (filled) markers.

Finally, the treatment effects graphs can be misleading if a few estimates in the right tail (countries with many years above the threshold) visually dominate the overall evolution of the relationship. In order to counter this impression we transform

[13] These are not the $\hat{\delta}_i$ but the smoothed predictions from a multivariate running line regression.

the 'years in regime' variable on the x-axis using the inverse hyperbolic sine (IHS): like a log transformation this stretches out low values and bunches up high values of treatment years, with the practical effect that the mean and median years spent in treatment are typically situated close to the *centre* of the plot.[14]

3.2 'Too much Finance'

In Figure 4 the upper panel presents results for the 92% credit/GDP threshold, the lower for the 119% threshold; ATET estimates for these specifications are presented in Appendix Table B-1.[15] These PCDID estimates show the causal effect of years spent in the 'higher' regime on per capita income relative to those countries which permanently stayed below the respective threshold. The different prediction lines are for the same treatment sample, but use different control samples: the orange line includes *any* country which stayed below 92% credit/GDP, the pink line excludes those control countries which always stayed below the 40th percentile, and so on. Similarly for the 119% threshold in the lower panel.[16]

There are three insights from the results in panel (a): first, the choice of control group clearly matters — when Angola or Chad are part of the control group to investigate the 'too much finance' hypothesis in Germany, France or the UK, we find the treatment effect trajectory is initially negative and at points statistically significant (control group lower cut-off from 0th, 40th or 50th percentile, orange, pink and blue lines), moving towards a positive insignificant value around the sample mean years in 'treatment'. When the control country sample is further restricted from below (from 60th or 70th percentile, all other coloured lines), creating arguably a closer match to the countries in the treatment sample, the treatment effect trajectories turn positive and significant. Second, if we focus on the mean (14.6) or

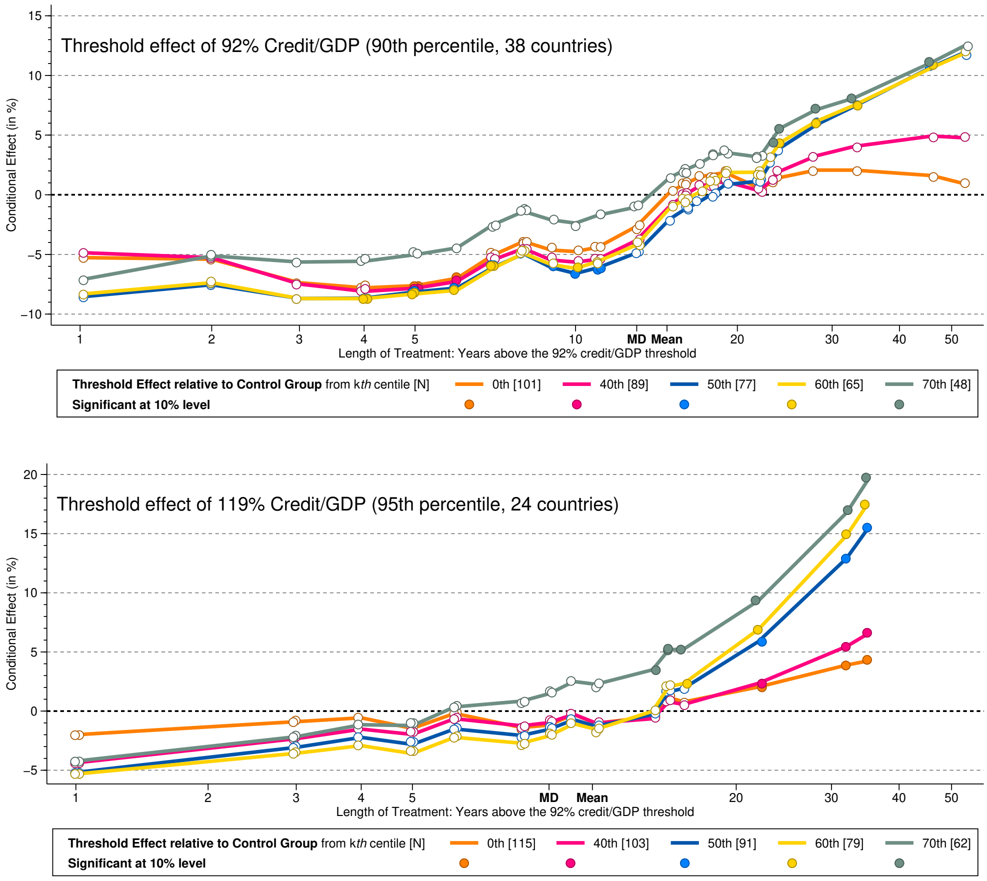
[14]This also distracts from the 'extremes' of the result plots (0-5 years or >30 years): these sections of our plots likely do not speak to the aim of studying the *long-run* effects of 'too much finance'. For the countries with few years in regime this is obvious; for those with many years the time spent in the lower regime is very small, so that the within-country difference is likely imprecise.

[15]In Appendix Figure B-1, Panel (a) we provide detailed robustness checks by varying the 'too much finance' threshold from 65% to 115% (k) of the credit/GDP level, specifying control groups that are below this cut-off but have at least breached $k-25$%. We cannot see anything approaching systematic negative effects for longer treatment in these results.

[16]Appendix Table B-1 reports the p-values for Wald tests in each specification: we regress the treatment dummy on the controls and one to six common factors, testing the hypothesis that the controls are jointly insignificant, which is the case in virtually all models.

median (13) years of treatment, all estimates across different control samples find a small negative, albeit statistically insignificant effect: for the average country 'too much finance' does not appear to benefit economic performance... but does no harm either. Third, countries which spend only a handful of years in the 'higher' regime appear to have negative treatment effects.[17]

Figure 4: Too much Finance? Running line presentation of PCDID results



Notes: Each plot investigates the prospect of 'too much finance' by studying the effect of being above the 90th or 95th percentile of the credit/GDP distribution. We consider restricted control group samples by dropping countries with very low financial development (below the 40th, 50th, 60th and 70th percentile of the credit/GDP distribution). The first plot (0th percentile), is for the unrestricted control group. A filled (hollow) marker indicates statistical (in)significance at the 10% level. Mean and median (MD) treatment lengths and control sample sizes (N) are also reported.

[17]With the exception of Zimbabwe, all of these represent events in the aftermath of the Global Financial Crisis, a clear sign of *short-run* economic contraction: six of the eight countries with five or fewer years of treatment have *negative* average GDP pc growth at the time they cross the threshold.

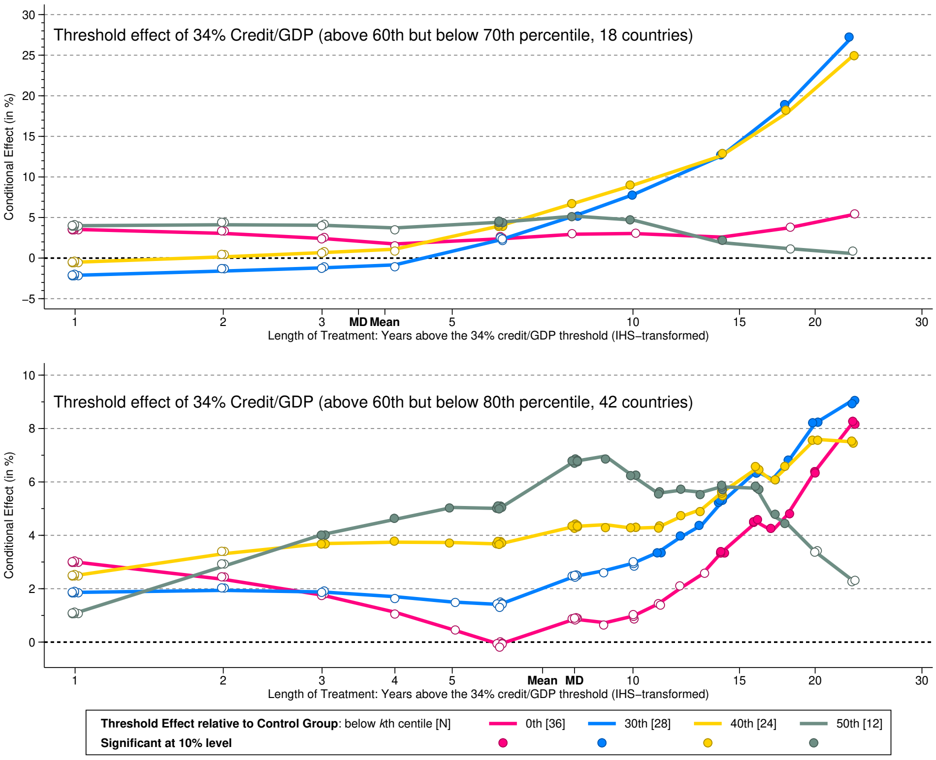
The analysis of the 119% credit/GDP threshold in the lower panel of the Figure provides similar evidence but with a strong divergence in the long-run between specifications with (relatively) indiscriminate control samples (orange and pink lines) and the more restricted control samples (all other coloured lines). For the latter, statistically significant treatment effects eventually reach around 15% higher per capita income after 30 years above the threshold, for the former the effect remains more modest but statistically significant. Predictions for countries with just a few years of treatment are again all negative, and as before none of the estimates below five years of treatment are statistically significant. The treatment effects for median and mean length of treatment measure are effectively zero.

In Appendix C we estimate treatment effects in a production function specification using PWT data for per capita GDP and capital stock: the inclusion of the latter is controversial, in that higher financial development should raise gross fixed capital formation, implying that the finance effect in a production function should be interpreted as *relative* investment efficiency. An alternative view could argue that finance should be interpreted as an element of TFP exclusively. In the above results we followed the literature in excluding any proxies for investment in the estimation equation — here we compare and contrast the results when capital stock per capita is included or excluded.[18] Regardless of the inclusion or exclusion of capital stock the trajectories of the treatment effects in Appendix Figure C-1 are qualitatively very similar to each other and in terms of effect at the mean/median as well as for countries with few years of treatment in line with those discussed above.

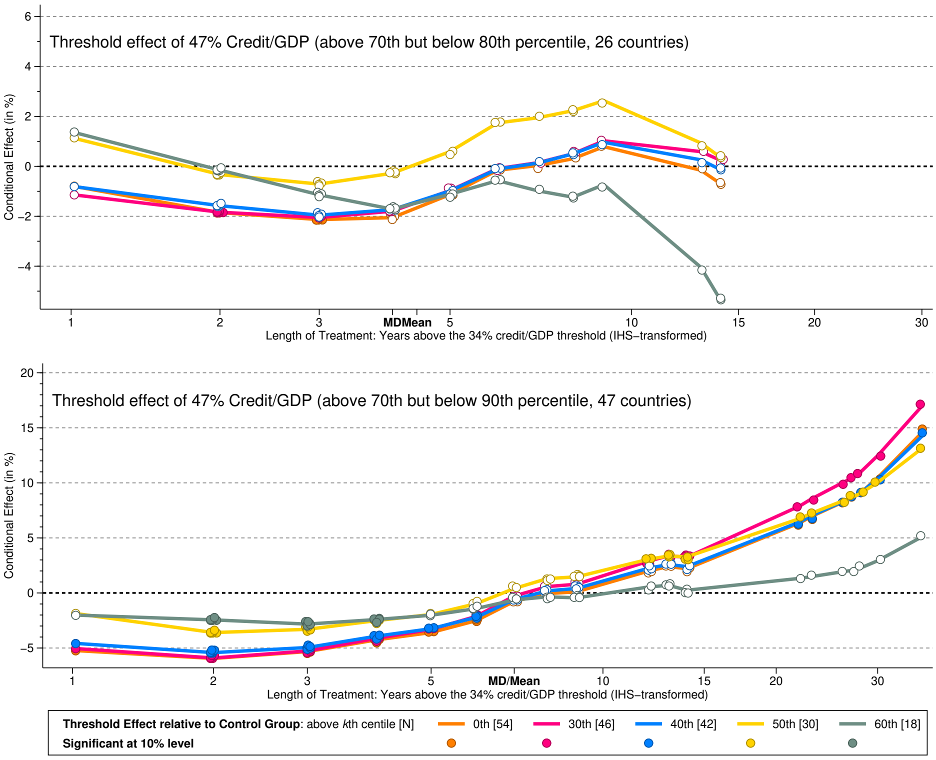
Taken together, these results provide strong evidence that when appropriate control samples are considered the effect of 'too much finance' is either meandering around zero and insignificant or rising over time and eventually, from around mean treatment length, positive and statistically significant. Our preferred estimates cannot provide definitive evidence for a positive or even positive *linear* treatment effect (the latter would imply a perpetual growth effect), but they clearly rule out any *dramatic* long-run decline in economic prosperity as has been suggested in the existing literature.

[18] The latter further acts as a robustness check on our main results which use WDI data for the dependent variable. We keep the same additional controls (openness and inflation).

Figure 5: Finance for Development? Running line presentation of PCDID results



(a) 34% Credit/GDP Threshold(60th-70th %ile, top, 60th-80th %ile)



(b) 47% Credit/GDP Threshold (70th-80th %ile, top, 70th-90th %ile)

Notes: The different specifications in each plot are for control samples dropping countries with a maximum credit/GDP below 16% (30th percentile) 20% (40th), 26% (50th), 34% (60th) or, in the lower panel also 47% (60th). See Figure 4 for additional details.

3.3 Finance for Development?

Panel (a) in Figure 5 presents results for the 34% credit/GDP threshold, Panel (b) for the 47% threshold.[19] In either case the treated sample is curtailed as indicated. The different running regression lines in each plot are for the same treated sample but correspond to different control samples: again we curtail the control sample from below. In Panel (a) the treatment effects are positive for virtually all specifications and at times statistically significant and rising with treatment length.

For the 47% threshold in Panel (b), the more curtailed specifications similarly yield positive significant and rising effects. Countries with just a few years above the threshold as well as those at mean or median treatment length often have positive but largely insignificant coefficients.

While the existing literature has acknowledged the beneficial aspects of finance at different levels of development (Levine 2005, Gambacorta et al. 2014), there has been no consensus over the *relative* benefit experienced between these two — our analysis suggests the effects for 'developing countries' are surprisingly similar to those at the very top of the credit/GDP distribution (119% threshold).

3.4 Household versus Corporate Debt

Background While the 'credit booms gone bust' narrative in the financial crisis literature is now well-established, the more recent work has asked whether this relationship is crucially influenced by 'who borrows' (e.g. Beck et al. 2009, Mian et al. 2017, Müller & Verner 2021, among others). From a theoretical point of view, sectoral heterogeneity does not feature prominently in credit cycle theories (see discussion in Müller & Verner 2021), though most of the empirical literature has suggested household credit as the major driver of the aggregate credit-crisis relationship (Jordà et al. 2016*a*, Mian et al. 2017).[20] As an exploratory exercise,

[19]In Appendix Figure B-1, Panel (b) we provide detailed robustness checks by varying the 'too much finance' threshold from 30% to 65% (k) of the credit/GDP level, further restricting the treated sample to those countries which stayed below $k+$ 25%. The control sample is always all countries with a peak below k. The effects for around 30 years in treatment are between 5 and 10%. Appendix Tables B-2 and B-3 report ATET estimates and p-values from Wald tests carried out in analogy to those for the advanced country samples: basic assumptions of the model are violated in the 47-65% treatment sample but largely confirmed in all other specifications.

[20]Recent work by Müller & Verner (2021) tackles the corporate credit side and demonstrates that, similar to household credit booms, lending to *non-tradable* sectors constitutes the 'bad booms'

limited to 'too much finance' at the top of the credit/GDP distribution, we investigate whether the use of household credit and corporate credit leads to different insights in the finance-growth relationship.

Thresholds Like in our analysis of aggregate credit we adopt specific percentiles of the distribution of household credit/GDP and firm credit/GDP to define respective thresholds for 'too much finance'; due to modest sample sizes all countries permanently below the respective threshold are in the control sample. We adopt the 80th, 85th and 90th percentiles — highlighted in Appendix Figure D-1 — but the treated sample sizes are modest (12 to 19 countries), so that our results need to be interpreted with caution. In order to capture an imbalance between household and corporate credit[21] we construct a variable representing the *share* of household to total credit and take its 80th, 85th and 90th percentiles as alternative thresholds. Again, the treated sample size is small, only ten countries.

Results Empirical findings for the PCDID estimator are presented in Appendix Table D-2. These give no indication of a negative average treatment effect (ATET – computed using the robust mean across heterogeneous country estimates),[22] in fact two out of three household credit specifications have large positive results (13% higher income per capita). Our Wald tests that control variables are jointly insignificant in an auxiliary regression of the treatment dummy on the controls as well as the estimated factors are somewhat mixed: the null of no statistical significance is *not universally* maintained. With this and other caveats in mind, we conclude that once again *on average* there was no evidence for a detrimental 'too much finance' effect on economic development.[23]

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leading to productivity slowdowns and financial vulnerability. Analysis using their rich sectoral credit data is left for future research once these are publicly available.

[21] We experiment with including the household credit/GDP variable in the treatment equation for 'too much corporate credit' and vice versa, but our Wald tests always reject the null that the threshold variable is exogenous to the controls.

[22] Given the small number of treated countries the running line regression tool does not yield any reliable insights.

[23] Given the sample sizes involved we did not pursue the analysis of banking crisis in this dataset. Substantially larger panel data from Müller & Verner (2021) are due to be made public in the second half of 2022.

4 Financial Development and Systemic Vulnerability

A large literature on financial crises has (re)emerged following the Global Financial Crisis of 2007/8, with the "new (near consensus) view" (Bordo & Meissner 2016, 31) that banking crises are 'credit booms gone bust' (Schularick & Taylor 2012). Yet the drivers of banking sector distress have been shown to differ across economies given their different structural characteristics (Hardy & Pazarbaşioğlu 1999) and the differences in the identity of lenders (private in advanced, predominantly official in developing countries) and borrowers (private in advanced, government-owned banks in developing economies: Caprio & Klingebiel 1996).

In this section we connect the empirical literatures on 'high' financial development and financial crises: adopting banking crisis data from Reinhart & Rogoff (2009) and Laeven & Valencia (2020) we compare the propensity of credit booms (Giannetti 2007, Jordà et al. 2011, Schularick & Taylor 2012), unfettered capital inflows (Reinhart & Rogoff 2013, Caballero 2016), and aggregate commodity price movements (Eichengreen 2003, Eberhardt & Presbitero 2021) in predicting banking crises above and below different cutoffs of 'too much finance'. Our research question is whether *within countries* these 'dominant narratives' for banking crises are comparatively more compelling when countries are in the higher finance regime: if 'too much finance' goes hand in hand with increased vulnerability, then we would expect the dominant crisis determinants established by the literature to be the primary suspects for driving this process (see also Kaminsky & Reinhart 1999), and our empirics should be able to detect increased vulnerability in the higher regime.

In the remainder of this section we introduce the additional data used as well as our EWS methodology (Section 4.1), before we discuss our findings for the countries at the top (4.2) and at intermediate levels (4.3) of the credit/GDP distribution.

4.1 Data and Methodology

Data and Transformations In addition to the credit/GDP data (see Section 3.1 above) we adopt the banking crisis data collated by Carmen Reinhart and co-authors, augmented with data from Laeven & Valencia (2020) to maximise coverage for the 1960-2016 period. Gross capital inflows (in percent of GDP) are taken from the

IMF Financial Flows Analytics database.[24] In order to capture 'excessive' capital inflows, the literature has adopted bonanza (Caballero 2016) or surge indicators (Ghosh et al. 2014) based on capital flow data. These dummy variables severely curtail the sample in our heterogeneous EWS analysis, since our lower versus higher regime setup is only identified if there are surges or bonanzas *in both regimes* of a country.[25] Our approach is thus wedded to the *continuous* financial flow variable (growth in capital flows/GDP), but in order to mimic the nature of capital inflow *spikes* we alternatively adopt the square of capital inflows/GDP *levels* for analysis.[26]

In the developing country sample we add commodity price movements, constructed using data from Gruss & Kebhaj (2019): we employ the monthly country-specific aggregate commodity price index (based on country averages of net export/GDP weights) for 1962-2016 to construct two variables: (a) the first difference of the index, and (b) the predicted volatility from a simple GARCH(1,1) model for commodity price growth with just an intercept term (following Cavalcanti et al. 2015). We sum the monthly growth terms to compute annual values and take the annual mean of monthly volatility (see Eberhardt & Presbitero 2021, for details).

One important issue is how to capture the 'trigger' dynamics of crisis determinants but not to rule out slower-moving fundamentals (Eichengreen 2003): in their analysis over three centuries Schularick & Taylor (2012) adopt a fifth-order lag polynomial specification for credit growth and controls — in our dataset this would take up too many degrees of freedom in the country regressions, hence we resort to specifying moving averages to capture pre-crisis dynamics, following Reinhart & Rogoff (2011) and Jordà et al. (2011, 2016*b*). In line with Eberhardt & Presbitero (2021) we adopt an MA(3) transformation: $\overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} = (1/3)\sum_{s=1}^{3}\Delta(\text{credit/GDP})_{i,t-s}$, and similarly for all other controls.

Regarding additional control variables we follow the practice in Schularick & Taylor (2012) for the 'advanced country' analysis: our simplest empirical model in-

[24]In line with Caballero (2016) we find the most robust results using gross rather than net inflows. Our findings are qualitatively similar if we adopt gross non-official flows rather than gross total flows.

[25]Further problems arise if there are comparatively few years spent in the higher or lower regime.

[26]This square is not included alongside the inflows/GDP 'levels' variable to detect a concave or convex relationship with crisis propensity, but it is entered *on its own* as an accentuated measure for large swings in capital movements.

cludes only the MA(3)-transformed credit/GDP growth or capital flow variables; we then present results for a 'full model' where we add MA(3)-transformations of per capita GDP growth, the change in gross fixed capital formation over GDP, and inflation as additional controls — these variables are taken from the World Bank *WDI*. For the developing country analysis we adopt inflation and trade openness taken from the same source in the specifications with additional controls.[27] These choices and restrictions regarding the operationalisation of our variables of interest and the limited additional controls represent a caveat for our analysis. Our set of additional controls represents a bare minimum compared with pooled empirical models in the existing literature (see Demirgüç-Kunt & Detragiache 1998, Kaminsky & Reinhart 1999, Papi et al. 2015, for additional crisis predictors); however, the parsimony imposed by our methodology as well as (in some cases) data availability at least avoids the concerns regarding overfitting when studying rare events like banking crises. We also gain insights by comparing results for specifications without additional controls with those when we, in comparative terms, saturate the model. Finally, it bears emphasising that our adopted methodology (discussed below) includes estimated effects of unobserved common factors in the spirit of Boneva & Linton (2017), which can capture relevant crisis determinants omitted from the model as well as global shocks or crisis spillovers (Cesa-Bianchi et al. 2019).

Factor-Augmented Early Warning System We specify a latent variable model of banking sector vulnerability Y^*_{it} as a function of the dominant crisis predictors in the literature (illustrated below using credit/GDP growth in the MA(3) transformation) for each country in a 'treated' sample of (a) highly financially developed economies; or (b) intermediate-level financially developed countries, respectively:

$$\begin{aligned} Y^*_{it} &= \alpha_i' d_t + \beta_i^A \, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} \\ &\quad + \beta_i^B \, \mathbf{1}_{\{t>T_{0i}\}} \, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} + \gamma_i' \overline{x}_{i,t-1\,/\,t-3} + \kappa_i' f_t + \varepsilon_{it}, \end{aligned} \tag{5}$$

where f is a set of unobserved common factors with heterogeneous factor loadings $\boldsymbol{\kappa}$ and additional controls are represented by x — these always include the 'rival'

[27] GFCF data are sparser for developing economies, while GDP growth was not found to be a significant crisis predictor in Eberhardt & Presbitero (2021), in contrast to inflation and openness.

dominant crisis predictors (i.e. in the present case capital flows in both samples and further aggregate commodity price movements in the developing country sample) alongside the other controls. The indicator variable $\mathbf{1}_{\{.\}}$ captures the time periods spent in the 'higher regime' above the credit/GDP threshold.[28]

We implement this model by combining work on common factors in a generalised linear model (Boneva & Linton 2017) with that on the PCDID (Chan & Kwok 2022) to create a factor-augmented EWS approach.[29] We adopt a linear probability model for banking crises Y_{it} (crisis start year)[30] in those countries which crossed the credit/GDP threshold. The country-specific estimation equation is augmented with up to k common factors, estimated from countries which always remained below the 'too much finance' threshold (for convenience: 'control sample').

For illustration, in the credit/GDP growth case: $\forall\, i \in E$

$$\begin{aligned} &Pr(Y_{it} = 1 \mid \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3},\, \overline{x}_{i,t-1/t-3},\, d_t,\, f_t) \qquad (6)\\ &= [\alpha_i + \tilde{f}_t'\kappa_i]d_t + \beta_i^A\, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} \\ &\quad + \beta_i^B\, \mathbf{1}_{\{t>T_{0i}\}}\, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} + \delta_i'\overline{x}_{i,t-1/t-3} + \psi_i'\hat{f}_t. \end{aligned}$$

In this specification the change in credit/GDP is split in two by means of the interaction with the 'higher regime' dummy $\mathbf{1}_{\{t>T_{0i}\}}$; as a benchmark we also provide results for a model where there is only one credit/GDP growth term. The common factors $\hat{f}$ are estimated via PCA from the residuals of the same model in the control group (albeit by construction with just a single credit/GDP growth term).[31]

[28]The more general setup with d_t allows for the inclusion of 'observed' common factors.

[29]Boneva & Linton (2017) provide an extension to the Pesaran (2006) common correlated effects estimator in the context of the probit model but also support the linear probability model. In contrast to our implementation in their model the common factors are proxied by the cross-section averages (CA) of all regressors in the model. We could have adopted this strategy, using the CA based on control sample variables, but opted to keep the estimation approach as similar as possible to the linear PCDID adopted in the finance-growth regressions above.

[30]Subsequent 'ongoing crisis years' are dropped from the sample as per practice in this literature.

[31]The term in square brackets in equation (6) includes some estimation error of this process, $\tilde{f}_t$, which vanishes as $\sqrt{T}/N_C \to 0$ for T the time series dimension and N_C the number of control group countries, in which case this term in square brackets is time-invariant. Note further that the estimated factors are *not* MA(3)-transformed since they are estimated from the residuals of a regression analogous to equation (7) in which all regressors are already MA(3)-transformed.

We assume $d_t = 1$ and estimate for treated countries $i \in E$

$$\begin{aligned} Y_{it} &= a_i + b_i^A\,\overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} + b_i^B\,\mathbf{1}_{\{t>T_{0i}\}}\,\overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} \\ &\quad + c_{1i}\overline{\Delta(\text{cap inflow/GDP})}_{i,t-1/t-3} + c_{2i}\overline{\Delta(\text{GDP pc})}_{i,t-1/t-3} \\ &\quad + c_{3i}\overline{\Delta(\text{GFCF/GDP})}_{i,t-1/t-3} + c_{4i}\overline{(\text{inflation})}_{i,t-1/t-3} + d_i'\hat{f}_t + \varepsilon_{it}, \end{aligned} \tag{7}$$

where we spell out the control variables in detail. ε is a well-behaved error term, which can be heteroskedastic and/or serially correlated. Alternative specifications focusing on excessive capital flow (and in the LDC analysis, aggregate commodity price movements) are constructed analogously, with the credit growth variable as additional control. The factor augmentation captures the developments in the countries which never crossed the specified credit/GDP threshold, while the interaction term setup allows us to investigate differential effects of dominant crisis predictors below and above the financial development threshold *within* individual countries. A positive (negative) significant interaction term suggests that being in the higher, 'too much finance' regime implies a higher (lower) propensity of banking crises for the dominant crisis predictors in the literature than in the lower regime. Note that we study the dominant crisis predictors in *separate* regressions, i.e. there is only ever one interaction term effect, not one for each of the covariates, to keep the empirical model parsimonious and hence feasible for estimation.

Robust mean marginal effects and inference We present the robust mean estimates for the dominant crisis determinants (and the interaction with 'high financial development', if applicable) and do not, as in the previous section, follow a strategy of highlighting the crisis propensity effect across time spent in the higher regime: the EWS analysis focuses on *short-run* trigger effects, and it therefore seems more natural *not* to take time in the higher regime into account. Our reported estimates are Mean Group estimates computed using robust regression (Hamilton 1992) with associated standard errors computed non-parametrically (Pesaran 2006, Chan & Kwok 2022). All results are expressed as marginal effects (in percent) of a one standard deviation increase in the variable of interest. In the interaction specifications we still adopt the full sample standard deviation for ease of comparability.

4.2 Systemic Vulnerability due to Too Much Finance?

Table 1 presents the results for two thresholds of 'high financial development', the 90th percentile (92% credit/GDP) and the 95th percentile (119% credit/GDP): there are 30 and 23 countries in these samples, which experienced 47 and 38 banking crises, respectively (see final section of the table). The unconditional crisis propensities in our treated samples (4.8% and 5.0%) are broadly similar to those in the various 'control samples' (5.5-5.9% and 5.3-5.5%). The different columns of the table represent different 'control samples', which as we move to the right are defined with higher and higher cutoffs: the results in columns (5) and (10) adopt the 'control group' of countries with a peak of credit/GDP between the 70th and 90th percentiles. In each of the three results panels (A-C) the first set of marginal effect estimates ignores separating out the effect of the variable of interest into a 'low' versus 'high' finance regime. These marginal effects (labelled $\hat{\beta}^{MG}$) are positive and significant in all specifications for credit/GDP growth and in many specifications of squared capital flow/GDP (but only sporadically significant for change in capital flow/GDP).

Regarding credit booms in Panel (A), highly financially developed economies experience a 2-3% higher propensity of a banking crisis for a one standard deviation increase in credit/GDP growth when we estimate the EWS without any control variables, rising to 2.5%-5% with the full set of controls. Hence, as is well-established in the literature, credit booms have a substantial positive effect on crisis propensity, up to the magnitude of the unconditional crisis propensity of around 5%.[32] While the simple change in the capital flows/GDP measure yields disappointing, largely statistically insignificant results in Panel (B), our attempt at capturing *excessive* capital movements in Panel (C) suggests that a one standard deviation of the squared capital flows/GDP ratio leads to a 1.5-5.8% increase in the propensity of a banking crisis — these are the results for the sample of countries which crossed the 90th percentile of the credit/GDP distribution, the estimates for the countries which crossed the 95th percentile are more modest (around 1%) and only consistently statistically significant in the model without additional controls. We take these

[32] Comparing AUROCs between models with control variables which include or exclude the credit/GDP growth variable suggests that including them has significantly higher predictive power in the Group I results, but only in the specification in column (10) in the Group II results.

results as confirmation that our samples (and empirical methodology) can replicate the current consensus in the literature that credit booms and perhaps to a somewhat lesser extent large capital inflows play an important role in triggering banking crises.

We now turn to the main purpose of this EWS exercise, the question whether *within* highly financially developed countries these credit boom and excessive capital flow effects are comparatively larger when countries were in the higher 'regime' of financial development compared with the effects in the lower 'regime'.[33] For credit booms in Panel (A), focusing on the specifications with additional controls the below-threshold estimates in Group I are large, between 3.7% and 4.8%, and statistically significant, whereas the above-threshold effects, to be interpreted as deviations from these benchmark effects, are all negative and statistically insignificant. In column (5), for instance, the below-threshold effect is 4.8%, whereas the relative effect above the threshold is -1.8%, albeit statistically insignificant. In Group II the benchmark estimates for below the threshold are substantially lower and statistically insignificant, now the above threshold deviations are positive and comparatively larger but still statistically insignificant.[34] Given that the $\hat{\beta}^{MG}$ estimates *ignoring the financial development regime* are all statistically significant, we interpret these findings as suggesting that a differential effect *by regime* in Group II is not supported by the data. Taken together, our results suggest that there is no evidence that credit booms create more substantial financial vulnerability in form of a higher susceptibility to banking crises when economies have very high levels of financial development.

Turning to the analysis of capital inflows, the change in capital flows/GDP measure in Panel (B) typically reveals patterns whereby the effect in the lower regime is negative while that in the higher regime is positive and often of substantially greater magnitude — however, none of these results are anywhere near statistical significance in the specifications which include additional controls. For the models using squared capital flows/GDP levels in Panel (C) the Group I results for specifications with all controls follow the same pattern as those discussed above for credit

[33] A simple count of banking crises in the two regimes already indicates that 50% more crises (100% in case of the 95th percentile cutoff in columns (6) to (10)) occurred in the *lower* regime.

[34] AUROC comparison in either Group I or II results indicate that including the two credit growth terms has higher predictive power than a model with just a single credit growth term ignoring financial development.

Table 1: Too Much Finance & Banking Crises

Higher Cutoff	**Group I** 92% Credit/GDP (90th percentile)					**Group II** 119% Credit/GDP (95th percentile)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Control Above	0%	20%	26%	34%	47%	0%	20%	26%	34%	47%
Percentile	0th	40th	50th	60th	70th	0th	40th	50th	60th	70th
Panel A: Change in Credit/GDP (Credit Booms Gone Bust)										
Without additional controls										
$\hat{\beta}^{MG}$	2.933	2.457	2.379	2.302	1.836	2.311	2.628	2.369	2.247	2.460
	[3.27]	[2.60]	[2.21]	[2.34]	[2.01]	[2.38]	[2.78]	[2.47]	[2.61]	[2.66]
$\hat{\beta}^{A}$	2.463	1.692	2.371	2.330	2.417	1.408	0.828	2.491	1.270	0.942
	[1.48]	[1.18]	[1.54]	[1.68]	[1.62]	[1.01]	[0.71]	[1.67]	[0.90]	[0.72]
$\hat{\beta}^{B}$	-2.263	-1.914	-2.488	-2.975	-2.769	0.464	1.374	0.132	2.797	2.186
	[0.90]	[0.79]	[0.96]	[1.11]	[1.11]	[0.18]	[0.55]	[0.06]	[0.83]	[0.57]
ROC Inter (p)	0.164	0.116	0.054	0.082	0.064	0.302	0.218	0.074	0.202	0.118
Controlling for Change in Capital Inflows/GDP, Inflation, GDPpc Growth and Change in GFCF/GDP (MA)										
$\hat{\beta}^{MG}$	4.909	3.736	4.103	4.721	4.214	2.607	2.592	2.454	2.770	2.484
	[4.25]	[3.20]	[3.39]	[3.83]	[3.80]	[2.39]	[2.37]	[2.22]	[2.55]	[2.20]
ROC Comp (p)	0.012	0.039	0.016	0.009	0.004	0.252	0.332	0.165	0.222	0.054
$\hat{\beta}^{A}$	4.267	3.753	3.923	4.474	4.833	0.895	0.830	0.763	0.922	0.248
	[1.89]	[1.81]	[1.76]	[1.96]	[1.96]	[0.86]	[0.75]	[0.60]	[0.68]	[0.15]
$\hat{\beta}^{B}$	-1.721	-1.088	-0.874	-1.063	-1.793	2.590	2.752	2.048	4.377	4.241
	[0.54]	[0.69]	[0.74]	[0.69]	[0.52]	[0.45]	[0.43]	[0.54]	[0.27]	[0.32]
ROC Comp (p)	0.014	0.011	0.010	0.002	0.005	0.023	0.035	0.026	0.038	0.039
ROC Inter (p)	0.483	0.102	0.187	0.054	0.143	0.070	0.077	0.152	0.144	0.204
Panel B: Change in capital flows/GDP (Excessive Capital Flows I)										
Without additional controls										
$\hat{\beta}^{MG}$	2.125	6.237	3.818	6.204	4.558	-1.130	-0.756	-0.356	0.926	0.860
	[0.55]	[2.02]	[1.19]	[1.95]	[1.55]	[0.70]	[0.51]	[0.22]	[0.71]	[0.47]
$\hat{\beta}^{A}$	-9.189	-5.559	-5.203	-2.916	-4.788	-2.874	-2.736	-3.348	-2.916	-0.881
	[0.99]	[0.55]	[0.57]	[1.38]	[0.58]	[1.02]	[1.04]	[1.52]	[1.38]	[0.30]
$\hat{\beta}^{B}$	19.642	20.899	14.848	5.189	8.271	5.510	5.467	7.021	5.189	4.968
	[1.46]	[1.47]	[1.17]	[1.75]	[0.86]	[1.09]	[1.14]	[1.89]	[1.75]	[1.23]
ROC Inter (p)	0.001	0.002	0.008	0.005	0.006	0.031	0.031	0.026	0.038	0.016
Controlling for Change in Credit/GDP, Inflation, GDPpc Growth and Change in GFCF/GDP (MA)										
$\hat{\beta}^{MG}$	4.02	1.832	0.235	3.12	4.513	-0.16	0.281	-1.051	-1.004	-0.717
	[0.80]	[0.39]	[0.04]	[0.57]	[0.94]	[0.08]	[0.14]	[0.45]	[0.34]	[0.27]
ROC Comp (p)	0.062	0.061	0.064	0.021	0.010	0.028	0.037	0.023	0.016	0.020
$\hat{\beta}^{A}$	-9.912	-2.821	-1.562	-1.773	5.071	-2.207	-2.349	-2.403	-2.433	-0.761
	[0.86]	[0.79]	[0.89]	[0.85]	[0.62]	[0.74]	[0.51]	[0.47]	[0.49]	[0.83]
$\hat{\beta}^{B}$	15.274	6.173	3.384	9.824	-1.380	0.739	1.354	3.075	1.310	2.453
	[0.97]	[0.45]	[0.81]	[0.48]	[0.92]	[0.90]	[0.83]	[0.57]	[0.81]	[0.70]
ROC Comp (p)	0.005	0.008	0.005	0.002	0.003	0.035	0.039	0.034	0.018	0.032
ROC Inter (p)	0.483	0.102	0.187	0.054	0.143	0.070	0.077	0.152	0.144	0.204

(Continued Overleaf)

Table 1: Too Much Finance and Banking Crises (continued)

Higher Cutoff	Group I 92% Credit/GDP (90th percentile)					Group II 119% Credit/GDP (95th percentile)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Control Above	0%	20%	26%	34%	47%	0%	20%	26%	34%	47%
Percentile	0th	40th	50th	60th	70th	0th	40th	50th	60th	70th
Panel C: Square of gross capital flows/GDP (Excessive Capital Flows II)										
Without additional controls										
$\hat{\beta}^{MG}$	1.597	2.145	2.534	3.067	2.671	0.636	0.810	0.745	0.726	0.718
	[1.71]	[2.13]	[2.93]	[3.01]	[3.03]	[2.46]	[2.52]	[2.60]	[2.49]	[2.53]
$\hat{\beta}^{A}$	2.011	1.872	1.912	2.721	2.884	0.309	0.468	0.831	0.771	0.726
	[1.29]	[1.33]	[1.23]	[1.38]	[1.34]	[0.89]	[1.34]	[1.73]	[1.72]	[1.55]
$\hat{\beta}^{B}$	-1.233	-1.352	-0.753	-0.720	-1.639	-0.690	-0.555	-0.687	-0.702	-0.936
	[0.54]	[0.65]	[0.39]	[0.35]	[0.70]	[1.20]	[0.76]	[0.83]	[0.85]	[1.25]
ROC Inter (p)	0.078	0.081	0.087	0.065	0.050	0.102	0.161	0.081	0.092	0.046
Controlling for Change in Credit/GDP, Inflation, GDPpc Growth and Change in GFCF/GDP (MA)										
$\hat{\beta}^{MG}$	5.313	4.758	5.737	5.816	5.760	0.930	1.135	1.360	1.035	0.806
	[3.31]	[2.79]	[2.87]	[2.98]	[2.90]	[1.55]	[1.61]	[1.73]	[1.60]	[1.21]
ROC Comp (p)	0.088	0.049	0.062	0.138	0.176	0.441	0.592	0.144	0.172	0.077
$\hat{\beta}^{A}$	4.621	4.691	4.845	4.489	4.115	0.915	0.902	1.071	1.617	1.622
	[2.06]	[2.27]	[1.77]	[1.72]	[1.63]	[0.91]	[0.89]	[0.86]	[1.42]	[1.43]
$\hat{\beta}^{B}$	-2.708	-3.767	-1.015	-1.878	-1.923	-0.018	-0.034	0.122	0.315	-0.523
	[0.80]	[1.06]	[0.35]	[0.59]	[0.57]	[0.02]	[0.04]	[0.15]	[0.34]	[0.56]
ROC Comp (p)	0.031	0.009	0.030	0.040	0.073	0.034	0.040	0.025	0.020	0.024
ROC Inter (p)	0.292	0.238	0.349	0.301	0.241	0.152	0.139	0.200	0.146	0.177
Treated Sample										
Countries	30	30	30	30	30	23	23	23	23	23
Observations	987	987	987	987	987	767	767	767	767	767
Crisis Prop.	0.048	0.048	0.048	0.048	0.048	0.050	0.050	0.050	0.050	0.050
Crises<cutoff	29	29	29	29	29	25	25	25	25	25
Crises>cutoff	18	18	18	18	18	13	13	13	13	13
Control Sample										
Countries	52	48	44	38	28	61	57	53	47	37
Observations	1518	1409	1289	1104	778	1807	1698	1578	1393	1067
Crises Prop.	0.055	0.056	0.056	0.055	0.059	0.053	0.054	0.054	0.053	0.055

Notes: We present robust means for country estimates (marginal effect of a one standard deviation in the variable, in percent) of MA(3)-transformed Δcredit/GDP or ΔCap Flows/GDP or (Cap Flows/GDP)^2 in the 'treated' sample of countries, where treatment is defined by having crossed a threshold of 92% or 119% of credit/GDP. These estimates derive from our factor-augmented linear probability model for banking crises in equation (5). We present marginal effects for a lower regime, $\hat{\beta}^{A}$, and their *deviation* for a higher regime, $\hat{\beta}^{B}$. $\hat{\beta}^{MG}$ is the marginal effect when we ignore regimes. Across columns we vary the control sample by setting a lower cutoff: countries below this cutoff are dropped from the control group. The full sample is labelled as 0th percentile. We include (MA(3)-transformed) controls as indicated. These results include four common factors estimated from the control samples, results for 1-6 factors are available on request. In each model we confirm that the factor-augmented model has better predictive power than that without factors using comparison of AUROC statistics (not reported). In rows labelled 'ROC Comp (p)' we carry out an equivalent test for the exclusion of the variable of interest. In rows labelled 'ROC Inter (p)' we carry out an equivalent test for the exclusion of the interaction effect. The median number of years countries spend in the 'lower' and 'higher' regime is 19 and 14 in Group I and 24 and 9 in Group II. 'Crisis Prop' is the unconditional propensity of a banking crisis in the sample indicated.

booms: below the threshold the effect is around 4.5% and almost always statistically significant, whereas the above-threshold results indicate a negative, i.e. lower, effect although this deviation is never statistically significant. In Group II we have mixed patterns though at times identical to those just described, with none of the estimates statistically significant. Our attempts at capturing excessive capital inflows have yielded no evidence that very high levels of financial development make countries more susceptible to banking crises through this channel.

For both crisis narratives investigated, the absence of evidence is of course not evidence for the absence of an effect, but the overall pattern of results — positive and significant effects when ignoring regimes, positive and at times significant effects for the benchmark lower regime alongside frequently negative albeit insignificant coefficients for the higher regime — suggests our finding is consistent across a great many specifications: having previously established that 'too much finance' on average does not affect relative long-run economic development, we can now conclude that on average it also does not *systematically* raise the propensity of financial crises vis-à-vis more moderate levels. It bears reminding that we carried out this EWS analysis in a factor-augmented regression framework (Boneva & Linton 2017, Chan & Kwok 2022), conditioning on the unobservables driving banking sector vulnerability in very similar economies,[35] and comparing the effects *within* individual highly financially developed countries below and above the threshold.

4.3 Exposing Developing Countries to Financial Vulnerability?

In this section we investigate 'too much finance' for countries which either crossed the 34% or 47% credit/GDP. These samples are made up of 27 and 30 countries, respectively. The treated countries experienced 47 and 50 banking crises, respectively, of which 34% and 32% occurred in the higher regimes. The unconditional crisis propensity is around 6%, compared with 4.9-5.5% in the control samples.

Our analysis follows the same approach as that for the finance-growth nexus in Section 3.3, limiting control samples 'from below' and treatment samples to specific ranges. Given that middle-income economies have rarely been studied on their own

[35]The most restricted control sample for Group II for instance includes Austria and Italy.

we adopt all three dominant banking crisis determinants found in existing studies of advanced and low-income economies. We again begin by estimating the effect of the canonical crisis predictors for the full sample of 'treated' countries, ignoring 'higher' and 'lower' regimes. In Panels (A) to (C) of Table 2 these are the estimates marked $\hat{\beta}^{MG}$. While credit growth on its own yields positive but not consistently significant results, once we include the controls we find a strong effect across all samples: a one standard deviation increase in credit/GDP growth is associated with a 3.1-4.4% increase in the propensity of a banking crisis. These are substantial economic magnitudes, given an unconditional crisis propensity of 6%. For the change in capital inflows/GDP in Panel (B) the simple specification yields statistically significant results in only two of the models, in the results with additional controls the coefficient magnitude drops substantially and none are statistically significant. The findings for excessive capital inflows are hence somewhat mixed for this group of countries.[36]

The results for commodity price (ACP) growth and volatility (Panel C) in the simple models are weak and counter-intuitive: since improving commodity terms of trade should improve an economy's external balance while increased volatility should weaken it, the pattern of signs is the opposite to what we would expect. This result is rectified in the models including additional controls, where the volatility terms now have large positive coefficients, which are statistically significant in a number of specifications. Hence, ignoring financial thresholds, our analysis confirms the 'credit booms gone bust' and commodity price movement narratives, but finds only limited evidence for the relevance of excessive capital flows in these samples.

Our second step repeats this analysis adding interaction terms for the credit, capital flow or ACP variables with a 'higher regime' dummy. For credit growth in Panel (A), while the simple model yields some evidence that this mechanism has a stronger effect for countries above the threshold, results for models with additional controls are mixed and very imprecise. This undermines the notion that 'credit boom gone bust' cycles could be more prevalent in 'too much finance' regimes for this sample.

[36]This highlights that the sample in Caballero (2016) was dominated by high-income countries (fewer than 18% of observations for middle-income countries). If we adopt the squared capital flow/GDP measure this yields insignificant results, whether we distinguish 'low' versus 'high' regimes or not (results available on request).

Table 2: 'Too Much Finance' and Banking Crises in Developing Countries (LDCs)

Treatment Range	**34-65% Credit/GDP** (60th-80th percentile)			**47-92% Credit/GDP** (70th-90th percentile)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Control: Above	0%	16%	20%	0%	16%	20%	26%
Percentile	0th	30th	40th	0th	30th	40th	50th
Panel A: Change in Credit/GDP (Credit Booms Gone Bust)							
Without controls							
$\hat{\beta}^{MG}$	1.683	1.635	2.032	1.933	2.211	1.312	1.081
	[1.17]	[1.21]	[1.27]	[1.25]	[1.31]	[0.83]	[0.84]
$\hat{\beta}^{A}$	-1.291	-2.190	-1.423	-0.621	-0.252	-1.782	-1.252
	[0.50]	[0.90]	[0.53]	[0.46]	[0.15]	[1.06]	[0.95]
$\hat{\beta}^{B}$	5.332	4.563	4.232	6.860	5.579	6.166	4.965
	[1.54]	[1.34]	[1.26]	[1.90]	[1.64]	[1.57]	[1.44]
ROC Inter (p)	0.010	0.006	0.008	0.027	0.012	0.010	0.120
Controls: Change in Capital Inflows/GDP, Inflation, Openness and ACP movements							
$\hat{\beta}^{MG}$	4.400	3.344	3.881	3.429	3.099	3.654	3.773
	[3.45]	[2.27]	[2.52]	[2.19]	[1.98]	[2.24]	[2.52]
ROC Comp (p)	0.028	0.026	0.031	0.086	0.051	0.057	0.060
$\hat{\beta}^{A}$	2.964	2.589	3.508	2.712	0.844	2.796	2.537
	[1.02]	[0.90]	[1.31]	[1.19]	[0.42]	[1.20]	[1.23]
$\hat{\beta}^{B}$	5.282	4.620	3.838	0.508	1.409	1.932	2.058
	[1.01]	[0.94]	[0.77]	[0.13]	[0.39]	[0.50]	[0.52]
ROC Comp (p)	0.017	0.028	0.029	0.045	0.094	0.058	0.078
ROC Inter (p)	0.012	0.008	0.010	0.030	0.011	0.015	0.025
Panel B: Change in capital flows/GDP (Excessive Capital Flows)							
Without controls							
$\hat{\beta}^{MG}$	1.043	0.984	0.689	6.080	5.198	5.948	1.358
	[0.90]	[0.82]	[0.51]	[1.77]	[1.61]	[1.90]	[0.67]
$\hat{\beta}^{A}$	1.200	1.204	1.089	1.308	1.864	1.704	2.687
	[1.10]	[1.04]	[1.09]	[0.36]	[0.55]	[0.55]	[0.79]
$\hat{\beta}^{B}$	6.882	6.427	4.234	17.677	18.636	13.765	11.327
	[2.03]	[1.72]	[1.21]	[1.39]	[1.43]	[1.07]	[1.11]
ROC Inter (p)	0.053	0.109	0.073	0.001	0.004	0.002	0.004
Controls: Change in Credit/GDP, Inflation, Openness and ACP movements							
$\hat{\beta}^{MG}$	0.192	-0.115	0.124	1.723	3.284	1.455	0.670
	[0.12]	[0.06]	[0.07]	[0.48]	[0.87]	[0.41]	[0.19]
ROC Comp (p)	0.197	0.264	0.076	0.148	0.033	0.062	0.138
$\hat{\beta}^{A}$	-1.230	-1.114	-0.278	3.076	3.398	4.407	3.449
	[0.52]	[0.50]	[0.13]	[0.80]	[0.85]	[0.93]	[0.95]
$\hat{\beta}^{B}$	10.841	7.502	9.452	9.685	8.286	6.972	11.857
	[1.90]	[1.16]	[1.28]	[1.08]	[0.80]	[0.64]	[1.18]
ROC Comp (p)	0.035	0.111	0.018	0.099	0.131	0.076	0.078
ROC Inter (p)	0.015	0.031	0.006	0.048	0.012	0.023	0.029

(Continued Overleaf)

Table 2: 'Too Much Finance' and Banking Crises in LDCs (continued)

Treatment Range	**34-65% Credit/GDP** (60th-80th percentile)			**47-92% Credit/GDP** (70th-90th percentile)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Control: Above	0%	16%	20%	0%	16%	20%	26%
Percentile	0th	30th	40th	0th	30th	40th	50th
Panel C: Aggregate Commodity Price (ACP) Growth and Volatility							
Without additional controls							
ΔACP $\hat{\beta}^{MG}$	2.397	2.168	1.695	0.888	-0.091	2.057	0.168
	[1.31]	[1.14]	[0.94]	[0.41]	[0.04]	[0.94]	[0.07]
ACP Vol $\hat{\beta}^{MG}$	-2.675	-1.556	-4.264	-8.646	-12.273	-14.614	-9.545
	[0.71]	[0.52]	[2.07]	[0.84]	[1.09]	[1.40]	[1.19]
ΔACP $\hat{\beta}^{A}$	2.904	2.928	3.035	2.686	3.824	2.201	2.331
	[1.77]	[1.57]	[1.75]	[1.11]	[1.29]	[0.84]	[0.91]
ACP Vol $\hat{\beta}^{A}$	-1.128	-1.762	-2.723	4.227	8.226	0.551	2.919
	[0.19]	[0.32]	[0.58]	[0.43]	[1.60]	[0.06]	[0.54]
ΔACP $\hat{\beta}^{B}$	-6.144	-6.732	-6.317	-6.162	-5.656	-8.971	-9.728
	[1.54]	[1.86]	[1.56]	[1.15]	[1.15]	[1.61]	[1.83]
ACP Vol $\hat{\beta}^{B}$	-0.370	-1.029	-0.513	-5.040	-1.044	-3.051	-4.081
	[0.19]	[0.41]	[0.20]	[0.91]	[0.22]	[0.63]	[0.76]
ROC Inter (p)	0.000	0.001	0.000	0.001	0.000	0.002	0.004
Controls: Change in Capital Inflows/GDP, Inflation, Openness and Credit/GDP Growth							
ΔACP $\hat{\beta}^{MG}$	0.664	-0.189	0.933	1.649	2.066	1.363	1.682
	[0.31]	[0.09]	[0.44]	[0.66]	[0.16]	[0.57]	[0.34]
ACP Vol $\hat{\beta}^{MG}$	15.931	4.475	7.190	24.510	23.763	25.729	14.621
	[2.23]	[0.82]	[1.02]	[1.83]	[2.05]	[1.50]	[1.17]
ROC Comp (p)	0.000	0.000	0.001	0.000	0.002	0.000	0.000
ΔACP $\hat{\beta}^{A}$	1.244	0.434	1.186	-0.412	-3.048	-0.032	-3.169
	[0.53]	[0.23]	[0.54]	[0.15]	[1.27]	[0.01]	[1.19]
ACP Vol $\hat{\beta}^{A}$	18.111	6.303	-0.225	25.984	27.541	27.002	18.722
	[1.54]	[0.71]	[0.02]	[1.59]	[2.37]	[2.53]	[2.00]
ΔACP $\hat{\beta}^{B}$	-2.762	-7.282	-4.718	-4.895	-7.197	-9.922	-9.123
	[0.47]	[1.28]	[0.78]	[0.92]	[1.16]	[1.49]	[1.74]
ACP Vol $\hat{\beta}^{B}$	6.836	3.876	5.761	1.350	2.468	0.142	-2.245
	[1.42]	[0.86]	[0.99]	[0.21]	[0.54]	[0.02]	[0.43]
ROC Comp (p)	0.003	0.001	0.000	0.001	0.001	0.000	0.000
ROC Inter (p)	0.093	0.037	0.020	0.048	0.053	0.034	0.063
Treated Sample							
Crisis Prop.	0.058	0.058	0.058	0.060	0.060	0.060	0.060
Crises<cutoff	31	31	31	34	34	34	34
Crises>cutoff	16	16	16	16	16	16	16
Control Sample							
Countries	24	19	16	34	29	26	20
Observations	710	599	492	1018	907	800	631
Crisis Prop.	0.051	0.052	0.055	0.051	0.052	0.054	0.049

Notes: We present robust means for country estimates (marginal effect of a 1sd increase in the variable, expressed in percent) of MA(3)-transformed Δcredit/GDP or ΔCap Flows/GDP or Aggregate Commodity Price Movements in the 'treated' sample of countries (N=27, n=810; N=30, n=839, respectively), where treatment is defined by having crossed a threshold of 34% or 47% of credit/GDP (but staying below 65% and 92%, respectively). See also notes to Table 1.

The capital inflow results in Panel (B), especially for the 34% threshold, present a different outcome with, broadly, agreement between the simple specifications and those with additional controls: we see a consistent pattern of high and in one case statistically significant results for the periods above the cutoff, while the estimates for the lower regime are all negative, of small magnitude and statistically insignificant. While all estimates using the 47% threshold are insignificant, those for the higher regime are typically a multiple of those for the lower regime. Overall, the evidence, although weak, is suggestive of a systemic effect of capital inflows affecting countries *more* at high levels of finance.

Finally, the results for commodity prices in Panel (C) provide some evidence that this channel has a systematic bearing on banking crises: once controls are included the 47% threshold indicates strong volatility effects below the threshold but also, though just in one specification, evidence for ACP growth effects above the threshold. Although less robust, these results are in line with the findings for low-income countries (Eberhardt & Presbitero 2021).[37]

Taken together, our benchmark analysis confirms general narratives in the literature of credit boom cycles and ACP movements in their relevance for vulnerability to banking crises. Once we account for different financial development regimes we found some indicative evidence that large increases in capital inflows and commodity price movements affect financial vulnerability in the higher regime.

5 Concluding remarks

Until quite recently, there was little doubt in the literature about the economic benefits from financial development. The experience of the Global Financial Crisis then led to the suggestion that while financial development overall was good for growth, economies could experience 'too much of a good thing', and the work by Arcand et al. (2015) and others established the presence of such a 'non-linearity' in the finance-growth relationship. Our paper challenges this conclusion by analysing

[37]All models presented in Table 2 indicate that the inclusion of the specific variable(s) of interest significantly add(s) to the predictive power of the model (ROC Comp p-values). The interaction terms for most samples of the 34% and 47% thresholds statistically significantly increase predictive power over the models not distinguishing financial development regimes (ROC Inter p-values). All models have better predictive power when estimated factors are included (not presented).

this relationship with (i) more flexible empirical specifications embedded in a causal treatment effects framework, (ii) a focus on country-specific effects, treatment length and the long-run equilibrium, and (iii) a methodological extension to study the impact of finance on the dominant banking crisis determinants in a factor-augmented EWS approach which focuses on the short-run and crisis 'triggers'.

Our analysis provides the following new insights into the implications of 'too much finance': there is no evidence that highly financially developed countries experience lower economic growth or are more susceptible to systemic banking crises above a certain threshold. In a moderate sample of advanced and emerging economies we are similarly unable to trace any detrimental growth effects when distinguishing whether financial development is driven by credit to households or to firms. Studying countries at intermediate levels of financial development we find that income per capita actually tends to *rise* with time spent in the 'high(er)' regime. Elevated levels of finance in this group of countries are however suggested to increase the risk of banking crises due to capital inflows and/or commodity price movements *in the short-run*. The empirical evidence is statistically weak, but the patterns are clearly more suggestive of a detrimental effect than in the advanced country sample. While some may disagree with this interpretation, it bears reminding that the *long-run* growth results suggest that however (in)substantial the increased vulnerability to banking crises in developing countries may be, these *short-run* implications of financial development do not hamper growth in the long-term. Hence, what remains of the 'too much finance' narrative? We would argue that for advanced and emerging economies on average there simply is no clear evidence for a large detrimental effect.

There are at least three important caveats for our analysis: first, we recognise that our proxy for financial deepening may not be *equally-suitable* at different points of the credit/GDP distribution (Popov 2018). We share this caveat with the entire empirical literature on the finance-growth nexus. However, if credit/GDP 'means different things' in different countries, then our heterogeneous model should go some way to weaken the bias relative to the pooled models studied in the existing literature.

Second, by moving away from pooled models with thousands of observations, our heterogeneous treatment effect analysis is *by construction* built on vastly fewer degrees of freedom. With this come imprecision, exaggerated idiosyncracies, and hence more uncertainty in the estimates we present. We have deliberately discussed and interpreted our results in broad brushes, trying to emphasise obvious commonalities across alternative specifications. We believe that the caution we employ in discussing our results and in drawing conclusions is reflected in the language we use, and that the patterns we detect stand out even to a more critical eye.

And third, although we have not ignored recent developments in unpacking private credit into more granular components (e.g. Beck et al. 2009, Mian et al. 2017, Müller & Verner 2021), our analysis of 'too much household credit' and 'too much corporate credit' could only rely on very modest 'treated' samples and did not unpack corporate credit into its sectoral components. Future work could build on the data collection effort by Müller & Verner (2021) to arrive at robust results in the analysis of a significantly larger sample.

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Online Appendix – Not Intended for Publication

A Data: Sample Makeup and Descriptives

Table A-1: Sample Makeup

	ISO	Country	Start	End	Obs	Miss	GDP pc (2008 US$)			Private Credit/GDP					'Adv. Countries'				'Developing Countries'			
							Start	End	∆pa	Start	End	∆pa	Min	Max	92	C	119	C	34-47	34-65	47-65	47-92
1	AGO	Angola	2000	2016	17	0	2196	3530	78.5	1	22	1.25	1	25								
2	ALB	Albania	1994	2016	23	0	1494	4682	138.6	3	36	1.44	3	40					8	8		
3	ARE	UAE	2001	2016	16	0	60861	41045	-1238.5	33	83	3.16	33	84		×		×				11
4	ARG	Argentina	1960	2016	57	0	5643	10240	80.6	18	12	-0.10	8	25								
5	ARM	Armenia	1992	2016	25	0	948	3917	118.8	40	45	0.16	3	45					6			
6	AUS	Australia	1960	2016	57	0	19378	55729	637.7	18	142	2.18	17	142	13		9					
7	AUT	Austria	1970	2016	45	2	19574	48260	610.3	42	83	0.88	42	98	9							
8	AZE	Azerbaijan	1992	2016	25	0	2361	5813	138.1	4	31	1.09	1	36					1	1		
9	BDI	Burundi	1964	2016	53	0	205	220	0.3	3	17	0.27	2	22								
10	BEL	Belgium	1970	2016	45	2	19808	45943	556.1	17	62	0.97	17	77		×		×				23
11	BEN	Benin	1982	2016	35	0	864	1135	7.7	28	21	-0.20	6	31								
12	BFA	Burkina Faso	1979	2016	38	0	356	748	10.3	14	27	0.32	6	28								
13	BGD	Bangladesh	1980	2016	30	7	359	1062	19.0	4	39	0.95	4	39					6	6		
14	BGR	Bulgaria	1991	2016	26	0	4360	8009	140.4	61	52	-0.33	8	69		×		×				14
15	BHR	Bahrain	1980	2015	33	3	21185	22436	34.7	34	105	1.95	26	114	7			×				
16	BHS	Bahamas,	1977	2016	39	1	18600	27370	219.3	28	72	1.08	24	84		×		×				20
17	BIH	Bosnia & H	1997	2016	20	0	2267	5595	166.4	60	52	-0.41	27	60		×		×		17	12	12
18	BLR	Belarus	1994	2016	23	0	2252	6216	172.3	18	27	0.39	4	35					1	1		
19	BLZ	Belize	1980	2016	36	1	2269	4217	52.6	27	58	0.83	27	65		×		×	15	32	17	18
20	BOL	Bolivia	1970	2016	47	0	1400	2426	21.8	8	61	1.12	6	63		×		×		23	9	9
21	BRA	Brazil	1970	2016	47	0	4704	10966	133.2	20	68	1.01	10	70		×		×				8
22	BRB	Barbados	1975	2009	35	0	10881	16492	160.3	28	78	1.42	26	78		×		×				9
23	BRN	Brunei D.	1999	2016	18	0	35681	31685	-222.0	54	45	-0.50	28	54		×		×		11	3	3
24	BTN	Bhutan	1983	2016	34	0	473	2971	73.5	3	57	1.58	3	57		×		×		8	6	6
25	BWA	Botswana	1972	2016	45	0	1114	7797	148.5	9	30	0.47	6	33								
26	CAF	Central Af R	1977	2015	39	0	643	347	-7.6	10	13	0.08	4	16								
27	CAN	Canada	1970	2008	39	0	22844	48495	657.7	32	123	2.33	32	177	11		8					
28	CHE	Switzerland	1970	2016	47	0	49581	77026	583.9	103	172	1.48	86	172	45		32					
29	CHL	Chile	1960	2016	57	0	3612	14777	195.9	23	109	1.49	3	109	8			×				
30	CHN	China	1985	2016	32	0	538	6908	199.1	65	149	2.63	65	149	19		5					
31	CIV	Cote d'Ivoire	1965	2016	52	0	1475	1530	1.1	18	22	0.07	13	42					14	14		
32	CMR	Cameroon	1975	2016	42	0	1123	1469	8.2	15	16	0.03	7	25								
33	COD	Congo, DR	2000	2016	17	0	290	407	6.9	0	6	0.32	0	6								
34	COG	Congo, R	1974	2015	36	6	2040	3013	23.1	12	21	0.21	2	29								
35	COL	Colombia	1960	2016	55	2	2339	7634	92.9	20	46	0.46	12	50		×		×		8	3	3
36	COM	Comoros	1982	2016	35	0	1460	1367	-2.7	10	26	0.47	8	26								
37	CRI	Costa Rica	1960	2016	57	0	2911	9510	115.8	26	56	0.53	10	56		×		×		10	4	4
38	CYP	Cyprus	1975	2015	41	0	7360	27898	500.9	79	248	4.12	54	261	22		15					
39	CZE	Czech R	1993	2016	24	0	12313	21864	397.9	59	50	-0.40	27	62		×		×		20	14	14
40	DEU	Germany	1970	2016	47	0	19680	45960	559.2	57	76	0.41	57	116	17			×				
41	DNK	Denmark	1966	2016	51	0	26032	61878	702.9	27	169	2.78	21	212	16		16					
42	DOM	Dominican R	1960	2016	57	0	1324	7026	100.0	5	26	0.36	5	30								
43	DZA	Algeria	1973	2016	44	0	2925	4830	43.3	35	22	-0.28	4	68		×		×				13
44	ECU	Ecuador	1960	2016	57	0	2238	5176	51.5	20	29	0.16	11	34					2	2		
45	EGY	Egypt	1960	2016	57	0	578	2761	38.3	18	28	0.18	10	51		×		×	7	13	6	6
46	ERI	Eritrea	1995	2011	17	0	568	537	-1.8	15	13	-0.14	13	35					1	1		
47	ESP	Spain	1972	2016	45	0	15010	31449	365.3	65	112	1.03	61	173	16		11					
48	EST	Estonia	1993	2016	24	0	6743	18092	472.9	9	70	2.51	9	103	2			×				
49	FIN	Finland	1970	2016	47	0	18267	46750	606.0	37	93	1.19	37	93	4			×				
50	FRA	France	1960	2016	55	2	12744	42140	515.7	20	95	1.32	20	96	8			×				
51	GAB	Gabon	1970	2016	47	0	7206	9429	47.3	5	14	0.19	5	28								
52	GBR	UK	1970	2016	47	0	17923	42500	522.9	19	130	2.38	19	196	28		15					
53	GEO	Georgia	1995	2016	22	0	1077	4305	146.7	5	56	2.33	3	56		×		×		5	2	2
54	GHA	Ghana	1967	2016	43	7	991	1645	13.1	8	18	0.20	1	18								
55	GIN	Guinea	1989	2016	28	0	535	810	9.8	3	10	0.25	2	10								
56	GMB	The Gambia	1981	2014	34	0	874	748	-3.7	15	13	-0.04	6	17								
57	GNB	Guinea-B.	1990	2016	27	0	637	595	-1.5	2	8	0.23	1	13								
58	GRC	Greece	1960	2016	57	0	6260	22666	287.8	10	110	1.75	10	121	7		3					
59	GTM	Guatemala	1960	2016	57	0	1491	3243	30.7	10	33	0.41	10	33								
60	GUY	Guyana	1960	2016	57	0	1699	3793	36.8	11	45	0.60	9	48		×		×		14	1	1

(Continued overleaf)

Table A-1: Sample Makeup (continued)

	ISO	Country	Start	End	Obs	Miss	GDP pc (2008 US$)			Private Credit/GDP					'Adv. Countries'				'Developing Countries'			
							Start	End	Δpa	Start	End	Δpa	Min	Max	92	C	119	C	34-47	34-65	47-65	47-92
61	HKG	Hong Kong	1990	2016	27	0	18251	36819	687.7	153	202	1.79	124	219	27		27					
62	HND	Honduras	1960	2016	57	0	1096	2111	17.8	10	54	0.78	10	54		×		×		16	8	8
63	HRV	Croatia	1995	2016	22	0	8568	14706	279.0	24	61	1.70	24	71		×		×				12
64	HUN	Hungary	1991	2016	26	0	8858	15114	240.6	41	34	-0.25	20	66		×		×				8
65	IDN	Indonesia	1980	2016	37	0	1231	3968	74.0	6	38	0.84	6	44					10	10		
66	IND	India	1960	2016	57	0	330	1876	27.1	9	49	0.71	9	50		×		×		12	6	6
67	IRL	Ireland	1970	2016	47	0	12745	67078	1156.0	25	49	0.52	25	174	11		6					
68	IRN	Iran	1961	2016	53	3	3236	6791	63.5	14	61	0.83	14	61		×		×		14	7	7
69	ISL	Iceland	1970	2016	47	0	16240	49985	718.0	30	84	1.14	21	263	13		9					
70	ISR	Israel	1970	2016	47	0	13965	33721	420.3	24	64	0.87	23	90		×		×				26
71	ITA	Italy	1970	2016	47	0	17671	34459	357.2	62	85	0.50	45	96	3							
72	JAM	Jamaica	1966	2016	51	0	3796	4762	18.9	17	30	0.26	14	37					1	1		
73	JOR	Jordan	1976	2016	41	0	2037	3271	30.1	30	71	0.99	30	85		×		×				36
74	JPN	Japan	1970	2016	47	0	18700	47403	610.7	82	160	1.65	82	192	46		35					
75	KAZ	Kazakhstan	1993	2016	24	0	4513	10583	252.9	15	34	0.80	5	49		×		×		7	2	2
76	KEN	Kenya	1964	2016	53	0	545	1130	11.0	13	31	0.35	10	32								
77	KGZ	Kyrgyz Rep	1995	2016	22	0	535	1044	23.1	11	21	0.42	4	21								
78	KHM	Cambodia	1993	2016	24	0	510	1080	23.7	2	74	2.98	2	74		×		×				3
79	KOR	Korea	1960	2016	57	0	932	26726	452.5	11	139	2.23	11	139	6		3					
80	LBN	Lebanon	1990	2016	27	0	3006	6412	126.1	61	97	1.32	36	97	2			×				
81	LBR	Liberia	2000	2013	14	0	614	597	-1.2	113	17	-6.86	14	906	11		10					
82	LKA	Sri Lanka	1961	2016	56	0	586	3769	56.8	7	37	0.53	7	37					1	1		
83	LSO	Lesotho	1973	2016	19	25	432	1422	22.5	0	17	0.38	0	17								
84	LTU	Lithuania	1995	2016	21	1	5318	15944	483.0	14	41	1.25	10	58		×		×		10	4	4
85	LUX	Luxembourg	1970	2016	44	3	35457	110162	1589.5	41	98	1.21	41	108	8			×				
86	LVA	Latvia	1995	2016	21	1	5141	14736	436.1	11	47	1.66	7	95	1			×				
87	MAC	Macao	1984	2016	33	0	18134	52163	1031.2	53	112	1.80	39	112	2			×				
88	MAR	Morocco	1966	2016	51	0	815	3213	47.0	13	63	0.99	9	73		×		×				12
89	MDA	Moldova	1995	2016	22	0	1624	3120	68.0	4	31	1.19	4	39					6	6		
90	MDG	Madagascar	1970	2016	47	0	854	476	-8.0	13	13	-0.01	8	18								
91	MEX	Mexico	1960	2016	57	0	3907	10206	110.5	20	32	0.20	8	32								
92	MKD	N Macedonia	1993	2016	24	0	3146	5247	87.6	36	48	0.48	16	49		×		×		11	2	2
93	MLI	Mali	1967	2016	45	5	341	749	8.2	1	23	0.44	1	23								
94	MLT	Malta	1970	2016	47	0	3746	26788	490.2	43	83	0.86	21	120	16		1					
95	MNE	Montenegro	2002	2016	15	0	5059	7493	162.2	8	47	2.60	8	83		×		×				10
96	MNG	Mongolia	1991	2016	26	0	1584	3866	87.8	11	53	1.61	5	55		×		×		6	4	4
97	MOZ	Mozambique	1992	2016	25	0	200	584	15.4	13	32	0.76	8	32								
98	MRT	Mauritania	1961	2012	39	13	1382	1653	5.2	3	21	0.35	3	30								
99	MUS	Mauritius	1976	2016	41	0	2405	9834	181.2	22	98	1.85	21	103	5			×				
100	MWI	Malawi	1973	2016	44	0	342	506	3.7	6	10	0.10	2	13								
101	MYS	Malaysia	1960	2016	57	0	1354	11244	173.5	8	120	1.97	8	145	23		5					
102	NAM	Namibia	1990	2016	27	0	3501	6143	97.8	19	64	1.69	19	64		×		×		24	5	5
103	NER	Niger	1980	2016	37	0	695	527	-4.6	16	15	-0.02	4	18								
104	NGA	Nigeria	1981	2016	36	0	1742	2456	19.8	14	15	0.02	5	20								
105	NIC	Nicaragua	1960	2016	45	12	1506	1895	6.8	15	36	0.37	3	39					3	3		
106	NLD	Netherlands	1969	2016	46	2	23389	52727	611.2	30	113	1.72	29	125	18		1					
107	NOR	Norway	1970	2016	47	0	32245	90196	1233.0	50	143	1.97	48	143	10		8					
108	NPL	Nepal	1975	2016	42	0	280	730	10.7	4	71	1.59	4	71		×		×				7
109	NZL	New Zealand	1970	2010	41	0	19989	33700	334.4	11	146	3.29	10	146	15		6					
110	OMN	Oman	1972	2016	38	7	9286	16226	154.2	2	73	1.56	2	73		×		×				2
111	PAK	Pakistan	1960	2016	57	0	302	1118	14.3	9	15	0.12	9	27								
112	PAN	Panama	1960	2016	57	0	2139	11107	157.3	12	81	1.23	11	92		×		×				30
113	PER	Peru	1960	2016	57	0	2660	6262	63.2	16	41	0.44	5	41					4	4		
114	PHL	Philippines	1960	2016	57	0	1100	2887	31.3	15	41	0.46	15	51		×		×	14	16	2	2
115	PNG	Papua NG	1973	2004	32	0	1774	1582	-6.0	11	8	-0.07	7	19								
116	POL	Poland	1995	2016	22	0	6540	15102	389.2	15	53	1.74	15	53		×		×		10	7	7
117	PRT	Portugal	1970	2016	47	0	8760	22534	293.1	46	114	1.45	42	159	18		11					
118	PRY	Paraguay	1962	2016	55	0	1430	5090	66.5	5	54	0.89	5	54		×		×		6	2	2
119	PSE	W Bank/Gaza	1996	2016	21	0	1879	2695	38.8	12	42	1.44	12	42					2	2		
120	ROU	Romania	1990	2016	23	4	5379	10237	179.9	70	34	-1.34	4	70		×		×				1
121	RUS	Russian Fed	1993	2016	24	0	7071	11356	178.6	6	56	2.10	6	56		×		×		9	3	3
122	RWA	Rwanda	1965	2016	51	1	288	793	9.7	0	20	0.37	0	20								
123	SAU	Saudi Arabia	1970	2016	47	0	22134	21271	-18.4	5	69	1.35	4	74		×		×				27
124	SDN	Sudan	1976	2015	40	0	946	1826	22.0	9	8	-0.02	2	15								
125	SEN	Senegal	1965	2016	52	0	1308	1432	2.4	17	32	0.29	13	35					3	3		
126	SGP	Singapore	1963	2016	54	0	4113	55043	943.2	33	132	1.84	33	132	22		4					
127	SLE	Sierra Leone	1980	2016	36	1	485	458	-0.7	4	5	0.02	1	7								
128	SLV	El Salvador	1965	2016	52	0	2358	3383	19.7	19	44	0.49	17	44					23	23		
129	SRB	Serbia	1997	2015	19	0	3504	6155	139.6	21	43	1.16	16	47		×		×	6	8	2	2
130	SVK	Slovak R	1993	2016	24	0	7821	19274	477.2	52	54	0.11	29	54		×		×		20	8	8

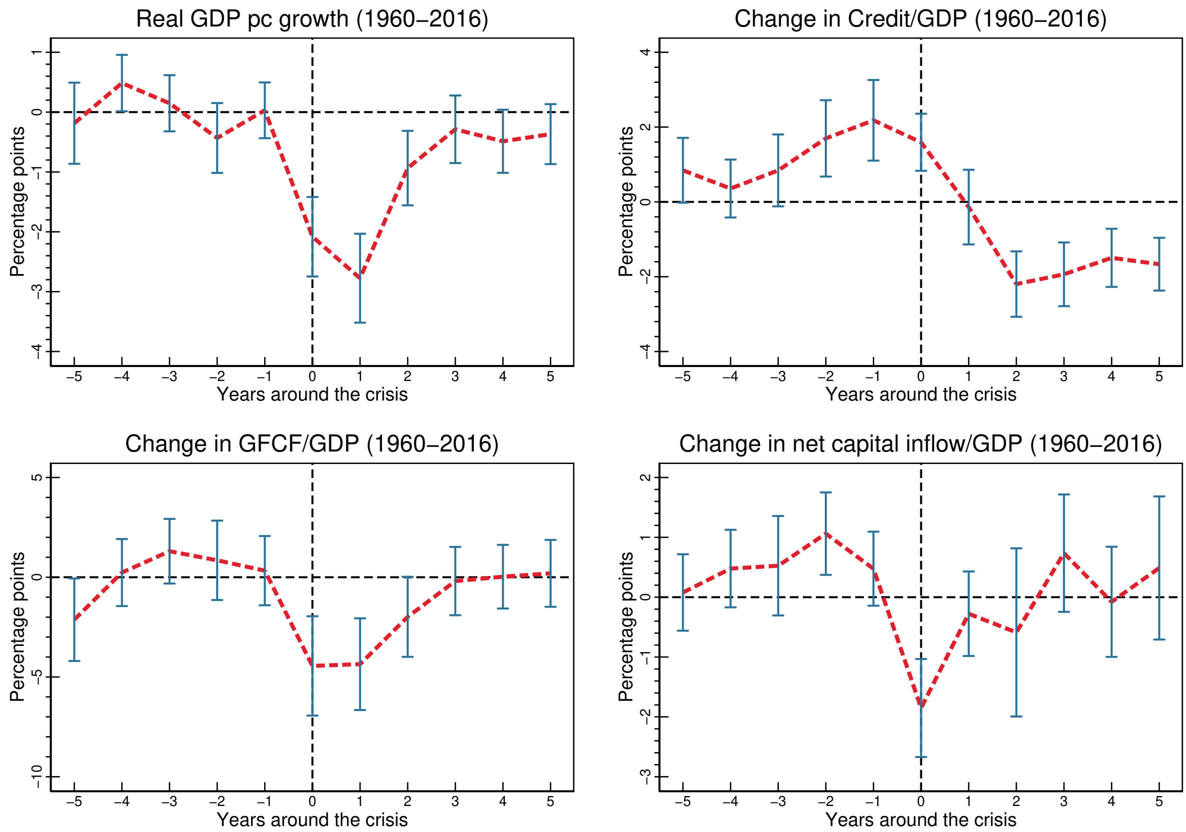
(Continued overleaf)

Table A-1: Sample Makeup (continued)

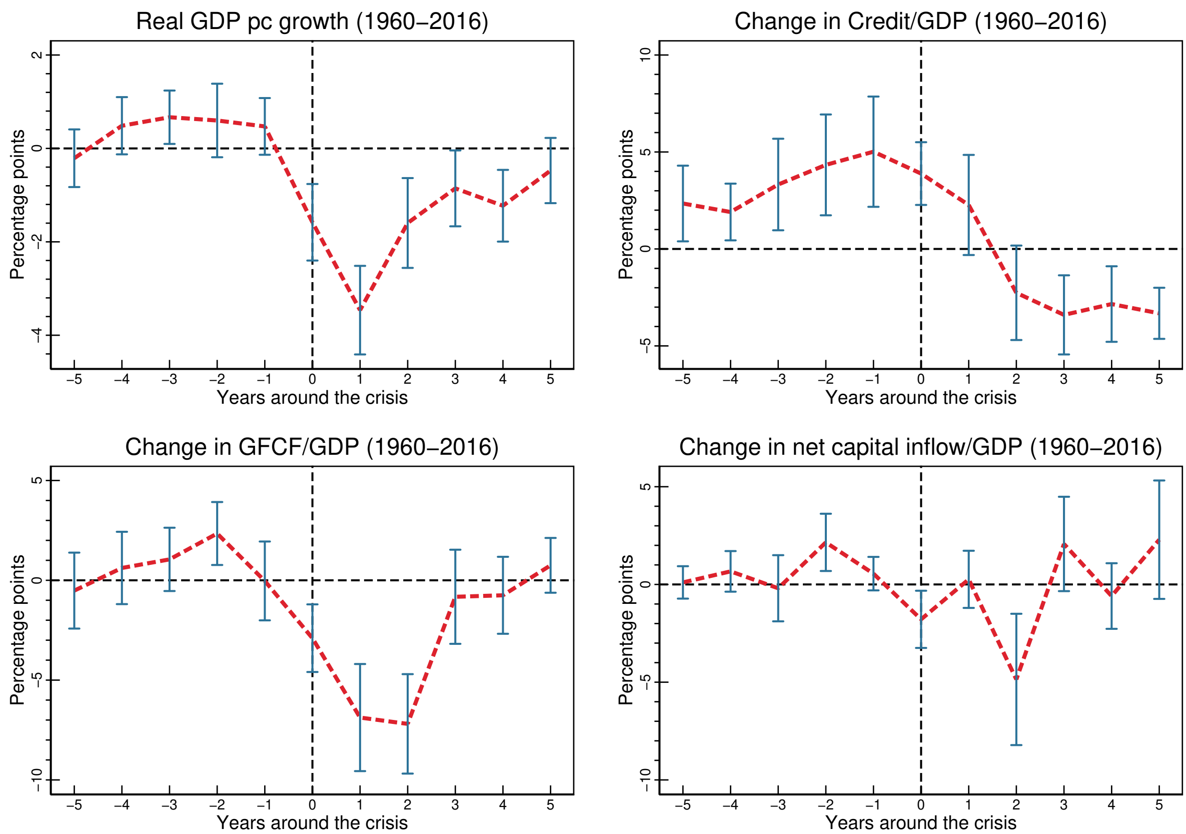
	ISO	Country	Start	End	Obs	Miss	GDP pc (2008 US$)			Private Credit/GDP					'Adv. Countries'				'Developing Countries'			
							Start	End	Δpa	Start	End	Δpa	Min	Max	92	C	119	C	34-47	34-65	47-65	47-92
131	SVN	Slovenia	1991	2016	26	0	14135	24552	400.6	35	47	0.47	19	85		×		×				12
132	SWE	Sweden	1960	2016	57	0	18050	56789	679.6	40	125	1.49	39	129	22		10					
133	SWZ	Eswatini	1970	2016	47	0	1226	4663	73.1	8	20	0.26	7	21								
134	SYC	Sychelles	1976	2012	35	2	5078	12000	187.1	19	24	0.15	9	30								
135	TCD	Tchad	1982	2015	33	1	418	957	15.8	11	8	-0.06	2	18								
136	TGO	Togo	1980	2016	37	0	733	649	-2.3	27	36	0.25	13	36					1	1		
137	THA	Thailand	1964	2016	53	0	662	5916	99.1	14	145	2.47	14	163	19		11					
138	TJK	Tajikistan	1998	2016	19	0	381	976	31.3	11	19	0.40	10	24								
139	TLS	East Timor	2002	2016	15	0	667	923	17.1	1	8	0.42	1	8								
140	TON	Tonga	1981	2012	32	0	2206	3730	47.6	12	32	0.60	12	52		×		×		15	3	3
141	TUN	Tunisia	1965	2016	48	4	1113	4311	61.5	27	77	0.97	24	77		×		×				28
142	TUR	Turkey	1968	2016	49	0	4120	14063	202.9	17	65	0.97	11	65		×		×		8	5	5
143	TZA	Tanzania	1990	2016	27	0	516	904	14.4	15	14	-0.06	3	16								
144	UGA	Uganda	1982	2016	35	0	401	910	14.5	3	14	0.33	1	14								
145	UKR	Ukraine	1992	2016	25	0	3263	2904	-14.4	1	47	1.84	1	90		×		×				9
146	URY	Uruguay	1970	2016	47	0	5671	14124	179.9	7	29	0.46	6	61		×		×		8	4	4
147	USA	United States	1972	2016	45	0	24650	52556	620.1	89	179	2.00	85	196	33		22					
148	VEN	Venezuela	1960	2014	55	0	12457	14025	28.5	16	30	0.26	7	66		×		×				9
149	VNM	Vietnam	1995	2016	22	0	583	1753	53.1	18	114	4.36	17	114	5							
150	VUT	Vanuatu	1983	2014	32	0	2531	2853	10.1	29	69	1.24	26	69		×		×				6
151	ZAF	South Africa	1961	2016	56	0	4685	7477	49.8	19	143	2.22	18	147	24		14					
152	ZWE	Zimbabwe	1975	2016	39	3	1388	1224	-3.9	9	22	0.32	0	137	1		1					

Notes: We provide details on the 152 countries in the full sample of analysis, including Start and End Year of the country time series, the number of observations (Obs) and hence the number of missing observations (Miss). Real GDP pc is in US$ 2008 values for the first and final year of the country sample, Δpa refers to the average annual change in GDPpc over the country-specific sample period. We provide the same quantities for Credit/GDP, alongside with the minimum and maximum values. The final set of columns indicates a number of 'treated' samples: in the analysis 'Advanced Countries' we provide details on the number of observations in the 'higher' regime for the 92% and 119% cut-offs (the 'treated' relative to the 'untreated' observations in the 'treated countries' make up the first 'difference' of the Diff-in-Diff specification), alongside with the respective control samples ('C'), where we limit the presentation to the controls samples where credit/GDP peaks between 47 and 92% — all observations of a 'control' country enter the control sample (the second 'difference'), marked with ×. In the analysis of 'Developing Countries' we only present the number of observations in the treated sample for the four samples we analyse: 34-47% credit/GDP, 34-65%, 47-65% and 47-92%.

Figure A-1: Event Analysis — Banking Crises



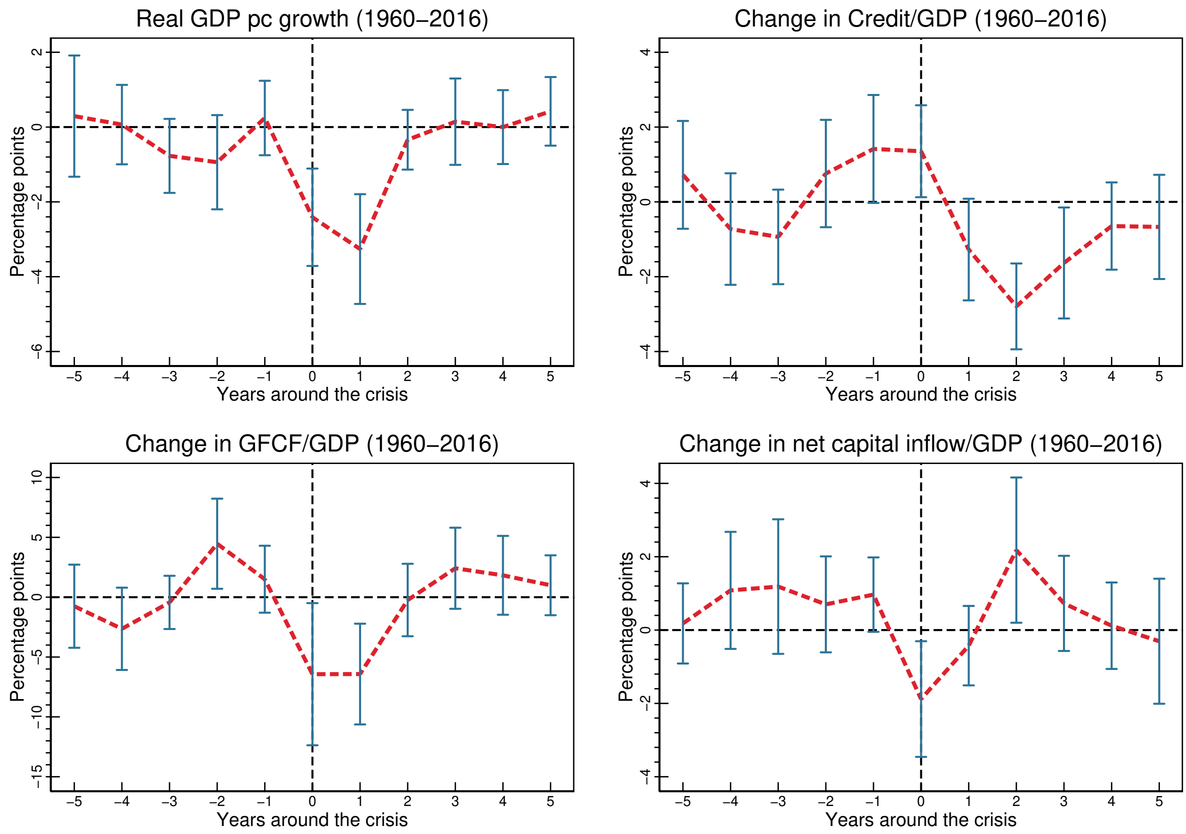
(a) 102 Countries which experienced a banking crisis



(b) 34 Highly financially developed countries (92% credit/GDP)

(continued overleaf)

Figure A-1: Event Analysis — Banking Crises (cont'd)

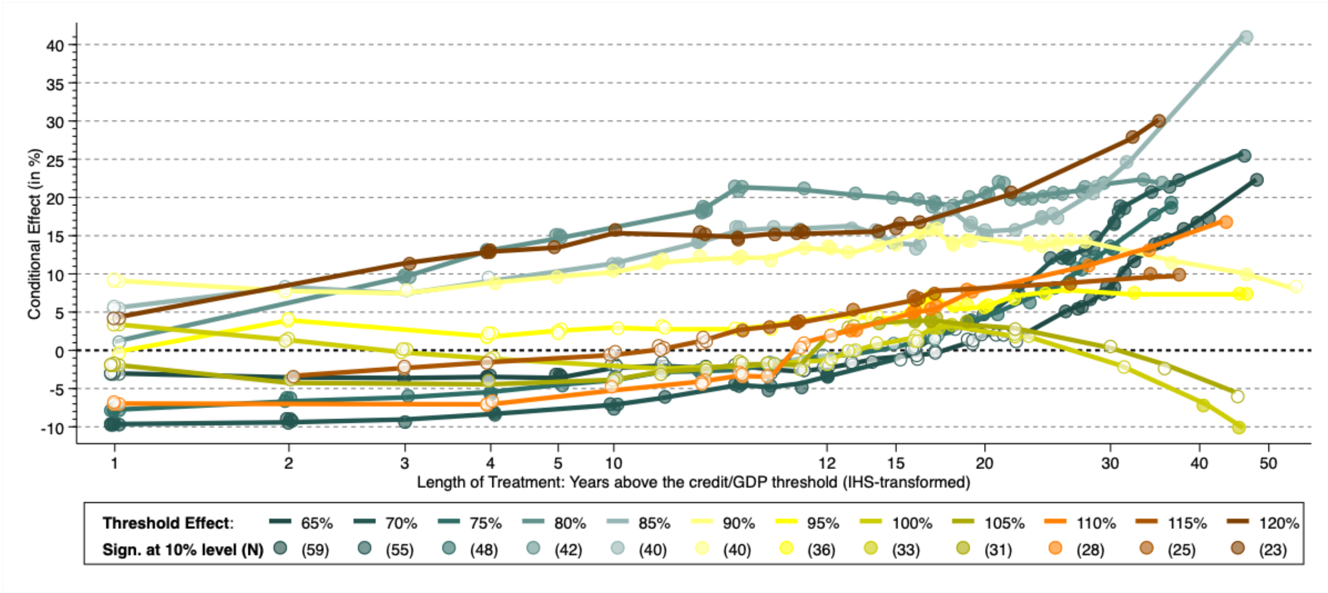


(c) 30 countries at intermediate levels of financial development (47% credit/GDP)

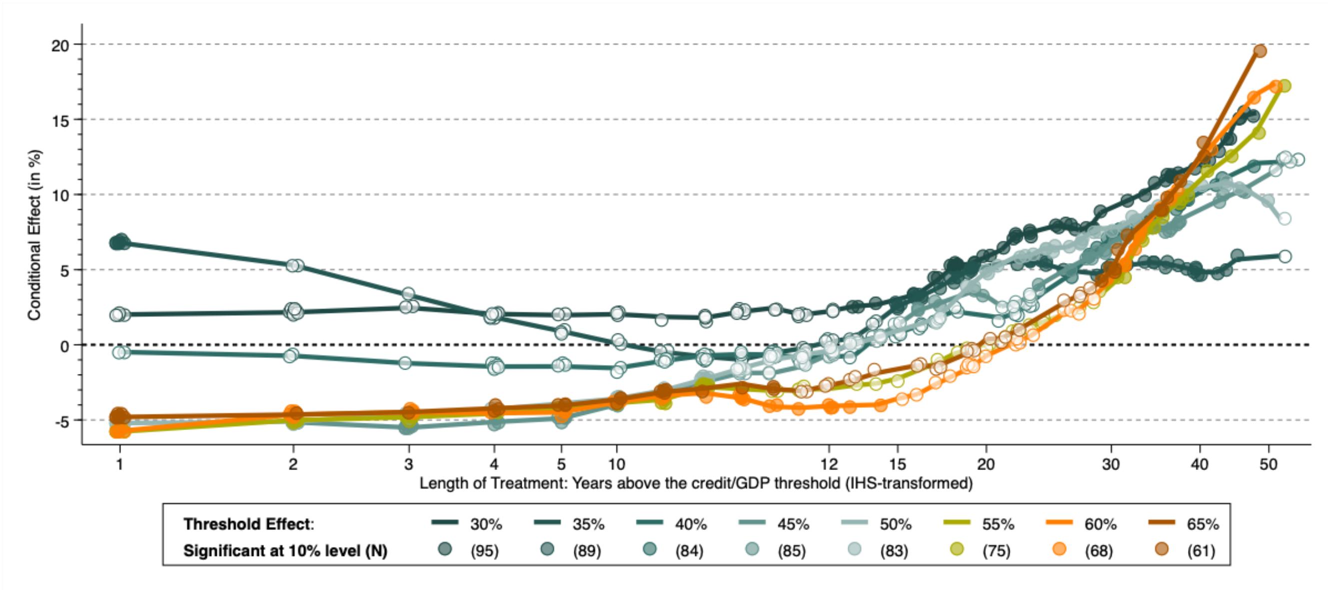
Notes: These plots present the results from event analyses in the eleven years surrounding banking crises, accounting for country fixed effects. The blue bars are the 90% confidence intervals, based on standard errors clustered at the country-level. Panel (a) is for all 102 countries (which experienced a banking crisis), panel (b) for countries which had credit/GDP in excess of 92% at one point in their sample period (dto.), panel (c) is for the 47% 'intermediate level' cut-off. Ongoing crisis years are omitted.

B Robustness Checks and Full Results

Figure B-1: Too much Finance — Alternative Cut-offs



(a) Threshold effects of 65% to 120% credit/GDP



(b) Threshold effects of 30% to 65% credit/GDP

Notes: Panel (a) is for the analysis of financial development at the top end of the distribution, broadly defined (k = 65-120% credit/GDP), where the control sample is made up of all those countries which have reached at least k−25% (so as to omit countries with very low financial development). Panel (b) for the analysis of financial development at the intermediate level (k = 35-65% credit/GDP), where the *treated* sample is curtailed to those countries which stayed below k+25%. The control sample is all countries which stayed below k. A filled (hollow) marker indicates statistical (in)significance at the 10% level. In the respective plot legend we report the number of countries in the treated sample in parentheses. All models presented include four estimated factors.

Table B-1: Too much Finance? PCDID Threshold regression ATET results (92% and 119% thresholds)

Higher Cutoff	92% Credit/GDP (90th pctile)					119% Credit/GDP (95th pctile)				
Lower cutoff	0	20	26	34	47	0	20	26	34	47
Percentile		40th	50th	60th	70th		40th	50th	60th	70th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Threshold Effect (ATET)	1.690	1.276	0.373	0.883	2.783	0.040	-0.084	-2.172	-2.387	0.496
	[1.514]	[1.592]	[1.754]	[1.906]	[2.005]	[2.031]	[2.064]	[1.692]	[1.872]	[1.857]
Inflation	0.390***	0.268*	0.190	0.238	0.152	0.455***	0.366**	0.530***	0.536***	0.241
	[0.141]	[0.138]	[0.176]	[0.193]	[0.192]	[0.148]	[0.164]	[0.178]	[0.201]	[0.252]
Trade Openness	22.491***	23.126***	31.875***	33.412***	42.447***	40.599***	40.487***	34.926***	40.454***	51.586***
	[6.901]	[6.754]	[7.254]	[7.567]	[8.691]	[10.385]	[9.721]	[8.606]	[9.612]	[10.387]
Treated Countries	38	38	38	38	38	24	24	24	24	24
Treated Observations	1678	1678	1678	1678	1678	1157	1157	1157	1157	1157
Share above threshold	0.34	0.34	0.34	0.34	0.34	0.23	0.23	0.23	0.23	0.23
Wald test controls (p)	0.27	0.31	0.22	0.28	0.32	0.08	0.33	0.13	0.06	0.21
Control Countries	101	89	77	65	48	115	103	91	79	62
Control Observations	3667	3279	2868	2327	1688	4188	3800	3389	2848	2209
Alternative Factor specifications: Coefficient for Threshold Effect										
1 Factor	2.621†	2.605†	2.624†	3.045†	4.496†	3.457	3.538	3.641	3.693	3.772
2 Factors	-0.426	-0.284	-0.454	0.018	2.726†	3.460	2.135	1.356	1.748	4.383
3 Factors	0.580	0.550	0.929	-0.290	3.388	1.486†	-0.115	-0.503	-0.125	3.282†
4 Factor	1.690	1.276	0.373	0.883	2.783	0.040†	-0.084	-2.172	-2.387†	0.496
5 Factors	0.287	0.345	0.236	1.858	3.919*	-1.591	-0.364	-2.818	-2.172	1.621†
6 Factors	1.035	1.578	-0.519	1.612	2.043	-0.401	-0.511	-2.192	0.343†	2.328*†

Notes: We present robust means for the PCDID country estimates in the 'treated' sample of countries, where treatment is defined by having overcome a threshold of 92% or 119% of credit/GDP, respectively. The estimates here are averages across heterogeneous treatments in terms of length of treatment and potential cross-country heterogeneity. Within each block of results we vary the control sample for this difference-in-difference estimator, by setting a second, lower, threshold for the 40th, 50th, 60th or 70th percentile of the credit/GDP distribution. The results use data for 1960 to 2016 and include four common factors estimated from the two control samples, in a lower panel of the table we present the threshold estimates for alternative factor specifications. † indicates where specifications fail a Wald test (10% level) that the additional controls are jointly statistically insignificant in a heterogeneous parameter linear probability model of the 'too much finance' dummy regressed on the controls and estimated factors from the control sample. For the model reported in detail the p-value for this test is provided in the table.

Table B-2: Finance for Development? PCDID Threshold regression ATET results (34% Credit/GDP)

	34-47% Credit/GDP (60th-70th pctile)				**34-65% Credit/GDP (60th-80th pctile)**			
Lower Cutoff	0	16	20	26	0	16	20	26
Percentile		30th	40th	50th		30th	40th	50th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Threshold Effect (ATET)	3.180	3.385	4.664*	3.041	2.107	2.710*	3.955**	3.104*
	[2.603]	[2.662]	[2.578]	[2.661]	[1.539]	[1.605]	[1.659]	[1.710]
Inflation	0.007	-0.050	-0.059*	0.016	-0.054	-0.108***	-0.112**	-0.133***
	[0.058]	[0.047]	[0.036]	[0.036]	[0.040]	[0.041]	[0.045]	[0.043]
Trade Openness	14.752**	16.097**	18.812**	20.098**	19.516***	22.052***	23.351***	25.949***
	[6.464]	[7.084]	[7.381]	[8.144]	[3.987]	[4.160]	[4.327]	[4.811]
Treated Countries	18	18	18	18	42	42	42	42
Treated Observations	661	661	661	661	1567	1567	1567	1567
Wald test controls (p)	0.25	0.47	0.50	0.67	0.20	0.42	0.39	0.53
Control Countries	36	28	24	12	36	28	24	12
Control Observations	1340	1124	952	541	1340	1124	952	541
Alternative Factor specifications: Coefficient for Threshold Effect								
1 Factor	2.713	3.129	3.435	3.711	2.919	3.572*	3.801**	3.704*
2 Factors	1.622	2.665	1.600	3.554	1.425	2.674	1.746	3.574*
3 Factors	1.518	2.259	2.855	4.560*	1.560	2.281	3.184*	3.439*
4 Factor	3.180	3.385	4.664*	3.041	2.107	2.710*	3.955**	3.104*
5 Factors	4.252*	2.953	4.000*	4.797**	2.542*	3.002**	3.903***	4.053**
6 Factors	4.477*	2.523	3.480	4.837*	2.814*	2.794**	3.642**	3.987**

Notes: We present robust means for the PCDID country estimates in the 'treated' sample of countries, where treatment is defined by having overcome a threshold of 34% of credit/GDP ('intermediate level of financial development'). Within each block of results we vary the control sample for this difference-in-difference estimator, by setting a second, lower, threshold for the 30th, 40th, or 50th percentile of the credit/GDP distribution. See Table B-1 for further details. Note that no specification rejected the Wald test.

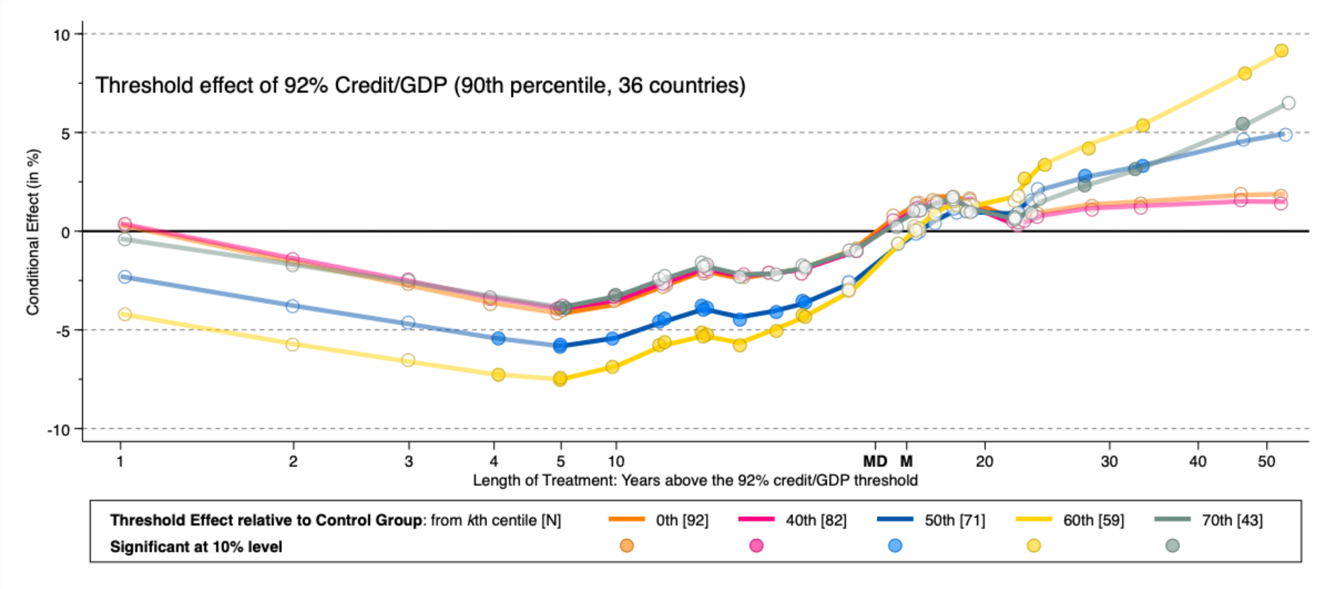
Table B-3: Finance for Development? PCDID Threshold regression ATET results (47% Credit/GDP)

Higher Cutoff	**47-65% Credit/GDP (70th-80th pctile)**					**47-92% Credit/GDP (70th-90th pctile)**				
Lower Cutoff	0	16	20	26	34	0	16	20	26	34
Percentile		30th	40th	50th	60th		30th	40th	50th	60th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Threshold Effect (ATET)	-0.849	-0.754	-0.772	0.069	-0.950	-0.865	-0.366	-0.477	0.767	-0.566
	[0.969]	[0.939]	[0.959]	[1.131]	[1.336]	[0.962]	[0.995]	[1.017]	[1.187]	[1.039]
Inflation	-0.009	-0.060	-0.065	-0.083	-0.098	-0.001	-0.044	-0.048	-0.059	0.009
	[0.050]	[0.060]	[0.062]	[0.063]	[0.089]	[0.048]	[0.051]	[0.046]	[0.042]	[0.078]
Trade Openness	21.394***	21.606***	20.781***	21.862***	24.998***	20.241***	20.903***	20.378***	22.180***	23.627***
	[4.083]	[4.021]	[4.193]	[3.991]	[5.484]	[2.975]	[2.774]	[2.873]	[3.046]	[3.958]
Treated Countries	26	26	26	26	26	47	47	47	47	47
Treated Observations	941	941	941	941	941	1666	1666	1666	1666	1666
Wald test controls (p)	0.01	0.00	0.00	0.01	0.07	0.14	0.22	0.21	0.17	0.15
Control Countries	101	89	77	65	48	115	103	91	79	62
Control Countries	54	46	42	30	18	54	46	42	30	18
Control Observations	2001	1785	1613	1202	661	2001	1785	1613	1202	661
Alternative Factor specifications: Coefficient for Threshold Effect										
1 Factor	1.757†	1.523†	1.758†	1.290†	3.354*†	2.172†	2.225†	2.382†	2.030†	3.317*†
2 Factors	1.231†	1.080†	0.898†	-0.301†	0.558†	1.037†	0.786†	0.695†	0.043†	0.704
3 Factors	-0.185†	0.075†	-0.009†	-0.590†	1.362†	-0.094	0.485	0.157	0.195	0.942
4 Factors	-0.849†	-0.754†	-0.772†	0.069†	-0.950†	-0.865	-0.366	-0.477	0.767	-0.566
5 Factors	-0.739†	-0.867†	-0.916†	0.365†	0.476	-0.237	0.069	-0.220	0.597	-0.046†
6 Factors	-0.065†	-0.274†	-0.122†	0.949†	-0.210	0.303	0.766	0.717	1.216	-0.133

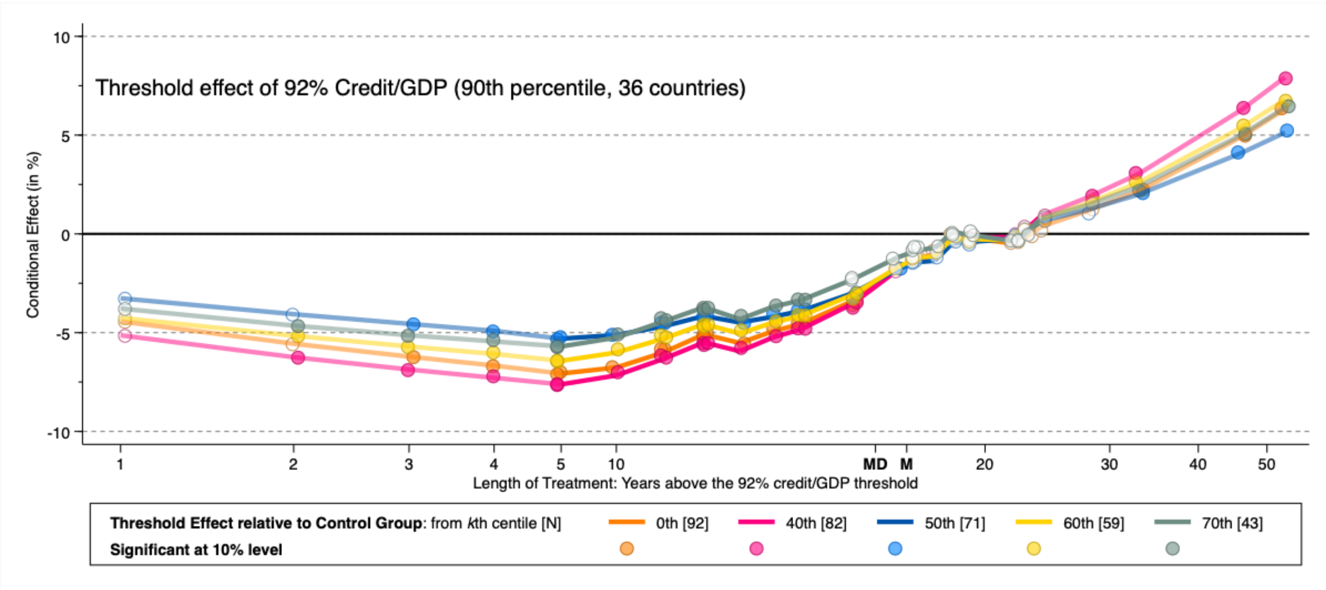
Notes: We present robust means for the PCDID country estimates in the 'treated' sample of countries, where treatment is defined by having overcome a threshold of 47% of credit/GDP ('intermediate level of financial development'). The estimates here are averages across heterogeneous treatments in terms of length of treatment and potential cross-country heterogeneity. Within each block of results we vary the control sample for this difference-in-difference estimator, by setting a second, lower, threshold for the 30th, 40th, 50th or 60th percentile of the credit/GDP distribution. The main results use data for 1960 to 2016 and include four common factors estimated from the two control samples, in a lower panel of the table we present the threshold estimates for alternative factor specifications (1-6 factors).

C PWT Production Function. . . or Not

Figure C-1: Too much Finance — Production Functions (or not) Using PWT Data



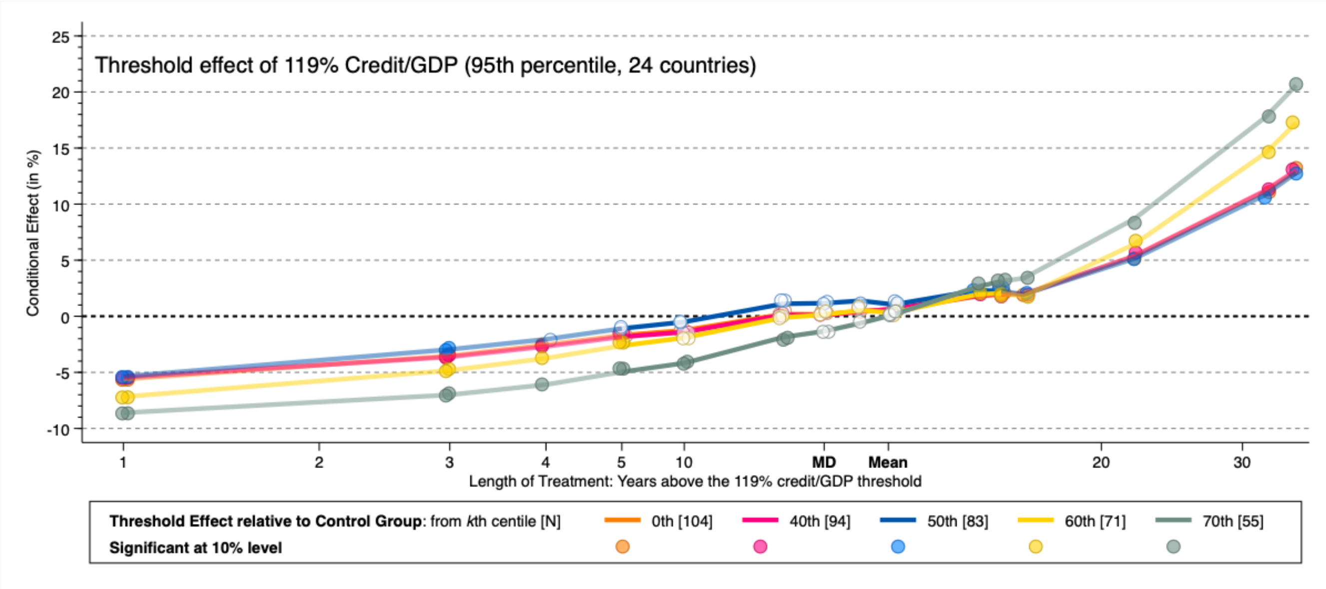
(a) Empirical Model without Capital Stock as Control — 92% Threshold



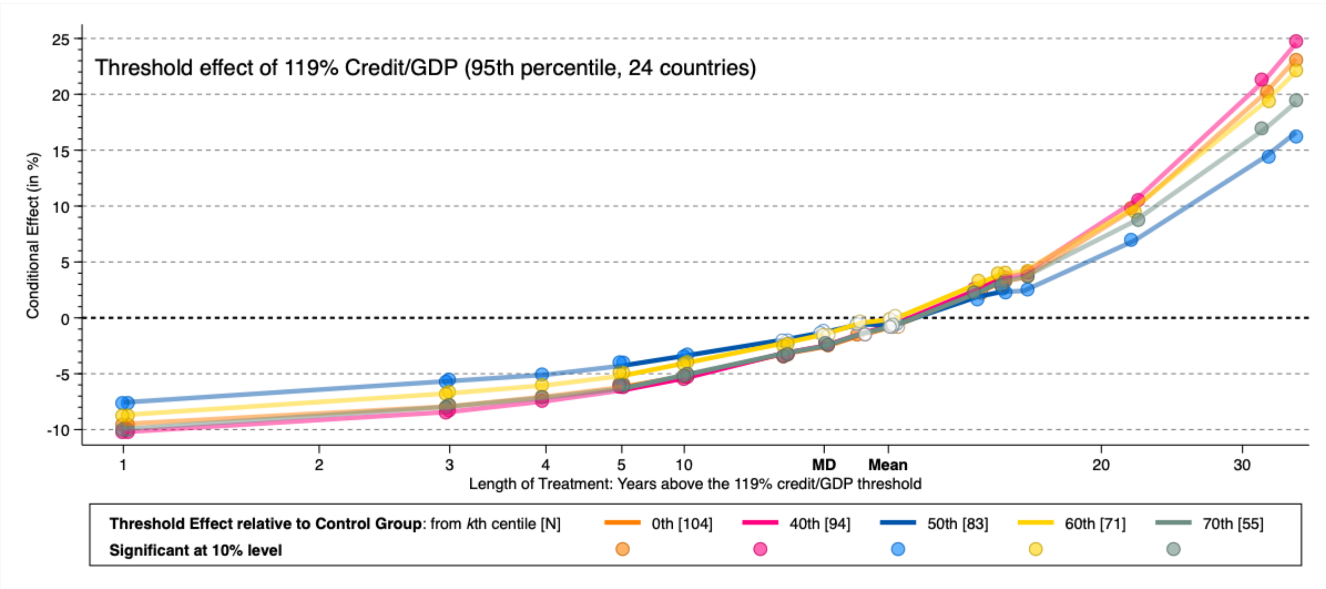
(b) Empirical Model with Capital Stock as Control (Production Function) — 92% Threshold

(Continued overleaf)

Figure C-1: Too much Finance — Production Functions (or not) Using PWT Data (cont'd)



(c) Empirical Model without Capital Stock as Control — 119% Threshold

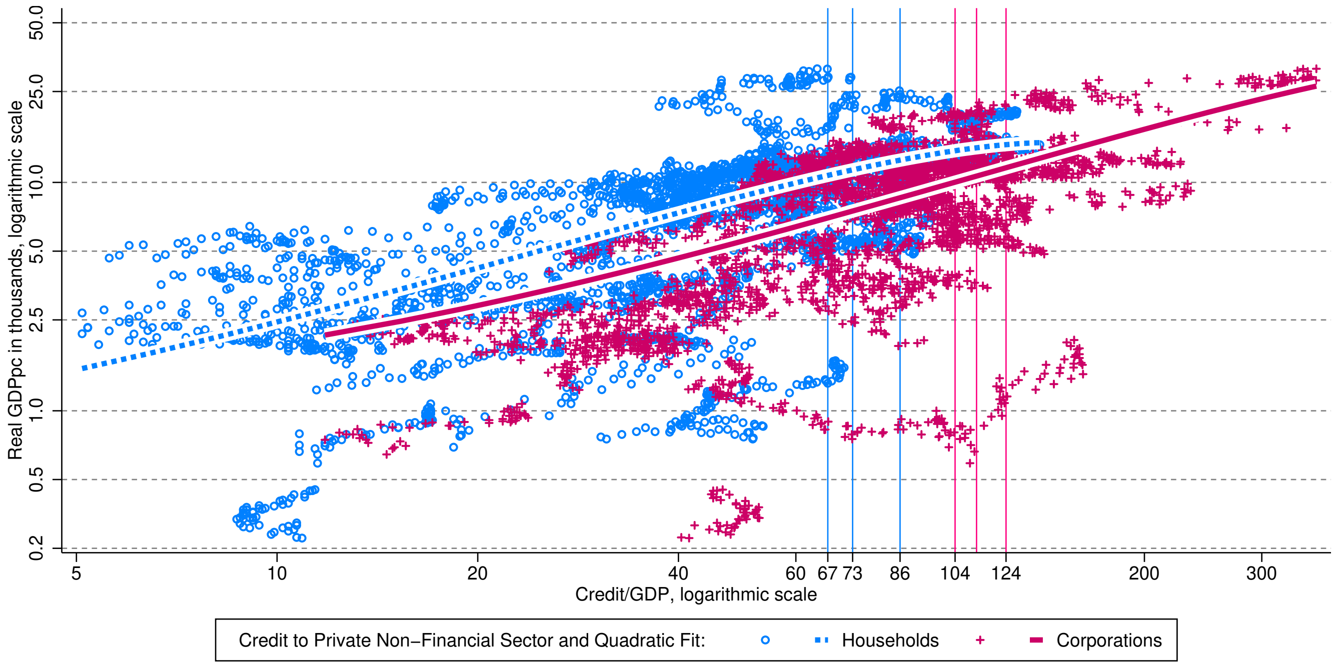


(d) Empirical Model with Capital Stock as Control (Production Function) — 119% Threshold

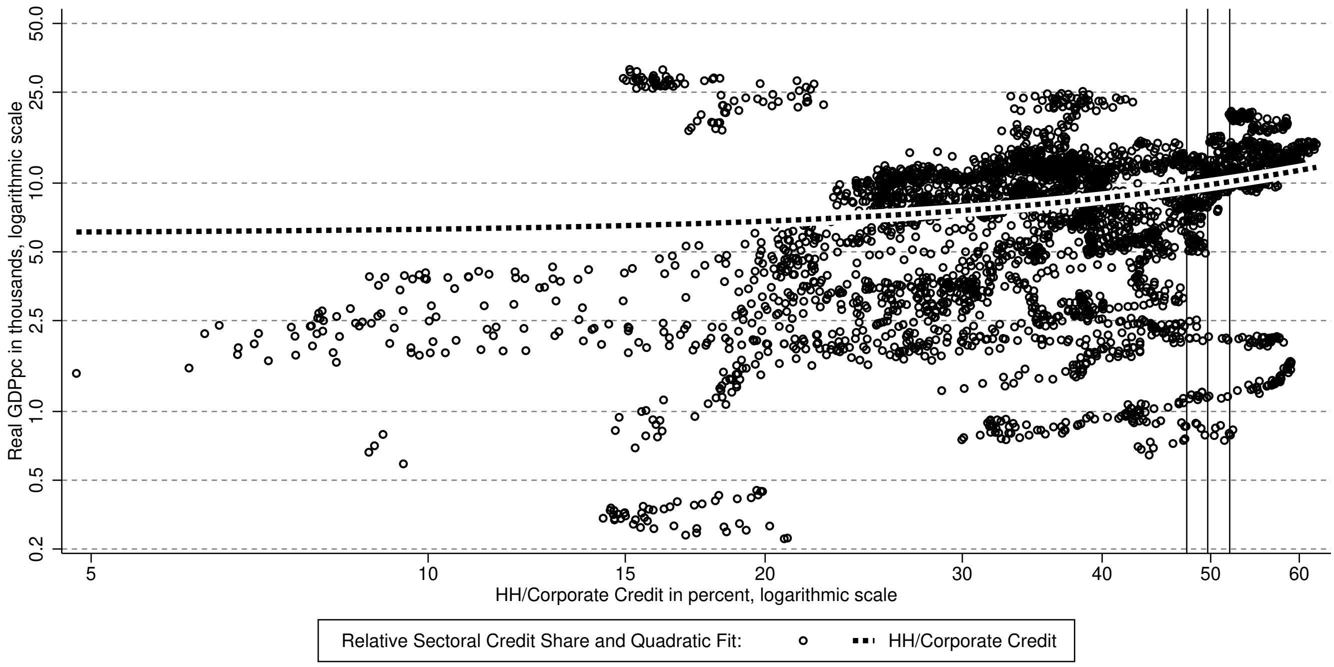
Notes: The figure presents mean estimates for a variety of Difference-in-Difference estimators; in contrast to the results in the maintext of the paper we here compare and contrast treatment effect results for a 'high financial development' dummy in a production function (Y/L regressed on K/L) using PWT data in (a) and (c) with an alternative specification without K/L as additional control in (b) and (d). Trade openness and inflation are included as controls in all models. In each plot we consider a number of alternative counterfactuals (control groups), by dropping countries with very low financial development (below 40th, 50th, 60th and 70th percentile of the credit/GDP distribution). The first plot, marked 0th percentile, is for a control group which includes all countries which stayed below the credit/GDP threshold. A filled (hollow) marker indicates statistical (in)significance at the 10% level. Mean and median length of treatment and treatment sample size are indicated in the graph.

D Distinguishing Household and Firm Credit

Figure D-1: Too much Finance? Quarterly Data for Household and Corporate Credit



(a) Income per capita and Sectoral Credit



(b) Income per capita and Relative Sectoral Credit Allocation

Notes: These are scatter plots for log real GDP pc (in thousands of US$ in 2010 values) and, in panel (a) the log of household credit/GDP (blue circles) and log of corporate (non-financial firm) credit/GDP (pink plus signs), as well as in panel (b) the log of household to corporate credit (in percent). The fitted lines are constructed using quadratic regression models, for the household credit data in panel (a) observations <5% credit/GDP are omitted to ease illustration.

Table D-1: Sample make-up — Quarterly Credit Data for HH and Corporations

					Household Credit/GDP			Firm Credit/GDP		
Country	ISO	Obs	Start	End	67%	73%	86%	104%	112%	124%
Australia	AUS	111	1991	2018	76	69	61	C	C	C
Austria	AUT	92	1995	2018	C	C	C	C	C	C
Belgium	BEL	87	1997	2018	C	C	C	72	20	43
Brazil	BRA	91	1996	2018	C	C	C	C	C	C
Canada	CAN	111	1991	2018	57	48	38	15	3	
Switzerland	CHE	76	1999	2018				22	28	
Chile	CHL	64	2002	2018	C	C	C	2	C	C
China	CHN	51	2006	2018	C	C	C	43	51	27
Colombia	COL	55	2005	2018	C	C	C	C	C	C
Czech Republic	CZE	92	1995	2018	C	C	C	C	C	C
Germany	DEU	111	1991	2018	30	C	C	C	C	C
Denmark	DNK	95	1995	2018		92	71	44	46	
Spain	ESP	95	1995	2018	43	34	C	42	25	20
Finland	FIN	111	1991	2018	2	C	C	47	68	3
France	FRA	111	1991	2018	C	C	C	72	44	28
United Kingdom	GBR	95	1995	2018	68	62	40	C	C	C
Greece	GRC	95	1995	2018	C	C	C	C	C	C
Hong Kong SAR	HKG	111	1991	2018	9	C	C	92	22	51
Hungary	HUN	95	1995	2018	C	C	C	C	C	C
Indonesia	IDN	43	2008	2018	C	C	C	C	C	C
India	IND	46	2007	2018	C	C	C	C	C	C
Ireland	IRL	67	2002	2018	43	39	33	50	44	46
Israel	ISR	95	1995	2018	C	C	C	C	C	C
Italy	ITA	95	1995	2018	C	C	C	C	C	C
Japan	JPN	99	1994	2018	41	C	C	53	36	25
Korea	KOR	111	1991	2018	50	42	13	16	2	
Luxembourg	LUX	67	2002	2018	4	C	C			
Mexico	MEX	96	1994	2018	C	C	C	C	C	C
Netherlands	NLD	95	1995	2018	87	85	73			68
Norway	NOR	111	1991	2018	61	41	18	80	64	44
New Zealand	NZL	82	1998	2018	62	57	49	5	C	C
Poland	POL	92	1995	2018	C	C	C	C	C	C
Portugal	PRT	95	1995	2018	64	50	25	52	37	22
Russia	RUS	63	2003	2018	C	C	C	C	C	C
Saudi Arabia	SAU	55	2005	2018	C	C	C	C	C	C
Singapore	SGP	111	1991	2018	C	C	C	17	6	
Sweden	SWE	103	1993	2018	42	37	8	68	47	44
Thailand	THA	103	1993	2018	17	C	C	5	1	
Turkey	TUR	83	1998	2018	C	C	C	C	C	C
United States	USA	111	1991	2018	79	69	32	C	C	C
South Africa	ZAF	43	2008	2018	C	C	C	C	C	C

Notes: The table indicates the full sample make-up for the 41 countries with quarterly data. The columns in the right part indicate the number of quarters a country was above the indicated thresholds ('in treatment'), with 'C' indicating that the country never breached the threshold and hence is part of the control sample.

Table D-2: Too much Finance? PCDID Threshold regression ATET results for Household and Firm Credit

	Household Credit/GDP			Firm Credit/GDP			Share of HH/Total Credit		
Cutoff	67%	73%	86%	104%	112%	124%	48%	50%	52%
Percentile	80th	85th	90th	80th	85th	90th	80th	85th	90th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Threshold Effect (ATET)	12.925***	13.748***	4.073	2.014	1.670	0.039	3.319**	2.904*	1.521
	[3.744]	[3.733]	[4.446]	[1.686]	[1.497]	[1.129]	[1.683]	[1.504]	[0.973]
Export/Trade	-22.055	-0.438	0.391	-1.418	-19.089	-30.026	-23.535	-16.017	-16.856
	[52.158]	[38.991]	[71.676]	[36.222]	[41.801]	[21.448]	[39.183]	[21.344]	[17.252]
Inflation	-1.210***	-1.065*	-1.595**	0.545*	0.523*	0.253	-0.642**	-1.479***	-0.775**
	[0.419]	[0.565]	[0.641]	[0.281]	[0.310]	[0.281]	[0.283]	[0.445]	[0.352]
Treated Countries	18	13	12	19	17	12	10	10	10
Treated Observations	1789	1282	1187	1794	1648	1136	890	905	870
Wald test controls (p)	0.53	0.03	0.05	0.29	0.03	0.11	0.60	0.23	0.00
Control Countries	21	27	28	20	22	28	27	30	31
Control Observations	1655	2257	2352	1659	1805	2412	2332	2634	2745
Alternative Factor specifications: Coefficient for Threshold Effect									
1 Factor	12.666***	13.067***†	7.352	0.288	-0.236†	0.298†	4.871	2.806*	1.903**
2 Factors	13.150***	9.891**†	5.347	-0.143	0.138†	1.743†	2.986	3.891*	2.320†
3 Factors	13.514***	14.903***†	4.656	1.584	0.699†	2.995***†	3.349	3.383**	1.695**†
4 Factors	12.925***	13.748***†	4.073†	2.014	1.670†	0.039	3.319**	2.904*	1.521†
5 Factors	11.058***	8.017***†	4.914*	2.692**†	1.424†	1.593†	4.432*	2.175	0.276†
6 Factors	10.915***	7.686***†	4.909**	2.732**†	2.249†	2.212	3.662**	1.279	0.294†

Notes: We present robust means for the PCDID country estimates in the 'treated' sample of countries, where treatment is defined by having overcome a threshold of the equivalent of the 80th, 85th and 90th percentile of the 'credit'/GDP, respectively — 'credit' here captures either 'household' or 'firm' credit (more formally: ' credit to households and non-profit institutions serving households' and 'credit to non-financial corporations'. Columns (7)-(9) use the share of Household to Total (Household and Firm) Credit as the threshold variable. The estimates are averages across heterogeneous treatments in terms of length of treatment and potential cross-country heterogeneity. The control sample for this difference-in-difference estimator is always the set of countries which stayed below the cut-off. The results use quarterly data for 1991Q1 to 2018Q3 and include four common factors estimated from the control samples, in a lower panel of the table we present the threshold estimates for alternative factor specifications. † indicates where specifications fail a Wald test (10% level) that the additional controls are jointly statistically insignificant in a heterogeneous parameter linear probability model of the 'too much finance' dummy regressed on the controls and estimated factors from the control sample. For the model reported in detail the p-value for this test is provided in the table.