Banking Crises and the International Transmission of Vulnerability

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Abstract: We investigate early warning systems for banking crises in a new dataset of 21 economies covering the past two centuries. A 'near-consensus view' has recently emerged in the empirical macro literature assigning credit growth – perhaps in interaction with other macroeconomic factors – a primary role in predicting crisis events. In this paper we investigate the role and patterns of cross-country spillovers of 'vulnerability' in helping to predict banking crises. Employing novel panel time series methods we highlight secular changes in the impact and predictive power of credit growth as well as of unobserved factors (including but not limited to spillovers and the transmission of vulnerability). A weakening in the part played by credit growth over the 20th century, concurrent with a rise in predictive power assigned to unobservables question the apparent weight given to historical analyses for present-day policy. Our next analytical steps will be (i) to consider additional variables including measures for broad money and bank asset, (ii) to attempt to provide more structure for the patterns and dynamics of these nebulous unobservables, and (iii) to highlight country- or group-specific idiosyncracies in the evolution of the credit-crisis nexus.

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1 Introduction

The theoretical and empirical investigation of the determinants of banking crises, along with the construction of early warning systems, has seen a wave of renewed interest among academics and policymakers in the aftermath of the Global Financial Crisis of 2007/8, and a number of important contributions in this field have been made over recent years. Perhaps most relevant in the context of financial crises in (modern day) advanced economies is the insight that the relative rarity of crises in this group over the post-WWII period necessitates a *broadening* of the sample so as to increase the number of crisis events and thus provide 'better' identification of crisis determinants in empirical studies. Extending the sample to include *more countries* will likely not serve as a suitable solution, given the heterogeneity in the development of macroeconomic institutions (public and private) between advanced, emerging and developing economies. Instead, economic historians have come to address this issue by delving deep into the archives of central banks and statistical offices to provide datasets with substantially increased time series (Reinhart and Rogoff, 2009; Jorda, Schularick and Taylor, 2017) along with careful empirical analysis which has not only fashioned a "new (near consensus) view" (Bordo and Meissner, 2016: 31) of crisis determinants, centred on the dynamics of private credit growth, but has also set new benchmarks for this literature in terms of empirical implementation and testing.

Our main empirical point of departure from this vibrant and emerging literature is to suggest that existing work on financial crises (i) has (tacitly) treated the relationship between crises and their 'determinants' as common across all sample countries; (ii) has behaved in a somewhat cavalier fashion with regards to possible cross-country spillover effects (crisis transmission),[1] the possibility that some crises may have been triggered by events beyond the control of the individual country or that latent crisis signals may not not reflect the economic conditions within the country in question but instead in the world at large (van den Berg, Candelon and Urbain, 2008); and furthermore (iii) has *primarily* focused on analysing the entire sample period, which in case of Schularick and Taylor (2012) and related work amounts to 140 years and for Danielsson, Valenzuela and Zer (2016) to over 200 years. These studies provide results for sub-periods but lose out on the rich narrative arising from the analysis of a rolling time window we pursue in the present study. Perhaps most significantly, this allows us to gauge the relative dynamic evolution of contagion/unobservables and the transmission of international vulnerability on the one hand, and domestic fundamentals, referring to any observed variable of country characteristics included in the empirical model, on the other.

Our empirical approach implements (among other specifications) the widely favoured binary choice model approach (e.g. Schularick and Taylor, 2012; Anundsen, Gerdrup, Hansen and Kragh-Sørensen, 2016; Danielsson, Valenzuela and Zer, 2016) on crisis events data but allows for unrestricted cross-country heterogeneity in crisis determinants and accounts for unobserved spillover and contagion effects. Thus we seek to go beyond the standard approach which provides crisis determinants and the predictive fit of the empirical model *for the entire sample of countries*; instead we offer insights into the differential patterns of these determinants and their empirical fit *across countries*.

The remainder of this study is organised as follows: we discuss some of the main findings of the recent literature in Section 2, then discuss some general aspects of the empirical modelling of financial crises along with the data sources in Section 3. Empirical estimators are discussed in Section 4, 5 presents the descriptive and regression results, before we conclude.

[1] We avoid the use of 'contagion' in our discussion since this is typically associated with 'irrational' behaviour of individual agents or an increase in stock price (index) correlation across countries following a financial shock or crisis in country a. The spillover of vulnerability we attempt to capture is not necessarily irrational and in terms of timing of a spillover is not limited to or even necessarily linked to a financial crisis in country a.

2 A History of Banking and Credit since 1800

3 Modelling and Data

The empirical modelling of crisis events is in most cases carried out using logit or probit models with country fixed effects thrown in to allow for a 'within-country' interpretation of the result and therefore a more plausably causal interpretation. Here, crisis events are signified by their start date, and the binary dependent variable is set equal to unity in the crisis start years and to zero in all other periods. With some exceptions – e.g. Bussiere and Fratzscher (2006) and Caggiano, Calice and Leonida (2014) who analyse crisis start and continuation in a multinomial logit model – ongoing crisis years are dropped from the sample. Typically, the standard empirical models of crisis prediction are static models, given the difficulty of implementing and interpreting nonlinear models with lagged dependent variables.[2] In the following we describe a number of modelling choices and justify each of them in turn.

3.1 Dealing with Slow-Moving Dynamics and the Duration of Crisis Events

An important aspect of the empirical modelling of financial crises is how to take account of the pre-crisis 'dynamics' of macro variables in the construction of an 'early warning' approach to crisis prediction. The standard practice in the papers reviewed in Papi, Presbitero and Zazzaro (2015, Table 2) and Klomp (2010) is to lag the regressors, typically by just a single time period (year).

The seminal contribution by Schularick and Taylor (2012) in contrast develops a methodology repeated in many of the papers by these authors using long time series, whereby crises events are primarily modelled using lag polynomials of growth rates for single variables (credit, narrow or broad money, external imbalances). The combined marginal effects are then calculated along with a measure of in-sample predictive power (the area under the Receiver Operating Characteristic curve, AUROC); the AUROC statistic (values over 0.5 indicate meaningful predictive power for a model) lends itself to statistical comparison across models, which enables these authors to judge the relative merit of different specifications: their empirical strategy is to iterate the above with different macro-variables, either adopting alternative proxies to their preferred crisis determinant of credit (the 'credit view') or 'rival' determinants (the 'money view'), building on theoretical arguments and the vast new data series they have brought to this literature. This empirical strategy has the advantage of not merely comparing the statistical significance and magnitude of coefficients or marginal effects, but focusing on predictive power: 'rival' determinants then either show limited statistical or economic significance, or they provide inferior predictive power than the 'credit view,' which makes for a strategy based on economic theory and embedded in statistical testing. We adopt these tools throughout our empirical analysis below.

An alternative approach to capture the dynamics is to use simple averages of RHS variables over certain time periods – e.g. Danielsson, Valenzuela and Zer (2016) chose 5-year averages from $t-5$ to $t-1$ for all covariates – or to provide a broader definition of the dependent variable covering several time periods, e.g. Anundsen, Gerdrup, Hansen and Kragh-Sørensen (2016) pick a forward-looking crisis dummy equal to one if a crisis occurs 5 to 12 quarters ahead and zero otherwise. Bussiere and Fratzscher (2006), were among the first to adopt this specification using a 12-months forward-looking (currency) crisis dummy. Whether the crisis dummy is forward-looking or the covariates are lagged is a somewhat inconsequential choice.

[2] Danielsson, Valenzuela and Zer (2016) are an exception here, estimating the vast majority of their models with a five-year average of crisis events from $t-5$ to $t-1$ as additional covariate. Parametric fixed effects estimators are biased and inconsistent when lagged dependent variables are included as regressors (e.g. Kennedy, 2008: 291).

A third approach is to use the moving averages of regressors, as practiced by Jorda, Schularick and Taylor (2011, 2016) and Reinhart and Rogoff (2011). This has the advantage of being somewhat less wasteful in terms of degrees of freedom (important in our heterogeneous logit regression case), while also allowing for the convenient adoption of interaction effects. Due to the demands of our heterogeneous probit estimation we adopt this strategy throughout.

Next, there is the question of how to treat crisis years following the start of a crisis. In the present sample our 148 crisis start years (counting only those analysed in the regressions) are followed by 241 ongoing crisis years (around 5.5% of the sample) – this indicates that the average crisis lasted between one and two years. A second data concern relates to the Great War and World War II, both of which represent significant structural breaks in all economic aspects of our sample countries, and the observations for these periods are therefore typically omitted in long-run analysis: this leads to a further reduction by 315 country-years (just shy of 8% of the sample).[3]

3.2 Levels versus Growth Rates or Ratios

A common practice in parts of the empirical literature is to include macroeconomic variables in levels – primarily per capita GDP – to the crisis prediction model (*inter alia* Beck, Demirguc-Kunt and Levine, 2006; Aizenmann and Noy, 2013; Danielsson, Valenzuela and Zer, 2016). This is of concern when these macroeconomic variables display stochastic trends (i.e. if these variables are nonstationary): the theoretical time series literature (Park and Phillips, 2000)[4] suggests that this data property leads to stark outcomes whereby the sample proportion of binary choices follows an arc sine law, meaning it is either close to zero or close to unity most of the time, implying either large numbers of repeated crises in individual countries alongside the virtual absence of crisis in all others. Although our sample countries experience up to nine crises in the case of Italy and the US, it would be difficult to suggest our data represent an empirical example of the stochastic process just described, especially given the sparsity of crisis events post-1945 (see Section 3.4 for details). In our empirical application we therefore focus on growth rates or variables with natural threshold character (e.g. ratios relative to GDP such as M2/GDP) which are less likely to be characterised by a stochastic trend.

3.3 Capturing International Transmission of Vulnerability

Our empirical setup employs a common factor model, which enables us to allow for time-varying unobserved heterogeneity, to model spillovers of instability across countries and omitted crisis determinants (see Section 4 below). The notion of 'waves' of crises in several countries is widely recognised in the (empirical) literature (Reinhart and Rogoff, 2009), though few studies attempt to model or exploit this in their empirics. Eichengreen, Rose and Wyploc (2000) include a dummy for 'crisis elsewhere' in their analysis of currency crises in advanced economies. They provide two conceptual motivations for the international transmission of vulnerability: first, currency crises or speculative attacks on currencies may have detrimental effects on the international competitivenss of major trading partners,[5] such as Ireland in the case of the UK's 1992 sterling crisis or Portugal in the case of attacks on the Spanish pesata in 1992/3. More recently, Cesa-Bianchi, Martin and Thwaites (2017) have revisited this notion in the context of banking crises, adding weighted cross-section averages of the variable of interest (in their case credit growth) for all $N-1$ countries to covariates of country i in their pooled logit regressions, arguing that banking crises have a global dimension

[3] The time period is not limited to the years of active conflict, we follow the suggestion in Schularick and Taylor (2012) and drop 1914-9 and 1939-1947 from the sample.

[4] To the best of our knowledge there does not exist any literature on this topic in the pooled panel context.

[5] The authors also cite the case of Sweden following Finland's devaluation in 1992 which was said to have been caused not by mutual trade but by export competition in similar third markets.

and that foreign booms can lead to domestic busts. This notion of a global dimension of financial crises and the high level of interconnectedness of all types of markets in advanced economies is very close what we focus on in this paper. While our empirical approach in the first instance is agnostic about the nature of the unobservables (be they omitted variables or contagion/spillovers), we subsequently attempt to uncover the relevance of 'core-country' variables for the rest of the panel, creating country-specific core country measures using time-varying (bilateral trade) weights inspired by the literature on knowledge spillovers (Keller, 2011; Ang and Madsen, 2013). An alternative approach would be to consider the 'bilateral dependence' measure introduced in Morelli and Sonno (2017).

3.4 Data

Many of the insights into the empirical specification as well as diagnostic testing we pursue were gained from the set of studies using historical data for a limited number of modern-day advanced economies by Oscar Jorda, Moritz Schularick and Alan Taylor — these authors have recently released the Jorda-Schularick-Taylor (JST) Macrohistory Database (Jorda, Schularick and Taylor, 2017) which provides data for 17 countries for the 1870-2010 time horizon (unbalanced panel). As these authors have repeatedly suggested and in line with the arguments put forth by Reinhart and Rogoff (2009) the study of financial crises needs to go back in time to provide a richer analysis to inform present-day policy: the number of financial crises in the post-WWII period, at least for advanced economies, is simply quite limited and crisis determinants might therefore be only weakly or imprecisely identified.

In this study we adopt an alternative dataset to invest crises in the long-run based on Madsen and Ang (2016), who cover 1870-2010 for 21 countries (balanced panel). Through archival work and additional manipulation we push the starting point of these series back to 1800. We further hope to extend this dataset further to include countries in South America. In this study we aim to focus on the broader Madsen data but (eventually) refer to JST for robustness checks.

The data collated by Reinhart and Rogoff (2009) provides information on crisis events from the start of the 19th century for a large number of countries to match the Madsen data. For more recent years we adopt an update of Laeven and Valencia (2013) which brings the crisis events up to 2015. The availability of house price series in the JST and Madsen data enable us further to investigate the suggestion that their cycles/bubbles play "a central role" in financial crises (Reinhart and Rogoff, 2009: 142; Jorda, Schularick and Taylor, 2015; Anundsen, Gerdrup, Hansen and Kragh-Sørensen, 2016). We further plan to use historical stock price data to capture volatility and construct indicators for very high or very low volatility in the vein of Danielsson, Valenzuela and Zer (2016).

4 Empirical Implementation

In our empirical analysis we adopt a novel estimator developed for nonlinear panel models by Boneva and Linton (2017). Their implementation builds on the existing linear 'common correlated effects' (CCE) estimators developed by Pesaran (2006) and then adapted for the use in dynamic panels by Chudik and Pesaran (2015). In previous work (Eberhardt and Presbitero, 2015) we have applied these linear estimators to provide insights into the differences in the debt-growth nexus across countries; our present research on banking crises benefits from the advances in econometric theory to the study of (nonlinear) models (Chen, 2014; Chen, Fernandez-Val and Weidner, 2014) such as logit or probit estimators widely applied in the financial crisis literature.

Our empirical setup is based on a static binary choice model:

$$Y_{it}^* = \boldsymbol{\alpha}_i' \boldsymbol{d}_t + \boldsymbol{\beta}_i' \boldsymbol{X}_{it} + e_{it}, \tag{1}$$

where Y_{it}^* is a latent variable relating to the observed response variable, Y_{it} (in our case the crisis start date), via the indicator function $Y_{it}=\mathbb{1}(Y_{it}^*)$. Thus Y_{it} is equal to unity if $Y_{it}^* > 0$ and zero otherwise. d_t may include observed common factors (e.g. the global oil price) and country fixed effects (when $d_t = 1\,\forall t$).

So far the setup is identical to that in a standard probit or logit model, with the exception that in the panel we do not estimate a single common slope coefficient β but country-specific coefficients β_i. This cross-country heterogeneity in the impact of the crisis determinants is theoretically desirable from both an economic and econometric stand point: (a) economic arguments highlight the heterogeneity of causes of financial crises, even though in empirical practice this is typically only shown by splitting samples into advanced and emerging economies (Quintyn and Taylor, 2003; Klomp, 2010; Broner and Ventura, 2016); (b) econometric theory developed in Boneva and Linton (2017) shows that the incidental parameter problem arising for fixed effects estimation in discrete choice models (Neyman and Scott, 1948) does not arise if the coefficients are heterogeneous across countries.[6] Furthermore, the econometric argument suggests that naïve pooling of countries to improve efficiency may be counterproductive if predictive power of the model deteriorates in the process due to potential heterogeneity in the crisis determinants across countries (Van den Berg, Candelon and Urbain, 2008). The only existing empirical studies we are aware of which use a comparable heterogeneous parameter setup are Klomp (2010) who analyses a large sample of over 100 countries with a random coefficient logit estimator; and Summers (2017), who extends the analysis of Schularick and Taylor (2012) by estimating static and dynamic Bayesian panel probit models, finding considerable heterogeneity in average marginal effects across countries.[7] Alternative nonlinear estimators allowing for common factors (Chen, Fernandez-Val and Weidner, 2014; Chen, 2016) assume parameter homogeneity, but neither of these have been applied to the analysis of financial crises.

The international connectedness of economies and the related susceptibility to spillovers of financial stress (contagion) is widely recognised: as Bordo and Meissner (2016: 357) remark, "[b]anking crises have often been global or regional events," citing the 1890/1 Baring Crisis, the global instability of 1907, the Credit Anstalt Crisis of 1931 and the East Asian Financial Crisis of 1997/8 as events with "an international dimension" (ibid., 359). In these cases financial stress in the home country is initiated or exacerbated through capital flows or via fixed exchange rate arrangements amenable to interest rate shocks in foreign banks or foreign countries. The propagation of financial stress abroad to financial crisis at home via such contagion is a central theme of this study and we note a possible distinction between economic fundamentals and (pure) sentiment triggering crises. The formulation we just adopted also highlights the difficulty in adopting a straightforward indicator approach – e.g. using a dummy for 'crisis elsewhere' – in cases where financial stress elsewhere leads to crisis in the home but not the foreign economy. The second innovation in our empirical setup therefore is the presence of a multifactor error structure:

$$e_{it} = \boldsymbol{\kappa}_i' \boldsymbol{f}_t + \varepsilon_{it}. \tag{2}$$

[6] The problem arises from the limited number of observations available to estimate the country-fixed effects, which are 'nuisance' parameters in the sense that we are typically not interested in the fixed effects themselves but what they do to the slope coefficients on the variable(s) of interest. When N rises (asymptotically) and T is fixed, the number of these nuisance parameters to be estimated grows as quickly as N, which gives rise to the asymptotic bias. In the present data context the full sample regressions over 140 years are unlikely to be substantially biased, though the rolling window regressions over the shorter time horizon are much more susceptible.

[7] It is difficult to compare this approach with that pursued in the present paper, but it should be noted that Bayesian or frequentist approach aside the inclusion or omission of common factors leads to substantially different results in linear (e.g. De Visscher, Eberhardt and Everaert, 2017) and nonlinear models (e.g. Boneva and Linton, 2017).

The factor setup is equivalently referred to as 'interactive fixed effects' (IFE; Bai, 2009; Chen, Fernandez-Val and Weidner, 2014; Chen, 2016) or 'common correlated effects' (CCE; Pesaran, 2006) in the panel time series literature. On its own the cross-section dependence this setup induces in the model error terms (and if not addressed in the regression residuals) would merely affect the efficiency of the estimator. However, in most empirical applications it is appropriate to assume that the common factors are not merely correlated with the response variable, but also with the regressors:

$$X_{it} = \boldsymbol{A}_i'\boldsymbol{d}_t + \boldsymbol{K}_i'\boldsymbol{f}_t + u_{it}. \tag{3}$$

This ubiquity of common factors induces endogeneity between the observed regressors and the error term e_{it}, such that a naïve estimator which ignores the factor structure is asymptotically biased. An example in the case of crisis prediction would be that cross-country spillovers do not merely affect the propensity of a crisis occurring, but at the same time also influence the regressors in our model employed to predict crises: for instance, in a large sample of advanced and emerging economies Gourinchas and Obstfeld (2012) find that economic growth persistently stays below trend before a crisis-event.

The implementation adopted here follows Boneva and Linton (2017), who investigate a number of alternatives to address the presence of unobserved common factors in a heterogeneous probit or logit model. All of these share the assumption that the cross-section averages of the regressors X_{it} and the observed factor(s) d_t span the unobserved factors, which means we assume that the regressors are driven by the same common factors as the response variable Y_{it}. Furthermore it is assumed that the number of unobserved common factors does not exceed the number of regressors in the model, and that all of these are stationary. The common factors are proxied by the cross-section averages of the regressors:

$$Pr(Y_{it} = 1 \,|\, X_{it}', d_t, f_t) = \Phi(\alpha_i' d_t + \beta_i' X_{it} + \kappa_i' f_t) = \Phi([\alpha_i + \tilde{K}'\kappa_i] d_t + \beta_i' X_{it} + \psi_i' \overline{X}_t), \tag{4}$$

where ψ_i refers to country-specific coefficients on the cross-section averages and $[\alpha_i + \tilde{K}'\kappa_i]$ highlights that the intercept terms no longer have the same interpretation as in an unaugmented model. Like in the linear heterogeneous model (Pesaran and Smith, 1995) the 'Mean Group' Probit-CCE estimator $\hat{\beta}^{MG}$ of the slope parameters is then obtained as an average of the country coefficients.[8] Conveniently, the standard errors for $\hat{\beta}^{MG}$ can be computed in identical fashion (non-parametrically as the variation of the country estimates around the cross-country mean) to those for the linear estimator. Each estimate $\hat{\beta}_i$ is associated with a marginal effect (ϵ_i^{β}), and in our results we report average marginal effects and related standard errors using the above Mean Group principle.

Our third innovation is to treat the empirical model of banking crises as subject to structural breaks and changes in equilibrium. Elaborate. Until deposit insurance and lender of last resort arrangements became widespread in the aftermath of the Great Depression, banking crises we banking panics and the responsibility of markets rather than state institutions (Bordo and Meissner, 2016), whereas in the context of liberalised domestic and international financial markets the resolution of present-day banking crises is typically managed by governments, with all the implications for pre-crisis expectations and moral hazard this setup entails. Even just a casual look at the frequency distribution of banking crises in Figure 1 highlights different 'crisis regimes' – frequent and widespread crises until WWII, then entirely absent in the Bretton Woods period, finally rising since the mid-1960s albeit with a deceptive lull from the mid-1990s to mid-2000s – which point to fundamentally different policy regimes.

Our choice of empirical determinants (X) will stay close to theoretical narratives, such that empirical specifications can to speak to or reject economic arguments. We investigate 'single-factor' models

[8] We employ robust regression to compute the means, which is the standard practice in the application of linear CCE models – see Eberhardt and Teal (2013) for a discussion of the rationale.

in the mould of Schularick and Taylor (2012) to analyse the determinants of banking crises in a balanced sample of 21 primarily advanced economies over the 1870-2009 time horizon: we estimate separate empirical models for each of credit growth, money growth and bank asset growth adopting the following empirical specification:

$$\Phi(p_{it}) = \Phi(\alpha_i + \boldsymbol{\delta}_i' \boldsymbol{d}_t + \sum_{\ell=1}^{5} \beta_{i\ell} \Delta ln(X)_{i,t-\ell} + \boldsymbol{\lambda}_i' \boldsymbol{f}_t + \varepsilon_{it},) \tag{5}$$

where we use $\Phi(\cdot)$ as a shorthand for a logit or probit implementation. X is defined as credit/GDP, money/GDP and bank assets/GDP, adopting the extended data from Madsen and Ang (2016), and p is a dummy for the start year t of a banking crisis event in country i using the data from Reinhart and Rogoff (2009). α_i is a country fixed effect, which allows us to interpret the results as relative to the evolution *within* each country. $\boldsymbol{d}_t$ represents observed common factors such as the oil price or other 'global' variables – to avoid confusion in this empirical representation we have separated the unobserved country fixed effects from the observed common factor and its loadings. For different implementations we make different assumptions about the nature of the crisis determinants, in the sense that we follow the literature and assume slope homogeneity $\beta_{i\ell} = \beta_\ell \, \forall \ell$ in some models and allow for heterogeneity as specified in equation (5) in others: we start with pooled logit models (with and without country fixed effects) as favoured in the existing literature, then compare results with those from two versions of bias-corrected pooled logit models with country fixed effects (Fernandez-Val and Weidner, 2016). These pooled model results are then contrasted with robust sample averages from a naïve heterogeneous estimator (probit estimation at the country level without augmentation to capture common factors) and the Boneva and Linton (2017) Probit-CCE.

5 Results and Discussion

5.1 Descriptive Analysis

In this analysis we do not exclude any 'ongoing-crisis' years or conflict years.

Banking Crises Distribution of banking crises since 1800 – Figure 1.

Financial deepening This section highlights the difficulty of using credit/GDP as a measure to predict banking crises. In Figure 2 we present a number of sample descriptives for financial deepening and real credit growth. In panel (a) we combine the median credit/GDP ratio (thick black line) with the standard deviation of this variable (shaded area) – since one country (Sweden) experienced rather extreme levels during the early 20th century of up to 400% credit/GDP (the resulting standard deviation for the sample including Sweden is indicated) we focus on the sample of 20 countries excluding Sweden. Both median and standard deviation for credit/GDP rise relatively monotonically from 1800 to the Great Depression, whereupon they drop dramatically. Closer inspection however suggests the period up to the early 1930s can be divided further into an early modern phase ending around 1870 and the phase of the first financial globalisation from the 1860s onwards. With a few notable exceptions credit markets were still largely undeveloped or underdeveloped during the first half of the 19th century: the number of countries with a credit/GDP ratio of less than 1% (5%) only drops very gradually from 11 (14) in 1810, to 8 (14) in 1820, 7 (14) in 1830, 5 (13) in 1840, 4 (10) in 1850, 2 (8) in 1860 and (7) 1870 and 1 (3) in 1880 – Greece remained an outlier by this measure until 1941. Reaching 18%, 1863 represents the year in which median credit/GDP reaches double figures for the first time, rising dramatically from 8% the previous year.

Interestingly, while the effect of the Great War is easily detected in the median series, World War II is primarily disinct as the end point to a significant decline in financial development which started in the aftermath of the Great Depression. Although the figures do not match up perfectly, one could suggest that in terms of financial development the 1950s were roughly on par with the 1860s: a 'Stunde Null' moment of sort.

The evolution of median credit/GDP since 1944 then represents an unprecedented rise, briefly interrupted in the early 1990s, which has recently come to a halt in the aftermath of the Global Financial Crisis in 2007/8. Interestingly, during this rise the *trend* in the standard deviation was relatively flat to begin with, temporarily bumped up by a credit boom between 1985 and 1990, driven by the dramatic rise in four economies: SWE (10% pa; crisis in 1991), JPN (10%; 1992), GBR (8%; 1991, 1995), and CHE (8%; no crisis). By the early 2000s the standard deviation of credit/GDP had returned to the pre-1985 trend, only to be dramatically inflated again from 36% in 2006 to 43% in 2008. Again a relatively small number of countries has seen the bulk of the increase driving this development: IRL (26% pa, crisis in 2007), SWE (12%, no crisis), ESP (10%, 2008), DNK (9%, 2008), NOR (9%, no crisis), BEL (9%, 2008).

As we demonstrated with the case of Sweden, individual countries can quite significantly influence these summary statistics, so that we chart the range for the 10th to the 90th percentile alongside the median in panel (b) – the seemingly unstoppable convergence in financial development is somewhat accentuated by the log scale, which exaggerates the slow development in the early years and dampens the unprecedented growth in the post-WWII period. This representation however does indicate the above narrative for the middle-period, where global financial development ended up in 1944 where it began in the late 1800s.

The above discussion of individual countries and their crisis experience highlights the major difficulty in using the credit/GDP variable for empirical analysis: its dual characteristic as a measure for financial deepening – which has quite robustly been linked to economic growth, at least for low and intermediate levels of development – and a statistic from which information about credit booms (essentially 'excessive' growth) can be gleaned.

Real credit growth Figure 3 charts the standard deviation and median for real credit growth (panels a and b) and inflation (panel c). In all cases the series are smoothed using an MA(10) – which actually is not really be necessary for the standard deviations.

Event analysis As an initial descriptive tool we follow the practice in *inter alia* Gourincheas and Obstfeld (2012) and Anundsen, Gerdrup, Hansen and Kragh-Sorensen (2016) and conduct an event analysis – a univariate test of variable behaviour in the vicinity of the banking crisis event. We estimate the following fixed effects model separately for each variable k

$$y_{it}^{k} = \alpha_{i}^{k} + \beta_{s}^{k}\delta_{is} + \varepsilon_{it}^{k}, \tag{6}$$

where δ_{is} is a dummy variable equal to one when country i is s years away from the crisis, α is the country fixed effect and ε is a white noise error term. We let s vary from -10 to $+10$, such that we evaluate each variable in the lead-up and aftermath of a banking crisis relative to the observations *outside* this 21-year window, with the latter interpreted as 'tranquil' times. We estimate this equation using robust regression to weigh down the impact of influential outliers. A robustness check adopts $-10 \leq s \leq +10$ for robustness (see Appendix): this changes/increases the pool of observations labelled as 'tranquil', but the differences in the outcome are near undetectable.

Figures 4 to 6 presents the results: we begin with the full sample (in terms of the time period) for all 21 countries in Figure 4, panel a, and then crudely split the sample by high and low credit/GDP ratio, ignoring the secular rise in this measure over time. Figure 5 instead splits the data into three sub-periods – note that the scales in this figure are the same for all event plots. The relatively strong full sample evolution in the lead-up and aftermath of crises is somewhat attenuated here. The final Figure 6 adopts real credit growth series which have been adjusted by the recursive mean and standard deviation computed country-by-country as suggested in Baron and Wei (2017).

5.2 Regression Results – Full Sample

Tables 1 and 2 for real credit and credit/GDP. These are the results excluding ongoing crisis years and war years (1914-9, 1939-47 are the suggested years to omit – bit longer than the actual conflict periods – which has strong implications for the analysis given all the lags in the model(s)). It is perhaps not really appropriate to compare these full sample results to those in Schularick and Taylor (2012) since the early 19th century included in our sample (but not in theirs) represents a period where financial development was still in its infancy.

Figure 7 presentes the associated ROC curves of the real credit growth and credit/GDP growth models.

5.3 Regression Results – Rolling Windows

The specification with lags created a lot of problems for the rolling window analysis: many countries were dropped from each window because the collinearity in the lagged variables predicted the outcomes perfectly. This is an artefact of our empirical setup: we have a minimal empirical model (just one variable, albeit with five lags) and relatively short time series – here: 60 year-windows. Instead of dropping some of the lags for each country, I found that using the variables transformed into moving averages works much better on that front. This is standard practice in the literature, e.g. Jorda, Schularick and Taylor (2011, 2016) and Reinhart and Rogoff (2011). In each case the variable for time t is now an average of the values for $t-1$ to $t-k$, where I investigated $k = \{3, 4, 5, 6\}$. The figures present results for $k = \{5, 6\}$ which show very similar patters.

5.4 Modelling Spillovers of Vulnerability

6 Concluding Remarks

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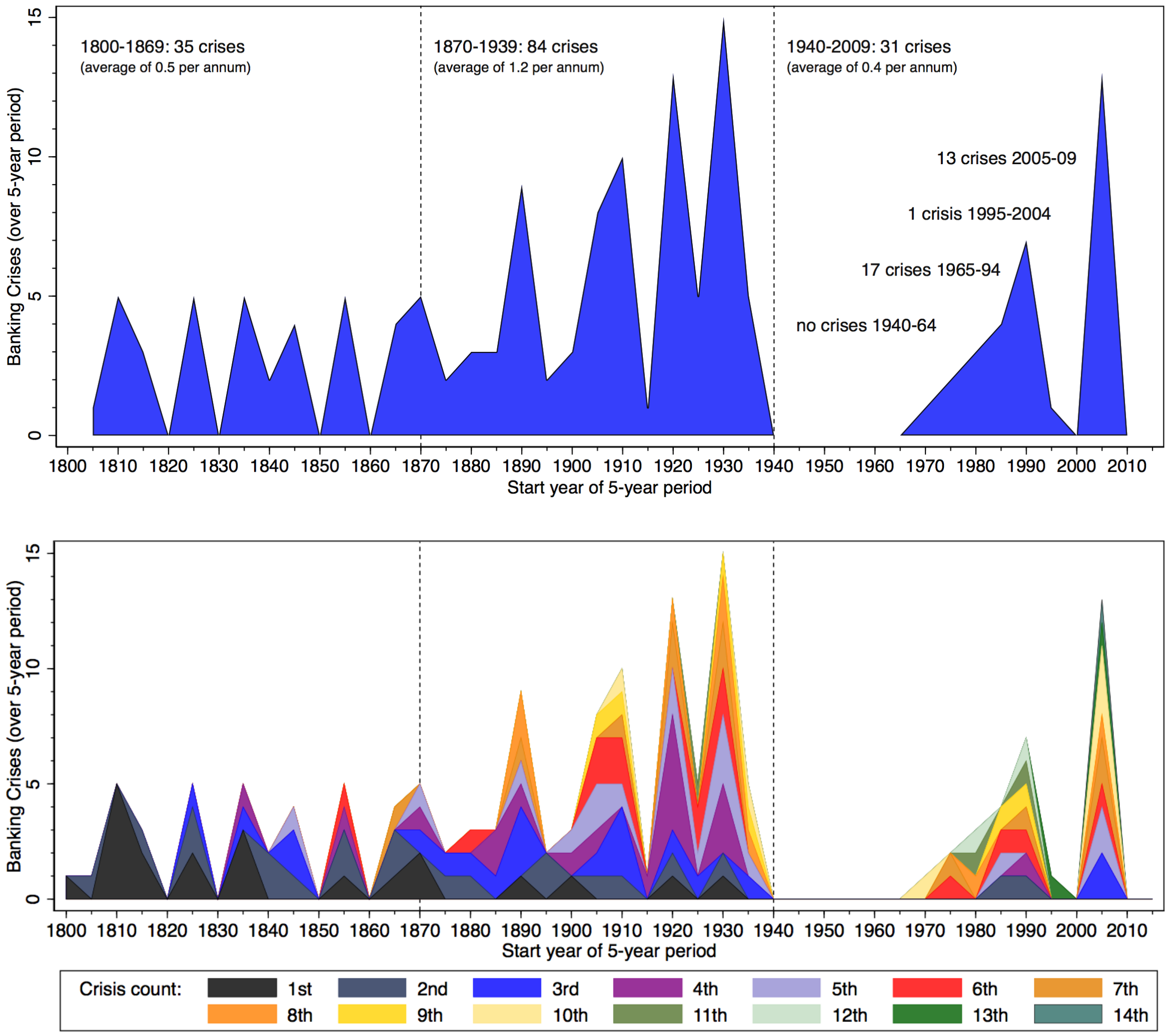
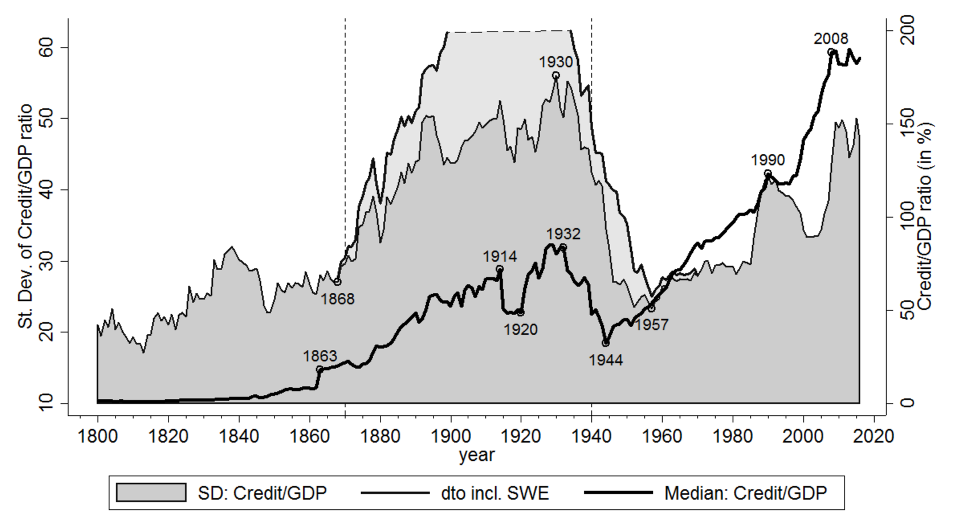
Figures and Tables

Figure 1: Frequency Counts of Banking Crises (summed over 5-year periods)

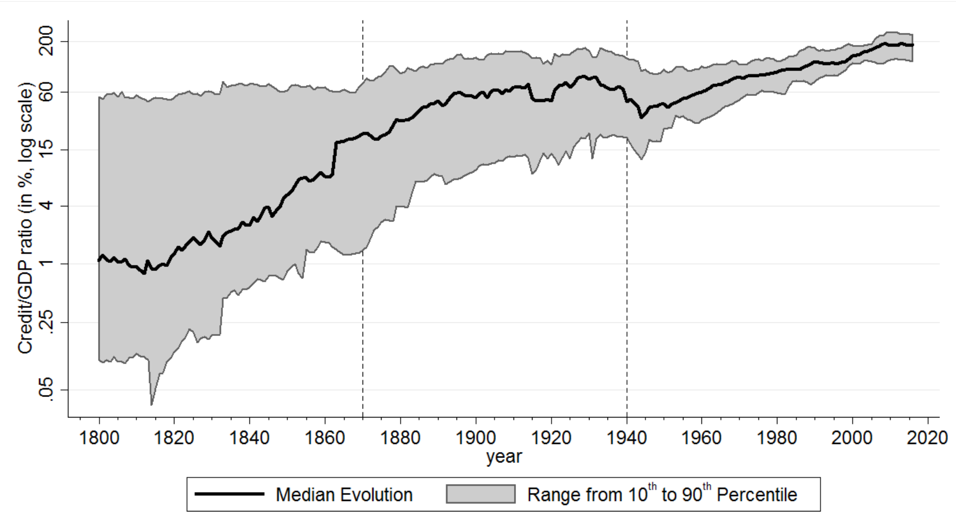


Notes: We chart the evolution of banking crises in our sample of 21 economies for the 1800-2015 time horizon. The frequency count is summed over 5-year periods, with the x-axis indicating the start year of this 5-year period. In the lower panel we distinguish crises by repetition (for sake of simplicity we pretend in this representation that there were no crises before 1800).

Figure 2: Financial Deepening in a Global Perspective

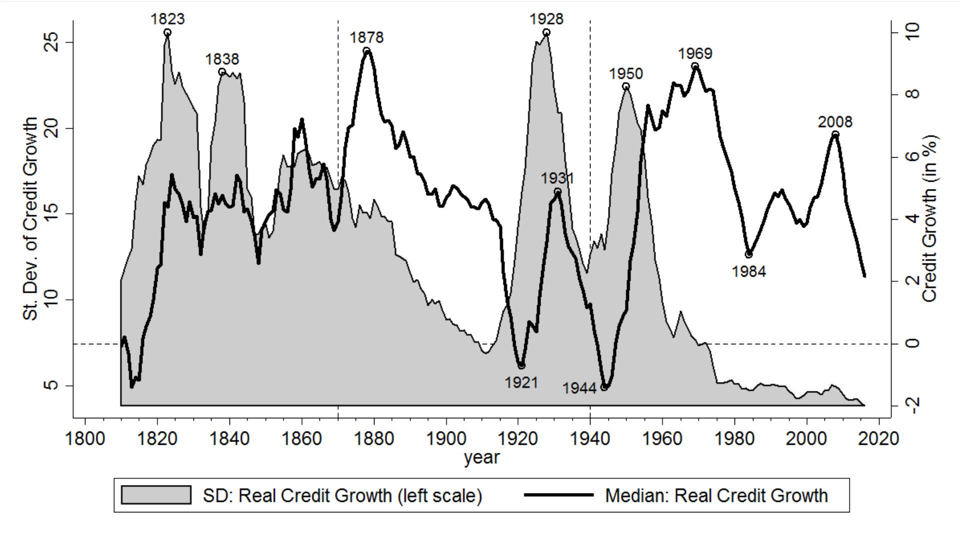


(a) Financial Deepening: Average Evolution and Spread of Credit/GDP

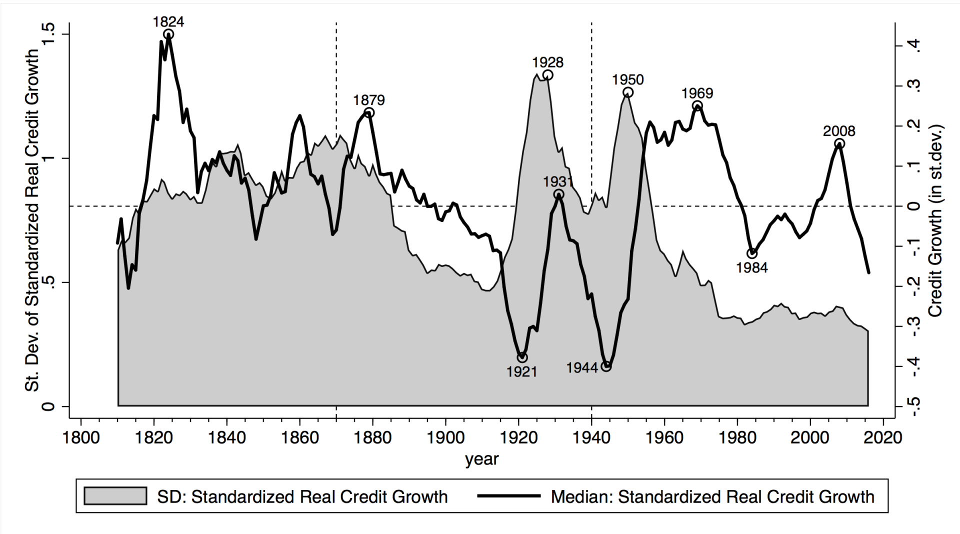


(b) Convergence in Financial Deepending: Median and 10-90th %ile Range (log scale)

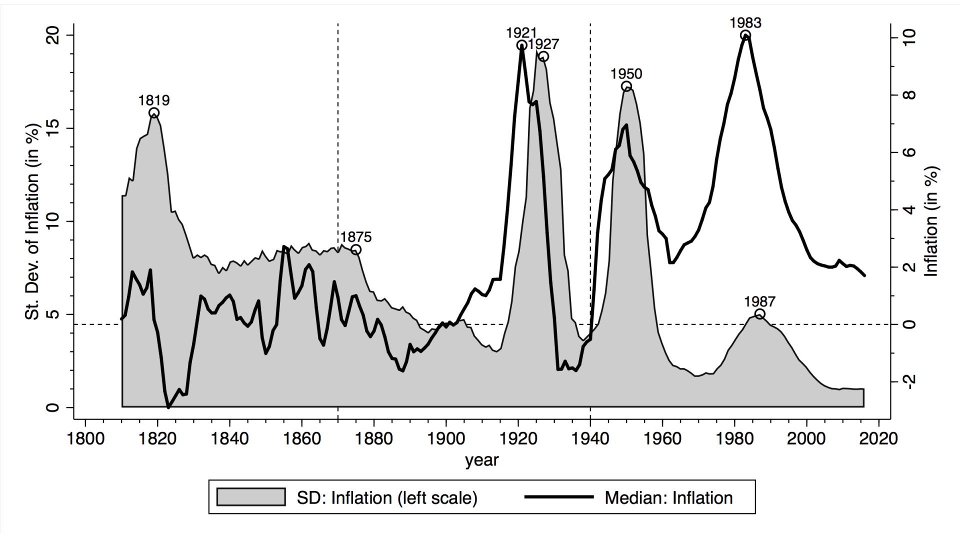
Figure 3: Real Credit Growth and Inflation in a Global Perspective



(a) Real Credit Growth: Average Evolution and Spread (MA(10)-transformed)

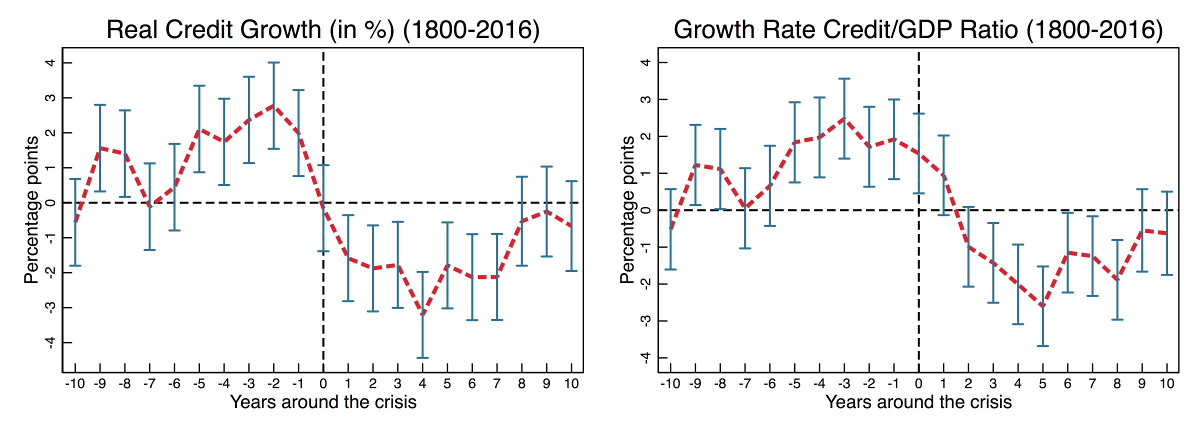


(b) Standardized Real Credit Growth: Average Evolution and Spread (MA(10)-transformed)

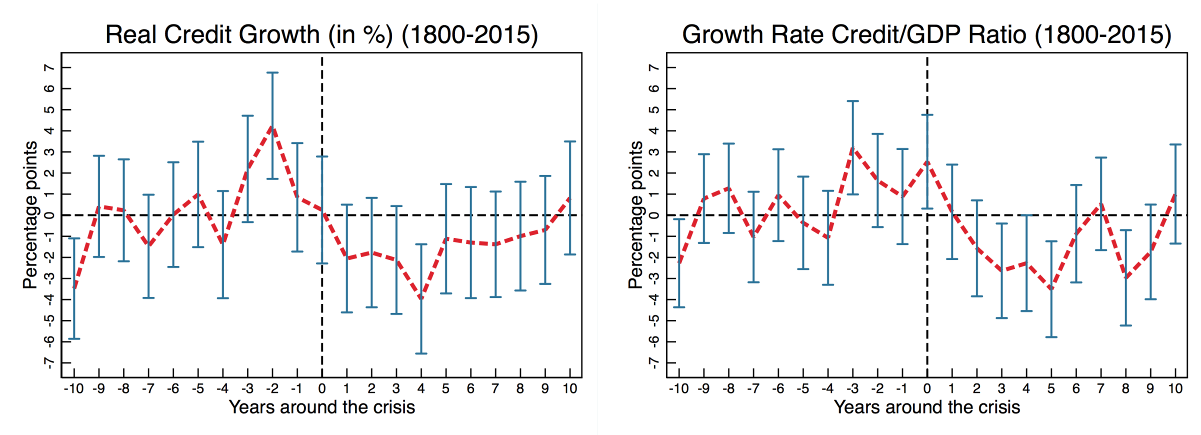


(c) Inflation: Average Evolution and Spread (MA(10)-transformed)

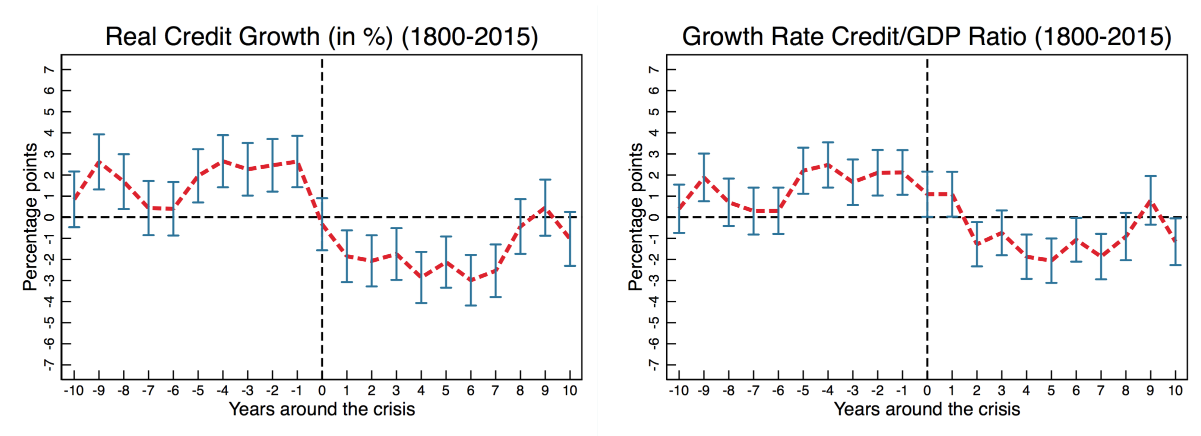
Figure 4: Event Analysis



(a) Full Sample



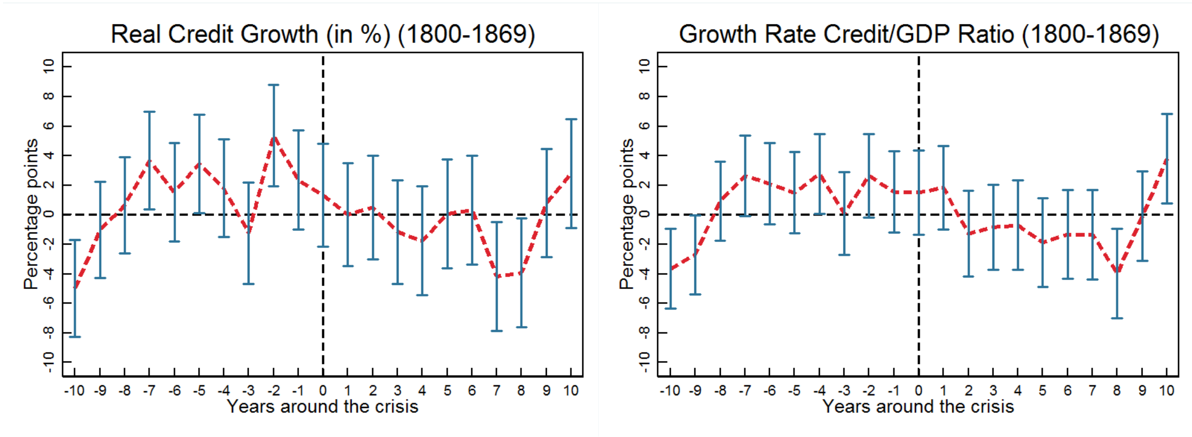
(b) Sample with Credit/GDP < median: 66 banking crises



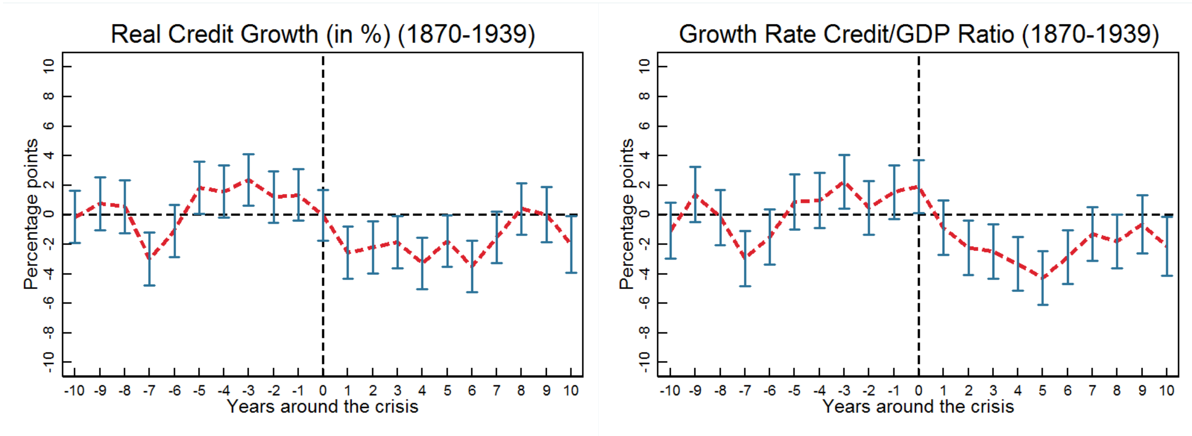
(c) Sample with Credit/GDP > median: 84 banking crises

Notes: We present selected event analysis plots for the period 1800-2015 in all 21 economies. We do not exclude ongoing crisis years or the period of the two world wars in this descriptive analysis. The estimates are derived from coefficients on crisis dummy leads and lags in a pooled regression with country fixed effects ('within-country' interpretation of plots). The implementation adopts a robust regression method, so as to weigh down the impact of outliers. The bars indicate the 90% confidence bands. In panels (b) and (c) we investigate observations with low and high credit/GDP ratio (below and above the 55% median), respectively – this ultimately amounts to a split in terms of early vs late period (see next Figure). In this sub-sample analysis we use the same scale on the y-axis for ease of comparability.

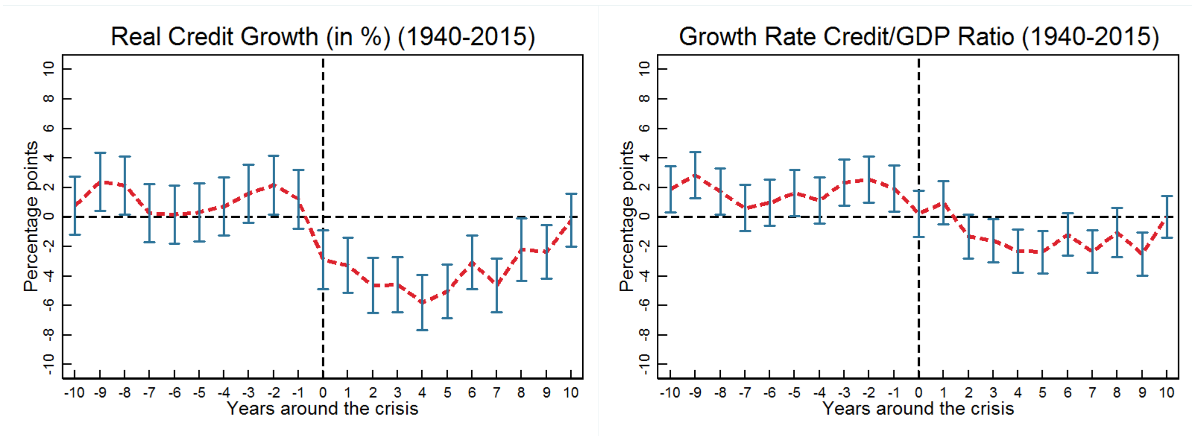
Figure 5: Event Analysis (Sub-periods)



(a) Sample years 1800-69: 35 banking crises



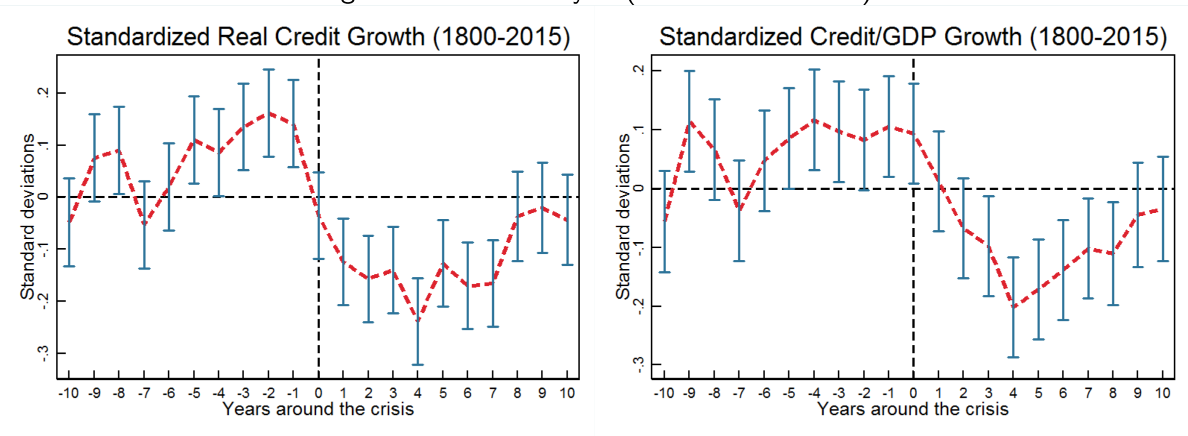
(b) Sample years 1870-1939: 84 banking crises



(c) Sample years 1940-2015: 31 banking crises

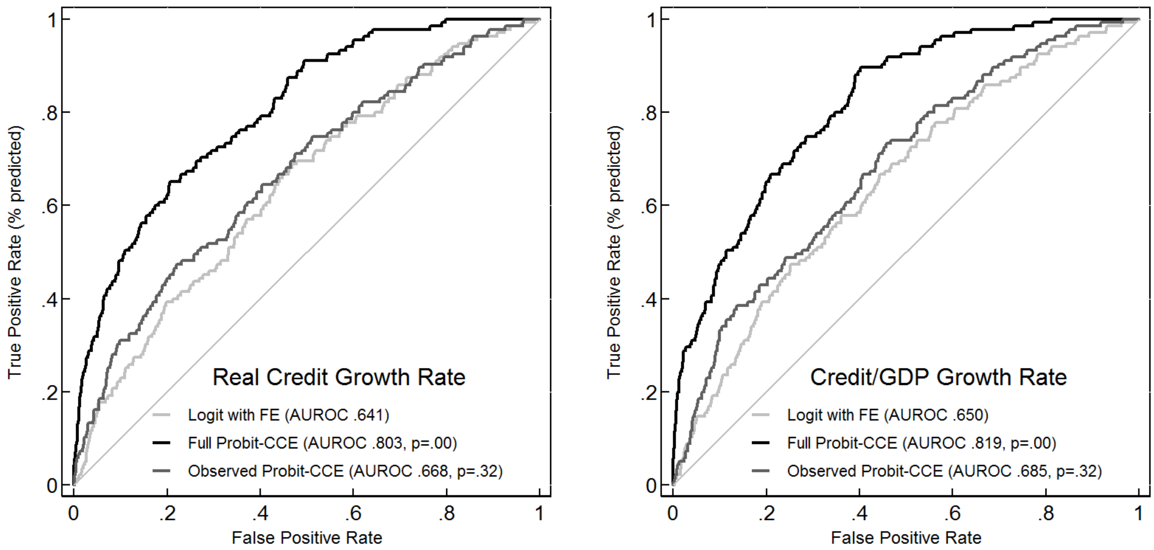
Notes: We present event analysis plots for all 21 economies. In panel (a) we present the results for the early 1800-70 period, panel (b) covers 1870-1940, and panel (c) the 1940-2015 period. The number of crisis events differs across these subsamples as indicated in the captions. The implementation adopts a robust regression method, so as to weigh down the impact of outliers. The bars indicate the 90% confidence bands. We do *not* exclude ongoing crisis years or the period of the two world wars in this descriptive analysis – the average patterns are virtually unchanged if we exclude the war years, years of ongoing crises or both – at times confidence bands are somewhat wider, especially for post-crisis years during 1940-2015. The estimates are derived from coefficients on crisis dummy leads and lags in a pooled regression with country fixed effects ('within-country' interpretation of plots).

Figure 6: Event Analysis (Standardized Data)



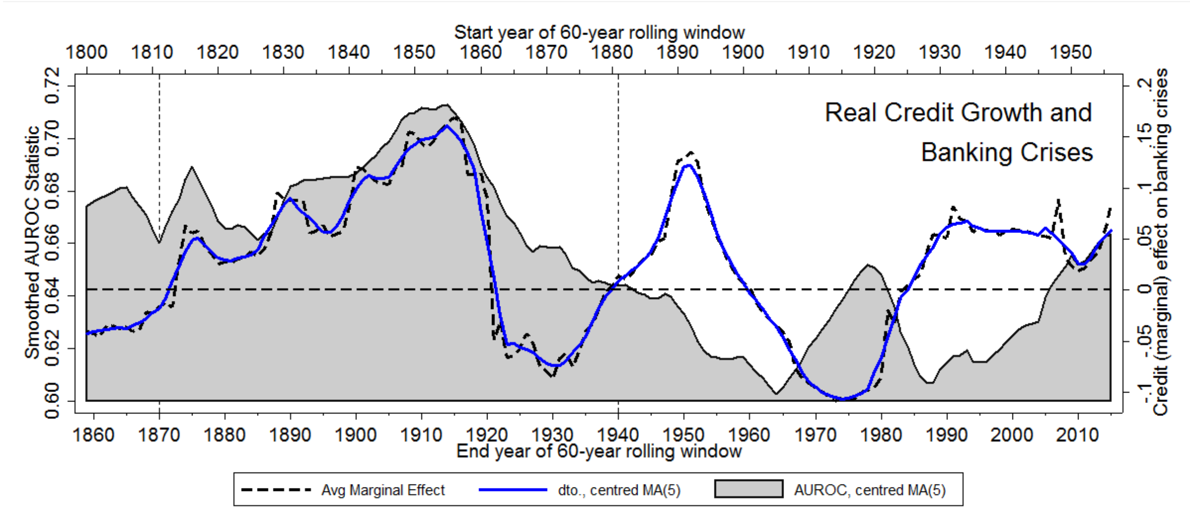
Notes: We present event analysis plots for all 21 economies using standardized real credit growth and credit/GDP growth. Growth rates here are adjusted by the recursive mean and standard deviation computed separately country-by-country. The bars indicate the 90% confidence bands. It is notable that in this event analysis the difference between results for this and the untransformed data (other than in scale, which here is in terms of standard deviations rather than in percent) is marginal.

Figure 7: ROC Curves for Full Sample Models

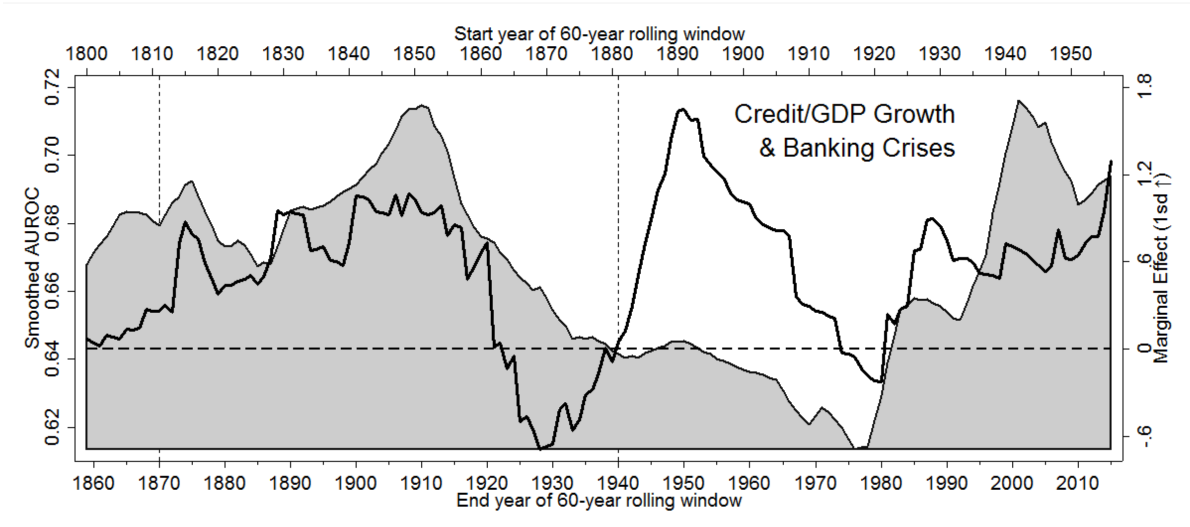


Notes: The p-values refer to ROC comparison tests relative to the logit with FE benchmark (model (2) in the results Tables 1 and 2), the null here is that there is no statistically significant difference in the area of the two model ROC curves. We can offer two ROC curves for the Probit-CCE models since we limit predictions in one case to those based on the observed variables only, whereas the other is based on the full model (including cross-section averages to capture unobserved heterogeneity/contagion).

Figure 8: Logit FE models – Rolling Window Analysis



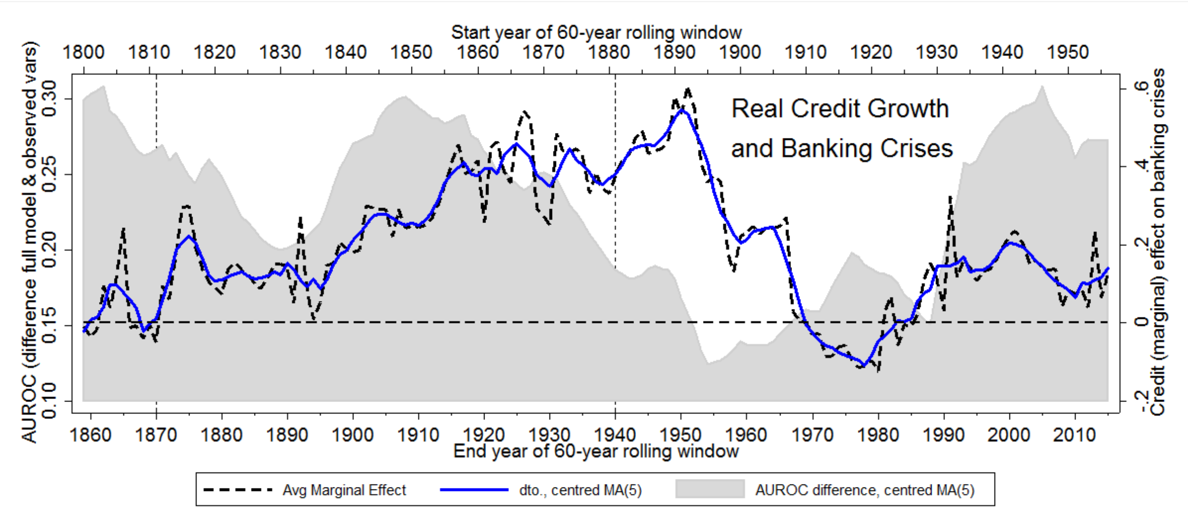
(a) Real Credit Growth, MA(6) transformed



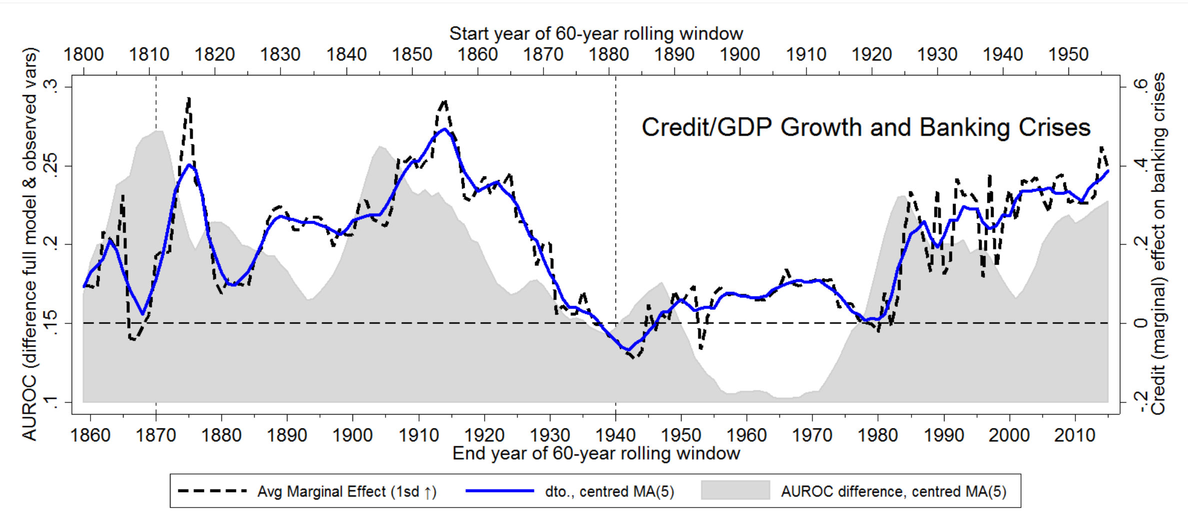
(b) Credit/GDP Growth, MA(6) transformed

Notes: The solid black line in each plot represents the marginal effects of a one standard deviation increase in (a) real credit growth and (b) credit/GDP growth (both variables transformed into MA(6) processes) on the propensity of a banking crisis. These results are derived from a simple logit regression with country fixed effects over a 60-year rolling window (start and end years as indicated on the x-axes). The gray area represents a smoothed evolution of the AUROC test statistic for each regression. The sample covers 1800-2015 and excludes ongoing crisis years but not the two world wars.

Figure 9: Probit CCE models – Rolling Window Analysis



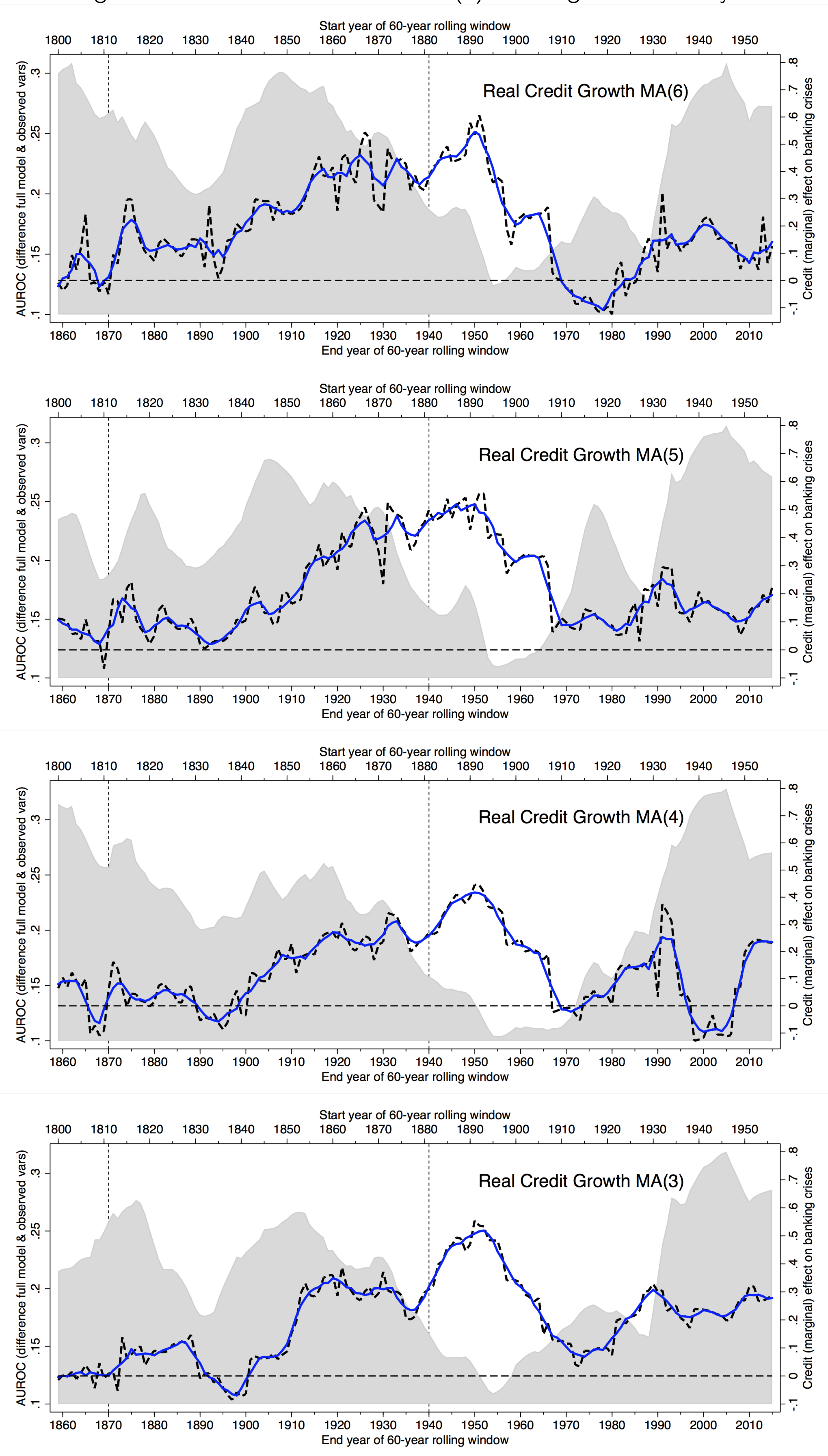
(a) Real Credit Growth, MA(6) transformed



(b) Credit/GDP Growth, MA(6) transformed

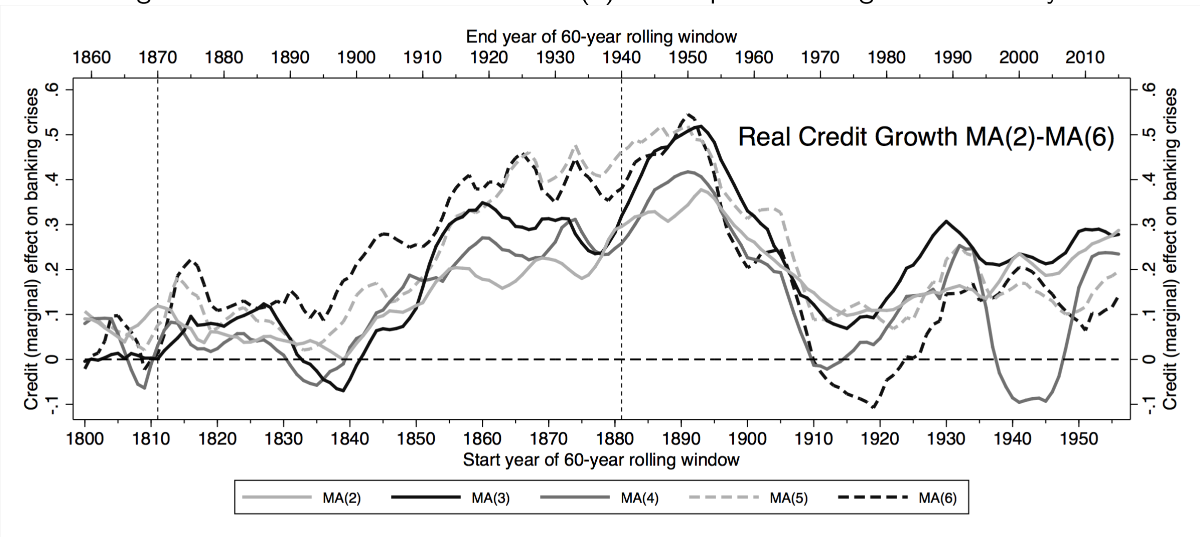
Notes: Panel (a) presents the average marginal effect of a 1sd increase in real credit growth (transformed into an MA(6) process) on the propensity of a banking crisis – the dashed black line are the robust means across sample countries (60-year rolling window), the solid blue line is smoothened version of this average estimate. The grey shading indicates the magnitude of the gap between the overall AUROC for the Probit CCE model (including unobservables) and the AUROC for the model just including the observable credit growth variable (but not the country fixed effects). In Panel (b) we present the same analysis but for credit/GDP growth. The sample covers 1800-2015 and excludes ongoing crisis years but not the two world wars.

Figure 10: Probit CCE models for MA(k) – Rolling Window Analysis



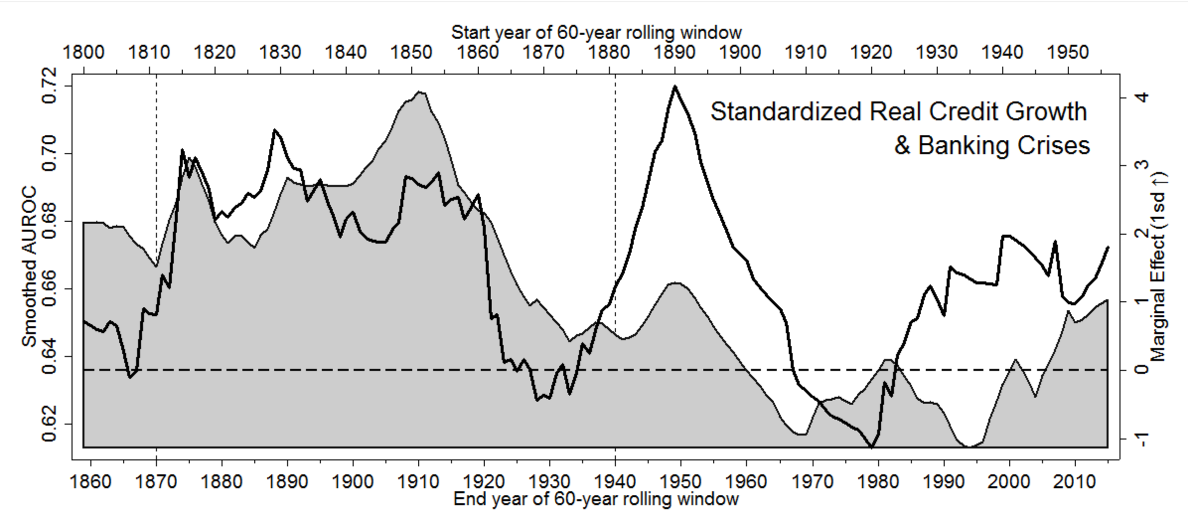
Notes: These plots follow the identical empirical strategy to the results presented in Figure 9, Panel (a), but vary the dynamic setup – MA-transformation – of the real credit growth variable as indicated.

Figure 11: Probit CCE models for MA(k) in one plot – Rolling Window Analysis

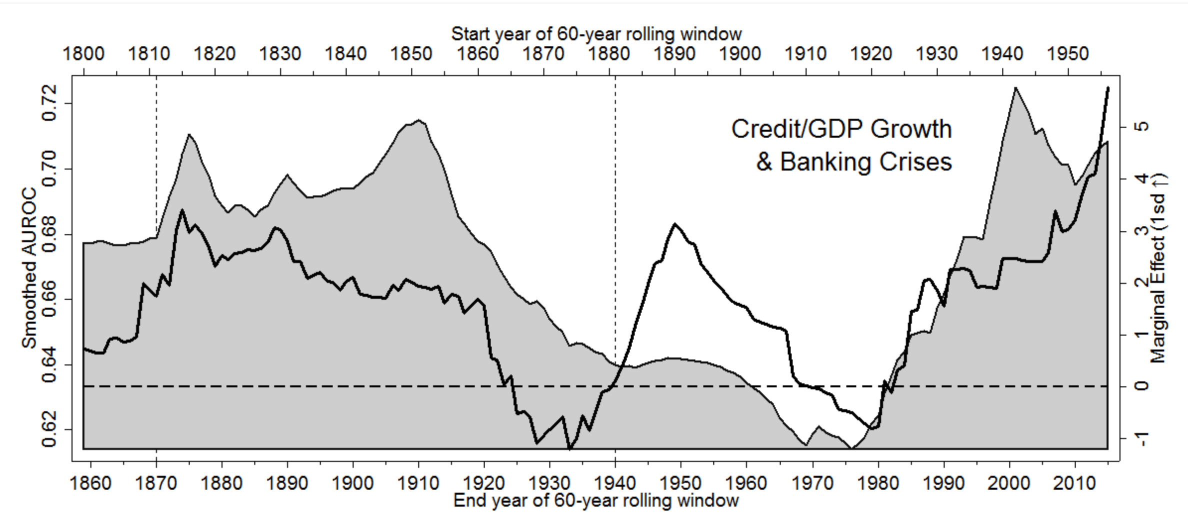


Notes: This plot follows the identical empirical strategy to the results presented in Figure 9, Panel (a), but varies the dynamic setup – MA-transformation – of the real credit growth variable as indicated. For ease of visual presentation only the smoothed average marginal effects are plotted here.

Figure 12: Logit FE models with Standardized Data – Rolling Window Analysis



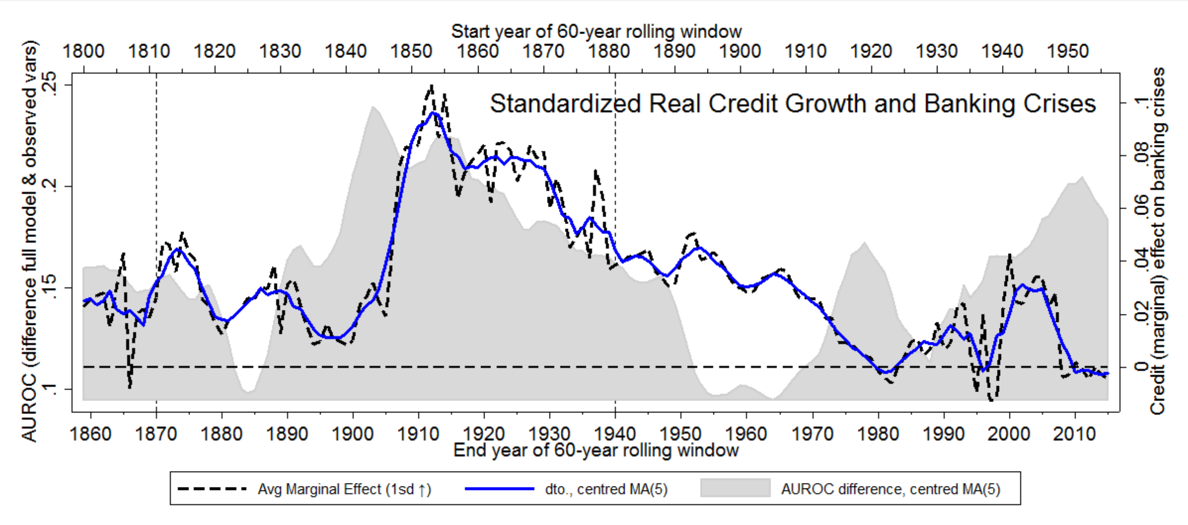
(a) Real Credit Growth, Standardized, MA(6) transformed



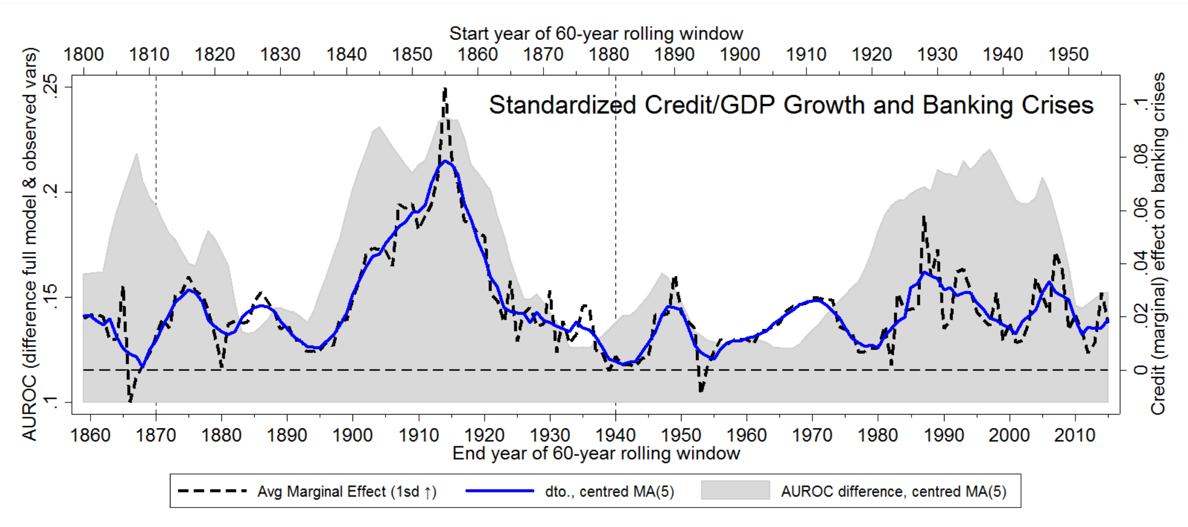
(b) Credit/GDP Growth, Standardized, MA(6) transformed

Notes: The solid black line in each plot represents the marginal effects of a one standard deviation increase in (a) real credit growth and (b) credit/GDP growth (both variables transformed into MA(6) processes) on the propensity of a banking crisis. These results are derived from a simple logit regression with country fixed effects over a 60-year rolling window (start and end years as indicated on the x-axes). The gray area represents a smoothed evolution of the AUROC test statistic for each regression. The sample covers 1800-2015 and excludes ongoing crisis years but not the two world wars. The credit data used in this analysis has been standardized by subtracting the recursive mean and dividing by the recursive standard deviation separately for each country.

Figure 13: Probit CCE models with Standardized Data – Rolling Window Analysis



(a) Real Credit Growth, MA(6) transformed



(b) Credit/GDP Growth, MA(6) transformed

Notes: Panel (a) presents the average marginal effect of a 1sd increase in real credit growth (transformed into an MA(6) process) on the propensity of a banking crisis – the dashed black line are the robust means across sample countries (60-year rolling window), the solid blue line is smoothened version of this average estimate. The grey shading indicates the magnitude of the gap between the overall AUROC for the Probit CCE model (including unobservables) and the AUROC for the model just including the observable credit growth variable (but not the country fixed effects). In Panel (b) we present the same analysis but for credit/GDP growth. The sample covers 1800-2015 and excludes ongoing crisis years but not the two world wars. The credit data used in this analysis has been standardized by subtracting the recursive mean and dividing by the recursive standard deviation separately for each country.

Table 1: Regression Results – Real Credit Growth Models

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Logit	Logit FE	Logit FE	Logit FE	Probit-MG	Probit-CCE
FE Bias Correction		none	jacknife	analytical		
Δln(Real Credit) 1st Lag	0.290	0.327	-0.012	0.611	0.124	0.351
	(0.408)	(0.396)	(0.611)	(0.611)	(0.391)	(0.436)
Δln(Real Credit) 2nd Lag	0.640	0.672	-0.712	0.897	1.038	1.198
	(0.300)**	(0.326)**	(0.497)	(0.497)*	(0.432)**	(0.531)**
Δln(Real Credit) 3rd Lag	0.154	0.183	-0.464	0.285	-0.053	0.401
	(0.493)	(0.486)	(0.550)	(0.550)	(0.424)	(0.491)
Δln(Real Credit) 4th Lag	-0.513	-0.502	-1.283	-0.450	0.151	0.147
	(0.309)*	(0.339)	(0.580)**	(0.580)	(0.247)	(0.288)
Δln(Real Credit) 5th Lag	-0.554	-0.536	0.521	-0.092	0.059	0.295
	(0.535)	(0.557)	(0.668)	(0.668)	(0.260)	(0.304)
$\sum \Delta ln$(Real Credit) Lags	0.016	0.145	-1.949	1.251	1.319	2.393
	(0.724)	(0.803)	(1.102)	(1.102)	(0.805)*	(0.943)***
$\sum$ Margins (1sd $\uparrow$) in %	0.009	0.083	-0.756	0.692	1.421	2.452
	(0.419)	(0.461)			(0.927)	(1.057)**
Observations	3,822	3,822	1,360	1,360	3,822	3,822
Crises	135	135	135	135	135	135
Country Fixed Effects		×	×	×	×	×
Time Fixed Effects			×	×		(×)
Wald test joint sign. (p)	0.00	0.00	0.07	0.29	0.28	0.18
AUROC	0.562	0.641	0.456	0.596	0.747	0.803
se(AUROC)	0.025	0.024	0.025	0.025	0.021	0.018
Observed Vars AUROC						0.668
se(AUROC)						0.024

Notes: We compare results for the empirical models laid out in equation (5), with the growth rate of real private credit as the variable of interest. We exclude ongoing crisis years as well as wars – see maintext for details. All models contain 21 countries, capturing 135 banking crises over the 1806-2016 time horizon. The unconditional probability of a banking crisis in this sample is 3.5%.

Models (1) and (2) are standard logit estimators, with (2) including fixed effects. Models (3) and (4) recognise the incidental parameter bias induced by including country fixed effects and provide results for the Fernández-Val and Weidner (2016) estimators with additional time fixed effects, adopting jacknife and analytical bias correction, respectively. Model (5) estimates probit regressions in each country and averages the results, Model (6) uses the Boneva and Linton (2017) Probit-CCE estimator – the latter two models present robust means to weigh down the impact of outliers.

Standard errors for the models in (1) and (2) are clustered at the country-level; the expressions for the variance-covariance matrix applied in models (3) and (4) can be found in the Fernández-Val and Weidner (2016) article; the standard errors in models (5) and (6) are constructed nonparametrically in the same manner as for linear models (Pesaran and Smith, 1995).

'$\sum$ Margins' reports the average marginal effect in percent of a one standard deviation increase in the credit growth variable as implied by the respective logit/probit model results.

AUROC is the area under the Receiver Operating Characteristic (ROC) curve, for which we also report the associated standard error. Note that for the Probit-CCE model we report two AUROC statistics: apart from the standard one for the full model we are able to isolate the predictions based on the observable variables only ('Observed Vars'), which is reported along with its standard error. A test for AUROC equality between models (2) and (5) is rejected for the full predictions from the Probit-CCE model (p=.000) but not for the predictions based on observed variables only (p=.436).

Table 2: Regression Results – Credit/GDP Growth Models

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Logit	Logit FE	Logit FE	Logit FE	Probit-MG	Probit-CCE
FE Bias Correction		none	jacknife	analytical		
Δln(Credit/GDP) 1st Lag	0.567	0.597	0.129	1.348	0.340	0.444
	(0.417)	(0.428)	(0.680)	(0.680)**	(0.376)	(0.492)
Δln(Credit/GDP) 2nd Lag	0.650	0.677	-0.466	0.812	0.563	1.117
	(0.316)**	(0.373)*	(0.526)	(0.526)	(0.520)	(0.420)***
Δln(Credit/GDP) 3rd Lag	-0.154	-0.141	-1.708	-0.368	0.535	0.859
	(0.519)	(0.544)	(0.677)**	(0.677)	(0.512)	(0.660)
Δln(Credit/GDP) 4th Lag	-0.306	-0.328	-1.019	-0.344	-0.068	0.074
	(0.326)	(0.358)	(0.617)*	(0.617)	(0.293)	(0.380)
Δln(Credit/GDP) 5th Lag	-0.273	-0.263	0.485	0.278	0.037	0.130
	(0.563)	(0.594)	(0.795)	(0.795)	(0.450)	(0.460)
$\sum \Delta ln$(Real Credit) Lags	0.485	0.542	-2.580	1.727	1.406	2.624
	(0.833)	(0.991)	(1.253)**	(1.253)	(0.981)	(1.100)**
$\sum$ Margins (1sd $\uparrow$) in %	0.254	0.281	-0.813	0.842	1.070	2.208
	(0.431)	(0.513)			(0.899)	(1.121)**
Observations	3,875	3,875	1,381	1,381	3,875	3,875
Crises	135	135	135	135	135	135
Country Fixed Effects		×	×	×	×	×
Time Fixed Effects			×	×		(×)
Wald test joint sign. (p)	0.00	0.01	0.01	0.29	0.68	0.08
AUROC	0.584	0.650	0.450	0.591	0.776	0.819
se(AUROC)	0.025	0.024	0.026	0.025	0.019	0.017
Observed Vars AUROC						0.685
se(AUROC)						0.023

Notes: We compare results for the empirical models laid out in equation (5), with the growth rate of private credit-to-GDP as the variable of interest. We exclude ongoing crisis years as well as wars – see maintext for details. All models contain 21 countries, capturing 135 banking crises over the 1806-2016 time horizon. The unconditional probability of a banking crisis in this sample is 3.5%.

Models (1) and (2) are standard logit estimators, with (2) including fixed effects. Models (3) and (4) recognise the incidental parameter bias induced by including country fixed effects and provide results for the Fernández-Val and Weidner (2016) estimators with additional time fixed effects, adopting jacknife and analytical bias correction, respectively. Model (5) estimates probit regressions in each country and averages the results, Model (6) uses the Boneva and Linton (2017) Probit-CCE estimator – the latter two models present robust means to weigh down the impact of outliers.

Standard errors for the models in (1) and (2) are clustered at the country-level; the expressions for the variance-covariance matrix applied in models (3) and (4) can be found in the Fernández-Val and Weidner (2016) article; the standard errors in models (5) and (6) are constructed nonparametrically in the same manner as for linear models (Pesaran and Smith, 1995).

'$\sum$ Margins' reports the average marginal effect in percent of a one standard deviation increase in the credit growth variable as implied by the respective logit/probit model results.

AUROC is the area under the Receiver Operating Characteristic (ROC) curve, for which we also report the associated standard error. Note that for the Probit-CCE model we report two AUROC statistics: apart from the standard one for the full model we are able to isolate the predictions based on the observable variables only ('Observed Vars'), which is reported along with its standard error. A test for AUROC equality between models (2) and (5) is rejected for the full predictions from the Probit-CCE model (p=.000) but not for the predictions based on observed variables only (p=.315).

Appendix

A Sample Makeup and Descriptives

Table A-1: Sample Makeup – Banking Crisis Events

Country	Obs	Crisis Years														Total
AUS♯	215	1828	1843	1893	1931	**1989**										5
AUT	215	1873	1924	1929	1931	**2008**										5
BEL	215	1838	1842	1848	1870	1914	1925	1931	1934	1939	**2008**					10
CAN♯	215	1837	1866	1873	1906	1908	1912	1923	**1983**							8
CHE♯	215	1870	1910	1921	1931	**2008**										5
DNK♯	215	1813	1857	1877	1885	1902	1907	1921	1931	**1987**	**2008**					10
ESP♯	215	1814	1829	1846	1920	1931	1977	**2008**								7
FIN	215	1921	1931	1939	**1991**											4
FRA♯	215	(1802)	(1805)	1881	1889	1907	1914	1930	1939	**1994**	**2008**					10
GBR♯	215	1810	1815	1825	1837	1847	1857	1866	1890	1914	**1974**	**1984**	**1991**	**1995**	**2007**	14
GER♯	215	1857	1880	1891	1901	1925	1931	**1977**	**2008**							8
GRC	182	1931	**1991**	**2008**												3
IRL	215	1836	1856	**2007**												3
ITA♯	215	1866	1868	1887	1891	1893	1907	1914	1921	1930	1935	**1990**				11
JPN♯	215	1901	1907	1914	1917	1923	1927	**1992**								7
NLD♯	215	1819	1897	1914	1921	1939	**2008**									6
NOR♯	215	1814	1898	1914	1921	1931	**1987**									6
NZL	195	1890	**1987**													2
PRT	215	1828	1846	1890	1920	1923	1931	**2008**								7
SWE♯	215	1811	1876	1907	1922	1931	**1991**									6
USA♯	215	1818	1825	1836	1857	1873	1884	1890	1893	1907	1914	1929	**1984**	**2007**		13
Total	4,462															150

Notes: We report the start years of banking crisis by sample country from the Reinhart and Rogoff (2009) database augmented with the updated Laeven and Valencia (2013) data – N=21 countries over the 1800-2015 period. In the second column we report the number of observations for which we have credit data. This is prior to any omissions for 'ongoing' crisis years and/or international conflict. We highlight the post-WWII events in bold – these make up exactly one fifth of all 150 crises in our regression sample. We further highlight crisis years during the 1914-9 and 1939-47 (immediate post-)conflict periods, which are excluded in the Schularick and Taylor (2012) analysis as well as in our regressions; these amount to 13 crises. Since the MA-transformation pursued in the rolling window analysis further excludes the 1802 and 1805 French crises the main results in our paper are based on 135 crisis events. We mark those countries appearing in the Schularick and Taylor (2012) sample with ♯ – the same sample restrictions apply.