Finance, Growth, and Crises*

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Abstract: We study the causal implications of high levels of financial deepening for economic development and banking crises in a panel of countries over the past seven decades. We adopt a factor-augmented heterogeneous difference-in-difference estimator and find, in contrast to the existing literature, that very high levels of financial development do not lead to lower long-term economic growth or a higher likelihood of banking crises associated with 'credit booms gone bust' cycles or excessive capital inflows. We submit this null result to a battery of tests adopting alternative specifications and robustness checks. Additional analysis of quarterly data on household and corporate credit suggests that our findings are not driven by aggregation bias.

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

JEL codes: F43, G01, G21, O40

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1 Introduction

The link between finance and growth has been studied extensively[1] and the various beneficial aspects of finance for development are well-known (Schumpeter 1912, Greenwood & Jovanovic 1990, Beck, Levine & Loayza 2000, Levine et al. 2000, Levine 2005). In the wake of the 2007/8 Global Financial Crisis this literature experienced a paradigm shift (Carré & L'Œillet 2018), whereby widespread agreement of a strictly positive and linear relationship between finance and development was replaced by the new consensus of a more complex, likely concave relationship, giving rise to concerns over 'too much finance'. On the darker side of financial development (Loayza et al. 2018) there are two main worries: firstly, finance potentially crowds out productive activity (Rioja & Valev 2004, Cecchetti & Kharroubi 2012, Law & Singh 2014, Arcand et al. 2015). 'Excessive' financial deepening may advance sectors with lower growth potential (e.g. household rather than firm credit, see Beck et al. 2009, Jordà et al. 2015, Müller & Verner 2023), and/or foster a human capital brain drain to vacuous but highly-paid finance jobs away from the pursuit of real economy activity (Popov 2018). Secondly, it may lead to increased susceptibility to financial crises (Demirgüç-Kunt & Detragiache 1998, Kaminsky & Reinhart 1999, Loayza & Rancière 2006, Rancière et al. 2006), where first-order determinants include 'credit booms gone bust' (Schularick & Taylor 2012, Müller & Verner 2023) and 'excessive' capital inflows (Reinhart & Rogoff 2013, Caballero 2016).

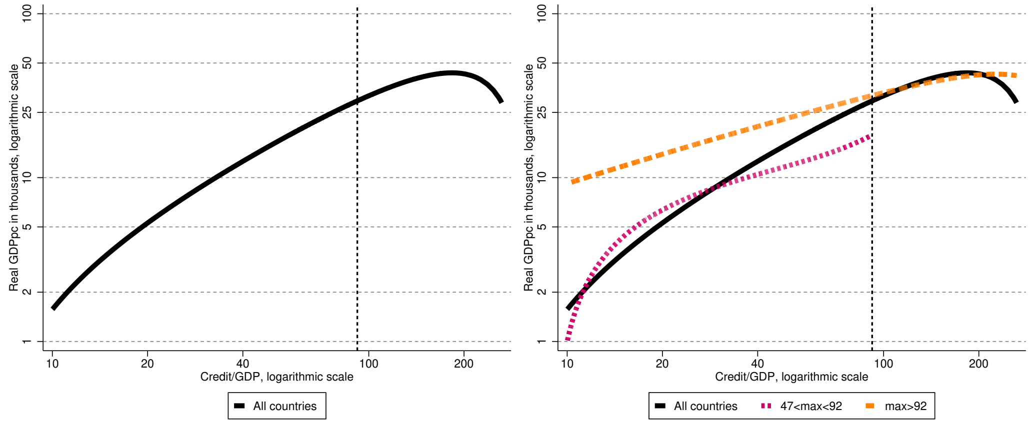
Few studies on financial development investigate growth and vulnerability in an integrated approach (Arcand et al. 2015, Rancière et al. 2006),[2] given that they concern different timings of effects: the link between finance and development should be viewed over the long term (Loayza

[1] Levine (2005), Carré & L'Œillet (2018), Loayza et al. (2018), and Popov (2018) provide comprehensive surveys.

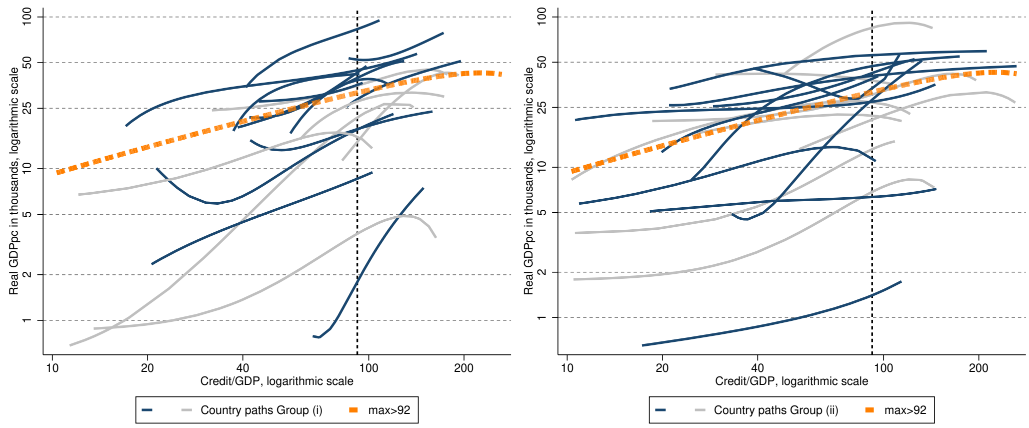
[2] Arcand et al. (2015) adopt a reduced form approach whereby their finance-growth model (levels and squared credit/GDP) is augmented with a crisis dummy and interaction terms. While their credit/GDP terms indicate a statistically significant concavity, the interaction terms between crises and financial development are insignificant. Rancière et al. (2006), whose analysis pre-dates the 'too much finance' debate and hence does not include a nonlinearity, adopt a more structural approach: in a first step they model financial crises, while the second-step equation for per capita GDP growth incorporates financial liberalisation, a crisis dummy and the estimated hazard rates from the first step. They show that finance is positive and significant in both equations, their decomposition however suggests that growth dominates substantially, by an order of 5/1 to 7/1.

& Rancière 2006), while the analysis of banking crises investigates the short-run trigger function of determinants in an early warning system (EWS) approach (e.g. Bussière & Fratzscher 2006).

Figure 1: Visualising 'Too Much Finance' in Pooled and Heterogeneous Models



(a) Pooled Fractional Polynomial Plots



(b) Country-Specific Fractional Polynomial Plots

Notes: We present fractional polynomial plots for per capita GDP (y) and credit/GDP (x) (both in logarithms). In (b) we distinguish those with 'excessive finance' above 92% (orange), and those with a finance peak between 47 and 92% (pink). In (c) and (d) we add country plots for 19 countries each — grey (navy) lines indicate country plots which do (not) conform with the 'too much finance' hypothesis (only 13 of 38 country plots conform).

In this paper we contribute to the 'too much finance' debate, modelling country experience of high levels of finance as a binary treatment and estimating treatment effects models. We do not take a stand on what constitutes 'excessive' levels of finance but consider the uniformity and robustness of results across a wide range of definitions (70% credit/GDP, 80%, 90%, etc). We further experiment with alternative sets of control countries to the 'excessive' finance economies.

Our investigation differs from related existing work in two important ways: First, we investigate the potential non-linearity of the finance-growth nexus in a heterogeneous parameter framework, where each country can have its own equilibrium relationship. The basic rationale for this modelling choice is illustrated in Figure 1 where we present fractional polynomial regression lines fitting GDP per capita on credit/GDP (both in logarithms to ease presentation): evidence of a concave non-linearity in line with a 'too much finance' effect when studying all countries in panel (a) is somewhat undermined in panel (b) where we compare countries with peak finance in excess of 92% of credit/GDP to those with peaks between 47% and 92%. Studying individual 'high finance' countries (38 with peaks exceeding 92% credit/GDP) in panels (c) and (d) indicates that only just over one third (grey lines) display a *concave* relationship. Hence, using simple descriptive analysis, the move from homogeneity (a) to heterogeneity (c, d) seems to do away with the evidence for a substantial 'too much finance' effect *in the raw data* — below we present estimates from state-of-the-art causal analysis which underscores this 'null result.'[3]

We adopt an empirical implementation, the Principal Component Difference-in-Difference (PCDID) estimator (Chan & Kwok 2022), which augments the treatment regression with common factors estimated from a control sample regression. Like in the pooled country fixed effects model, where unobserved time-*invariant* country effects (correlated with the other regressors) can be proxied by country dummies, the empirical implementations using a common factor framework create proxies that capture the unobserved time-*varying* heterogeneity (correlated with the other regressors) in the estimation equation (e.g. Pesaran 2006, Bai 2009, Gobillon & Magnac 2016, Xu 2017, Brown et al. 2023). Hence, a treatment regression, where a treatment variable like 'excessive' finance is likely endogenous to omitted unobservables, is augmented with *proxies for*

[3] In Appendix C we investigate the standard pooled models adopted in the literature on 'too much finance' using two-way fixed effects and popular dynamic panel estimators (Arellano & Bond 1991, Blundell & Bond 1998). While our diagnostics for the 1960-2015 data across 140 countries are not always favourable, we find strong evidence for a concave relationship in support of the 'too much finance' hypothesis.

these omitted unobservables and can therefore avoid the standard omitted variable bias. In the PCDID the 'proxies' are factors estimated *from the control sample* and they can have different impact across countries, most importantly between treated and control samples.[4] We do not know what these factors represent (we can of course speculate), in the same way that no researcher can know what is captured by the country fixed effects in their pooled regression. Our empirical approach enables us to isolate the causal treatment effect of 'excessive' finance.[5]

Second, we reason that if high levels of financial deepening are indeed detrimental to economic prosperity, then one ought to account for the length of time spent in the 'danger zone'. It is to be expected that longer 'exposure' would be linked to worse economic performance. We adopt a multivariate smoothing procedure for the country-specific PCDID estimates to present our results: running line regressions (Royston & Cox 2005), which can further control for sample differences related to panel unbalancedness and multiple crossings of the 'too much finance' threshold. The smoothed estimates can then be presented in results plots against the number of years spent above the 'excessive' finance threshold. This makes for a straightforward visual check on the 'too much finance' hypothesis, which would predict worse economic outcomes with longer treatment time.

In implementing these novel specifications for data from the last seven decades we find no evidence for a 'too much finance' effect on economic development. Since this finding constitutes a 'null result' we expend considerable efforts to check its robustness, adopting a large range of 'too much finance' cut-offs, alternative control samples, alternative economic theory-based specifications, dynamic treatment effects models, and more granular definitions of credit (household vs. corporate finance). We further demonstrate that the heterogeneity in the 'too much finance' effect is not linked to 'deep determinants' such as a country's legal origin, culture, or geography.

[4]The factors represent the common trends and the parameter estimates on the factors ('factor loadings') can capture the extent to which these trends are parallel across countries or not (Brown et al. 2023).

[5]Specifying a heterogeneous treatment estimator allows us to by-pass the concerns debated in the recent microeconometric literature on pooled difference-in-difference estimators (De Chaisemartin & d'Haultfoeuille 2020, Goodman-Bacon 2021, Athey & Imbens 2022).

One concern about our empirical setup is the scenario of a nefarious effect of 'too much finance' leading to a financial crash, economic contraction, and a subsequent drop in financial development below the treatment threshold. By construction, the poor performance due to 'too much finance' would not be attributed to this 'treatment', but to the period below the threshold, and attenuate any 'too much finance' effect. We address this concern by studying banking crises. We argue that a link from excessive finance to increased financial fragility is most likely propagated through channels that are already identified as first-order 'triggers' for banking crises, namely 'credit booms gone bust' and excessive capital inflows. We extend the heterogeneous treatment model to the study of financial vulnerability in a simple but intuitive way. Our approach here is novel because we are among the first to employ a heterogeneous banking 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 marry this with our PCDID setup. We test whether experiencing elevated levels of finance change the *within-country* effects of the dominant crisis triggers. Our benchmark results ignoring the level of financial development confirm the narratives in the financial crisis literature. When we contrast the results *within* countries below and above the 'too much finance' threshold we commonly find the former to be the driving force of the increased crisis vulnerability, in contradiction to a role for excessive finance in exacerbating financial vulnerability.

The remainder of this paper is structured as follows: in the next section, we study the finance-growth nexus, also in the context of household versus corporate finance, and dig deeper into possible (immutable) drivers for our results (the 'deep determinants'). Section 3 turns to the investigation of banking crises. In both sections, we first introduce the data and methods used and then present empirical findings. Section 4 concludes.

2 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 (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. In the following, we describe our data and methodology (Section 2.1), present our results along with a large range of robustness checks (2.2). We conclude with an exploratory analysis of disaggregated credit data in a moderate sample of advanced and emerging economies (2.3).

2.1 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 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, trade as a share of GDP) are taken from the World Bank *World Development Indicators* — all (except inflation) are log-transformed and the income variable is further multiplied by 100: our treatment estimates provide the percentage effect of 'excessive' 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). In robustness analysis we effectively estimate production functions augmented with a 'too much finance' dummy (with and without capital stock), using Penn World Table (Feenstra et al. 2015, PWT v. 10) data — see Appendix E. Following some restrictions on a minimal number of observations,[6] the full sample covers close to 5,400 observations in 140 countries (1960-2016). See Appendix Table A-1 for sample make-up and descriptive statistics.

Regime Thresholds and Sample Make-up For our main results we adopt the full range of credit/GDP thresholds from 65% to 120% (in 5% steps). In extensions we adopt the 90th and 95th percentiles of the credit/GDP variable in the full 140-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 90% threshold found by Cecchetti & Kharroubi (2012), the 88% threshold found by Law & Singh (2014) and the 100% found by Arcand et al. (2015).[7]

We experiment with a range of practices to curtail the control sample: in the main results we use a simple rule that only economies with peak financial development below the threshold k but above $k-25$ percentage points of credit/GDP are included. We then consider a range of the 'lower' cut-offs in the extended analysis.

[6]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).

[7]For these two thresholds we observe 38 and 24 treated countries, respectively; 80% are high-income countries.

Finally, some more details on the sample makeup: first, who's in? Among the 38 treated countries analysed with the 92% credit/GDP cut-off, we count 26 OECD countries and 12 non-OECD countries.[8] The five countries with the longest number of years above the threshold are Japan, Switzerland, the US, the UK and South Africa; Italy, Estonia, Kuwait, Macao and Latvia have the shortest number of years.[9] 85% of country observations are for High-Income Countries, the remainder (except Viet Nam) for Upper Middle-Income Countries (World Bank classification). Second, who's out? What if countries are always above the 'too much finance' threshold? We discard them. For the 92% credit/GDP cut-off we only exclude a single country (Hong Kong). Third, who's in the control sample? Of the 48 control countries which satisfy the most restrictive condition (credit/GDP peak between 47% and 92%) we find all remaining OECD countries except Mexico, large emerging economies like Brazil, India, and the Philippines, as well as a host of post-Soviet Republics.[10] Country observations fall in equal shares into the High, Lower Middle and Upper Middle Income categories.

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*. We covered the intuition and basic mechanics of this approach in the introduction, and focus on a more technical discussion in the following. Using potential outcomes, the observed outcome of treatment D_{it} for panel unit i at time T_0 can be written as

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

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

[8] The full list of the latter is BHR, CHN, CYP, KWT, MAC, MLT, MUS, MYS, SGP, THA, VNM, ZAF.

[9] The full list of treated countries can be viewed in Appendix Table A-1, the column labelled '92'.

[10] The full list of all controls countries can be viewed in Appendix Table A-1, the column immediately to the right of that labelled '92' — to take into account control sample restrictions the country 'Max Credit/GDP' is in the column immediately to the left of that labelled '92'.

where the two indicator variables $\mathbf{1}_{\{\cdot\}}$ are for the treated panel unit and time period, respectively, Δ_{it} is the time-varying heterogeneous treatment effect, x is a vector of 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

$$y_{it} = \overline{\Delta}_i \mathbf{1}_{\{i \in E\}} \mathbf{1}_{\{t > T_{0i}\}} + \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \epsilon_{it}, \tag{3}$$

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 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:

[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.

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

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

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).

Assumptions and Testing 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 variables x are jointly insignificant predictors for the treatment:[12] they do not constitute 'bad controls'. Since the factors are estimated with error, there is potentially correlation between the errors of treated and control countries, which will bias the treatment estimate. This bias can be removed if we require that

[12]We carry out Wald tests for this assumption — see Appendix Tables E-1 and E-2.

$\sqrt{T}/N_c \rightarrow 0$, where T is the time series dimension and N_c is the number of control countries. Like every Difference-in-Difference model, the PCDID must satisfy a version of the 'parallel trend' assumption for causal identification:[13] we require that the expected factor loadings are the same between treated and control units, which is investigated using the Chan & Kwok (2022) Alpha test. Informally, we can think of this as asking whether the unobserved 'information' captured in the control sample is equally 'relevant' for the treated sample. The Alpha test is implemented in two steps: first, instead of extracting factors from the control sample residuals ($\hat{e}_{it}$), we compute their cross-section average, $\bar{\hat{e}}_{Ct}$; second, we enter this estimate into an auxiliary regression $y_{it} = b_{0i} + b'_{1i}x_{it} + \delta_i \mathbf{1}_{\{t>T_0\}} + a_i\bar{\hat{e}}_{Ct} + e_{it}$ for all countries i in the treated sample. The Alpha test then amounts to testing whether the mean group average $\hat{a}$ is equal to 1.[14] Appendix Tables E-1 and E-2 report the p-values for this test.

Conditional Local Mean Results The standard approach in the treatment effects literature is to report the ATET, $\hat{\delta}^{MG}$ — we do so in an Appendix. However, this ignores the length of time a country has spent in the higher regime. 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 number of 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[15] 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

[13]For more details see Section 4.4 of Chan & Kwok (2022) which addresses the untestability (Callaway & Sant'Anna 2021) of the parallel trend assumption in fully nonparametric settings. The test is referred to as a 'weak parallel trends' test.

[14]We use the Pesaran & Smith (1995) Mean Group estimator and the associated variance estimator.

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

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 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.[16]

2.2 'Too much Finance'?

Main Results In Panel (a) of Figure 2 we plot predictions from multivariate running line regressions for twelve alternative thresholds (from 65% to 120%).[17] For the time being, we focus on the dark-brown line and markers, which show the results for the 120% threshold, to explain how to interpret this and the following results graphs. The legend indicates that the dark-brown line is based on 23 treated countries.[18] This line represents the smoothed treatment effect (based on estimates from the PCDID regressions) of years spent in the 'higher' regime (shown on the x-axis) on per capita income (shown on the y-axis), relative to countries in the control sample.[19] We can hence read off the effect on income per capita of having experienced 'too much finance' for a total of, say, 5 or 15 or 30 years. In the present case the dark-brown line is near-monotonically upward-sloping, from around 5% for one year of 'too much finance' to 30% after around thirty years. The markers dotting the smoothed line indicate the predictions for individual countries in

[16]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'.

[17]Average treatment effects are presented in Appendix Table E-1.

[18]Note that in the graphs we present in panels (b) and (c) of Figure 2 we have a fixed number of treated countries and the legend reports the number of countries in the control sample.

[19]Control samples are defined as countries with a credit/GDP peak in the range from 25% below the threshold to the threshold value.

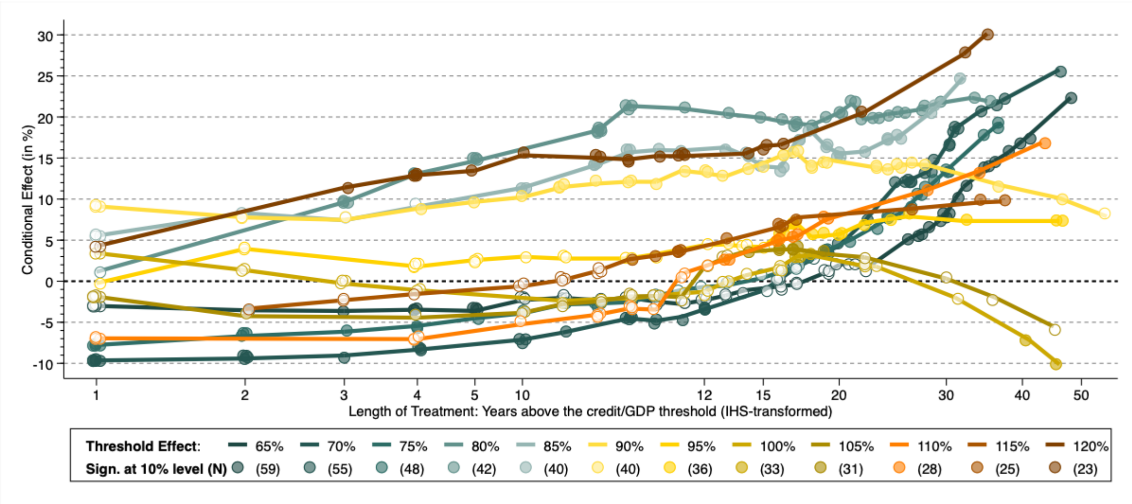
this treatment sample; we randomly perturb these markers so we can see, for instance, that two countries in our example of the 120% threshold have had one year of 'treatment', and two or three countries had four years. Individual markers are either hollow, like those for one or two years in treatment, which means that the effect is not statistically significantly different from zero, or they are filled (here in dark brown) to indicate that they are. All running line plots presented below follow this interpretation.

Considering now all smoothed lines for the twelve thresholds presented we can see that a number of line plots for comparatively low thresholds (marked in shades of teal) have negative significant effects on income in the first decade,[20] but thereafter slope upwards to yield positive and significant long-run effects. For virtually all other thresholds, we observe an evolution from low (negative or positive) and insignificant effects in early years to positive and significant effects beyond a dozen or so years. Strong evidence for a 'too much finance' effect would result in higher cut-offs being associated with clearly *negative* effects as the length of treatment increases — if anything the exact opposite of what we observe in the results.

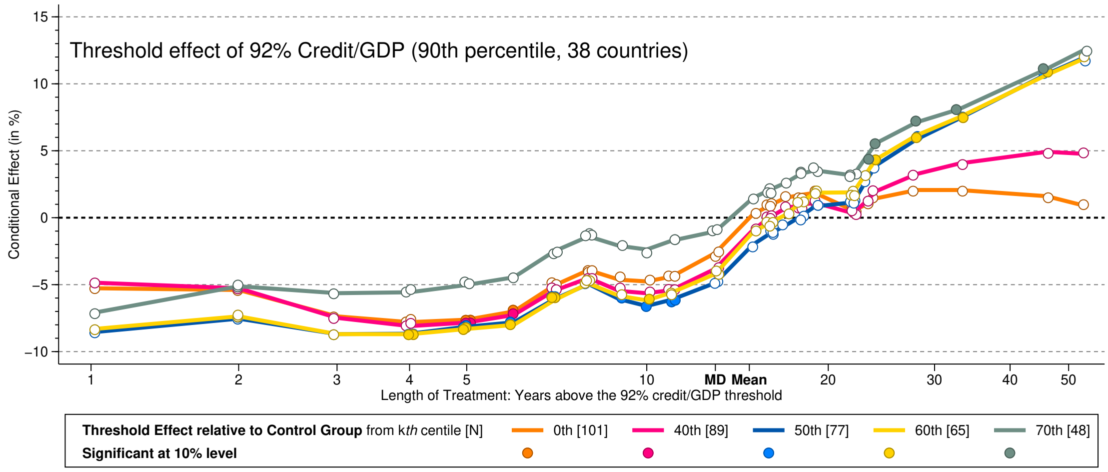
Naturally, there are multiple assumptions implicated in these results, prime amongst these the mechanical control sample cut-off, whereby the control sample is made up only of countries for which peak credit/GDP is up to 25 percentage points below the threshold studied. As the diagnostic tests presented in Appendix Table E-1 indicate, the Alpha test typically (but not uniformly) confirms the parallel trend assumption as valid in these specifications. In contrast, the Wald tests typically indicate that inflation and trade openness constitute 'bad controls'. The lower panel of Appendix Table E-1 highlights that adopting alternative factor augmentations (all results presented in the main part of the paper are based on augmentation with four estimated factors)

[20]This could either be explained by countries experiencing a credit surge or a negative shock to GDP, e.g. due to terms of trade deterioration, mechanically pushing them above the threshold into the 'treated' sample. Neither of these suggestions is about the impact of financial development on income per capita.

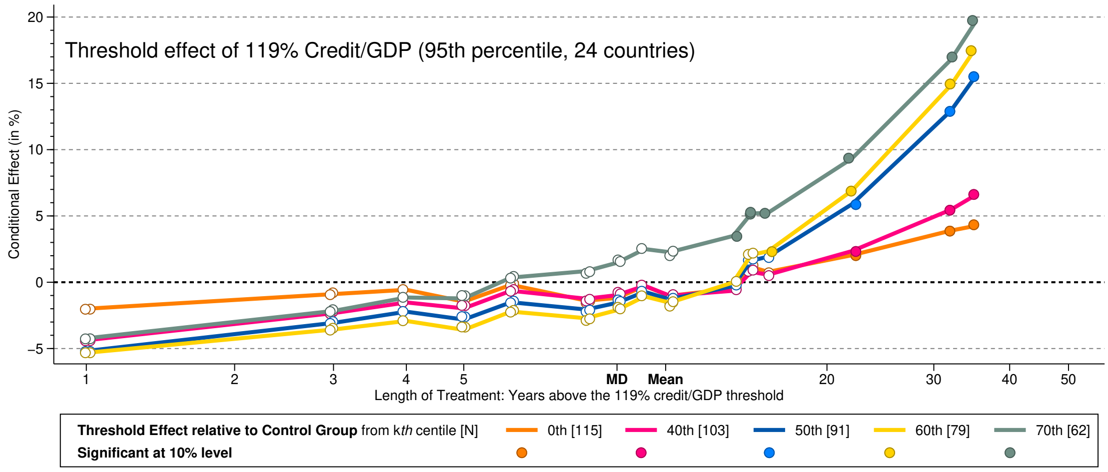
Figure 2: Too much Finance? Running line presentation of PCDID results



(a) Defining excessive finance with thresholds from 65% to 120% credit/GDP



(b) Defining excessive finance as 92% credit/GDP, alternative control samples



(c) Defining excessive finance as 119% credit/GDP, alternative control samples

Notes: Each plot investigates the prospect of ‘too much finance’ by studying the effect of being above a specified threshold of credit/GDP. In panels (b) and (c) we consider alternative control group samples by keeping or dropping countries with low financial development. 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.

does not make this problem go away. Failure in these tests may be due to the choice of control sample, so that in the following we zoom in on two specific thresholds (92% and 119%) but consider a range of alternative control samples.

Extensions Panels (b) and (c) of Figure 2 present the results for 92% and 119% credit/GDP thresholds, respectively — these are the 90th and 95th percentiles of the distribution of credit/GDP across 140 countries in the 1960-2016 period. Average (ATET) estimates for these specifications are presented in Appendix Table E-2. The different prediction lines are for the same treatment sample, but use different control samples: for instance, the orange line in Panel (b) includes *any* country which stayed below 92% credit/GDP, the pink line excludes those countries from the control sample which always stayed below the 40th percentile, and so on.

There are three insights from the results in Panel (b): first, the choice of control group clearly matters — when Angola or Mali 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 a closer match to the countries in the treated sample, the treatment effect trajectories eventually turn positive and significant. Second, if we focus on the mean (14.6) or 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.[21]

[21] All of these represent events in the aftermath of the GFC, a clear sign of *short-run* economic contraction: six

Additional insights derive from the results for the diagnostic tests presented in Appendix Table E-2: all specifications 'pass' the parallel trend test as well as the test whether inflation and trade openness are 'bad controls'. These findings are important since diagnostics were less favourable in our analysis of many thresholds k with the rule-based inclusion in the control sample.

The analysis of the 119% credit/GDP threshold in Panel (c) of the Figure provides similar evidence but with a stronger divergence in the long run between specifications with relatively indiscriminate control samples (orange and pink lines) and the more restricted control samples (other 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 none of the estimates with fewer than five years of treatment are statistically significant. The treatment effects for median and mean length of treatment measure are effectively zero. Diagnostic tests for these specifications (Appendix Table H-1) indicate that in most specifications inflation and trade openness are not bad controls while the parallel trend assumption is only narrowly rejected in the model with the most restricted control sample.

Finally, the above results are all based on the inclusion of four estimated factors extracted from the control sample, a reasonable but arbitrary choice. ATET results in Appendix Table E-2 provide the mean estimates for the range of specifications augmented with one to six estimated factors. While only a few of these yield statistically significant results — reiterating the importance of considering 'length of treatment' in the presentation of results — it is notable that few mean estimates have a negative sign (and these are never statistically significant): at worst the average effect of 'too much finance' is indistinguishable from zero.

In summary, moving from indiscriminate control samples to countries which are more similar

of the eight countries with five or fewer years of treatment have *negative* average GDP pc growth at the time they cross the threshold.

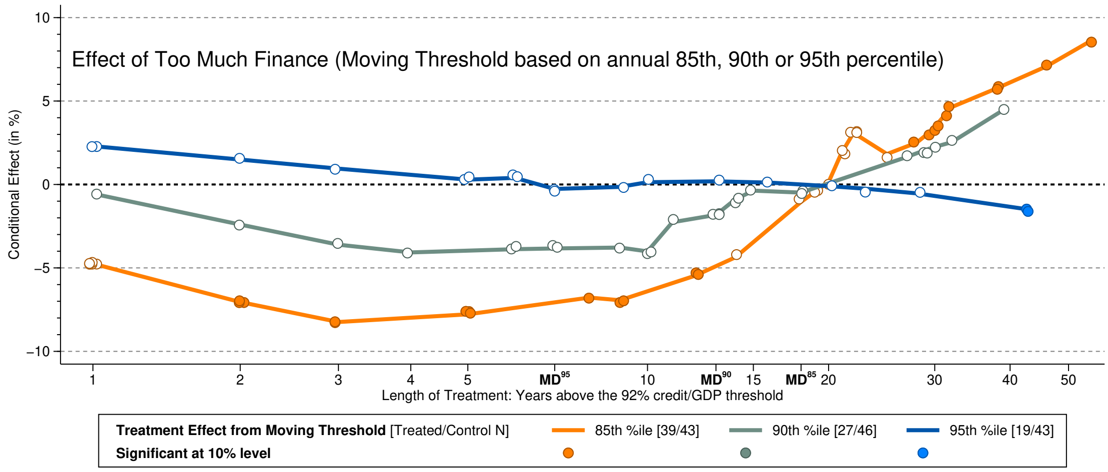
to the treated economies provides more conclusive evidence of a positive long-run effect, rather than of detrimental outcomes as found in the literature. Similarly, restricting treatment to a mere two dozen countries which experienced very high levels of credit/GDP in excess of 119% yields stronger positive results than those for the lower thresholds of 92%. Both findings are in contradiction to the findings of an inverted-U-shaped relationship in the existing literature.

Production Function Specification While we demonstrate that the additional controls included in the PCDID regressions, which form the basis of the above results, are not themselves outcomes of 'excessive' finance in our preferred specifications (Wald tests reported in Appendix Table E-2), we can also analyse excessive finance within a standard production function framework, akin to the study of the debt-growth nexus in Eberhardt & Presbitero (2015) among others. In Appendix E 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 would argue that financial intermediation 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 the results when capital stock per capita is included or excluded.[22] Regardless of the inclusion or exclusion of capital stock the trajectories of the treatment effects in Appendix Figure E-1 are qualitatively identical to our findings above.

Moving Threshold The above analysis adopts a 'static' threshold for too much finance for the entire sample period. But the secular evolution of the credit/GDP distribution, especially of the right tail, has seen a very substantial shift to the right: the 90th percentile of credit/GDP in

[22]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) and study the 92% and 119% thresholds along with a range of restricted control samples.

Figure 3: Too much Finance? Moving Threshold



Notes: We investigate 'too much finance' adopting a moving threshold of the 85th, 90th or 95th percentile of the credit/GDP distribution in every year from 1960-2016. The control samples have these thresholds as their upper bound and 25 percentage point lower credit/GDP as their lower bound for their credit/GDP peak. See Figure 2 for additional details.

1960 was 20%, in 1980 67%, in 2000 105% and at our sample endpoint in 2016 130%. A moving threshold could capture a more exclusive group of countries perennially close to 'excessive' finance. As a further robustness check on our findings, we study three moving thresholds, representing the 85th, 90th and 95th percentile of the annual credit/GDP distribution, with the control sample restricted to countries for which credit/GDP peaked between this moving target and 25 percentage points below it. Figure 3 presents the running line plots for these specifications. Two of the plots are upward-sloping over time, turning positive after twenty years of 'excessive finance', although only the version with the 85th percentile threshold turns statistically significant. The specification for the 95th percentile (just 19 treated countries) is flat around zero, with some small negative significant effect for the right tail. If we focus on the medians of these three specifications (marked MD^{85}, MD^{90} and MD^{95} along the x-axis) we can see that all three effects are effectively zero. Once again, there is little empirical evidence for detrimental effects of high levels of finance.

Dynamic Treatment Effects In our analysis so far we link each country's single treatment estimate for 'too much finance' to the number of years spent above some threshold (e.g. 92%

or 119% credit/GDP), adopting running line regressions. This 'ex-post' approach ignores any short-run dynamic treatment effects in the estimation equation and may lead to biased estimates. In an attempt to address this we adjust equation (4)

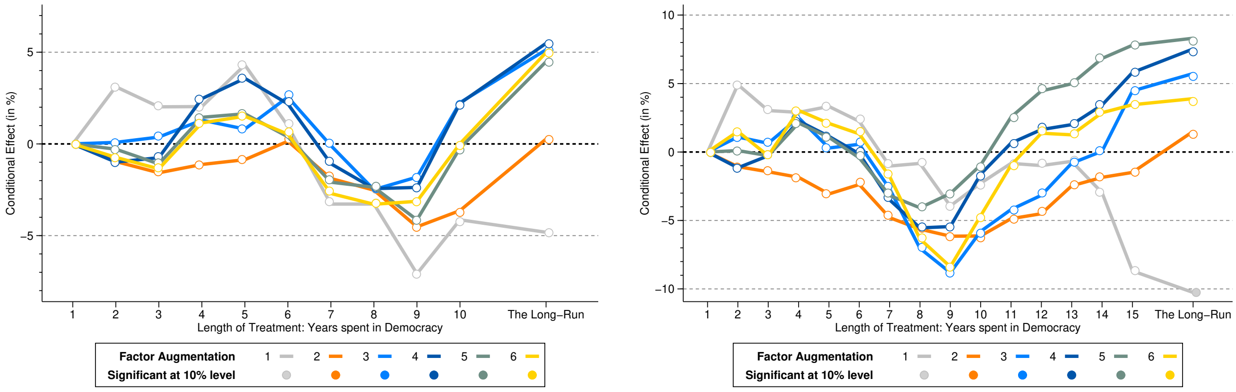
$$y_{it} = b_{0i} + \delta_i^{LR} \mathbf{1}_{\{t>T_{0i}\}} + \sum_{k=2}^{K} \delta_{ik}^{DTE} \mathbf{1}_{\{k=t-T_{0i}\}} + a_i' \hat{f}_t + b_{1i}' x_{it} + e_{it}, \tag{5}$$

where the indicator variable in the sum captures the $k = 2, \ldots, K$ years after the country exceeded <u>and</u> remained above the 'too much finance' threshold and δ_{ik}^{DTE} represents the associated effect on income per capita (the estimate for $k = 1$ is set to 0). We adopt $K = 15$ and $K = 10$.[23] What we term δ_i^{LR} is the 'Long-Run' effect of excessive finance: having spent more than 15 (or 10) years above the threshold. The results presented in Figure 4 focus on the specifications where the control sample is constrained to a maximum credit/GDP level between 47% and 92% or 47% and 119%, since these represent the most convincing counterfactuals.[24] Panel (a) uses the 92% credit/GDP threshold, panel (b) 119%, in each plot we show the results for the PCDID with 1 to 6 factors. These results indicate that once we add two or three estimated factors the long-run effect is positive, albeit insignificant, whether we adopt a ten- or fifteen-year horizon for the dynamic treatment effects (left and right plot, respectively). None of the dynamic treatment effects, which for the results in panel (a) follow inverted-U shapes, are statistically significant at the 10% level. Only the plot in the bottom right for the 119% threshold follows slightly different patterns. Note, however, that this includes 24 treated countries, a mere five of which have more than 15 years above the threshold (i.e. 'The Long-Run' is averaged from five estimates), with the treatment effects of 12 to 15 years each averaged from ten or fewer estimates.

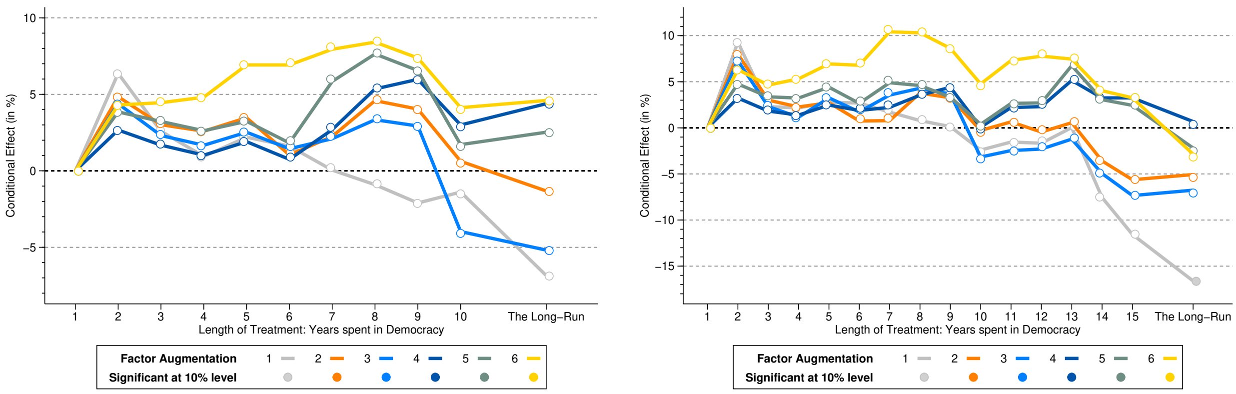
[23] Since our approach relies on country-specific regressions we cannot simply include a dummy *for each year* in treatment since for the 92% and 119% threshold models there would be 34 and 45 year dummies, respectively.

[24] In contrast to our earlier graphs these are not running line plots but robust Mean Group estimates across treated countries for each of the year dummies, $N^{-1}\sum_i \delta_i^{LR} + N_k^{-1}\sum_i \delta_{ik}^{DTE}$ (since they represent deviations from the long-run), and for the 'Long-Run' estimate, $N^{-1}\sum_i \delta_i^{LR}$. The inference for the latter is based on the nonparametric variance estimator defined in Chan & Kwok (2022), for the former we compute the p-values of a Wald test $N^{-1}\sum_i \delta_i^{LR} + N_k^{-1}\sum_i \delta_{ik}^{DTE} = 0$ for each $1 < k < K$.

Figure 4: Too much Finance? Dynamic Treatment Effects



(a) Defining excessive finance as 92% credit/GDP, alternative factor augmentations



(b) Defining excessive finance as 119% credit/GDP, alternative factor augmentations

Notes: We investigate 'too much finance' in the PCDID model but allow for dynamic treatment effects, including treatment year dummies up to $k = 10$ (left plot) and $k = 15$ (right plot) in the estimation equation. See text for further details on estimation and inference.

Heterogeneity We already pointed out in Section 2.1 that over 85% of country observations in the specification based on the 90th percentile of the credit/GDP distribution — results in Figure 2, panel (b) — are from high-income countries, two-thirds of which are present members of the OECD. Nevertheless, there may still be a concern that 'deeper determinants' may drive our results, one or more structural factors determining that some countries with many years above the 'too much finance' cut-off are qualitatively different from those which experienced just five or ten years. Such concerns undermine our approach to interpreting the estimated evolution of income per capita over the years of treatment. In the following, we explore the heterogeneity of our 'too much finance' results adopting proxies from the empirical literature on the deep determinants of comparative development (e.g. La Porta et al. 1997, Beck et al. 2003, Easterly & Levine 2003, Gorodnichenko & Roland 2017). This aside, although we capture the instances a country crosses the threshold in our running line regressions, it is of interest to know whether the timing and the instances of countries crossing the threshold provide any additional insights. Our analysis focuses on the specification using the 90th percentile of the credit/GDP distribution as a cut-off and with the control sample restriction of 47-91% (the line and markers in olive green in panel (b) of Figure 2): this represents a high threshold (92%), a conceptually appealing control sample restriction, as well as a sufficiently large treatment sample (N=38) to carry out sub-sample analysis.

Appendix Figure F-1 provides histograms for a number of proxies for 'deep determinants' (for details and sources see Appendix Section F) arranged in three groups: geography, culture, and (of particular interest given the vast literature on financial development) legal origin. The red bars are for the 38 treated countries, the light-blue bars are for the full sample. x-scales are at times reversed so that *values on the left* of each plot are hypothesised in the literature to be *more conducive* to long-run development. Patterns suggests that in terms of geography and legal origins our treated sample countries are very different from the full sample ones, but this is not the case for proxies of culture. Studying the homogeneity of the treated sample across proxies for deep determinants, in terms of geographical proxies we observe a relatively wide spread for historical disease prevalence and absolute latitude. For cultural proxies and legal origin, the two variables relating to European descendants and French legal origin are clearly polarised, while the language variables are again spread out widely.

In Appendix Figure F-2 we investigate whether such heterogeneities (between treated and all countries, within treated countries) can explain the patterns we observe in our 'too much finance' results. In each of the four running line plots, we reprint in pink the full sample (N=38) result from

Figure 2 Panel (b) alongside running line plots for subsamples defined by proxies for geography, culture and legal origin. We find that in all cases the predicted relationships are closely matched, with the exception of a small number of countries which have spent many years above the 'too much finance' threshold (note, this is not the case for legal origin). None of the subsample plots indicate a negative effect in the long run and given that only a handful of countries have treatment beyond 25 years we do not put much emphasis on these minor deviations.[25] We conclude that deep determinants do not appear to be primary drivers of the patterns we observe in our results.

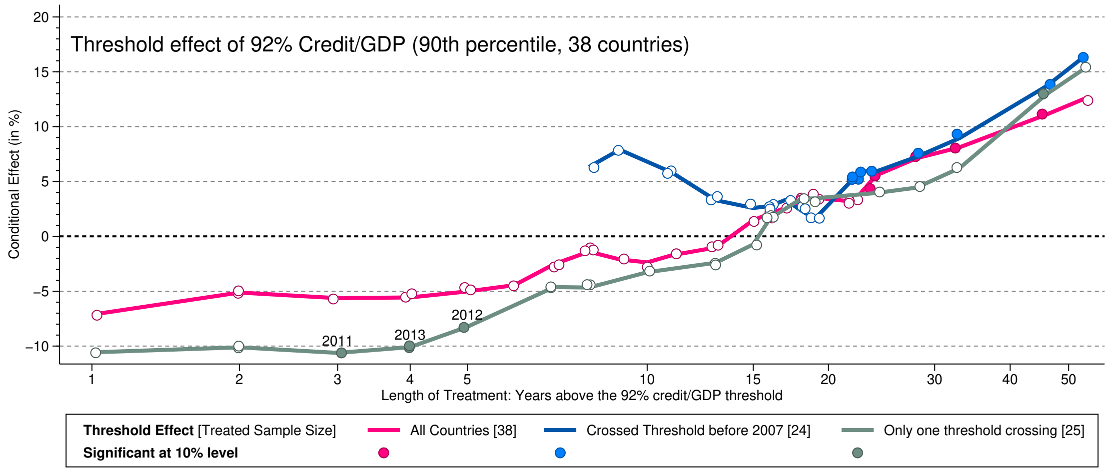
Finally, the timing and instances of a country crossing the 'too much finance' threshold are interesting heterogeneities to investigate further. Figure 5 does so, once again reprinting the benchmark full sample plot in pink. The olive green running line is for the 25 countries which crossed the threshold only once, and the blue running line is for the 24 countries which first crossed the line before the Global Financial Crisis in 2007. The long-run implications of all three models presented are remarkably similar. The negative significant results in the early years for countries which only crossed the threshold once are all for post-GFC years (green line, years highlighted), and we can refer to our explanations for these negative effects (see footnote 20): credit surges or negative shocks to GDP, neither related to financial development in its impact on growth.

2.3 Household versus Corporate Credit

Background While the 'credit booms gone bust' narrative in the financial crisis literature is now well-established, 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 2023, among others). From a theoretical point of view, sectoral heterogeneity does not feature prominently in credit cycle theories (see Müller & Verner 2023), though most of the empirical literature has suggested

[25] The analysis seems to suggest that countries with fewer European descendants and comparatively 'worse' geographical disposition drive the full treated sample result beyond 25 years of treatment.

Figure 5: Timing and Instances of 'Too much finance'



Notes: We investigate the heterogeneity of our 'too much finance' results by studying the countries with just a single crossing into 'too much finance' and those which first crossed the threshold before 2007. We adopt the PCDID estimates for the 92% credit/GDP threshold, with control countries those with a peak credit/GDP between 47% and 91%. A filled (hollow) marker indicates statistical (in)significance at the 10% level.

household credit as the major driver of the aggregate credit-crisis relationship (Jordà et al. 2016*a*, Mian et al. 2017).[26] As an exploratory exercise, we investigate whether the use of household credit and corporate credit lead to different insights into the finance-growth relationship.

Data 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 is from the same source, from the IMF Direction of Trade (DOT) data we construct the export/trade control variable. The sample makeup is reported in Appendix Table G-1.

[26] Müller & Verner (2023) demonstrate that similar to household credit booms, lending to *non-tradable* sectors constitutes the 'bad booms' leading to productivity slowdowns and financial vulnerability.

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 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[27] we also 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 G-2. These give no indication of a negative average treatment effect (ATET – computed using the robust mean across heterogeneous country estimates), 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. Furthermore, only the results for specifications using share of hh/corporate credit 'pass' the Alpha test. 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.[28]

3 Financial Development and Systemic Vulnerability

In this section we connect the empirical literatures on 'excessive' financial development and financial crises: we compare the propensity of credit booms and unfettered capital inflows in predicting systemic banking crises above and below different cut-offs of 'too much finance'. Our research

[27]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 suggest these are 'bad controls'.

[28]Given the sample sizes involved we did not pursue the analysis of banking crisis in this dataset.

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 suggested 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 first introduce the additional data used as well as our EWS methodology (Section 3.1), and then discuss our findings (3.2).

3.1 Data and Methodology

Data and Transformations In addition to the credit/GDP data (see Section 2.1 above) used to create the credit boom proxy, Δcredit/GDP, 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.[29] 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.[30] Our approach is thus wedded to the *continuous* financial flow variable (growth in capital flows/GDP), but to mimic the nature of capital flow *spikes* we alternatively adopt the square of capital inflows/GDP *levels*.[31]

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

[29] In line with Caballero (2016) we find more robust results using gross rather than net inflows.

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

[31] 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.

centuries Schularick & Taylor (2012) adopt a fifth-order lag polynomial specification — in our analysis, this would take up far too many degrees of freedom, 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): our simplest empirical model includes 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 — taken from the World Bank *WDI*. 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); however, the parsimony imposed by our methodology as well as data availability 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 includes proxies of unobserved common factors in the spirit of Boneva & Linton (2017), which can capture crisis determinants omitted from the model as well as global shocks or crisis spillovers (Cesa-Bianchi et al. 2019).

We close with a brief analysis of the dominant narratives for banking crisis prediction to demonstrate that these can be traced in our raw data. In Appendix Figure B-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. 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. These suggest that the 'credit boom gone bust' and capital flow bonanza narratives can be traced in our data.

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 using credit/GDP growth in the MA(3) transformation) for countries in the treated sample:

$$\begin{aligned} Y_{it}^* &= \alpha_i' d_t + \beta_i^A \, \overline{\Delta(\text{credit/GDP})}_{i,t-1/t-3} \qquad (6) \\ & \quad + \beta_i^B \, \mathbf{1}_{\{t>T_{0i}\}} \, \overline{\Delta(\text{credit/GDP})}_{i,t-1/t-3} + \gamma_i' \overline{x}_{i,t-1/t-3} + \kappa_i' f_t + \varepsilon_{it}, \end{aligned}$$

where f is a set of unobserved common factors with heterogeneous factor loadings κ and additional controls are represented by x — these always include the 'rival' dominant crisis predictors (i.e. capital flows), alongside the other controls. The indicator variable $\mathbf{1}_{\{\cdot\}}$ captures the time periods spent in the 'higher regime' above the credit/GDP threshold.

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.[32] We adopt a linear probability model for the start year of a banking crisis,[33] Y_{it}, in those countries which crossed the credit/GDP threshold (treated sample). 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 (control sample).

[32] 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 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.

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

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

$$Pr(Y_{it} = 1 \mid \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3},\, \overline{x}_{i,t-1/t-3},\, d_t,\, f_t) \tag{7}$$
$$= [\alpha_i + \tilde{f}_t'\kappa_i]d_t + \beta_i^A\, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3}$$
$$+ \beta_i^B\; \mathbf{1}_{\{t>T_{0i}\}}\, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} + \delta_i'\overline{x}_{i,t-1/t-3} + \psi_i'\hat{f}_t.$$

In this specification the change in credit/GDP is split in two by means of the interaction with the 'higher regime' dummy $\mathbf{1}_{\{t>T_{0i}\}}$. The common factors $\hat{f}$ are estimated via PCA from the residuals of the same model in the control group (with only a single credit/GDP growth term).[34] As a benchmark we provide results for a model where financial development is ignored and hence there is only one credit/GDP growth term.

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

$$\begin{aligned} Y_{it} \;=\;& a_i + b_i^A\, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} + b_i^B\, \mathbf{1}_{\{t>T_{0i}\}}\, \overline{\Delta(\text{credit/GDP})}_{i,t-1\,/\,t-3} \\ & + c_{1i}\overline{\Delta(\text{cap inflow/GDP})}_{i,t-1/t-3} + c_{2i}\overline{\Delta(\text{GDP pc})}_{i,t-1/t-3} \\ & + c_{3i}\overline{\Delta(\text{GFCF/GDP})}_{i,t-1/t-3} + c_{4i}\overline{(\text{inflation})}_{i,t-1/t-3} + d_i'\hat{f}_t + \varepsilon_{it}, \end{aligned} \tag{8}$$

where we spell out the control variables in detail. ε is the error term, which can be heteroskedastic and/or serially correlated. Alternative specifications focusing on excessive capital flow 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

[34] The term in square brackets in equation (7) 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 (8) in which all regressors are already MA(3)-transformed.

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 interaction with dominant crisis predictors in *separate* regressions, i.e. there is only ever one interaction term effect per specification, to keep the empirical model parsimonious and hence feasible for estimation.

Robust mean marginal effects and inference We compute 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 is more natural *not* to take time in the higher regime into account. Our reported results are Mean Group estimates computed using robust regression (Hamilton 1992) with associated standard errors computed non-parametrically (Pesaran 2006, Chan & Kwok 2022).

Graphical presentation of results We present the results for our banking crisis analysis using a graphical representation. The figures present robust mean effects of the prominent banking crisis triggers for what we previously referred to as 'treated countries'. A value of, say, 2.9 indicates that a one standard deviation increase in the crisis determinant is associated with a 2.9% increase in the propensity of a banking crisis. In each plot there are two different markers (circles and triangles) for the analysis of 'credit booms gone bust' and capital inflows, respectively. Dark blue markers are for results where we ignore financial development, light blue and pink markers are for the results below and above the 'too much finance' cut-off (or 'low' and 'high' regimes), respectively. The results for the 'high' regime are expressed *in deviation from* the estimates for the 'low' regime. Shaded markers are for statistically significant effects (10% level), and hollow markers are for insignificant effects. For this visualisation we minimally perturbed the empirical estimates in a random fashion to aid presentation — the point estimates are reported in Tables in Appendix Section H. The tables also make it straightforward to distinguish EWS results with and without additional controls — we do not make this distinction in the figure.

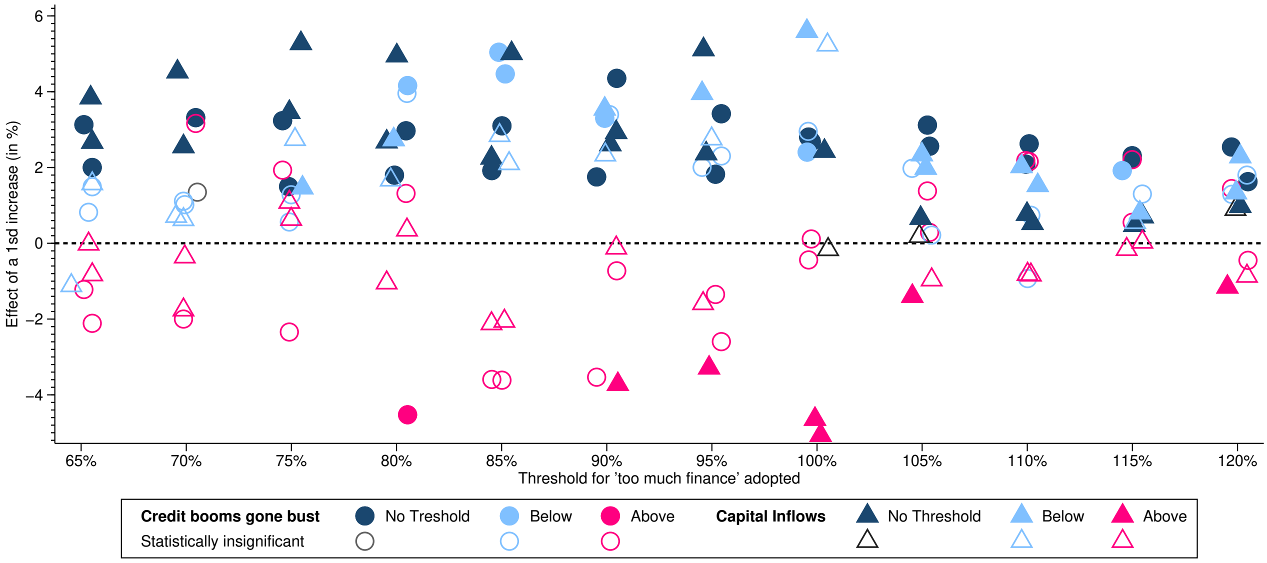
3.2 Systemic Vulnerability due to 'Too Much Finance'?

Main Results Panel (a) of Figure 6 shows the results when we select thresholds for 'high' financial development ranging from 65% to 120%.[35] We first focus on the specifications which ignore financial development (markers in dark blue): these are overwhelmingly statistically significant and positive, suggesting a 1 SD increase in the respective variable is associated with a higher propensity of banking crises by 1-5 percentage points. We take these results as confirmation that our samples and methodology can replicate the current consensus in the literature that credit booms and capital inflows are significant determinants of banking crises.

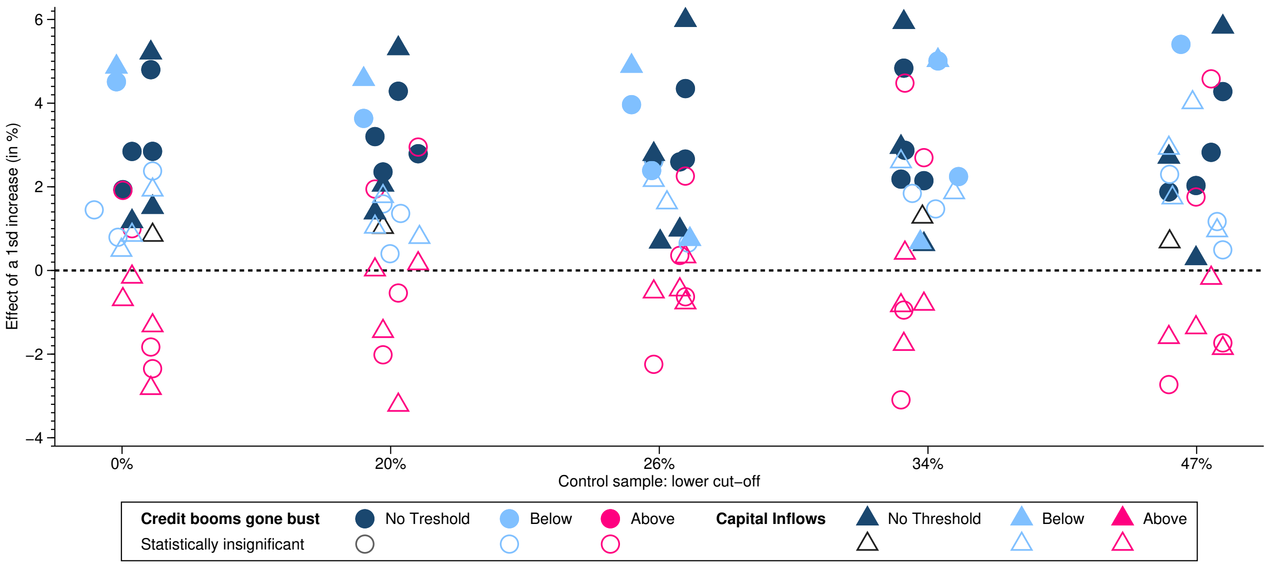
We now turn to the main purpose of this EWS exercise, the question of whether *within* highly financially developed countries these credit boom and capital flow effects are comparatively larger when countries are in the higher regime of financial development compared with the effects in the lower regime. Note that these specifications differ merely by the addition of a single (interaction) term, hence the number of parameters estimated in each country regression does only increase by one. The markers in light blue show the effect when countries are *below* the 'excessive' finance threshold: these are almost all positive, often significant, and of similar magnitude to the results just discussed. The pink markers capture the effect when the same countries are *below*

[35] These results for thresholds of k=65% to k=120% of credit/GDP have control samples fixed to countries which had a credit/GDP peak between $k-$25% and k: there are between 41 (65%) and 23 (120%) 'treated' countries in these samples, which experienced between 61 and 38 banking crises; control samples range from 20 (65%) to 6 (120%) countries (with 37 to 9 crises) — having just 6 control countries makes for limited counterfactual evidence, which is why below we provide alternative results for just two cut-offs (92% and 119% credit/GDP) for which the control samples are less restricted. The unconditional crisis propensities in our treated samples (between 4.7% and 5.0%) are broadly similar to those in the various 'control samples' (3.6 to 6.4%). This information and the empirical results are presented in Appendix Table H-2.

Figure 6: Too Much Finance and Systemic Vulnerability — Main Results



(a) Main Results



(b) Results for 92% and 119% Thresholds

Notes: We present the resulting effect on the propensity of a banking crisis following a standard deviation increase in the prominent crisis trigger: credit booms and capital inflows. Dark blue markers indicate the change in crisis propensity (in %) if we ignore 'too much finance', light-blue markers are for the effects *below* the 'too much finance' threshold, pink markers for those *above* the threshold — the latter are interaction effects (*in deviation* from the former). Filled (hollow) markers indicate statistical (in)significance at the 10% level. This visualisation is based on robust mean estimates reported in Appendix Tables H-2 and H-1. All markers are minimally perturbed to aid presentation.

the threshold and are expressed in deviation from the 'below threshold' results. Although only occasionally statistically significant, these are next to uniformly negative.[36] This implies that for the same countries and the same crisis trigger the propensity for banking crises is *lower* when they have *higher* levels of financial development and vice versa:[37] a pattern opposite to that if we suspected high levels of financed caused higher financial vulnerability.

Extensions Panel (b) of Figure 6 illustrates the empirical results when we focus on only two thresholds, 92 or 119% credit/GDP, but use alternative control samples:[38] the results labelled '0%' include all control countries which never reached 92% or 119% credit/GDP; for those labelled 20% we only include a subsample of control countries which reached at least 20% credit/GDP; and analogously for the remaining cut-offs.[39] Ignoring financial development, dark blue markers confirm that credit booms and capital inflows are statistically significant determinants of banking crises.[40] Depending on the specification credit booms increase the propensity of a banking crisis by around 2-5%, a sizeable effect which is always statistically significant. Results for capital inflows are not uniformly significant and vary more between models without controls (0.6-3%) and those which include them (1-6%). The markers in light blue are the results when countries are *below* the 'excessive' finance threshold. While these results are only occasionally statistically significant, it can be seen quite clearly that these effects generally cluster at 0.5-5.5%, whether we study credit booms or capital inflows. The pink markers are for results when countries are *above* the 'excessive'

[36] It is possible that the coefficient of interest has a large magnitude, but is statistically insignificant because of low variation in the data, measurement error, or too small a sample to detect an effect. This means we cannot say that the coefficient is economically insignificant; rather, we will say that it has a sizable magnitude, but it is imprecisely estimated.

[37] Note that control samples are reduced to a mere 9 and 6 countries for the 115% and 120% thresholds, so these two results should be taken with a grain of salt.

[38] There are 30 and 23 countries in these samples, which experienced 47 and 38 banking crises, respectively. 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%). These results are presented in Appendix Table H-1.

[39] A simple count of banking crises in the two regimes already indicates that 50% more crises (100% in case of the 119% threshold) occurred in the *lower* regime.

[40] In this figure we only present the latter results using the square gross capital inflows/GDP, while Appendix Table H-1 also studies change in capital flows/GDP.

finance threshold. These are never statistically significant, but again there is a clear tendency for these interaction effects to be negative and hence smaller than those in the 'low' regime.[41]

For the crisis narratives investigated, the absence of evidence is of course not evidence for the absence of an effect, but the overall pattern of results 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 negatively, we can now conclude that on average it also does not *systematically* raise the propensity of financial crises. 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, and comparing the effects *within* individual highly financially developed countries below and above the threshold.

4 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 was generally 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 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 on financial vulnerability in a factor-augmented EWS approach which focuses on the short-run and crisis triggers.

[41]AUROC comparison in the results for the 92% and 119% cut-offs 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.

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. The patterns we reveal appear not to be driven by deep determinants.

There are at least two caveats to our analysis: first, 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 most of the 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.

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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					Treatm. & Control			
							Start	End	Δpa	Start	End	Δpa	Min	Max	92	C	119	C
1	ABW	Aruba	1995	2015	21	0	26705	25822	-42.0	42	62	0.96	41	62	0	×	0	×
2	AGO	Angola	2003	2016	14	0	2423	3530	79.1	4	22	1.35	4	25	0		0	
3	ALB	Albania	1994	2016	23	0	1494	4682	138.6	3	36	1.44	3	40	0		0	
4	ARM	Armenia	1994	2016	23	0	956	3917	128.8	3	45	1.80	3	45	0		0	
5	AUS	Australia	1960	2016	57	0	19378	55729	637.7	18	142	2.18	17	142	13		9	
6	AUT	Austria	1970	2016	45	2	19574	48260	610.3	42	83	0.88	42	98	9		0	×
7	AZE	Azerbaijan	1992	2016	25	0	2361	5813	138.1	4	31	1.09	1	36	0		0	
8	BDI	Burundi	1966	2016	51	0	213	220	0.1	2	17	0.28	2	22	0		0	
9	BEL	Belgium	1970	2016	45	2	19808	45943	556.1	17	62	0.97	17	77	0	×	0	×
10	BEN	Benin	1993	2016	24	0	854	1135	11.7	11	21	0.44	6	22	0		0	
11	BFA	Burkina Faso	1960	2016	57	0	269	748	8.4	2	27	0.43	2	28	0		0	
12	BGD	Bangladesh	1993	2016	24	0	439	1062	26.0	13	39	1.11	12	39	0		0	
13	BGR	Bulgaria	1991	2016	26	0	4360	8009	140.4	61	52	-0.33	8	69	0	×	0	×
14	BHR	Bahrain	1980	2015	33	3	21185	22436	34.7	34	105	1.95	26	114	7		0	×
15	BHS	Bahamas, The	1977	2016	39	1	18600	27370	219.3	28	72	1.08	24	84	0	×	0	×
16	BLR	Belarus	1994	2016	23	0	2252	6216	172.3	18	27	0.39	4	35	0		0	
17	BOL	Bolivia	1960	2016	57	0	1005	2426	24.9	2	61	1.03	2	63	0	×	0	×
18	BRA	Brazil	1981	2016	36	0	7797	10966	88.0	26	68	1.17	10	70	0	×	0	×
19	BRB	Barbados	1975	2009	35	0	10881	16492	160.3	28	78	1.42	26	78	0	×	0	×
20	BRN	Brunei Darussalam	1999	2016	18	0	35681	31685	-222.0	54	45	-0.50	28	54	0	×	0	×
21	BTN	Bhutan	1983	2016	34	0	473	2971	73.5	3	57	1.58	3	57	0	×	0	×
22	BWA	Botswana	1975	2016	42	0	1435	7797	151.5	16	30	0.33	6	33	0		0	
23	CAN	Canada	1970	2008	39	0	22844	48495	657.7	32	123	2.33	32	177	11		8	
24	CHE	Switzerland	1970	2016	47	0	49581	77026	583.9	103	172	1.48	86	172	45		32	
25	CHL	Chile	1971	2016	46	0	4901	14777	214.7	7	109	2.21	3	109	8		0	×
26	CHN	China	1987	2016	30	0	634	6908	209.1	69	149	2.66	66	149	19		5	
27	CIV	Cote d'Ivoire	1961	2016	56	0	1300	1530	4.1	19	22	0.06	13	42	0		0	
28	CMR	Cameroon	1969	2016	48	0	926	1469	11.3	15	16	0.03	7	25	0		0	
29	COD	Congo, Dem Rep	2000	2016	17	0	290	407	6.9	0	6	0.32	0	6	0		0	
30	COG	Congo, Rep	1989	2015	25	2	2862	3013	5.6	15	21	0.22	2	21	0		0	
31	COL	Colombia	1960	2016	55	2	2339	7634	92.9	20	46	0.46	12	50	0	×	0	×
32	CRI	Costa Rica	1960	2016	57	0	2911	9510	115.8	26	56	0.53	10	56	0	×	0	×
33	CYP	Cyprus	1975	2015	41	0	7360	27898	500.9	79	248	4.12	54	261	22		15	
34	CZE	Czech Republic	1993	2016	24	0	12313	21864	397.9	59	50	-0.40	27	62	0	×	0	×
35	DEU	Germany	1970	2016	47	0	19680	45960	559.2	57	76	0.41	57	116	17		0	×
36	DNK	Denmark	1966	2016	51	0	26032	61878	702.9	27	169	2.78	21	212	16		16	
37	DOM	Dominican Rep	1960	2016	57	0	1324	7026	100.0	5	26	0.36	5	30	0		0	
38	DZA	Algeria	1973	2016	44	0	2925	4830	43.3	35	22	-0.28	4	68	0	×	0	×
39	ECU	Ecuador	1960	2016	57	0	2238	5176	51.5	20	29	0.16	11	34	0		0	
40	EGY	Egypt, Arab Rep	1960	2016	57	0	578	2761	38.3	18	28	0.18	10	51	0	×	0	×
41	ESP	Spain	1972	2016	45	0	15010	31449	365.3	65	112	1.03	61	173	16		11	
42	EST	Estonia	1993	2016	24	0	6743	18092	472.9	9	70	2.51	9	103	2		0	×
43	FIN	Finland	1970	2016	47	0	18267	46750	606.0	37	93	1.19	37	93	4		0	×
44	FRA	France	1960	2016	55	2	12744	42140	515.7	20	95	1.32	20	96	8		0	×
45	GAB	Gabon	1963	2016	54	0	5529	9429	72.2	18	14	-0.06	5	28	0		0	
46	GBR	United Kingdom	1970	2016	47	0	17923	42500	522.9	19	130	2.38	19	196	28		15	
47	GEO	Georgia	1995	2016	22	0	1077	4305	146.7	5	56	2.33	3	56	0	×	0	×
48	GHA	Ghana	1965	2016	52	0	1055	1645	11.3	7	18	0.20	1	18	0		0	
49	GMB	The Gambia	1977	2014	38	0	865	748	-3.1	9	13	0.11	6	17	0		0	
50	GNB	Guinea-Bissau	1990	2014	25	0	637	556	-3.2	2	12	0.42	1	13	0		0	

(Continued overleaf)

Table A-1: Sample Makeup (continued)

	ISO	Country	Start	End	Obs	Miss	GDP pc (2008 US$)			Private Credit/GDP					Treatm. & Control			
							Start	End	Δpa	Start	End	Δpa	Min	Max	92	C	119	C
51	GRC	Greece	1960	2016	57	0	6260	22666	287.8	10	110	1.75	10	121	7		3	
52	GTM	Guatemala	1960	2016	57	0	1491	3243	30.7	10	33	0.41	10	33	0		0	
53	GUY	Guyana	1995	2016	22	0	2060	3793	78.8	15	45	1.40	15	48	0	×	0	×
54	HKG	Hong Kong SAR	1990	2016	27	0	18251	36819	687.7	153	202	1.79	124	219	27		27	
55	HND	Honduras	1960	2016	35	22	1096	2111	17.8	10	54	0.78	10	54	0	×	0	×
56	HRV	Croatia	1995	2016	22	0	8568	14706	279.0	24	61	1.70	24	71	0	×	0	×
57	HUN	Hungary	1991	2016	26	0	8858	15114	240.6	41	34	-0.25	20	66	0	×	0	×
58	IDN	Indonesia	1980	2016	37	0	1231	3968	74.0	6	38	0.84	6	44	0		0	
59	IND	India	1960	2016	57	0	330	1876	27.1	9	49	0.71	9	50	0	×	0	×
60	IRL	Ireland	1970	2016	47	0	12745	67078	1156.0	25	49	0.52	25	174	11		6	
61	IRN	Iran	1961	2016	53	3	3236	6791	63.5	14	61	0.83	14	61	0	×	0	×
62	IRQ	Iraq	1970	2016	18	29	1466	5931	95.0	12	10	-0.06	2	12	0		0	
63	ISL	Iceland	1977	2016	40	0	23544	49985	661.0	21	84	1.57	21	263	13		9	
64	ISR	Israel	1970	2016	47	0	13965	33721	420.3	24	64	0.87	23	90	0	×	0	×
65	ITA	Italy	1970	2016	47	0	17671	34459	357.2	62	85	0.50	45	96	3	×	0	×
66	JAM	Jamaica	1966	2016	50	1	3796	4762	18.9	17	30	0.26	14	37	0		0	
67	JOR	Jordan	1976	2016	40	1	2037	3271	30.1	30	71	0.99	30	85	0	×	0	×
68	JPN	Japan	1970	2016	47	0	18700	47403	610.7	82	160	1.65	82	192	46		35	
69	KEN	Kenya	1961	2016	56	0	480	1130	11.6	12	31	0.34	10	32	0		0	
70	KGZ	Kyrgyz Rep	1996	2016	21	0	564	1044	22.8	9	21	0.55	4	21	0		0	
71	KHM	Cambodia	1995	2016	22	0	342	1080	33.5	3	74	3.21	3	74	0	×	0	×
72	KOR	Korea, Rep	1960	2016	57	0	932	26726	452.5	11	139	2.23	11	139	6		3	
73	KWT	Kuwait	1995	2016	22	0	41801	35887	-268.8	29	106	3.48	29	106	2		0	×
74	LKA	Sri Lanka	1961	2016	56	0	586	3769	56.8	7	37	0.53	7	37	0		0	
75	LSO	Lesotho	1974	2016	18	25	468	1422	22.2	0	17	0.39	0	17	0		0	
76	LTU	Lithuania	1995	2016	21	1	5318	15944	483.0	14	41	1.25	10	58	0	×	0	×
77	LUX	Luxembourg	1970	2016	44	3	35457	110162	1589.5	41	98	1.21	41	108	8		0	×
78	LVA	Latvia	1995	2016	21	1	5141	14736	436.1	11	47	1.66	7	95	1		0	×
79	MAC	Macao SAR	1989	2016	28	0	20609	52163	1126.9	58	112	1.94	39	112	2		0	×
80	MAR	Morocco	1966	2016	51	0	815	3213	47.0	13	63	0.99	9	73	0	×	0	×
81	MDA	Moldova	1995	2016	22	0	1624	3120	68.0	4	31	1.19	4	39	0		0	
82	MDG	Madagascar	1965	2016	52	0	774	476	-5.7	17	13	-0.09	8	18	0		0	
83	MEX	Mexico	1960	2016	57	0	3907	10206	110.5	20	32	0.20	8	32	0		0	
84	MKD	N Macedonia	1994	2016	23	0	3094	5247	93.6	37	48	0.47	16	49	0	×	0	×
85	MLI	Mali	1989	2016	28	0	504	749	8.8	10	23	0.45	7	23	0		0	
86	MLT	Malta	1970	2016	47	0	3746	26788	490.2	43	83	0.86	21	120	16		1	
87	MNG	Mongolia	1993	2016	24	0	1364	3866	104.3	5	53	2.00	5	55	0	×	0	×
88	MRT	Mauritania	1986	2012	14	13	1634	1653	0.7	26	21	-0.19	18	27	0		0	
89	MUS	Mauritius	1976	2016	41	0	2405	9834	181.2	22	98	1.85	21	103	5		0	×
90	MWI	Malawi	1981	2016	36	0	371	506	3.8	11	10	-0.03	2	13	0		0	
91	MYS	Malaysia	1960	2016	57	0	1354	11244	173.5	8	120	1.97	8	145	23		5	
92	NAM	Namibia	2003	2016	14	0	4229	6143	136.7	43	64	1.55	43	64	0	×	0	×
93	NER	Niger	1967	2016	50	0	906	527	-7.6	9	15	0.11	4	18	0		0	
94	NGA	Nigeria	1981	2016	36	0	1742	2456	19.8	14	15	0.02	5	20	0		0	
95	NIC	Nicaragua	2000	2016	17	0	1294	1895	35.4	24	36	0.75	14	36	0		0	
96	NLD	Netherlands	1969	2016	46	2	23389	52727	611.2	30	113	1.72	29	125	18		1	
97	NOR	Norway	1970	2016	47	0	32245	90196	1233.0	50	143	1.97	48	143	10		8	
98	NPL	Nepal	1975	2016	42	0	280	730	10.7	4	71	1.59	4	71	0	×	0	×
99	NZL	New Zealand	1970	2010	41	0	19989	33700	334.4	11	146	3.29	10	146	15		6	
100	OMN	Oman	2001	2016	16	0	18782	16226	-159.8	39	73	2.11	28	73	0	×	0	×
101	PAK	Pakistan	1960	2016	57	0	302	1118	14.3	9	15	0.12	9	27	0		0	
102	PAN	Panama	1960	2016	57	0	2139	11107	157.3	12	81	1.23	11	92	0	×	0	×
103	PER	Peru	1960	2016	57	0	2660	6262	63.2	16	41	0.44	5	41	0		0	
104	PHL	Philippines	1960	2016	57	0	1100	2887	31.3	15	41	0.46	15	51	0	×	0	×
105	PNG	Papua New Guinea	1973	2004	32	0	1774	1582	-6.0	11	8	-0.07	7	19	0		0	
106	POL	Poland	1995	2016	22	0	6540	15102	389.2	15	53	1.74	15	53	0	×	0	×
107	PRT	Portugal	1970	2016	47	0	8760	22534	293.1	46	114	1.45	42	159	18		11	
108	PRY	Paraguay	1962	2016	55	0	1430	5090	66.5	5	54	0.89	5	54	0	×	0	×
109	PSE	West Bank/Gaza	1997	2016	20	0	2060	2695	31.7	13	42	1.45	13	42	0		0	
110	QAT	Qatar	2002	2016	15	0	62632	64303	111.4	29	78	3.27	26	78	0	×	0	×

(Continued overleaf)

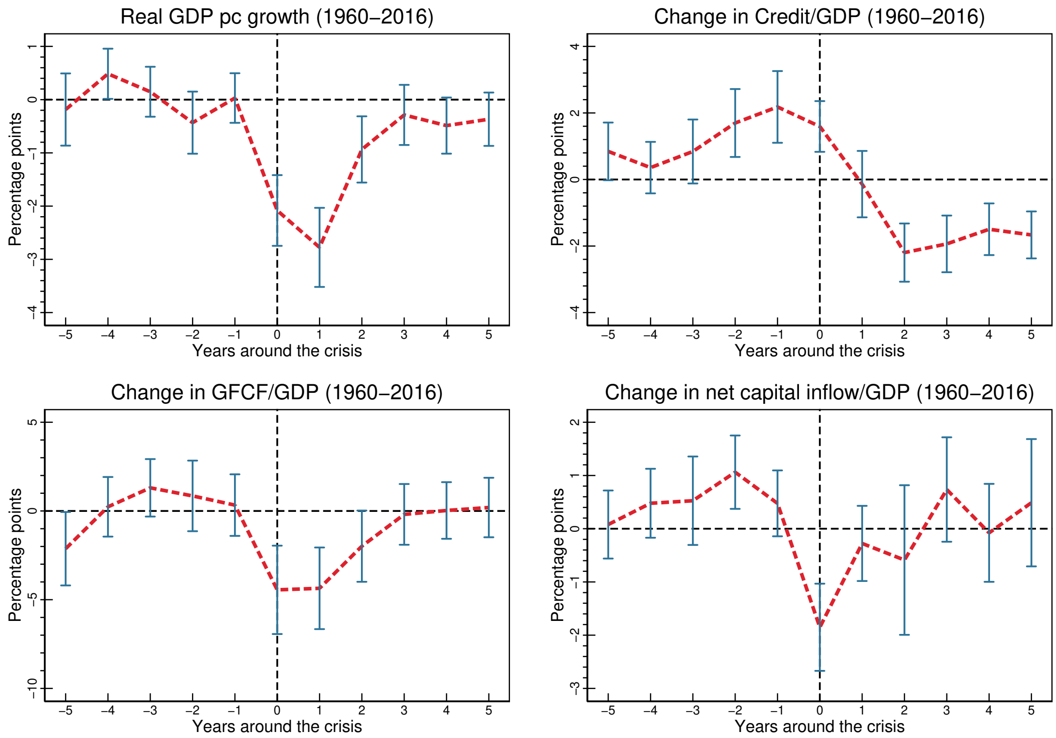
Table A-1: Sample Makeup (continued)

	ISO	Country	Start	End	Obs	Miss	GDP pc (2008 US$)			Private Credit/GDP					Treatm. & Control			
							Start	End	Δpa	Start	End	Δpa	Min	Max	92	C	119	C
111	ROU	Romania	1991	2016	22	4	4725	10237	212.0	31	34	0.10	4	44	0		0	
112	RUS	Russian Fed	1993	2016	24	0	7071	11356	178.6	6	56	2.10	6	56	0	×	0	×
113	RWA	Rwanda	1967	2016	48	2	312	793	9.6	1	20	0.38	1	20	0		0	
114	SAU	Saudi Arabia	1970	2016	47	0	22134	21271	-18.4	5	69	1.35	4	74	0	×	0	×
115	SDN	Sudan	1960	2015	56	0	876	1826	17.0	10	8	-0.03	2	15	0		0	
116	SEN	Senegal	1968	2016	49	0	1296	1432	2.8	13	32	0.38	13	35	0		0	
117	SGP	Singapore	1963	2016	54	0	4113	55043	943.2	33	132	1.84	33	132	22		4	
118	SLV	El Salvador	1965	2016	52	0	2358	3383	19.7	19	44	0.49	17	44	0		0	
119	SRB	Serbia	1997	2015	19	0	3504	6155	139.6	21	43	1.16	16	47	0	×	0	×
120	SVK	Slovak Rep	1993	2016	24	0	7821	19274	477.2	52	54	0.11	29	54	0	×	0	×
121	SVN	Slovenia	1991	2016	26	0	14135	24552	400.6	35	47	0.47	19	85	0	×	0	×
122	SWE	Sweden	1960	2016	57	0	18050	56789	679.6	40	125	1.49	39	129	22		10	
123	SWZ	Eswatini	1970	2016	47	0	1226	4663	73.1	8	20	0.26	7	21	0		0	
124	SYC	Sychelles	1976	2016	41	0	5078	13606	208.0	19	29	0.25	9	30	0		0	
125	TCD	Tchad	1984	2015	32	0	470	957	15.2	11	8	-0.07	2	18	0		0	
126	TGO	Togo	1969	2016	48	0	631	649	0.4	8	36	0.58	8	36	0		0	
127	THA	Thailand	1964	2016	53	0	662	5916	99.1	14	145	2.47	14	163	19		11	
128	TJK	Tajikistan	2001	2016	16	0	448	976	33.0	12	19	0.42	10	24	0		0	
129	TLS	East Timor	2003	2016	14	0	635	923	20.6	3	8	0.30	2	8	0		0	
130	TON	Tonga	1981	2012	32	0	2206	3730	47.6	12	32	0.60	12	52	0	×	0	×
131	TUN	Tunisia	1987	2016	30	0	2167	4311	71.5	45	77	1.09	44	77	0	×	0	×
132	TUR	Turkey	1960	2016	57	0	3175	14063	191.0	18	65	0.82	11	65	0	×	0	×
133	TZA	Tanzania	1990	2016	27	0	516	904	14.4	15	14	-0.06	3	16	0		0	
134	UGA	Uganda	1994	2016	23	0	439	910	20.5	4	14	0.45	4	14	0		0	
135	UKR	Ukraine	1993	2016	24	0	2798	2904	4.4	1	47	1.92	1	90	0	×	0	×
136	URY	Uruguay	1960	2016	57	0	5475	14124	151.7	19	29	0.18	6	61	0	×	0	×
137	USA	United States	1970	2016	47	0	23207	52556	624.4	85	179	2.00	85	196	33		22	
138	VNM	Vietnam	1996	2016	21	0	628	1753	53.5	17	114	4.60	17	114	5	×	0	×
139	VUT	Vanuatu	1980	2014	35	0	2071	2853	22.4	32	69	1.05	26	69	0	×	0	×
140	ZAF	South Africa	1961	2016	56	0	4685	7477	49.8	19	143	2.22	18	147	24		14	

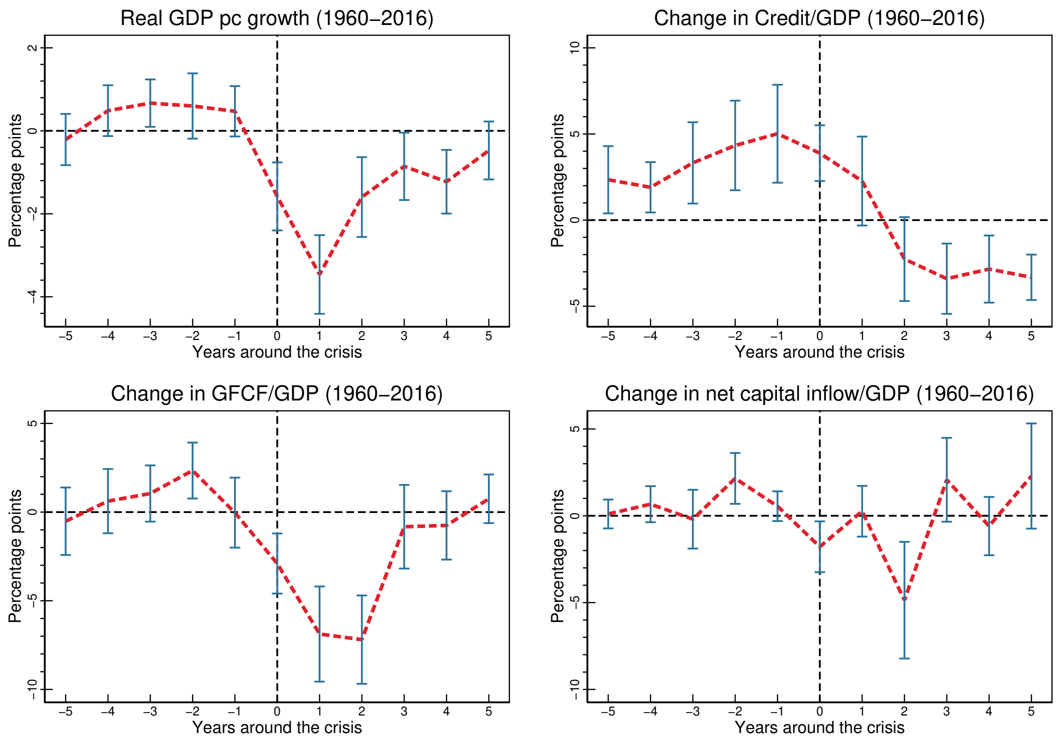
Notes: We provide details on the 140 countries in our sample of analysis, including Start and End Year of the country time series, the number of observations (Obs) and hence the number of missing observations (Miss). Real GDP pc is in US$ 2008 values for the first and final year of the country sample, 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: 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 ×.

B Banking Crisis Event Analysis

Figure B-1: Event Analysis — Banking Crises



(a) 102 Countries which experienced a banking crisis



(b) 34 Highly financially developed countries (92% 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.). Ongoing crisis years are omitted.

C Pooled Estimates

Table D-1: Too much Finance? Pooled Estimates

Estimator	(1) TWFE	(2) D-GMM	(3) S-GMM	(4) TWFE	(5) D-GMM	(6) S-GMM
Raw Estimates						
Lagged GDP pc	-0.041	-0.076	0.000	-0.041	-0.078	0.001
	[0.005]***	[0.016]***	[0.003]	[0.005]***	[0.017]***	[0.003]
Credit	1.307	4.411	2.788	1.733	4.846	6.466
	[0.565]**	[1.555]***	[1.198]**	[0.719]**	[2.225]**	[2.508]***
Lagged Credit				-0.338	-1.442	-3.545
				[0.451]	[1.411]	[1.830]*
Credit Squared	-0.252	-0.940	-0.473	-0.357	-0.982	-1.046
	[0.090]***	[0.305]***	[0.189]**	[0.119]***	[0.401]**	[0.405]***
Lagged Credit Squared				0.107	0.197	0.571
				[0.077]	[0.261]	[0.302]*
Inflation	-0.002	-0.001	-0.002	-0.002	-0.002	-0.002
	[0.000]***	[0.001]	[0.001]	[0.000]***	[0.002]	[0.001]
Lagged Inflation				-0.000	-0.003	-0.000
				[0.000]	[0.002]*	[0.002]
Openness	2.093	1.151	2.317	2.396	1.794	2.420
	[0.512]***	[1.417]	[0.693]***	[0.610]***	[1.729]	[1.476]
Lagged Openness				-0.540	-1.943	-0.359
				[0.447]	[1.547]	[1.276]
Cumulative Estimates						
Credit	1.307	4.411	2.788	1.395	3.404	2.921
	[0.565]**	[1.555]***	[1.198]**	[0.594]**	[1.592]**	[1.450]**
Credit Squared	-0.252	-0.940	-0.473	-0.249	-0.784	-0.475
	[0.090]***	[0.305]***	[0.189]**	[0.089]***	[0.309]**	[0.222]**
Observations	969	825	969	969	825	969
Countries	140	140	140	140	140	140
Instruments		84	125		84	125
Sargan p-value		0.00	0.00		0.01	0.00
Hansen p-value		0.14	0.22		0.12	0.23
AB AR(1) p-value		0.00	0.00		0.00	0.00
AB AR(2) p-value		0.40	0.02		0.49	0.05

Notes: We estimate dynamic pooled regressions for the average per capita GDP growth rate within a 5-year period on lagged GDP pc, credit, inflation, and openness (all regressors are 5-year averages of the logarithmic values). Models (1)-(3) are partial adjustment models where the regressors enter only in their contemporaneous values, models (4)-(6) are distributed lag models. 'Cumulative Estimates' sum up the lag coefficients for the latter and reprint the estimates for the former. Estimators are: TWFE — two-way fixed effects, D-GMM — Difference GMM (Arellano & Bond 1991), S-GMM — System GMM (Blundell & Bond 1998). Our sample is divided into 11 non-overlapping 5-year periods. Diagnostics report the number of instruments used (we restrict the instrument set to the 3rd and 4th lags to avoid instrument proliferation and hence overfitting bias), the Sargan and Hansen test p-values along with the residual serial correlation tests following Arellano & Bond (1991).

In Table D-1 we present results from pooled regression models of the finance-growth nexus adopting the empirical setup of Arcand et al. (2015), comprising 11 non-overlapping 5-year periods: all variables are 5-year averages. As in our 'treatment' analysis in the main text we stick to a limited set of controls (trade openness, inflation) so we can directly compare pooled and heterogeneous treatment results. Our first set of specifications in columns (1) to (3) adopt a partial adjustment model, including the lag of per capita GDP (in logs) and contemporaneous values of credit, credit squared, inflation and openness. In columns (4) to (6) we then adopt less restrictive dynamics by allowing all independent variables specified with their contemporaneous and lagged terms — in order to make results comparable across the two dynamic setups we report 'cumulative estimates' in a lower panel of the table. The dependent variable is per capita GDP growth, hence the specifications carry an element of error correction models (ECM), where we would expect the lagged GDP pc term to be negative significant.

The implementation is via standard two-way fixed effects (TWFE), the difference GMM (Arellano & Bond 1991) and the system GMM (Blundell & Bond 1998) estimators: as is well-known the GMM estimators suffer from over-fitting bias (in the direction of the TWFE results) if there are too many instruments adopted, and we hence adopt a common choice of moment restrictions for all GMM specifications (the 3rd and 4th lags) which yields *some* favourable diagnostic test results.

Across all specifications and estimators we find the statistically significant 'non-monotonicity' result for credit and credit squared characteristic of the 'too much finance' debate: positive credit and negative squared credit terms, typically significant at the 5% level. Focusing on the diagnostics for the GMM estimators, we find some evidence of the dynamic requirement of significant first-order but insignificant second-order serial correlation in the residuals — marginally so for the system GMM models. The test results for the validity of the overidentification restrictions are mixed: Sargan tests reject throughout, whereas Hansen tests look much better. Since we have moderate numbers of instruments, we put more emphasis on the latter results (which are robust but weakened by instrument proliferation).

D Full Results Tables

Overleaf:

Table E-1 – Advanced Country sample, thresholds 65% to 120% credit/GDP, control sample cut-off is threshold less 25%.

Table E-2 – Advanced Country sample, thresholds 92% and 119% credit/GDP, with a range of control sample cut-offs.

Table E-1: Too much Finance? PCDID Threshold regression ATET results (thresholds from 65% to 120%)

Higher cut-off	65	70	75	80	85	90	95	100	105	110	115	120
Lower cut-off	40	45	50	55	60	65	70	75	80	85	90	95
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Threshold Effect (ATET)	1.298	1.047	0.898	10.806***	14.460***	10.831***	7.844***	4.047***	2.824	4.449*	3.765*	10.478***
	[1.517]	[1.909]	[1.632]	[2.129]	[3.150]	[3.053]	[2.016]	[1.517]	[2.214]	[2.315]	[2.034]	[1.774]
Inflation	0.024	0.109	0.142	-0.711***	-0.975***	-0.335**	-0.661***	-0.723***	-0.791***	-0.809***	-0.969***	-1.182***
	[0.105]	[0.153]	[0.159]	[0.208]	[0.265]	[0.155]	[0.198]	[0.232]	[0.261]	[0.270]	[0.267]	[0.444]
Trade Openness	24.305***	25.615***	32.038***	67.525***	73.011***	70.632***	63.642***	51.577***	55.753***	52.019***	59.179***	74.146***
	[5.188]	[6.110]	[7.234]	[10.731]	[11.351]	[9.531]	[10.594]	[12.341]	[11.996]	[13.032]	[13.153]	[11.658]
Treated Countries	59	55	48	42	40	40	36	33	31	28	25	23
Treated Observations	2352	2214	1968	1810	1752	1740	1610	1463	1398	1286	1204	1110
Wald test controls (p)	0.62	0.06	0.30	0.00	0.00	0.00	0.00	0.02	0.04	0.06	0.02	0.00
Alpha test (p)	0.04	0.93	0.95	0.37	0.90	0.70	0.25	0.00	0.28	0.19	0.06	0.28
Control Countries	38	36	39	36	31	25	23	18	14	15	16	14
Control Observations	1378	1278	1403	1166	1037	785	767	646	553	600	618	540
Alternative Factor specifications: Coefficient for Threshold Effect												
1 Factor	7.476***	6.499***	6.431**	9.975***†	11.678***†	9.115***†	5.957**†	5.731*†	5.920*†	7.349*	9.260***	11.046***†
2 Factors	5.054**	5.122***	3.933**	14.122***†	11.171***†	11.198***†	7.482***†	6.664**†	6.915**†	6.748*†	8.367***†	11.264***†
3 Factors	5.236***	5.552***	4.288**	10.616***†	13.530***†	10.517***†	5.971***†	5.904***†	4.555†	3.652†	4.820†	14.210***†
4 Factors	1.298	1.047	0.898	10.806***†	14.460***†	10.831***†	7.844***†	4.047***†	2.824†	4.449*†	3.765*†	10.478***†
5 Factors	1.919	2.887*	1.569	9.988***†	11.329***†	10.252***†	4.748***†	5.497***†	2.137†	3.557	4.304**†	11.153***†
6 Factors	1.198	2.650	1.377	10.810***†	11.859***†	9.723***†	4.476***†	3.727**†	3.252†	4.145	3.844**†	13.880***†

Notes: We present robust means (and absolute standard errors) 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. The p-value for a Wald test 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 is provided. In the lower panel the failure of this test is indicated using †. The Alpha test is for weak parallel trends, the p-value for the test is provided (this applies to all factor augmentations).

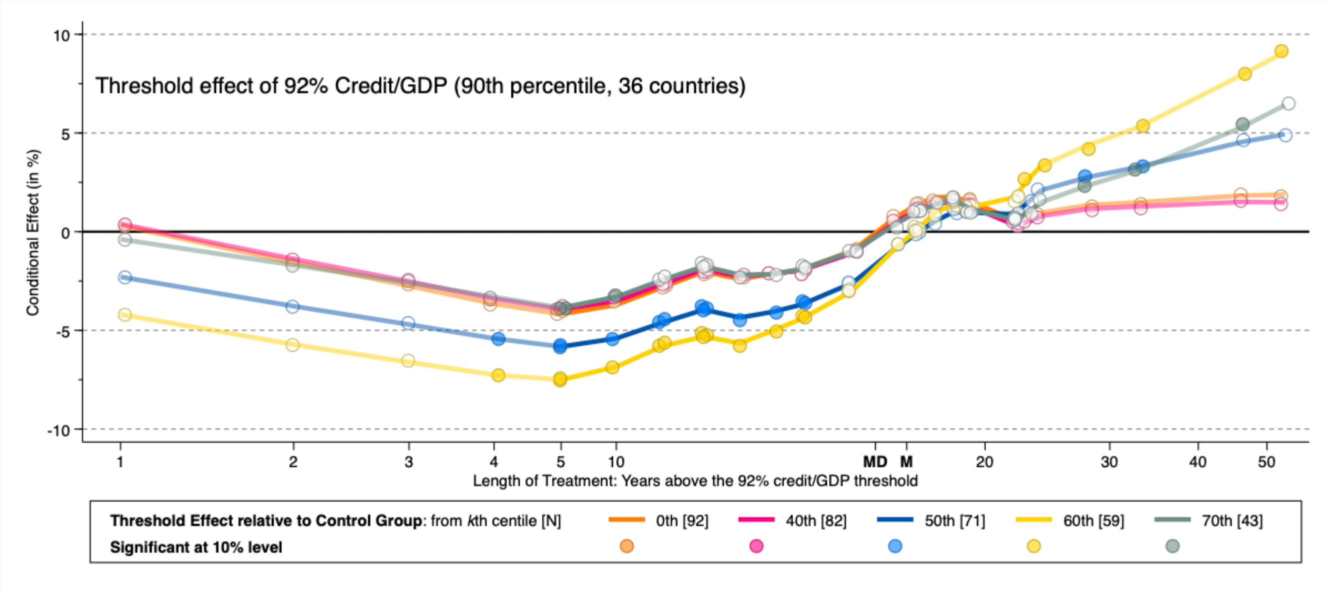
Table E-2: Too much Finance? PCDID Threshold regression ATET results (92% and 119% thresholds)

Higher cut-off	92% Credit/GDP (90th pctile)					119% Credit/GDP (95th pctile)				
Lower cut-off	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
Alpha test (p)	0.64	0.32	0.65	0.91	0.68	0.40	0.18	0.33	0.49	0.09
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*†

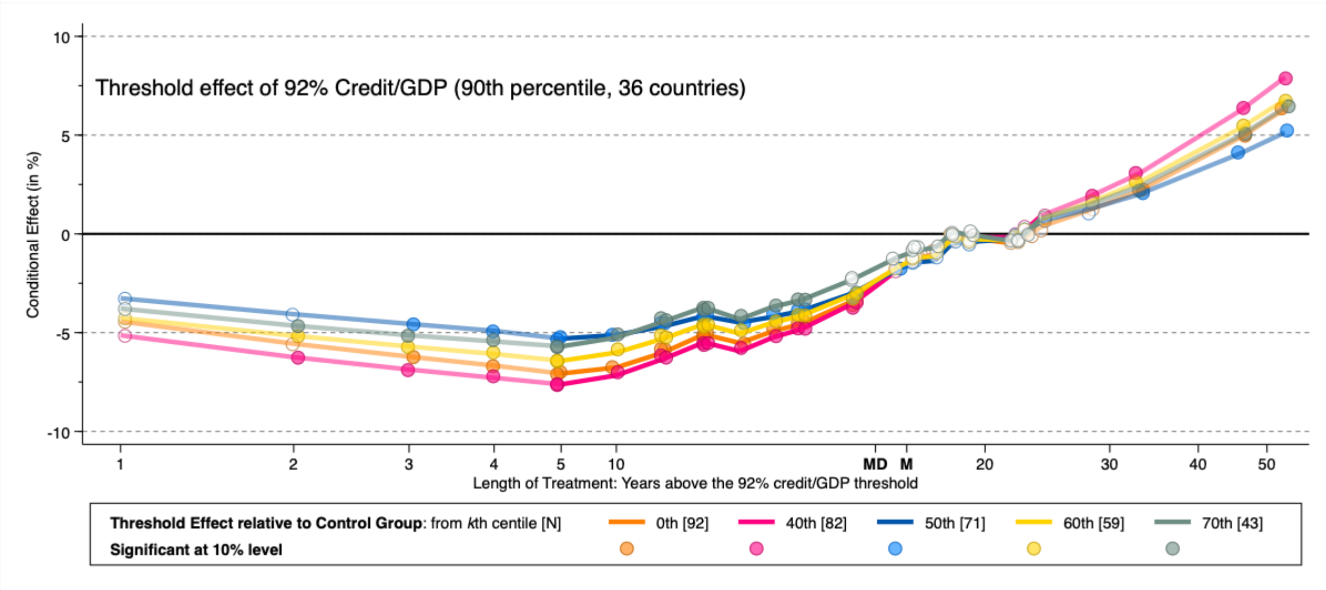
Notes: We present robust means (and absolute standard errors) for the PCDID country estimates in the 'treated' sample of countries, where treatment is defined by having overcome a threshold of 92% or 119% of credit/GDP, respectively. The estimates here are averages across heterogeneous treatments in 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.

E PWT Production Function. . . or Not

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



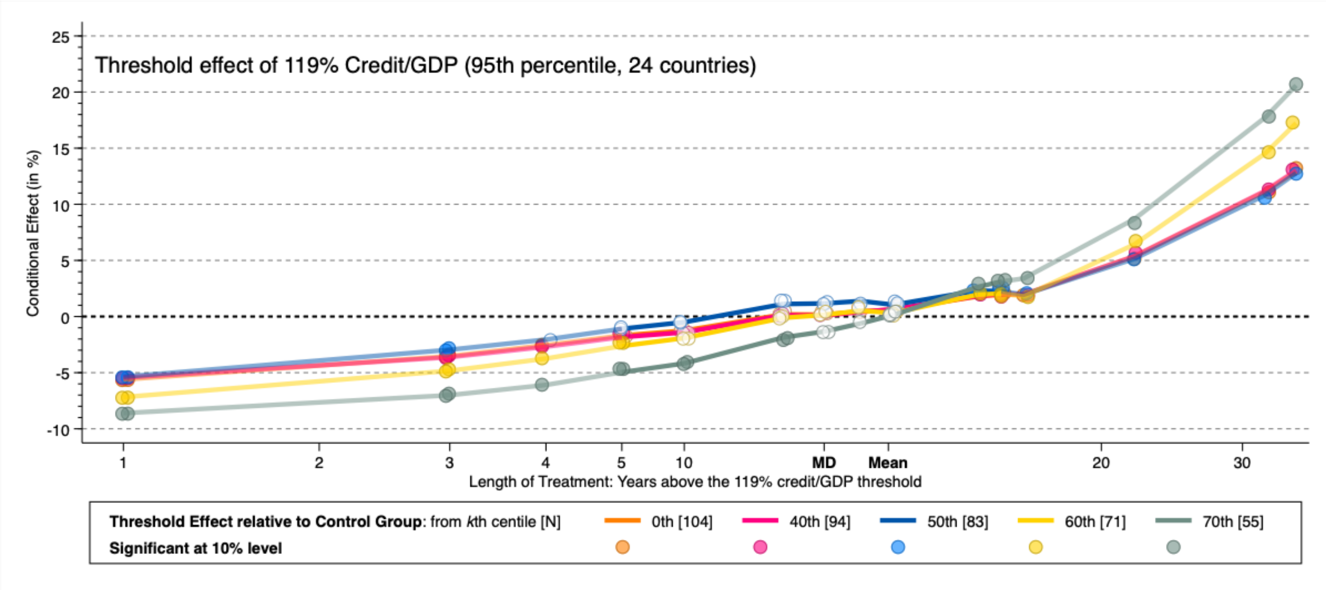
(a) Empirical Model without Capital Stock as Control — 92% Threshold



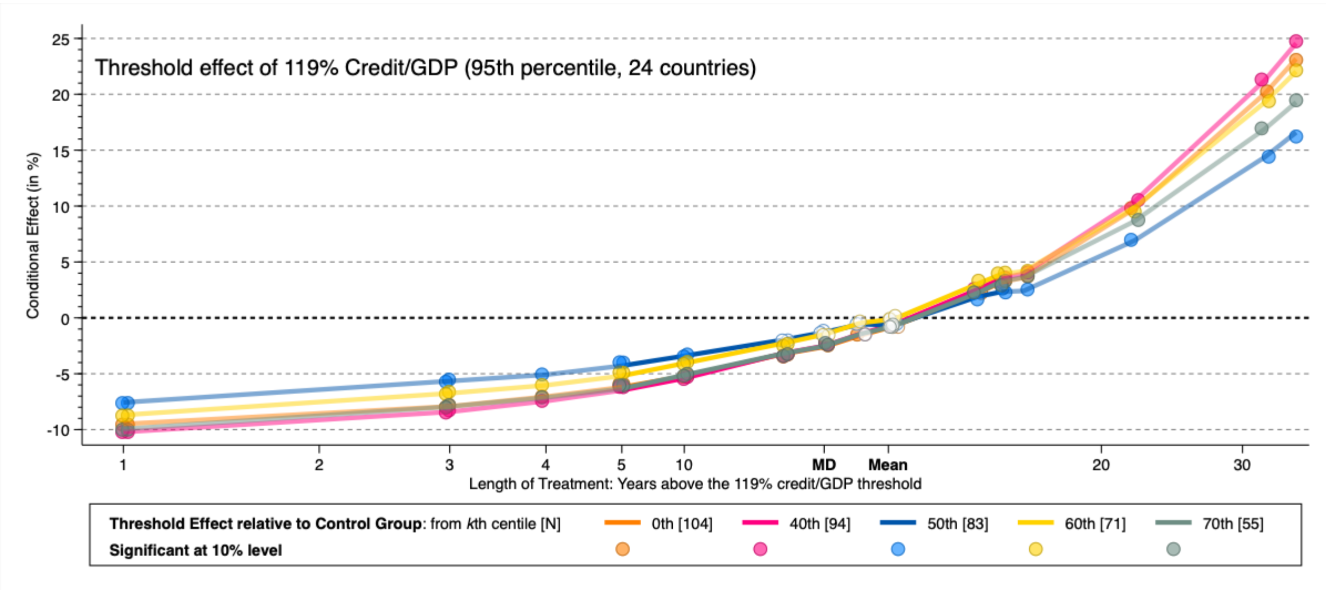
(b) Empirical Model with Capital Stock as Control (Production Function) — 92% Threshold

(Continued overleaf)

Figure E-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.

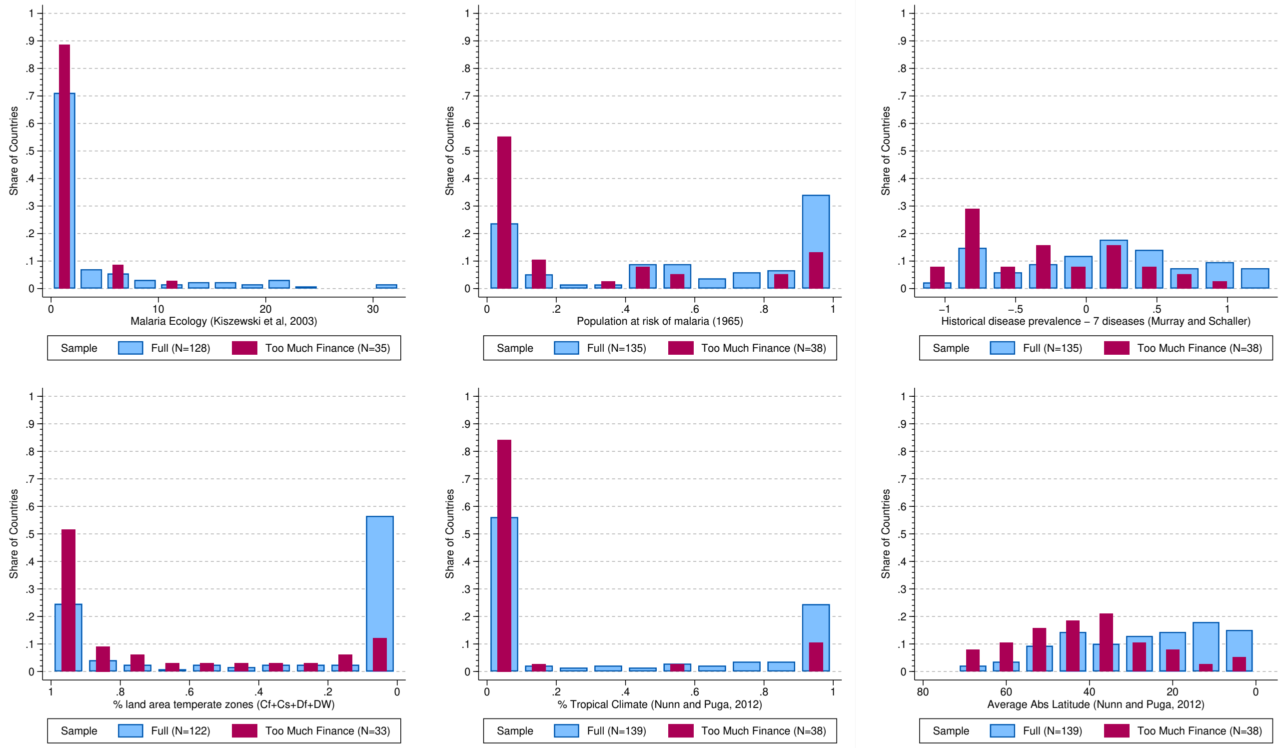
F Deep Determinants of Comparative Development

Legal Origin Data on British and French legal origin are taken from La Porta et al. (2008).

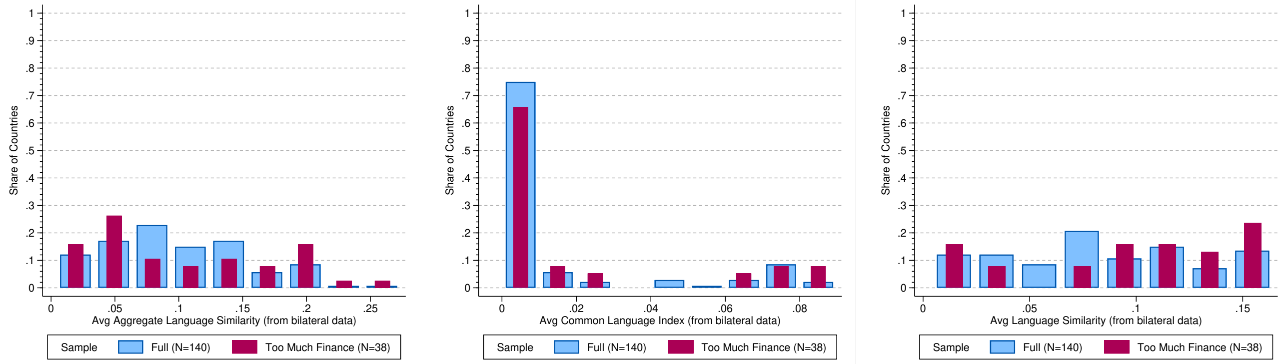
Geography Malaria ecology is taken from (Kiszewski et al. 2004), the share of population at malaria risk in 1965 from (Conley et al. 2007). Historical prevalence of seven infectious diseases is from the database of (Murray & Schaller 2010). Average absolute latitude and share of tropical climate are from Nunn & Puga (2012), share of temperate climate from Spolaore & Wacziarg (2013).

Culture The three language similarity measures are averages from bilateral data in DICL (Gurevich et al. 2021), share of European settlers in 1900 is from Gorodnichenko & Roland (2017) and share of descendants from Europeans is provided in Spolaore & Wacziarg (2013).

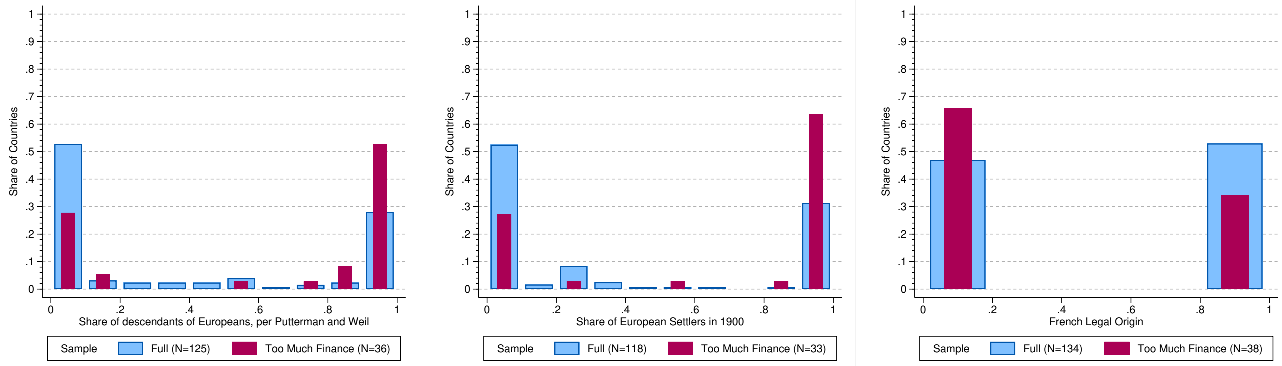
Figure F-1: Deep Determinants in our treated sample vs the full sample



(a) Geography



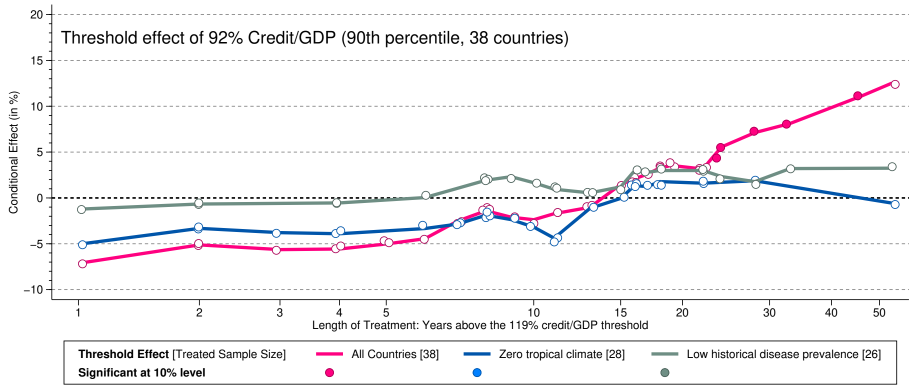
(b) Culture: Language



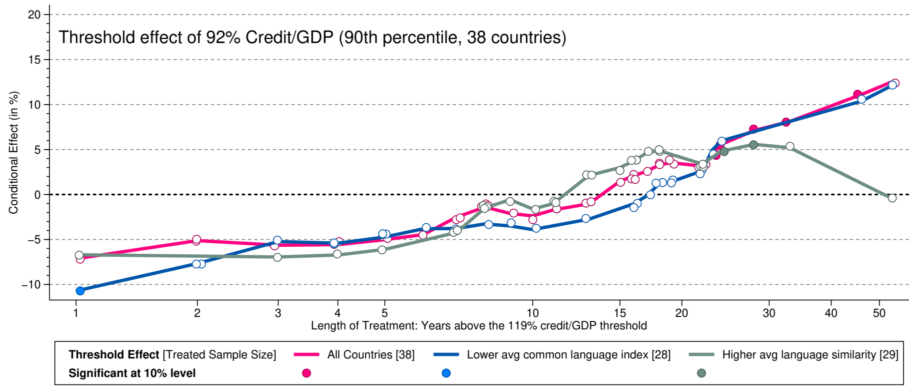
(c) Culture: European Origins (left two plots); Legal Origin: French LO (right plot)

Notes: Each plot provides a histogram of the distribution of countries in our treated sample (92% threshold, 38 countries) and the full sample (140 countries) for a proxy for geography, legal origins or culture. Some x-scales are reversed, so that values to the left are hypothesised in the literature to be more conducive to long-run development (e.g. low historical disease environment or not having French legal origin). The histogram for British legal origin indicates identical distribution between our treated sample and the full sample.

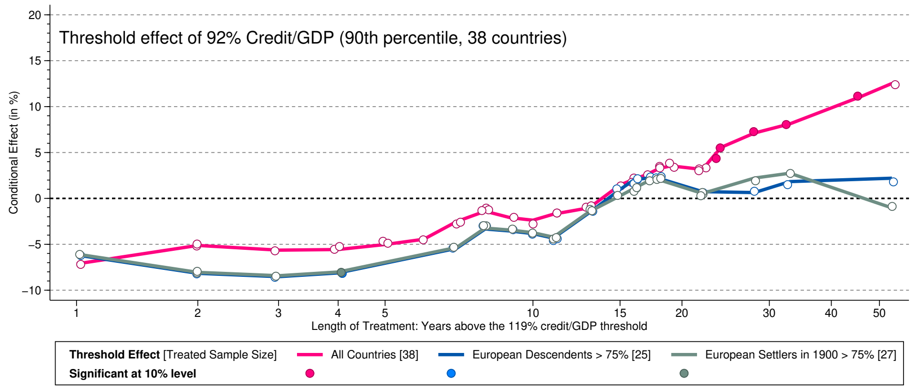
Figure F-2: Deep Determinants of 'Too much finance'?



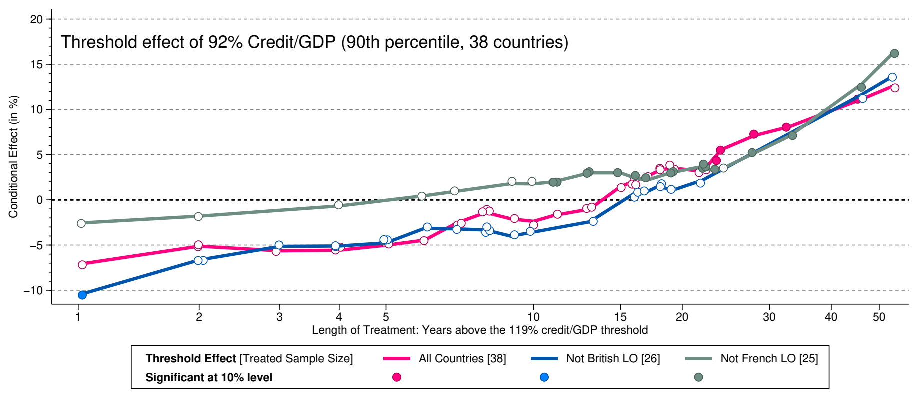
(a) Geography: Being outside the Tropics and Low Historical Disease Prevalence



(b) Culture: Language (Dis)Similarity



(c) Culture: European Descendants



(d) Legal Origin: Not to have French or British Legal Origin

Notes: We investigate the heterogeneity of our 'too much finance' results by studying the relationship with 'deep determinants'. We adopt the PCDID estimates for the 92% credit/GDP threshold, with control countries those with a peak credit/GDP between 47% and 91%. A filled (hollow) marker indicates statistical (in)significance at the 10% level.

G Distinguishing Household and Firm Credit

Table G-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 Rep	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 G-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		
cut-off	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
Alpha test (t)	3.31	6.06	4.88	2.63	2.95	4.55	1.67	1.05	0.36
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 (and absolute standard errors) 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.

H Mean (ATET) Estimates — banking crisis analysis

Table H-1: Too Much Finance & Banking Crises

Higher cut-off	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 H-1: Too Much Finance and Banking Crises (continued)

Higher cut-off	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<cut-off	29	29	29	29	29	25	25	25	25	25
Crises>cut-off	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, following our model in equation (6). 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; absolute t-ratios are reported in square brackets. Across columns we vary the control sample by setting a lower cut-off: countries below this cut-off are dropped from the control group. The full sample is labelled as 0th percentile. These results include four common factors estimated from the control samples, results for 1-6 factors are available on request. We confirm that the factor-augmented model has better predictive power than that without factors using comparison of AUROC statistics (not reported). 'ROC Comp (p)' reports p-values for equivalent tests for the exclusion of the variable of interest and 'ROC Inter (p)' for equivalent test for the exclusion of the interaction effect. 'Crisis Prop' is the unconditional propensity of a banking crisis in the sample indicated. 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.

Table H-2: Finance and Banking Crises — EWS results (thresholds 65% to 120%)

Threshold	65	70	75	80	85	90	95	100	105	110	115	120
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Change in Credit/GDP (Credit Booms Gone Bust)												
Without additional controls												
$\hat{\beta}^{MG}$	2.034	1.383	1.538	1.699	1.967	1.735	1.608	2.452	2.471	2.129	2.403	1.670
	[2.29]**	[1.50]	[1.97]**	[1.96]**	[2.45]**	[2.17]**	[1.69]*	[3.29]***	[3.21]***	[2.87]***	[2.92]***	[2.10]**
$\hat{\beta}^{A}$	1.527	1.152	0.464	4.197	5.083	3.203	2.052	2.453	1.962	0.526	1.903	1.063
	[0.68]	[0.84]	[0.37]	[1.78]*	[2.50]**	[2.03]**	[1.64]	[2.24]**	[1.53]	[0.47]	[1.87]*	[1.13]
$\hat{\beta}^{B}$	-2.078	-1.959	-2.44	-4.491	-3.546	-3.55	-1.565	-0.115	0.196	2.222	0.726	-0.406
	[0.72]	[0.66]	[0.70]	[1.72]*	[1.26]	[1.54]	[0.72]	[0.06]	[0.09]	[1.09]	[0.31]	[0.19]
ROC Inter (p)	0.005	0.023	0.011	0.050	0.030	0.039	0.039	0.101	0.108	0.119	0.105	0.093
Controlling for Change in capital inflow/GDP, Inflation, GDPpc Growth and Change in GFCF/GDP (MA)												
$\hat{\beta}^{MG}$	2.910	3.218	3.189	2.945	3.370	4.274	3.335	2.763	2.983	2.695	2.220	2.759
	[2.36]**	[2.83]***	[3.01]***	[2.66]***	[2.93]***	[4.43]***	[3.24]***	[2.68]***	[2.60]***	[2.64]***	[2.47]**	[2.51]**
$\hat{\beta}^{A}$	0.727	1.06	1.454	4	4.255	3.176	2.206	2.908	0.192	-0.660	1.220	1.717
	[0.18]	[0.34]	[0.39]	[1.08]	[2.03]**	[1.32]	[1.17]	[1.50]	[0.11]	[0.40]	[0.60]	[1.10]
$\hat{\beta}^{B}$	-1.441	3.065	1.884	1.291	-3.342	-0.806	-2.679	-0.48	1.245	2.226	2.116	1.662
	[0.31]	[0.78]	[0.40]	[0.26]	[1.47]	[0.32]	[0.94]	[0.20]	[0.49]	[0.94]	[0.88]	[0.74]
ROC Comp Inter (p)	0.017	0.063	0.045	0.031	0.064	0.204	0.531	0.404	0.948	0.806	0.659	0.131
Panel B: Change in capital flows/GDP (Excessive Capital Flows I)												
Without additional controls												
Δcap flows/GDP $\hat{\beta}^{MG}$	6.069	1.321	7.530	2.158	5.391	4.721	7.326	4.695	1.951	1.499	1.918	-0.413
	[1.74]*	[0.36]	[2.07]**	[0.60]	[1.52]	[1.39]	[2.62]***	[3.09]***	[2.01]**	[1.48]	[1.61]	[0.22]
Below cut-off $\hat{\beta}^{A}$	4.574	4.061	1.351	2.613	2.48	-1.549	-0.414	-7.851	-0.739	-0.301	-0.944	0.071
	[0.60]	[0.62]	[0.20]	[0.35]	[0.38]	[0.24]	[0.05]	[1.17]	[0.28]	[0.14]	[0.43]	[0.03]
Above cut-off $\hat{\beta}^{B}$	-5.595	-7.35	-0.056	3.779	6.026	8.96	0.957	4.681	5.493	2.646	4.678	7.01
	[0.53]	[0.87]	[0.01]	[0.43]	[0.76]	[0.96]	[0.11]	[0.46]	[0.91]	[0.58]	[1.13]	[1.45]
ROC Comp Inter (p)	0.001	0.001	0.001	0.001	0.001	0.002	0.002	0.003	0.009	0.020	0.041	0.009

(Continued Overleaf)

Table H-2: Finance and Banking Crises — EWS results (thresholds 65% to 120%) — continued

Threshold	65	70	75	80	85	90	95	100	105	110	115	120
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel B (continued): Change in capital flows/GDP (Excessive Capital Flows I)												
Controlling for Change in Credit/GDP, Inflation, GDPpc Growth and Change in GFCF/GDP (MA)												
$\hat{\beta}^{MG}$	0.865	2.992	5.138	7.524	8.719	-0.205	-1.015	-5.711	-1.046	-1.504	-1.536	-1.532
	[0.17]	[0.57]	[1.11]	[1.27]	[1.65]*	[0.04]	[0.19]	[1.03]	[0.55]	[0.72]	[0.77]	[0.75]
$\hat{\beta}^{A}$	-7.304	-0.138	0.98	5.061	4.333	1.836	-4.792	-7.08	-0.824	0.791	-1.379	-2.666
	[0.61]	[0.01]	[0.09]	[0.36]	[0.33]	[0.15]	[0.49]	[0.91]	[0.16]	[0.24]	[0.59]	[0.94]
$\hat{\beta}^{B}$	20.702	6.434	12.378	5.471	11.805	16.627	1.73	8.706	3.711	0.323	-0.816	5.116
	[1.48]	[0.44]	[0.84]	[0.38]	[0.71]	[0.94]	[0.19]	[0.80]	[0.69]	[0.07]	[0.18]	[1.08]
ROC Comp (p)	0.002	0.001	0.001	0.001	0	0.014	0.003	0.024	0.046	0.035	0.02	0.004
ROC Comp Inter (p)	0.029	0.006	0.044	0.054	0.08	0.171	0.049	0.13	0.487	0.24	0.448	0.133
Panel C: Square of gross capital flows/GDP (Excessive Capital Flows II)												
Without additional controls												
Δcredit/GDP $\hat{\beta}^{MG}$	2.704	2.594	3.361	2.725	2.236	2.391	2.138	2.342	0.704	0.941	0.745	0.758
	[3.06]***	[2.58]***	[3.26]***	[3.75]***	[2.92]***	[2.94]***	[2.55]**	[2.09]**	[2.61]***	[2.66]***	[2.41]**	[2.42]**
Below cut-off $\hat{\beta}^{A}$	1.607	0.668	1.498	2.778	2.748	3.447	4.004	5.581	1.77	1.799	0.715	1.375
	[0.94]	[0.43]	[1.77]*	[1.94]*	[1.56]	[1.99]**	[1.86]*	[2.81]***	[2.77]***	[3.26]***	[1.93]*	[2.09]**
Above cut-off $\hat{\beta}^{B}$	-0.794	-1.716	0.987	-0.996	-2.136	-3.688	-3.247	-4.738	-1.349	-1.05	-0.389	-1.169
	[0.57]	[0.68]	[0.54]	[0.81]	[0.99]	[1.92]*	[1.90]*	[2.29]**	[2.09]**	[1.64]	[0.93]	[1.65]*
ROC Comp Inter (p)	0.000	0.000	0.000	0.001	0.000	0.003	0.016	0.119	0.116	0.120	0.056	0.011

(Continued Overleaf)

Table H-2: Finance and Banking Crises — EWS results (thresholds 65% to 120%) — continued

Threshold	65	70	75	80	85	90	95	100	105	110	115	120
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel C (continued): Square of gross capital flows/GDP (Excessive Capital Flows II)												
Controlling for Change in Credit/GDP, Inflation, GDPpc Growth and Change in GFCF/GDP (MA)												
$\hat{\beta}^{MG}$	3.742	4.478	5.250	5.222	4.932	2.843	5.072	-0.129	0.225	0.390	0.545	0.800
	[2.66]***	[2.82]***	[3.11]***	[3.05]***	[2.64]***	[2.24]**	[2.14]**	[0.18]	[1.46]	[1.76]*	[2.23]**	[1.24]
ROC Comp (p)	0.011	0.008	0.007	0.016	0.007	0.036	0.181	0.069	0.028	0.092	0.059	0.021
$\hat{\beta}^{A}$	-1.068	0.697	2.527	1.443	2.013	2.373	2.929	5.279	2.512	1.572	0.331	2.067
	[0.45]	[0.30]	[0.81]	[0.93]	[0.97]	[0.91]	[1.23]	[1.61]	[2.66]***	[1.96]**	[0.94]	[2.14]**
$\hat{\beta}^{B}$	-0.110	-0.317	0.807	0.396	-2.275	-0.221	-1.636	-5.281	-0.988	-0.546	-0.048	-0.950
	[0.04]	[0.10]	[0.38]	[0.18]	[0.72]	[0.08]	[0.61]	[1.94]*	[1.52]	[1.09]	[0.07]	[1.43]
ROC Comp (p)	0.001	0.001	0.002	0.001	0.001	0.004	0.085	0.076	0.081	0.125	0.057	0.018
ROC Inter (p)	0.022	0.018	0.052	0.011	0.014	0.085	0.336	0.265	0.282	0.280	0.219	0.183
Treated Sample												
Countries	41	38	35	34	33	32	29	26	24	23	24	23
Observations	1267	1210	1125	1060	1042	1006	969	869	813	778	806	767
Crisis Prop.	0.048	0.048	0.047	0.048	0.048	0.048	0.047	0.047	0.049	0.05	0.05	0.05
Treated Crises<cut-off	29	27	27	27	29	29	30	26	26	25	26	25
Treated Crises>cut-off	32	31	26	24	21	19	16	15	14	14	14	13
Control Sample												
Countries	20	19	20	15	13	11	12	11	11	11	9	6
Observations	576	505	547	386	338	273	347	324	339	356	304	212
Crisis Prop.	0.064	0.063	0.064	0.065	0.062	0.055	0.046	0.046	0.044	0.042	0.036	0.042

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 k of between 65% and 120% of credit/GDP, following our model in equation (6). The control sample is defined as countries with peak credit/GDP between $k - 25\%$ and k. 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; absolute t-ratios are reported in square brackets. We confirm that the factor-augmented model has better predictive power than that without factors using comparison of AUROC statistics (not reported). 'ROC Comp (p)' reports p-values for equivalent tests for the exclusion of the variable of interest and 'ROC Inter (p)' for equivalent test for the exclusion of the interaction effect. 'Crisis Prop' is the unconditional propensity of a banking crisis in the sample indicated.