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Dealing with bank distress

Insights from a comprehensive database

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Abstract

We study the effectiveness of policy tools that deal with bank distress (i.e. central bank lending, asset purchases, bank liability guarantees, impaired asset segregation schemes). We present and draw on a novel database that tracks the use of such tools in 29 countries between 1980 and 2016. To keep "all else" equal, we test whether different policies explain differences in how countries fared through bank distress episodes that feature observationally similar initial macro–financial vulnerabilities. We find that, altogether, policy interventions help restore GDP growth and normalize the economy when bank distress follows a period of high cross–border exposures. Central bank lending and asset purchase schemes are especially effective in the first and second years of distress, respectively, and when bank distress follows low asset valuations, high bank leverage and weak bank performance. Overall, our results suggest that swift and broad–ranging policies can mitigate the adverse economic effects of bank distress.

Keywords: Bank distress, distress mitigation policy **JEL Class.:** G01 – G38 – E60.

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"At the start of any crisis, there's an inevitable fog of diagnosis. You can recognize the kind of vulnerabilities that tend to precede severe bank distress episodes, (...) but you can't be sure whether the initial market turmoil is a healthy adjustment or the start of a systemic meltdown.", T. Geithner, *Stress test: reflections on financial crises*, p. 119.

1 Introduction

Bank distress episodes and banking crises have negative and persistent effects on output. The cumulative GDP loss (relative to pre–crisis peak) is above 9% in half of crises (Cecchetti, Kohler, and Upper (2009)) and still exceeds 6% ten years after the beginning of the crisis in most countries (Cerra and Saxena (2008)).[1] Losses vary widely across episodes and countries, though. For example, during the Great Financial Crisis (GFC), output fell from peak to trough by 0.16% in Switzerland and by almost 30% in Greece.

There are two interrelated sets of explanations for such variations. One relates to the initial economic conditions, notably the macro–financial imbalances with which countries enter a period of bank distress. For instance, bank distress associated with the unravelling of a domestic financial imbalance (e.g. a housing bubble) may have a very different impact than that stemming from an external event in the absence of such or similar imbalances (e.g. a crisis imported through cross–border exposures). The other set of explanations relates to the policies employed. The timing and degree of policy activity and the specific tools deployed (e.g. central bank lending, separation of impaired assets) differ considerably across episodes. To illustrate these differences, Figure 1 shows the number of tools deployed at the beginning of banking crises. During the first year, it ranges from zero, in 45% of distress episodes (first grey bar), to above eight in 12% of the crises (last five grey bars), which suggests that not all crises are fought with the same speed and force. These choices will likely influence the severity of the recession, not least if tools differ in terms of their effectiveness.

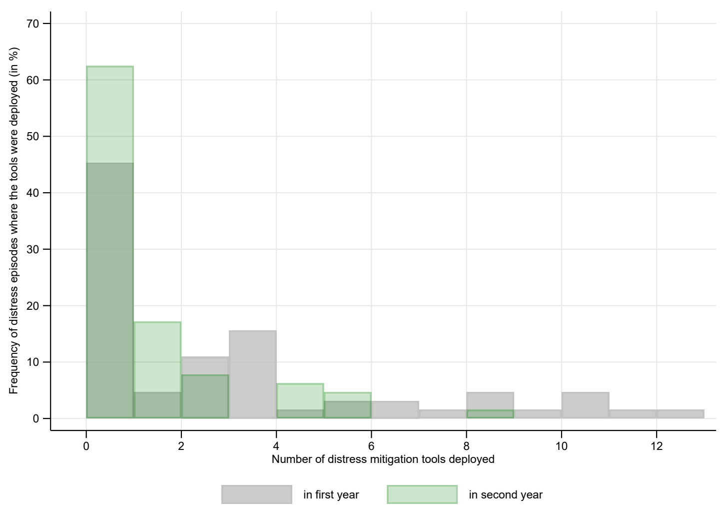
The aim of this paper is to review the use of bank distress mitigation tools and assess their effectiveness. Bank distress has various causes and can start from different initial conditions. Such differences must be accounted for when evaluating effectiveness. Our approach consists of testing whether the tools deployed can explain differences in countries' performance throughout similar bank distress episodes. Our identification strategy rests on the assumption that similar macro–financial anomalies are the symptoms of similar underlying factors, and that the latter should have similar economic consequences unless they are addressed with different policies. We gauge a tool as more effective if the country exhibits a higher cumulative growth rate of real GDP and/or returns faster to normal macro–economic conditions three years after the beginning of the distress episode.

[1] Ollivaud, Guillemette, and Turner (2018) show that the most severe bank distress episodes reduce not only output but also *potential* output.

Our analysis relies on a comprehensive set of quarterly macro–economic data and on a new database that contains information on more than 300 distress mitigation policies for 29 countries since 1980. One advantage of our database is that it records interventions at their precise deployment date (i.e. quarter), which allows us to measure the lag between the beginning of a distress episode (i.e. when banks' stock prices crash) and a specific intervention. This, in turn, allows us to evaluate whether the specific timing of an intervention has different effects.

Formally, we identify pairs of similar bank distress episodes on the basis of their initial macro–financial anomalies. We track the evolution of macro–financial variables in the run–up to bank distress episodes. There is an anomaly whenever a variable take on abnormal values. We construct a synthetic measure of similarity between two bank distress episodes, as the total number of anomalies that they have in common. This measure is akin to the (opposite of the) "Hamming" distance, which is commonly used in information theory to measure the similarity of strings of characters or in biology to compare the DNA of different organisms.[2] We calculate this distance for all possible pairs of distress episodes, as listed in Baron, Verner, and Xiong (2020), and identify a pair of similar episodes when the distance is below a threshold.

Figure 1: Policy activity at the beginning of bank distress episodes



Note: Distribution of the number of new bank distress mitigation tools deployed in the first (grey) and second (green) year of distress episodes, based on a sample of 62 bank episodes for which we have information on policy interventions (see Section 2.3). Tools include central bank lending schemes, bank liability guarantee schemes, impaired asset segregation schemes, and asset purchase schemes.

We find that greater and swifter overall policy activity reduces the adverse impact of bank distress on economic activity regardless of the initial macro–financial anomalies. More particularly,

[2]The Hamming distance between two strings of equal length is the number of positions at which the corresponding symbols are different.

central bank lending and asset purchase schemes are relatively effective when bank distress follows low asset valuations, high bank leverage and weak bank performance. Central bank liquidity is also more effective when provided in the first year of distress episodes.

Related literature. This work belongs to the literature on financial crises. Kaminsky and Reinhart (1999), for example, show that bank distress episodes are often preceded by currency crises, which are themselves often due to financial liberalization. The consequence is a recession with a worsening of the terms of trade and a rising cost of credit. Similarly, Bordo, Eichengreen, Klingebiel, Martinez-Peria, and Rose (2001) find that bank distress episodes are more likely to occur in countries without capital controls and that the output cost of bank distress episodes is higher when an exchange rate peg is in place. Reinhart and Rogoff (2009) highlight that bank distress episodes are often preceded by equity and house prices booms as well as surges in capital inflows (so–called "capital bonanzas"). An important part of the literature focuses on the predictive power of credit booms (Borio and Lowe (2002)). Mendoza and Terrones (2008) propose a methodology for identifying and measuring credit booms. While not all credit booms end in financial crises, they show that most emerging market financial crises are associated with credit booms due to large capital inflows. In the case of advanced economies, Schularick and Taylor (2012) show that credit growth is the best predictor of financial crises. More recently, Mian, Sufi, and Verner (2017) find that past increases in household credit predict low future GDP growth. Claessens, Kose, and Terrones (2009) focus on the interaction between macro–economic and financial variables. They find evidence that recessions associated with credit crunches and house price busts tend to be deeper and longer than other recessions.

In contrast to the above studies, our focus is on the effectiveness of policy interventions that deal with severe bank distress episodes.[3] It is inherently difficult to measure effectiveness. For example, larger–scale distress calls for stronger interventions, but also makes success less certain. This may lead to the spurious conclusion that interventions are less effective.

A distinguishing feature of our analysis relates to the methodology. To address the endogeneity problem, we work on a sample of pairs of similar bank distress episodes, and study how policy interventions affect the relative economic performance within these pairs. To classify bank distress episodes, a popular approach consists in measuring a Euclidean distance, and in minimizing this distance within categories of episodes while maximizing it across categories (e.g. Cecchetti, Kohler, and Upper (2009), Dardac and Giba (2011)). One advantage of such "cluster" analysis is that

[3] Early cross–country analyses include Honohan and Klingebiel (2003), who evaluate the fiscal cost of a crisis and find that accommodative measures, such as blanket guarantees, open–ended liquidity support and repeated recapitalizations increase the fiscal cost. Cecchetti, Kohler, and Upper (2009) study the correlation between the policy response and the length, depth and cumulative loss in GDP after bank distress episodes. They find that the recovery takes longer, where authorities set up an asset management company. A related literature focuses the effectiveness of crisis management policies at the firm–level (e.g. Dell'Ariccia, Detragiache, and Rajan (2008), Laeven and Valencia (2013), Giannetti and Simonov (2013)) and at the bank–level (e.g. Hryckiewicz (2014), Li (2013), Poczter (2016)).

it does not prejudge of the number of categories. One limitation is that the categories are often difficult to interpret economically, and their definition varies over time. Another is that Euclidean –linear– projections are not well suited to deal with the non–linearities that typically surround bank distress episodes. In contrast, we classify bank distress episodes into canonical and time–invariant categories, based on the macro–financial anomalies that precede them. The Hamming distance, which compares binary data strings, also allows us to account for potential non–linearities.

Roadmap. The rest of this paper is structured as follows. In the second section, we present the data used in our analysis, including a new database on bank distress mitigation tools. In the third section, we describe our methodology to measure pre–distress similarities and to derive pairs of similar distress episodes. The fourth section formally tests the effectiveness of various policy interventions depending on their speed and the type of episode. A final section concludes.

2 Data used in the analysis

Our analysis draws on three types of data: (i) macro–financial variables; (ii) an exhaustive list of bank distress episodes; and (iii) comprehensive information on the bank distress mitigation tools deployed during distress episodes. All data are quarterly, and cover the period 1980q1–2020q2. We present them in turn.

2.1 Macro–financial variables

We consider a comprehensive quarterly data set of more than 70 macro–financial variables (in levels, growth rates, ratios to GDP) that the literature has identified as potential early warning indicators of bank distress (Table 1).[4] The whole data set covers 60 countries over the period 1980q1–2020q2. In line with with central banks' financial stability monitoring frameworks, and to fix ideas, we group these variables into five categories relating to different types of macro–financial vulnerabilities:[5] (V1) cross–border exposures; (V2) asset valuations; (V3) bank health; (V4) private non-financial sector (PNFS) leverage; and (V5) real economy performance.

2.2 List and dates of bank distress episodes

One important aspect in managing bank distress episodes is the speed and timing of policies. Assessing the effectiveness of these policies therefore requires identifying and dating the beginning

[4] We collect data from various sources, including the BIS, the IMF, Datastream, Fitch Connect, IHS Markit and national central banks and statistical agencies.

[5] For example, in its Financial Stability Report the US Federal Reserve Board emphasises four broad categories of vulnerability: valuation pressures; borrowing by businesses and households; leverage within the financial sector; and financial institutions' funding risks (FRB (2020)). Similarly, in its Financial Stability Review the ECB focuses on: macro–financial imbalances relating to the real economic outlook; leverage in the household and corporate sectors; financial market liquidity and asset valuations; and banks' financial health (ECB (2020)).

Table 1: List of macro–financial variables used for the pairing distress episodes

Category	**Variables included in the category**
V1: Cross–border exposures	Cross–border loans, bonds, short-term liabilities, foreign currency–denominated liabilities
V2: Asset valuation	House price index, stock price index (overall, banks, financials, consumption sector, industrial sector)
V3: Bank health	Bank assets, RoA, loans/assets, loans/deposits, deposit/assets, price-to-book value, leverage, interest rate margin, NPL/loans
V4: PNFS leverage	Credit to PNFS, HHs, NFCs, credit to HHs/credit to NFCs ratio, debt service ratio
V5: Real economy performance	Real GDP, consumption, investment, unemployment, short-term rate, government bond yield, PMIs (composite, manufacturing), inflation
Other	Total credit, government debt, interest rate slope, interest rate spread, deposit rate, loan rate, current account, exchange rate

Note: HHs = households; NFCs = non-financial corporations; NPL = non-performing loans; PMIs = purchasing managers' indices; PNFS = private non-financial sector; RoA = return on assets. Depending on the variable, we consider up to three transformations: in log or level; in change or growth rate; and ratios to GDP. All variables (and transformations thereof) used in the empirical analysis are de–trended (see Section 3.1).

of bank distress episodes as precisely as possible. There are several approaches to date bank distress episodes. Some rely on narratives (e.g. Reinhart and Rogoff (2009), Romer and Romer (2017), Laeven and Valencia (2012) and Laeven and Valencia (2018), henceforth LV); others on quantitative analysis (Basten, Bengtsson, Detken, Koban, Klaus, Lang, Lo-Duca, and Peltonen (2017), and Baron, Verner, and Xiong (2020), henceforth BVX).

In LV's narrative approach, for example, a bank distress episode is defined as an event where (i) the banking system shows significant signs of financial distress (e.g. losses, bank runs, bank liquidations) and (ii) this distress induces authorities to take significant measures (e.g. bank nationalisations, extensive liquidity support, asset purchases). This definition is similar to that of Bordo, Eichengreen, Klingebiel, Martinez-Peria, and Rose (2001) or Reinhart and Rogoff (2014), who "mark a bank distress episode by two types of events: (i) bank runs that lead to the closure, merging, or takeover by the public sector of one or more financial institutions; and (ii) if there are no runs, the closure, merging, takeover, or large-scale government assistance of an important financial institution (or group of institutions)".

BVX, in contrast, primarily identify and date bank distress episodes based on bank equity returns. They identify a bank distress episode when bank equity falls by more than 30% year–on–year and there are "widespread bank failures", after controlling for broader —non–financial— stock market conditions. Only then do they use narrative documentation (e.g. on the occurrence of events such as panic runs, or government intervention) to refine their list of bank distress episodes and determine, with the benefit of hindsight, whether the fall in bank equity prices indeed corresponded to a distress episode.[6]

While the lists broadly agree, there are nonetheless important discrepancies. Figures A–C in the Appendix, which compares the dates of bank distress episodes across five different lists, illustrates these discrepancies. For example, BVX, Basten, Bengtsson, Detken, Koban, Klaus, Lang, Lo-Duca, and Peltonen (2017), and Reinhart and Rogoff (2009) all detect severe bank distress in Denmark in the 1990s, whereas neither LV (2018) nor Romer and Romer (2017) do (Figure A). Even when the lists overlap, they may disagree about the starting dates. For example, in Reinhart and Rogoff (2009) the US Savings & Loans crisis runs from 1984 to 1991, whereas in LV (2018) it lasts only one year, 1988 (Figure C).

Each approach has its limitations. One important drawback of the narrative–based lists of bank distress episodes, though, is that they tend to disagree with each other on which episodes are regarded as bank distress episodes. Another is that, when bank distress episodes are dated based on the policy interventions that follow them, they are –by construction– endogenous to the interventions. This makes such lists ill–devised for studying the effect of policies. In this respect, lists based on hard data, like BVX's, seem more adequate. The BVX list also features more episodes than narrative–based lists, with notably many distress episodes that did not end in a crisis. For example, BVX contains about 50% more episodes than LV, for the same countries.

For the purposes of our analysis, we primarily work with BVX's list, which initially covers 46 countries. We expand this list along three dimensions. First, we refine BVX's annual starting dates by using banks' quarterly stock prices, and start a distress episode in the quarter stock prices start falling. Second, in some rare cases, a banking panic occurs *before* BVX's starting date. In those instances, we replace their date with the quarter of the panic. Third, for the countries in our macro–data set that are not in the BVX list, we complement the latter with the list of Basten, Bengtsson, Detken, Koban, Klaus, Lang, Lo-Duca, and Peltonen (2017). Ultimately, for 60 countries over the period 1980q1–2020q2, we identify a total of 110 bank distress episodes.

2.3 A new database on bank distress mitigation tools

We collected information on more than 300 policy interventions dealing with bank distress, for 29 countries over the period 1980q1–2016q4.[7] We organise these interventions based on our judgement, into four types: (T1) central banks' lending schemes; (T2) bank liability guarantee schemes; (T3) impaired asset segregation (IAS) schemes; and (T4) bond and other asset purchase schemes. We

[6] Basten, Bengtsson, Detken, Koban, Klaus, Lang, Lo-Duca, and Peltonen (2017) also use a hybrid approach that combines a financial stress index with local financial authorities' expert judgement.

[7] Argentina, Austria, Belgium, Colombia, Denmark, Estonia, Finland, France, Germany, Greece, Iceland, Indonesia, Ireland, Italy, Japan, Korea, Luxemburg, Malaysia, Netherlands, Norway, Portugal, Slovakia, Slovenia, Spain, Sweden, Thailand, Turkey, UK, and US.

Table 2: Comparison of our crisis management policy database with LV

Tool	**Starting date**	**Amount**	**Design features**
Central bank lending scheme	LV	LV	LV*
Asset purchase scheme	✓	✓	
Liability guarantee scheme			
Deposit insurance	✓	LV*	
Liability guarantee	LV	✓	✓
Impaired asset segregation schemes	✓	✓	LV*

Notes: "LV" refers to information already in LV, "LV*" to substantial additions to information already in LV, and "✓" to new information not in LV.

also record them at the time they are deployed (i.e. come into effect).[8] There are 62 bank distress episodes for which we have information on policy interventions.

One advantage of the database is that we can combine it with the list and dates of bank distress episodes (Section 2.2) to measure the lag between the beginning of an episode (i.e. when banks' stock prices crash) and a specific policy intervention, which allows us to evaluate whether the timing has an effect. A limitation of our database, though, is that it contains limited information on the size of policy interventions. Data on the size are often not available.[9] And even when they are, many issues prevent comparability across distress episodes (e.g. facilities of the same size may be used to different degrees, with liability guarantees and liquidity facilities being cases in point).[10]

2.3.1 Data sources

Our main sources of information are LV, OECD Economic Surveys (for OECD member countries) and IMF Staff Reports (for other countries). We use LV's information on crisis management policies as a starting point, expand the data coverage over the whole sample period (i.e. beyond crisis years), and collect additional information on the various policies (e.g. timing, amount, and design features). Table 2 compares our database with LV's.

Our data collection method follows Romer and Romer (2017). We first use a text–search algorithm to look for general keywords such as "bank" and similar terms as well as more specific ones relating to bank distress mitigation policies (e.g. "liability guarantees", "asset management company", etc) in the OECD Economic Surveys and IMF Staff Reports. We then read the relevant parts of the reports to extract information by hand. We check and complement the latter by using narrative accounts of state aid from European Commission Competition Cases, relevant authorities (e.g. central banks, deposit insurers, and national statistical agencies), Yale University's New Bagehot Project, and the literature.

[8] We did not manage to collect systematically information on when the programs were first announced, although this is an important aspect.

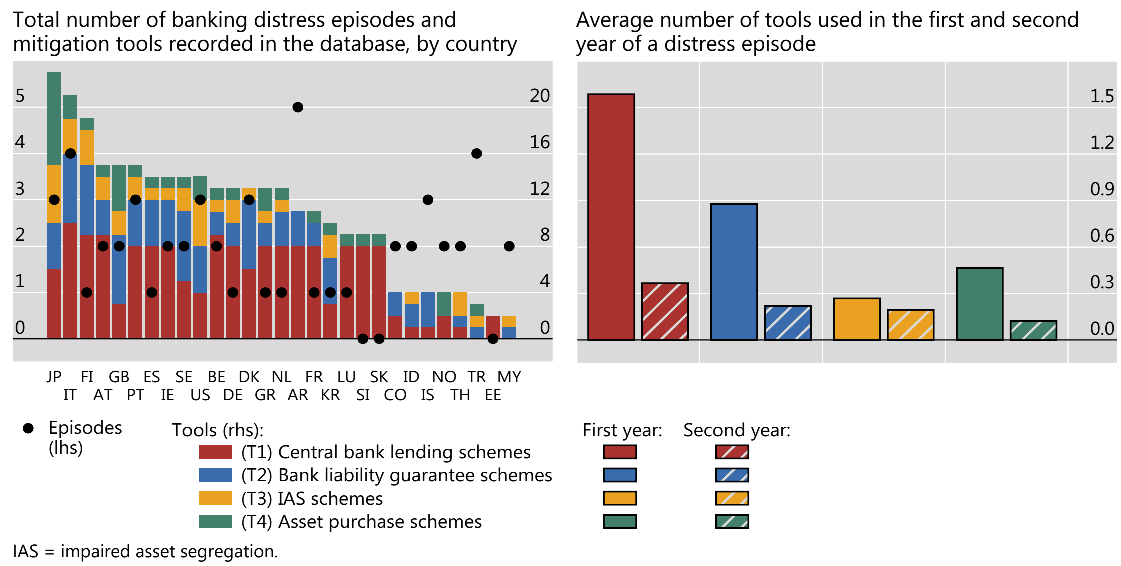
[9] Banks' participation to central bank lending schemes is rarely made public to avoid stigma effects.

[10] Similar caveats apply to studies on the effects of macro–prudential tools (e.g. Boar, Gambacorta, Lombardo, and Awazu Pereira da Silva (2017), Cerutti and Laeven (2017)).

2.3.2 Content of the database

Figure 2 illustrates the content of our database. The intensity of interventions varies across countries, with Japan, Italy and Finland recording the most interventions overall. Finland has the most interventions per distress episode. When they take place, most interventions occur in the first year (right–hand panel). Central banks' lending schemes are the most prevalent type of intervention, with an average of 1.6 schemes set up in the first year, followed by bank liability guarantee schemes.

Figure 2: Number of bank distress mitigation tools in the database



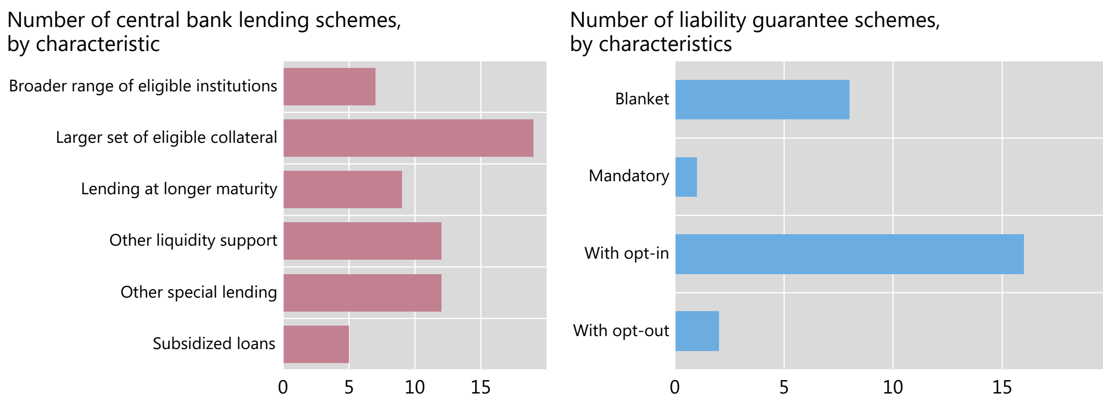
Note: For a sample of 29 countries between 1980q1 and 2016q4. Right–hand panel: statistics conditional on at least one tool being used within the first two years of the distress episode.

Central bank lending schemes (T1) This category consists of interventions providing funding directly to banks and other financial intermediaries. These include outright liquidity provision, special (e.g. long–term) lending and changes in central banks' collateral eligibility rules (e.g. extension of collateral frameworks to more institutions or asset classes). Among these tools, the last one is the most frequently employed (left–hand panel of Figure 3).

Liability guarantee schemes (T2) This category consists of fiscal authorities (partly) guaranteeing commercial banks' privately issued debts, sometimes in exchange for a fee. An example is an enhancement to an existing deposit insurance scheme. The right–hand panel of Figure 3 shows that optional schemes (i.e. with opt–in/out clauses), which potentially carry stigma effects, are more frequent than mandatory and "blanket" schemes. Most schemes recorded in the database only cover new debt issuances.

Impaired Asset segregation schemes (T3) The database records 40 IAS schemes, or so-called "bad banks". Most schemes in the database have a limited lifetime. About half are set up for one specific bank; the other half are "centralized", i.e. purchase assets from several banks, often with a limit on the amount purchased per bank. In our database, the size of a bad bank amounts to 7% of GDP on average, and a haircut of 25% is applied on the assets purchased (see Table 3).

Figure 3: Characteristics of central bank lending and liability guarantee schemes



Note: Left–hand panel: "Broader range of eligible institutions": more financial institutions are allowed to borrow directly from the central bank. "Larger set of eligible collateral": banks can use more varied (usually lower quality) assets as collateral when borrowing from the central bank. "Lending at longer maturity": the central bank offers longer (than usual) maturity loans to banks. "Subsidized loans": banks are given loans at rates below market rates. "Other special lending": Different types of special lending facilities offered by the central bank. "Other liquidity support": other types of liquidity support schemes. Right–hand panel: A bank liability guarantee scheme may be mandatory for all banks or optional. In the latter case, banks may have to opt in to participate, or opt out to withdraw. In the case of a "blanket" liability guarantee, banks are automatically covered by the scheme.

Table 3: Impaired asset segregation schemes

Variable	Unit	Obs	Mean	Median
Centralized AMC	Y/N	38	0.53	1
State bank assets only	Y/N	16	0.69	1
Sunset clause	Y/N	40	0.28	0
Haircut applied	%	28	24.18	0
Size	% of GDP	33	6.99	3.45
Net result	% of GDP	7	0.11	-0.03
Recovery rate	%	5	71.2	75

Asset purchase schemes (T4) This category consists in the central bank purchasing specific assets on secondary markets (e.g. corporate bonds, asset–backed securities), or offering banks to swap risky for safer assets (typically government bonds), asset purchases and asset swap schemes. The database includes 34 of such schemes, most of which posterior to 2008, with information on their starting dates and, in some cases, the type of asset purchased.

Other policy interventions Our database also includes information on nation–wide bank recapitalization schemes but, in many instances, banks' actual participation in these schemes was relatively low.[11] Low participation could reflect the strict conditions attached to participation or the fact that, in practice, most recapitalizations are bank–specific and take place outside of nation–wide schemes. In Section 4, we only use the deployment of bank recapitalization schemes as a control variable capturing distress intensity, with the contention that such schemes signal more severe distress.[12]

3 Pairing and classification of bank distress episodes

The aim of this section is to measure the similarity of, and classify, bank distress episodes, based on the macro–financial anomalies that preceded them.

3.1 Pairing

To identify pairs of similar past bank distress episodes, we proceed in two steps. In the first step, we track the evolution of our set of 70+ macro–financial variables (see Section 2.1) in the two–year run–up to each episode, and determine which macro–economic variables are "abnormally high", "abnormally low", or "normal" before a given episode. We say that a variable takes an abnormally high (resp. low) value in a given quarter, if its change compared with a quarter of reference is in the upper (resp. lower) 10% tail of this change's distribution in normal times.

Since the immediate lead–up and aftermath of the episode may distort the statistics, we follow Goldstein, Kaminsky, and Reinhart (2000) and consider the distribution only for "normal times", defined as the period that excludes the two years before and after the beginning of distress episodes.[13] As various vulnerabilities may not build up at the same pace and time, it is important to span the full run–up phase. Accordingly, we consider the dynamics of our macro–financial variables in 4 selected quarters before the episode: the starting quarter of the episode (0), and the first (-1), third (-3), and fifth (-5) quarters before.[14] This yields more that 280 (i.e. 70+ variables times 4 quarters) diagnoses, and therefore as many potential pre–crisis macro–financial anomalies.

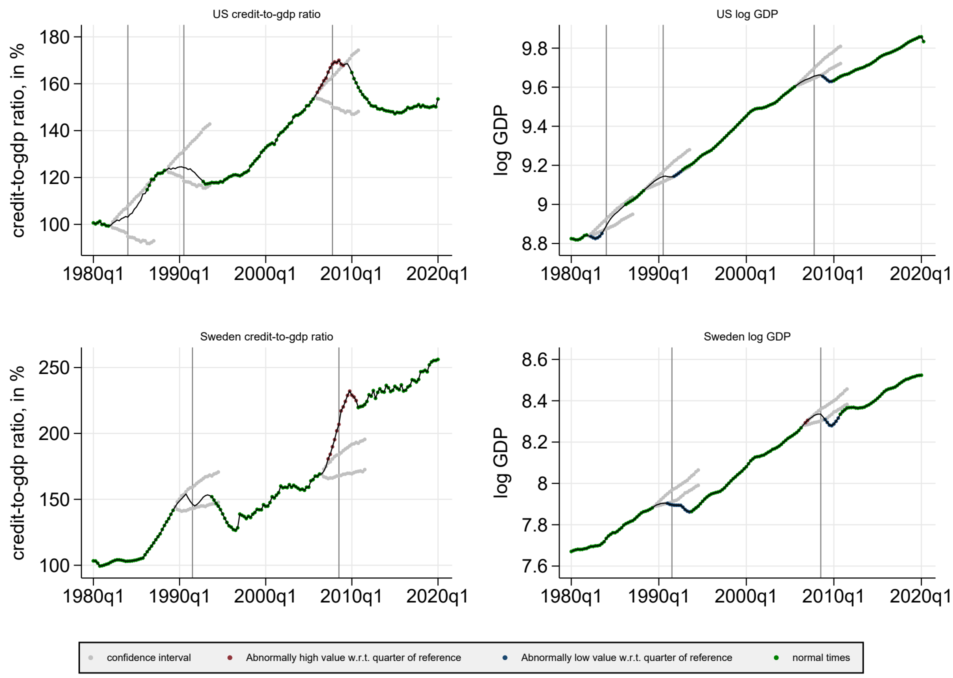
[11] In our database, the amount spent is less than half of the amount spent in half of the cases.

[12] For studies on the effects of bank recapitalizations on economic activity, see e.g. Laeven and Valencia (2013) and Giannetti and Simonov (2013).

[13] Goldstein, Kaminsky, and Reinhart (2000) define normal times slightly differently, and exclude the two years before and three years after a bank distress episode begins (p. 85). Our results are robust to this variation. The normal times distribution of a given variable is country–specific, and we require at least 20 observations to compute the tails.

[14] Our results do not rest on which (and how many) quarters we consider, provided that they span the two year run–up phase of the crisis.

Figure 4: Example of anomalies around bank distress episodes: US vs Sweden



Note: Normal times (green dots) are defined as periods outside of distress times, i.e. two years before and after the beginning of a distress episode. The vertical lines refer to the beginning of a distress episode, i.e. quarter $q = 0$. The 10% confidence intervals (gray dots), are constructed using the country's distribution of the cumulated change of variable v (here, v is the private non–financial sector's credit–to–GDP or log GDP), $v_q - v_{-8}$, with $q = -8, ..., -1, 0, +1, ..., +12$.

Formally, to determine whether a variable, say v, is abnormally high or low, we first de–trend it by calculating its cumulated change, $v_q - v_{-8}$ (where quarter -8 is used as reference), for $q = 0, -1, -3, -5$. We then calculate the 10% upper or lower tails of the normal times distribution of this cumulated change. The distribution is country–specific. If $v_q - v_{-8}$ is in the upper (lower) tail, we conclude that it is abnormally high (low). Figure 4 illustrates anomalies of the credit–to–GDP and log GDP in the case of the US and Sweden around the GFC. The black line corresponds to times around bank distress episodes. The red and blue dots correspond to the quarters around the distress episode, when the variable is off its normal times confidence interval, i.e. abnormally high or low, respectively.

Next, we measure the similarity of two given distress episodes by counting the number of anomalies they have in common, akin to Hamming's distance.[15] One important difference, though, is that we weigh each anomaly by its frequency in the whole history of bank distress episodes. Another is that we normalize the result using the worst–case scenario as benchmark. We thus normalize our measure of similarity between 0% and 100%, where 100% corresponds to a situation where all macro–financial variables take abnormal values in all four pre–distress quarters, i.e. those

[15]To fix ideas, we seek to identify pairs of bank distress episodes that have the same "genetics", i.e. similar initial macro–financial anomalies.

before the "perfect storm". Formally, our synthetic measure of similarity of two distress episodes e and e' is given by:

$$\mathscr{S}_{e,e'} = 100 \times \frac{\sum_v \sum_{q=0,-1,-3,-5} \sum_{g=L,H} \omega(v,q,g)\mathbb{I}(e;v,q,g)\mathbb{I}(e';v,q,g)}{\sum_v \sum_{q=0,-1,-3,-5} \max\left[\omega(v,q,L),\omega(v,q,H)\right]}. \quad (1)$$

in the above expression, the weight $\omega(v,q,g)$ corresponds to the percentage of bank distress episodes (in the whole history of bank distress episodes[16]) where the cumulated change of variable v in quarter q, $v_q - v_{-8}$ is in the 10%–lower ($g = L$) or 10%–upper ($g = H$) tail of its distribution, and $\mathbb{I}(e;v,q,g)$ is a dummy equal to one if (i) episode e features this anomaly and (ii) $\omega(v,q,g) \geq 10\%$, and to zero otherwise.[17]

We compute the measure $\mathscr{S}_{e,e'}$ for all possible pairs of distress episodes for which we have information on policy interventions, i.e. 1,891 (=62×61/2) distinct pairs. We deem two episodes e and e' similar if $\mathscr{S}_{e,e'} \geq 15\%$, which corresponds to the upper quartile of the distribution of the $\mathscr{S}_{e,e'}$s.

3.2 Classification

While the previous measure gives us a measure for the overall similarity of two crises, we are also interested along which dimensions two episodes are similar. For each variable category Vi in Table 1, we construct the following synthetic measure of pre–distress anomaly:

$$\mathscr{A}_{e;\mathrm{Vi}} = 100 \times \frac{\sum_{v\in Vi} \sum_{q=0,-1,-3,-5} \sum_{g=L,H} \omega(v,q,g)\mathbb{I}(e;v,q,g)}{\sum_{v\in Vi} \sum_{q=0,-1,-3,-5} \max\left[\omega(v,q,L),\omega(v,q,H)\right]}. \quad (2)$$

where the notations are the same as in relation (1). We classify episode e as preceded by anomalies of type Vi if $\mathscr{A}_{e;\mathrm{Vi}} \geq 25\%$, which corresponds to the upper quartile of the distribution of the $\mathscr{A}_{e;\mathrm{Vi}}$s in our sample of 62 distress episodes.

To help interpret the synthetic measure in (2), the left–hand panel of Figure 5 indicates the most frequent anomalies within each category. A bar corresponds to the frequency of abnormally low (left–hand side) or high (right–hand side) values in one of the quarters ($q = 0, -1, -3, -5$) preceding bank distress episodes, i.e. $\omega(v,q,L)$ or $\omega(v,q,H)$). Anomalies of type V1, for example, typically stem from domestic residents' cross–border liabilities in foreign currencies and cross–border bank loans. Those related to asset valuations (V2) show up as sharp drops in house and stock prices (e.g.

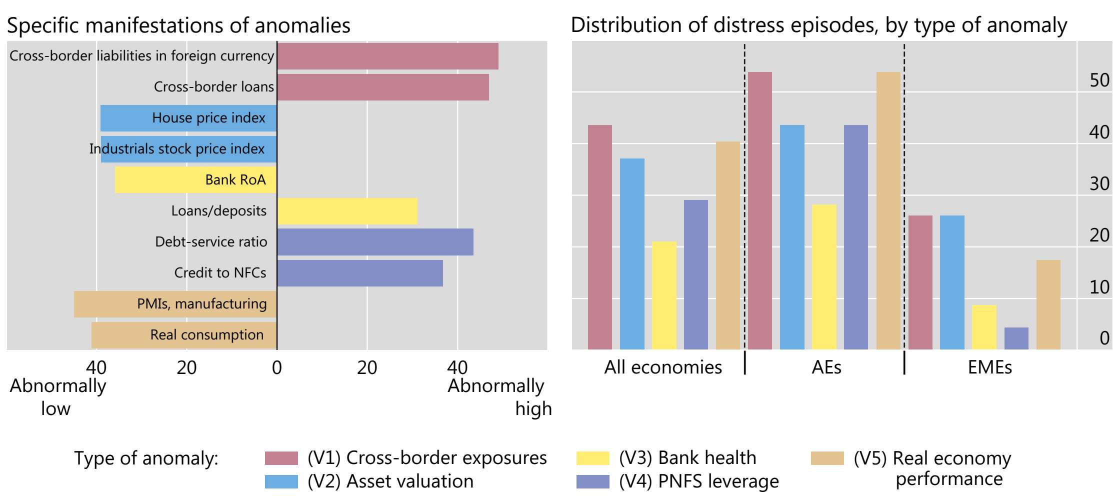
[16]To compute this frequency, we consider a full sample of 110 bank distress episodes in 60 countries over the period 1980q1–2016q4. The weights for all variables and quarters are reported in Figures D–H. For example, Figure D shows that $\omega(\text{X–border FC liab./GDP}, 0, H) = 0.40$ while $\omega(\text{X-border FC liab./GDP}, 0, L) = 0.19$, which means that the ratio of cross–border foreign currency liabilities to GDP is abnormally high at the beginning of a bank distress episode in 40% of the cases, and abnormally low in 19% of the cases (over a sample of 110 distress episodes for 60 countries since 1980q1).

[17]Note that our measure of similarity does not account for rare anomalies (i.e. those that precede less than 10% of distress episodes), and therefore is immune to adding irrelevant macro–financial variables to our database.

following the bust of an asset price bubble). Anomalies related to real economy performance (type V5) most often manifest themselves through a recession, with a reduction in manufacturing PMIs or consumption growth (see Boissay, Claessens, and Villegas (2020)).

Our classification highlights the variety of bank distress episodes. The right–hand panel of Figure 5 shows that distress is preceded by excessive cross–border exposure, severe asset price corrections, or weak real economy performance in about 40% of the episodes. Weak bank performance precedes only 20% of the episodes. The data also indicate some differences between advanced and emerging market economies (EMEs). In advanced economies (AEs), distress episodes tend to be preceded by widespread vulnerabilities, with notably excessive cross–border exposure and weak economic performance (55% of cases). In about 40% of the AE episodes, the initial conditions involve severe asset price corrections or high private sector leverage. In EMEs, it is harder to relate distress episodes to specific vulnerabilities. Just one in four episodes is preceded by excess cross–border exposure or a severe fall in asset prices, and few episodes by excess leverage, whether in the financial or non-financial sector. This suggests that bank distress in EMEs need not be the outcome of domestic imbalances, but could be triggered by external shocks.[18]

Figure 5: Bank distress is preceded by different macro–financial anomalies



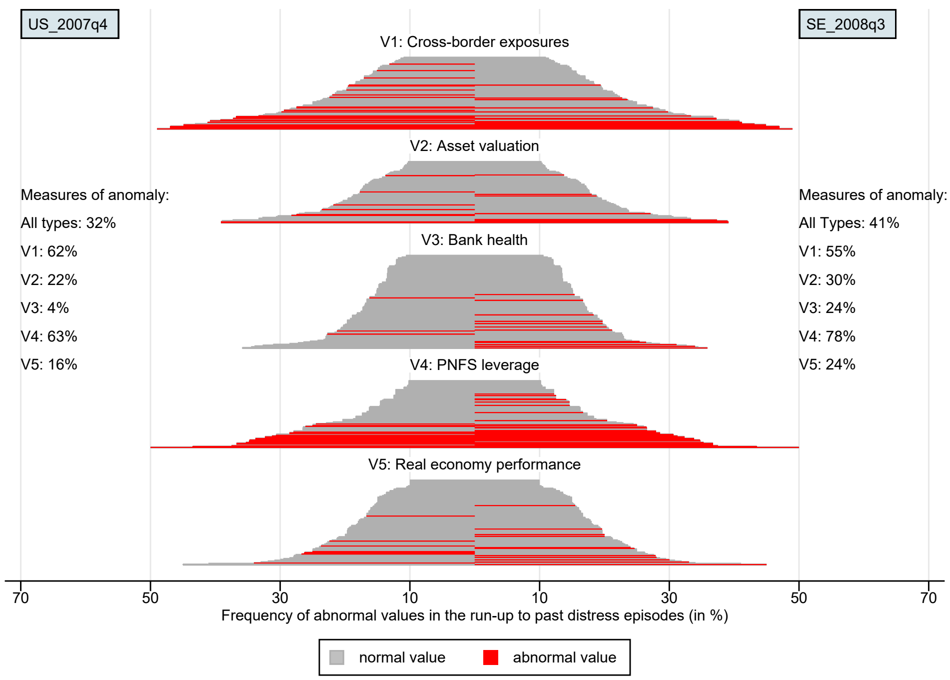
Note: NFCs = non-financial corporations; PMIs = purchasing managers' indices; PNFS = private non-financial sector; RoA = return on assets. Left–hand panel: a bar indicates the frequency of abnormally low (left-hand side) or high (right-hand side) values in the quarter preceding bank distress episodes, during the whole history of such episodes since 1980, for the 60 countries (and 110 distress episodes) for which we observe the macro–financial variables. Right–hand panel: a bar indicates the frequency of the five types of macro–financial anomalies, for the 29 countries (and 62 distress episodes) for which we observe both macro–financial variables and distress mitigation tools.

An illustration. Figure 6 shows an example of similar bank distress episodes –the GFC in the US and Sweden. For these two episodes, our measure of similarity, $\mathscr{S}_{US\_2007q4,SE\_2008q3} = 19\%$ is above our 15% criterion. We conclude that these two episodes are similar. Moreover, the measures

[18]This could also reflect the lack of data (notably cross–border borrowing data) before EMEs' distress episodes.

of pre–distress anomalies are above 25% for categories V1 and V4 for both episodes. This suggests that they are similar in terms of initial cross–border exposures and PNFS leverage.

Figure 6: Pre–GFC macro–financial anomalies: US vs Sweden



Note: This graph compares the macro–economic contexts in the US (left) and Sweden (right) as the GFC broke out. The gray and red bars indicate potential and realized pre-crisis anomalies, respectively. The length of a bar (measured along the x axis) indicates how frequently the underlying variable has had an abnormal value in the run–up to past bank distress episodes since 1980q1 (110 episodes over 60 countries). The longer the bar, the more prevalent the anomaly. The measures of anomaly are defined in expression (2). For example, $\mathscr{A}_{US\_2007q4;\mathrm{V1}} = 62\%$. Red bars that coincide on the left and right sides of the graph point to anomalies common to both episodes.

4 Effectiveness of bank distress mitigation tools

Our approach to evaluating empirically the effectiveness of policy interventions involves two steps. First, we seek to establish a causal relationship between the number of tools deployed and a measure of effectiveness. Second, we test whether the various tools are more effective when deployed more swiftly or in particular types of distress episodes.

4.1 Measures of effectiveness

We consider two distinct measure of effectiveness:[19] (i) the cumulated GDP growth rate between two years (8 quarters) before and three years (12 quarters) after the beginning of a distress event e,

$$\mathscr{G}_e = \ln(GDP_{+12}) - \ln(GDP_{-8}) \tag{3}$$

[19]Notice that both measures of effectiveness are de–trended and purged from distress episode fixed effects.

and (ii) a synthetic measure of macro–financial anomalies three years after the beginning of the distress episode,

$$\mathscr{P}_e = 100 \times \frac{\sum_v \sum_{g=L,H} \omega(v,+12,g)\mathbb{I}(e;v,+12,g)}{\sum_v \max\left[\omega(v,+12,L),\omega(v,+12,H)\right]}. \tag{4}$$

This measure is similar to that of pre–distress anomaly in (2), except for the quarter considered ($q = +12$). In the example in Figure 4, we consider 2011q3 for Sweden and 2010q4 for the US. By then, the credit–to–GDP ratio has returned back to its normal times' confidence band (gray dots) for the US, i.e. to where it should normally have been without the GFC (top left panel). Therefore, $\mathbb{I}(US\_2007q4;\text{credit–to–GDP},+12,L) = \mathbb{I}(US\_2007q4;\text{credit–to–GDP},+12,H) = 0$. For Sweden, this ratio is still 25pp higher than the normal times' upper confidence band (bottom left panel). Hence $\mathbb{I}(SE\_2008q3;\text{credit–to–GDP},+12,H) = 1$ and $\mathbb{I}(SE\_2008q3;\text{credit–to–GDP},+12,L) = 0$.[20] If the distress mitigation tools deployed during the first two years of episode e are effective, then we would expect the economy to have returned to normal, and therefore $\mathscr{P}_e$ to be small.

4.2 Baseline econometric model

Establishing a causal relationship between a given policy intervention and GDP growth is challenging. To keep "all else" equal, we focus on bank distress episodes that feature observationally similar initial macro–financial vulnerabilities. Our identification strategy rests on the assumption that similar vulnerabilities are the symptoms of similar underlying factors, and that the latter should have similar economic consequences unless they are addressed with different policies.[21]

Accordingly, the unit of analysis is a pair of similar episodes e and e', and we are interested in whether the difference in the evolution of GDP or post–distress anomalies throughout these episodes can be explained by differences in policy interventions. Our baseline econometric model is the following:

$$\Delta y_{e,e'} = \alpha + \beta_{(1)}\Delta T^{(1)}_{e,e'} + ... + \beta_{(4)}\Delta T^{(4)}_{e,e'} + \gamma\Delta Z_{e,e'} + \epsilon_{e,e'}, \tag{5}$$

where variable $\Delta y_{e,e'}$ is the difference in either (i) the average annualised real GDP growth rates (relation (3)) or (ii) the measure of post–distress anomalies (relation 4) between episodes e and e'.[22] Since different policy interventions often come together in a distress episode, we estimate the effects of the four types of distress mitigation tools simultaneously. Accordingly, variables $\Delta T^{(1)}_{e,e'}$,...,$\Delta T^{(4)}_{e,e'}$ are the differences between the number of tools of type (T1), ..., (T4) deployed within the first two

[20] For the weight of this anomaly, Figure G shows that the credit-to–GDP ratio is still abnormally high 12 quarters after the beginning of a distress episode in 22% of the episodes. Hence, $\omega(\text{credit–to–GDP},+12,H) = 0.22$.

[21] There are admittedly limitations to comparing like with like. Ideally, one would compare episodes that differ only in terms of the scope and timeliness of policy interventions, e.g. where policymakers also face similar institutional constraints and fiscal space, and not only episodes with similar initial macro–financial vulnerabilities. In practice, however, one cannot exclude that even observationally similar vulnerabilities have different causes (e.g. due to different shocks), and thus may call for different policy actions.

[22] To be sure: the dependent variable is either $\mathscr{G}_e - \mathscr{G}_{e'}$ or $\mathscr{P}_e - \mathscr{P}_{e'}$.

years of episode e versus episode e'. Of interest are the coefficients $\beta_{(1)}$, ...,$\beta_{(4)}$, which capture the effect of the corresponding tools. The regression features a set of additional control variables $\Delta Z_{e,e'}$ that account for drivers of GDP growth or post–distress anomalies other than policy interventions.

The controls capture the differences between countries in terms of exchange rate regimes, central bank independence, initial macro–financial anomalies ($\mathscr{A}_{e;\mathrm{Vi}}$), and other policy interventions, such as monetary policy rates and nation–wide recapitalization schemes. As mentioned earlier, the deployment of such schemes likely signals that bank distress morphs into a full–fledge banking crisis (as defined in LV). Therefore, we view the number of bank recapitalization schemes as a control for the intensity of bank distress, rather than as a policy intervention per se.[23]

4.3 Results for general effectiveness and timing

The analysis points to differences in the tools' effectiveness. The estimates of β in regression (5) are reported in Table 4, for GDP growth. In columns (1) and (2), we omit episode–specific controls such as initial conditions, the change in monetary policy rate, and the deployment of bank recapitalization schemes. In that case, only lending schemes show up has having a significant positive effect on GDP growth. As we control better for initial conditions (column (2)) and recapitalization schemes as proxies for the intensity of the distress, the effect of distress–mitigation tools turns positive (columns (3) and (4)). Central banks' lending schemes and asset purchases, in particular, have a statistically significant positive effect on post-crisis GDP growth. Every additional asset purchase scheme augments the annualised GDP growth rate by 0.51 percentage points on average. Additional bank liability guarantees also have a statistically significant positive effect, but marginally so. One reason could be that calibrating liability guarantee schemes (guarantee fee, requirements for participation, scope of the scheme) is challenging, and schemes may not initially be attractive to banks.

The effectiveness of policy interventions varies depending on the phase of the distress episode (column (5)). This result is obtained from a richer version of regression (5) distinguishing between tools deployed in the first or second year of the distress episode. We find that liquidity provision by central banks is effective in the first year, reflecting its stabilization role, but not in the second, when solvency issues tend to be more prominent. In contrast, impaired asset segregation is more effective in the second year, as non–performing assets are being recognized (Ari, Chen, and Ratnovski (2019)) and bank balance sheets must be repaired. Asset purchase schemes are effective whenever they are deployed, but more so in the second year. Liability guarantees are the only exception, equally effective in the first or the second year.

The results are broadly similar for post–distress anomalies (see Table 5). In this case, we expect policy interventions to speed up the normalization of the economy, and therefore negative βs if the

[23] The negative effect on GDP growth and positive effect on post–distress anomalies (see bottom of Tables 4 and 5) confirms our suspicion that the number of nation–wide recapitalization schemes deployed is especially likely to be endogenous.

Table 4: Effect of bank distress mitigation tools on GDP growth

	Dependent variable: GDP growth				
Main independent variables	(1)	(2)	(3)	(4)	(5)
CB Lending – first 2 years	0.36**	0.43**	0.33**	0.32**	
	(8.67)	(10.23)	(8.68)	(9.28)	
CB Lending – first year					0.34**
					(7.93)
CB Lending – second year					0.02
					(0.21)
Liability guarantee – first 2 years	-0.22**	-0.13	0.24**	0.13*	
	(-2.87)	(-1.08)	(2.14)	(1.91)	
Liability guarantee – first year					0.11
					(1.28)
Liability guarantee – second year					0.26*
					(1.70)
IAS – first 2 years	0.13	0.02	0.07	0.04	
	(1.29)	(0.20)	(0.66)	(0.41)	
IAS – first year					-0.15
					(-0.66)
IAS – second year					0.43**
					(4.43)
Asset purchases – first 2 years	-0.04	0.21*	0.69**	0.51**	
	(-0.47)	(1.89)	(6.17)	(5.36)	
Asset purchases – first year					0.47**
					(5.19)
Asset purchases – second year					1.00**
					(3.90)
Control variables					
FX peg	-1.99**	-2.11**	-1.54**	-1.69**	-1.42**
	(-11.89)	(-9.33)	(-6.52)	(-10.78)	(-7.95)
CB independence	-0.10	-0.38*	-0.81**	-0.44**	-0.56**
	(-0.64)	(-1.85)	(-3.92)	(-2.84)	(-2.29)
Advanced economy	-0.04	-0.08	-0.89**	-0.54**	-0.73**
	(-0.21)	(-0.32)	(-3.73)	(-3.01)	(-4.08)
$\mathscr{A}_{\mathrm{V}}$		0.09	0.07		
		(1.41)	(1.30)		
$\mathscr{A}_{\mathrm{V1}}$		-0.02	-0.02		
		(-1.53)	(-1.25)		
$\mathscr{A}_{\mathrm{V2}}$		-0.02*	-0.03**		
		(-1.87)	(-2.79)		
$\mathscr{A}_{\mathrm{V3}}$		-0.02	0.00		
		(-1.34)	(0.42)		
$\mathscr{A}_{\mathrm{V4}}$		-0.03**	-0.02*		
		(-2.91)	(-1.73)		
$\mathscr{A}_{\mathrm{V5}}$		0.00	-0.00		
		(0.12)	(-0.18)		
Policy rate – first 2 years	0.23**	0.20**	0.20**	0.20**	
	(6.16)	(5.28)	(5.07)	(5.37)	
Policy rate – first year					0.10*
					(1.83)
Policy rate – second year					0.18**
					(3.34)
Bank recap. scheme – first 2 years			-1.30**	-1.16**	
			(-8.73)	(-10.20)	
Bank recap. scheme – first year					-1.07**
					(-6.14)
Bank recap. scheme – second year					-2.22**
					(-11.00)
Constant	-0.40**	-0.41**	-0.25**	-0.26**	-0.19**
	(-3.94)	(-3.80)	(-2.44)	(-2.75)	(-2.14)
Nb Observations	385	385	385	385	385
R–squared	0.386	0.436	0.541	0.505	0.587

Note: Robust t–statistics in parentheses. ** p<0.05, * p<0.1. The dependent variable is the difference in GDP growth within pairs of similar episodes e and e' (i.e. $\mathscr{G}_e - \mathscr{G}_{e'}$). The main independent variables are the differences in the number of tools deployed within the first two years, between episodes e and e'.

interventions are effective. The coefficients in column (4) are negative, except in the case of liability guarantee schemes, which only reduce macro–financial anomalies when implemented in the second year (column (5)).

Overall, our results are consistent with those using richer, micro data sets. Despite the usual limitations inherent to cross-country analyses employing coarse data, our findings concur with those of more granular case studies. For example, Eser and Schwaab (2016) and Andrade, Breckenfelder, De Fiore, Karadi, and Tristani (2016) find that asset purchase schemes improve liquidity conditions, reduce risk premia and contribute to raising the equity price of the banks holding the assets covered by the scheme. Andrade, Cahn, Fraisse, and Mesonnier (2019) also find that, when central banks provide long-term liquidity to banks, the latter increase their loans to the real economy. And Laeven and Valencia (2008) find that extending blanket guarantees reduces banks' need for liquidity support from central banks.

4.4 Results for effectiveness by type of tool and category of bank distress

Next, we examine whether the effectiveness of individual tools depends on the category of initial vulnerability. To this end, we modify regression (5) by interacting a given tool with each macro–financial vulnerability:

$$\Delta y_{e,e'} = \alpha + \beta_{(1)}^{(1)} \Delta T_{e,e'}^{(1)} \times V_{e,e'}^{(1)} + ... + \beta_{(1)}^{(5)} \Delta T_{e,e'}^{(1)} \times V_{e,e'}^{(5)} + \beta_{(2)} \Delta T_{e,e'}^{(2)} + ... + \beta_{(4)} \Delta T_{e,e'}^{(4)} + \gamma \Delta Z_{e,e'} + \epsilon_{e,e'}, \quad (6)$$

Of interest in regression (6) are the coefficients $\beta_{(1)}^{(1)}, ..., \beta_{(1)}^{(5)}$, which capture the effects of deploying a tool of type (T1) during a distress episode preceded by macro–financial vulnerability of category (V1), ..., or (V5). We estimate this model separately for each type of tool and obtain 20 (four tools times five vulnerabilities) distinct estimates.

The results for GDP growth, shown in Table 6 (columns (1)–(6)), suggest that certain tools are especially effective under specific initial conditions. For instance, more central bank lending schemes and asset purchases boost GDP growth (by 0.2 percentage points (column (1)) and 0.35 percentage points (column (6)), respectively) when distress follows abnormally large cross–border exposures. Likewise, such interventions are effective on the heels of large asset price corrections, weak bank performance and excess private sector leverage. From the perspective of specific vulnerabilities, we find that all tools are effective in a context of excessive cross–border exposure. In contrast, no single tool is especially potent when a country enters a bank distress episode with weak economic performance. This could reflect the lack of direct central bank levers to address distress that originates in the real economy.

The estimates of the effect of policy interventions on post–distress anomalies are broadly consistent with the above results (Table 6, columns (7)–(12)). In that case, setting up impaired asset segregation schemes in the second phase of the distress episode helps normalize the economy if the asset prices were initially depressed.

Table 5: Effect of bank distress mitigation tools on normalization

Main independent variables	Dependent variable: anomalies after 3 years ($\mathscr{P}_e$) (1)	(2)	(3)	(4)	(5)
CB lending – first 2 years	-2.03**	-2.29**	-1.63**	-1.83**	
	(-7.63)	(-8.73)	(-7.32)	(-7.79)	
CB lending – first year					-2.58**
					(-8.98)
CB lending – second year					-2.01**
					(-3.90)
Liability guarantee – first 2 years	5.09**	4.65**	2.29**	2.95**	
	(10.44)	(7.16)	(3.86)	(5.80)	
Liability guarantee – first year					2.35**
					(5.06)
Liability guarantee – second year					-2.59**
					(-2.34)
IAS – first 2 years	-1.70**	-0.40	-0.71	-1.11	
	(-2.42)	(-0.54)	(-1.05)	(-1.63)	
IAS – first year					6.86**
					(6.12)
IAS – second year					-6.69**
					(-10.13)
Asset purchases – first 2 years	0.89*	0.80	-2.33**	-2.48**	
	(1.72)	(1.36)	(-3.65)	(-4.00)	
Asset purchases – first year					-2.76**
					(-5.34)
Asset purchases – second year					-1.88
					(-1.60)
Control variables					
FX peg	4.87**	3.65**	-0.03	3.06**	0.58
	(4.37)	(2.64)	(-0.02)	(3.05)	(0.47)
CB independence	-0.93	1.79	4.53**	1.14	8.50**
	(-0.81)	(1.26)	(3.63)	(1.03)	(4.99)
Advanced economy	2.13	-0.13	5.16**	5.16**	7.61**
	(1.64)	(-0.10)	(4.13)	(4.71)	(8.56)
$\mathscr{A}_{\mathrm{V}}$		1.50**	1.61**		
		(4.10)	(5.00)		
$\mathscr{A}_{\mathrm{V1}}$		-0.27**	-0.31**		
		(-2.89)	(-3.82)		
$\mathscr{A}_{\mathrm{V2}}$		-0.16**	-0.11**		
		(-2.72)	(-2.01)		
$\mathscr{A}_{\mathrm{V3}}$		-0.15	-0.29**		
		(-1.64)	(-3.87)		
$\mathscr{A}_{\mathrm{V4}}$		-0.20**	-0.31**		
		(-2.92)	(-4.87)		
$\mathscr{A}_{\mathrm{V5}}$		-0.28**	-0.25**		
		(-3.58)	(-3.84)		
Policy rate – first 2 years	-1.02**	-0.89**	-0.89**	-0.88**	
	(-4.94)	(-4.01)	(-4.31)	(-4.23)	
Policy rate – first year					-0.30
					(-0.74)
Policy rate – second year					0.29
					(0.83)
Bank recap. scheme – first 2 years			8.43**	7.08**	
			(10.14)	(8.92)	
Bank recap. scheme – first year					9.78**
					(8.36)
Bank recap. scheme – second year					8.64**
					(8.74)
Constant	1.55**	1.29**	0.25	0.71	0.11
	(2.49)	(2.03)	(0.43)	(1.22)	(0.21)
Nb Observations	385	385	385	385	385
R–squared	0.378	0.465	0.578	0.491	0.672

Note: Robust t–statistics in parentheses. ** p<0.05, * p<0.1. The dependent variable is the difference in the synthetic measure of anomalies 12 quarters after the start of a distress episode within pairs of similar episodes e and e' (i.e. $\mathscr{P}_e - \mathscr{P}_{e'}$). The main independent variables are the differences in the number of tools deployed within the first two years, between episodes e and e'.

Table 6: Effect of bank distress mitigation tools on GDP growth, by category of episode

	Dependent variable: GDP growth						Dependent variable: anomalies after 3 years ($\mathscr{P}_e$)					
Main independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CB lending – first 2 years × Cross–border exposures	0.20**						-1.81**					
	(3.74)						(-5.86)					
CB lending – first 2 years × Asset valuation	0.11**						-0.81**					
	(2.17)						(-2.75)					
CB lending – first 2 years × Bank health	0.00						0.00					
	(0.03)						(0.01)					
CB lending – first 2 years × PNFS leverage	0.10**						0.05					
	(2.09)						(0.15)					
CB lending – first 2 years × Real economy perf.	0.07						-0.01					
	(1.20)						(-0.03)					
Liability guarantees – first 2 years × Cross–border exposures		0.04						1.11*				
		(0.43)						(1.72)				
Liability guarantees – first 2 years × Asset valuation		0.03						-0.22				
		(0.38)						(-0.33)				
Liability guarantees – first 2 years × Bank health		0.00						-0.04				
		(0.04)						(-0.07)				
Liability guarantees – first 2 years × PNFS leverage		-0.00						1.81**				
		(-0.02)						(3.00)				
Liability guarantees – first 2 years × Real economy perf.		0.09						0.79				
		(0.99)						(1.27)				
Liability guarantees - second year × Cross–border exposures			0.42**						-3.30**			
			(2.10)						(-2.68)			
Liability guarantees - second year × Asset valuation			-0.09						-1.14			
			(-0.52)						(-0.92)			
Liability guarantees - second year × Bank health			-0.44**						1.77			
			(-2.28)						(1.40)			
Liability guarantees - second year × PNFS leverage			-0.14						1.17			
			(-0.82)						(1.02)			
Liability guarantees - second year × Real economy perf.			-0.00						-0.08			
			(-0.01)						(-0.06)			
IAS – first 2 years × Cross–border exposures				0.10						-0.00		
				(0.68)						(-0.00)		
IAS – first 2 years × Asset valuation				-0.22						-1.49		
				(-1.43)						(-1.49)		
IAS – first 2 years × Bank health				0.17						-0.70		
				(1.18)						(-0.67)		
IAS – first 2 years × PNFS leverage				-0.08						1.08		
				(-0.63)						(1.14)		
IAS – first 2 years × Real economy perf.				0.07						0.11		
				(0.46)						(0.11)		
IAS – second year × Cross–border exposures					0.68**						-3.94**	
					(3.66)						(-3.50)	
IAS – second year × Asset valuation					0.21						-4.33**	
					(1.04)						(-3.54)	
IAS – second year × Bank health					-0.05						-0.80	
					(-0.30)						(-0.69)	
IAS – second year × PNFS leverage					-0.16						0.53	
					(-0.84)						(0.48)	
IAS – second year × Real economy perf.					-0.26						-0.30	
					(-1.23)						(-0.24)	
Asset purchases – first 2 years × Cross–border exposures						0.35**						-2.06**
						(2.61)						(-2.58)
Asset purchases – first 2 years × Asset valuation						-0.04						0.65
						(-0.30)						(0.86)
Asset purchases – first 2 years × Bank health						0.21*						-2.10**
						(1.69)						(-2.64)
Asset purchases – first 2 years × PNFS leverage						0.11						0.92
						(0.96)						(1.22)
Asset purchases – first 2 years × Real economy perf.						0.05						-0.54
						(0.34)						(-0.70)
Control variables												
CB lending – first 2 years		0.33**	0.34**	0.32**	0.34**	0.33**		-1.69**	-1.53**	-1.85**	-2.08**	-1.83**
		(8.92)	(9.74)	(8.96)	(9.66)	(9.20)		(-7.14)	(-5.87)	(-7.88)	(-9.54)	(-7.76)
Liability guarantees – first 2 years	0.17**			0.13*	0.11	0.15**	2.64**			2.74**	3.07**	2.77**
	(2.45)			(1.85)	(1.59)	(2.09)	(5.35)			(5.57)	(7.87)	(5.54)
IAS – first 2 years	0.01	0.05	0.15*			0.04	-0.82	-0.60	0.82			-1.06
	(0.08)	(0.57)	(1.71)			(0.44)	(-1.21)	(-0.88)	(1.25)			(-1.57)
Asset purchases – first 2 years	0.51**	0.50**	0.48**	0.53**	0.48**		-2.45**	-2.65**	-3.26**	-2.53**	-2.04**	
	(5.45)	(5.23)	(5.02)	(5.51)	(5.12)		(-3.97)	(-4.09)	(-4.96)	(-4.03)	(-3.60)	
FX peg	-1.58**	-1.68**	-1.72**	-1.70**	-1.67**	-1.67**	2.14**	3.49**	2.70**	2.96**	1.94**	2.90**
	(-9.89)	(-10.10)	(-10.49)	(-10.60)	(-10.18)	(-10.51)	(2.14)	(3.42)	(2.51)	(2.96)	(1.98)	(2.89)
CB independence	-0.45**	-0.44**	-0.39**	-0.44**	-0.54**	-0.44**	1.16	0.81	2.22	1.45	2.93**	1.09
	(-2.78)	(-2.69)	(-2.06)	(-2.81)	(-3.52)	(-2.82)	(1.07)	(0.71)	(1.55)	(1.33)	(2.76)	(0.99)
Advanced economy	-0.52**	-0.52**	-0.44**	-0.55**	-0.57**	-0.51**	5.32**	6.13**	8.24**	5.47**	5.90**	5.30**
	(-2.95)	(-2.69)	(-2.70)	(-3.01)	(-3.15)	(-2.85)	(4.82)	(5.60)	(7.81)	(4.94)	(5.72)	(4.86)
Policy rate	0.20**	0.20**	0.21**	0.20**	0.19**	0.20**	-0.83**	-0.94**	-0.73**	-0.87**	-0.69**	-0.89**
	(5.13)	(5.24)	(4.24)	(5.33)	(4.83)	(5.40)	(-3.92)	(-4.46)	(-2.44)	(-4.16)	(-3.10)	(-4.29)
Bank recap. scheme	-1.14**	-1.14**	-1.07**	-1.17**	-1.18**	-1.12**	6.77**	7.73**	9.59**	7.24**	7.65**	6.83**
	(-9.80)	(-10.06)	(-8.85)	(-10.17)	(-10.43)	(-10.03)	(8.52)	(9.70)	(11.77)	(9.08)	(10.39)	(9.12)
Constant	-0.22**	-0.26**	-0.28**	-0.26**	-0.23**	-0.24**	0.44	0.53	-0.02	0.67	0.28	0.61
	(-2.27)	(-2.80)	(-2.97)	(-2.75)	(-2.45)	(-2.57)	(0.75)	(0.87)	(-0.04)	(1.14)	(0.49)	(1.04)
Nb Observations	385	385	385	385	385	385	385	385	385	385	385	385
R–squared	0.508	0.505	0.510	0.510	0.526	0.507	0.509	0.484	0.458	0.494	0.564	0.503

Note: Robust t–statistics in parentheses. ** p<0.05, * p<0.1. The dependent variable is the difference in GDP growth (columns (1)–(6)) or in the synthetic measure of anomalies 12 quarters after the start of a distress episode (columns (7)–(12)) within pairs of similar episodes e and e' (i.e. $\mathscr{G}_e - \mathscr{G}_{e'}$ or $\mathscr{P}_e - \mathscr{P}_{e'}$). The main independent variables are the differences in the number of tools deployed within the first two years, between episodes e and e', interacted with a dummy that is equal to one if episodes e and e' both belong to the same category of initial macro–financial anomalies (i.e. were both preceded by abnormal cross–border exposures, asset valuation, bank health, PNFS leverage, or real economy performance).

5 Conclusion

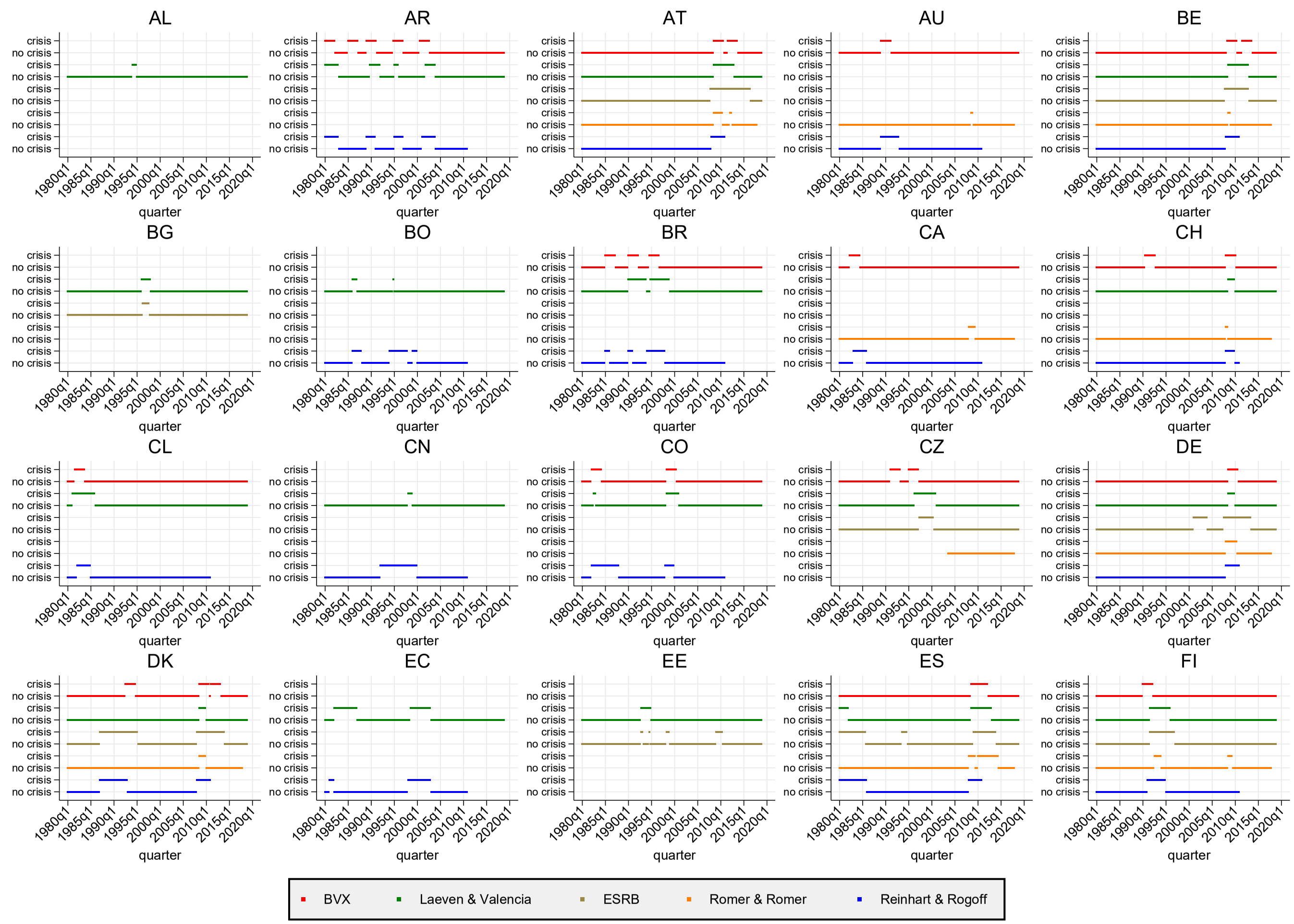
We study the effectiveness of policies that deal with bank distress, and make progress on three fronts. First, we propose a methodology to pair and classify bank distress episodes based on their initial macro–financial conditions. Second, we offer a novel database on policy interventions covering 29 countries over the period 1980–2016. Third, we present new results on the effects of bank distress mitigation policies on the economy.

Whether a policy is effective depends on its type, the speed at which it is deployed and the initial financial and economic vulnerabilities. Greater and swifter policy activity overall reduces the adverse impact of banking distress on economic activity, especially when bank distress follows a period of high cross–border exposures. Central bank lending and asset purchase schemes also help restore GDP growth and normalize the economy when distress follows low asset valuations, high bank leverage and weak bank performance.

Our analysis comes with many of the usual caveats. In particular, due to data limitations we measure policy intervention by the number of tools used —not their size– and we do not capture all the institutional and other country differences that may affect economic outcomes. We measure the effectiveness of policy interventions based on their short–term impact (within two years) on GDP growth and the normalization of the economy. And we ignore longer–term aspects, such as the potential effects on resource allocations, moral hazard or public finances.

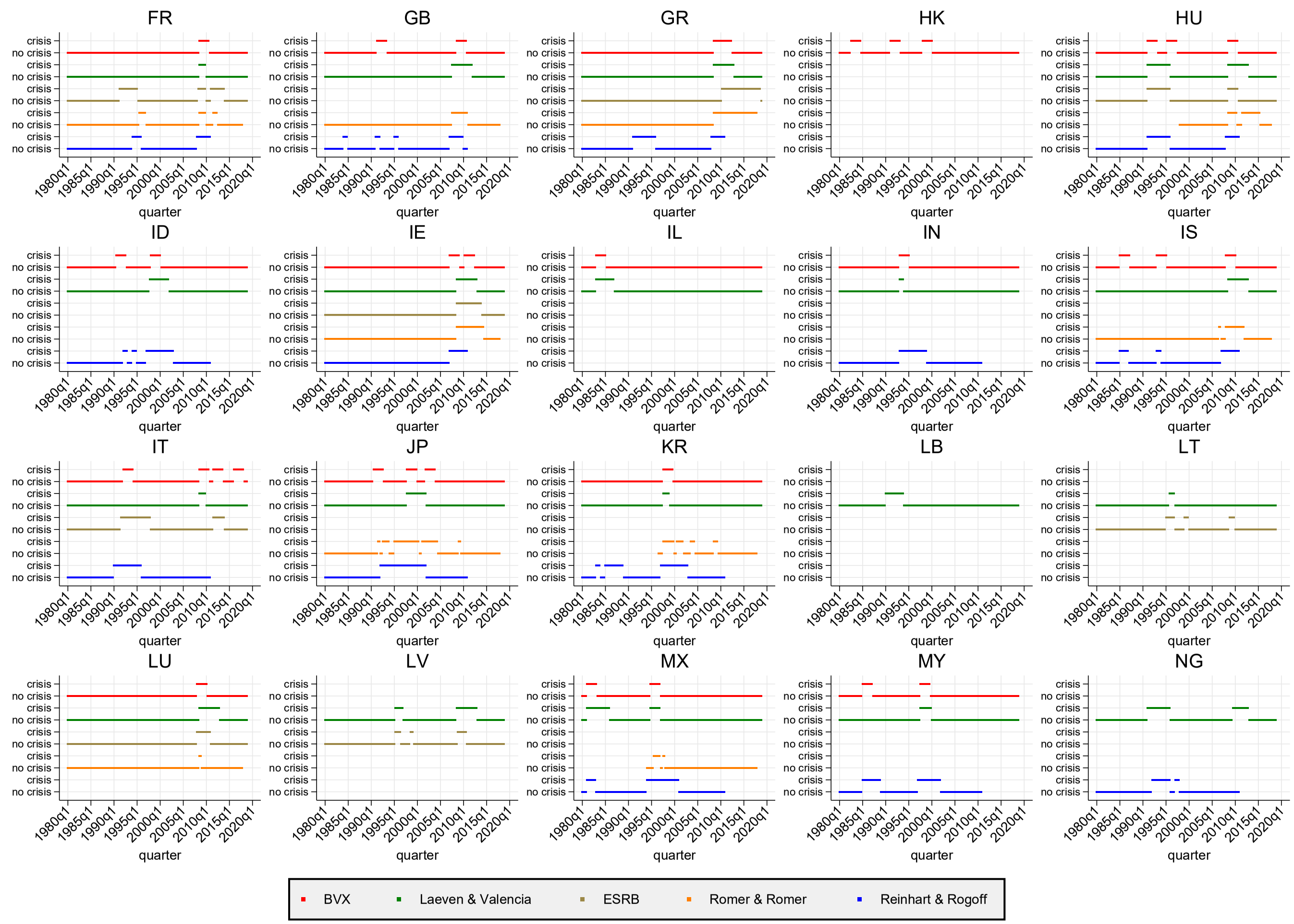
6 Appendix

Figure A: Comparison of bank distress episodes dates (I/III)



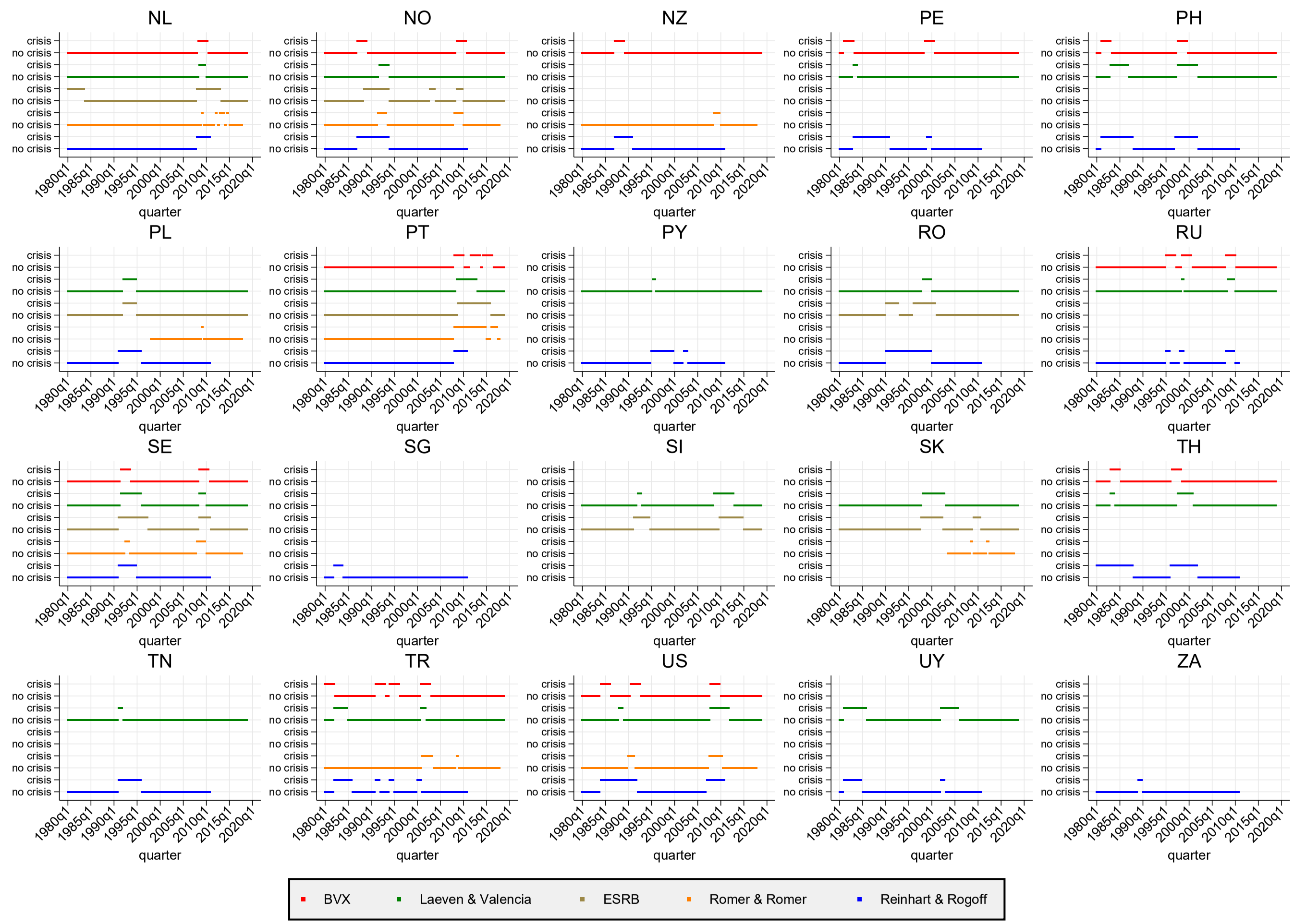
Note: Breaks in the horizontal lines indicates the starting/end dates of bank distress episodes. When the end date of a crisis is not reported, we assume that the crisis lasts eight quarters.

Figure B: Comparison of bank distress episodes dates (II/III)



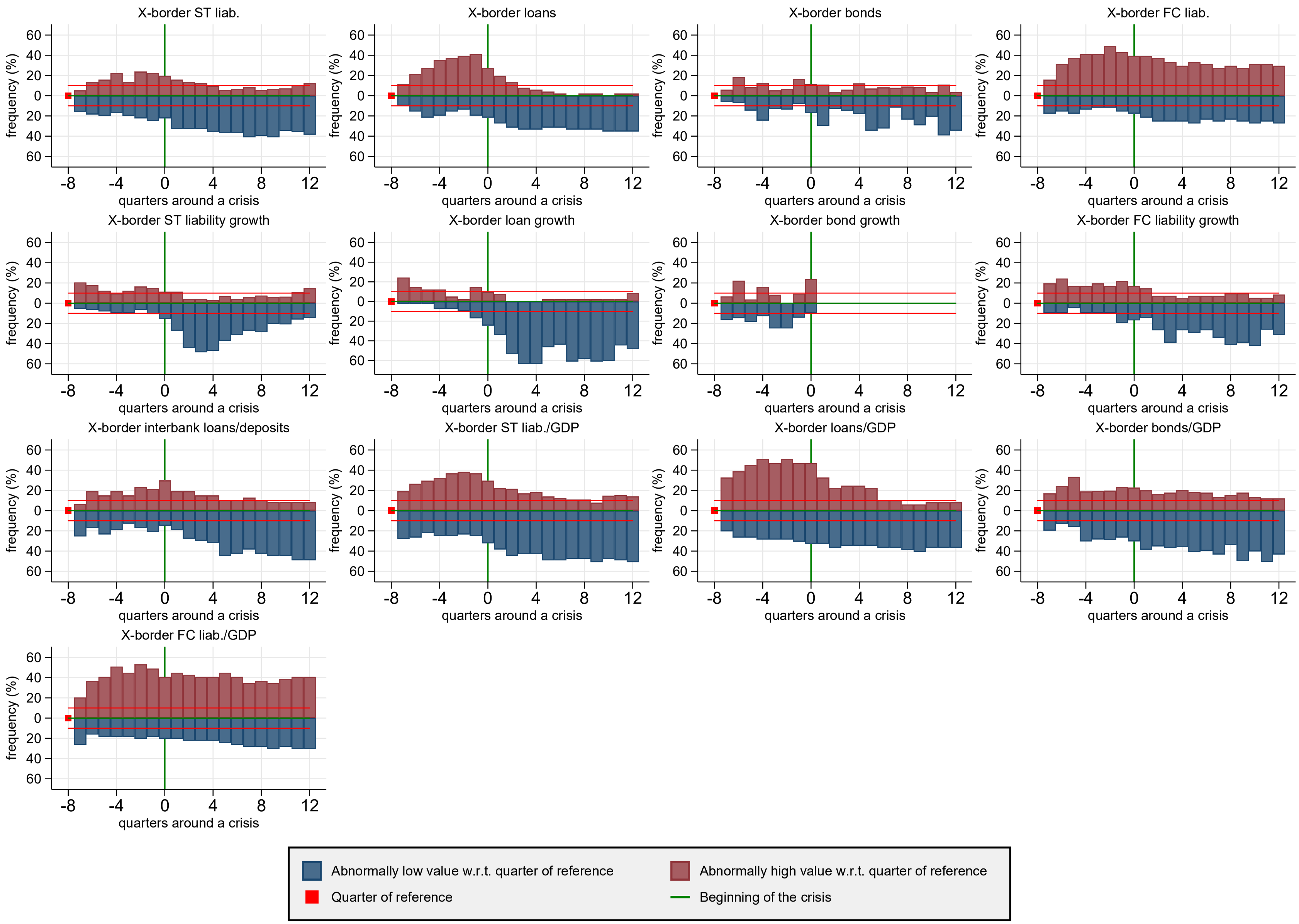
Note: Breaks in the horizontal lines indicates the starting/end dates of bank distress episodes. When the end date of a crisis is not reported, we assume that the crisis lasts eight quarters.

Figure C: Comparison of bank distress episodes dates (III/III)



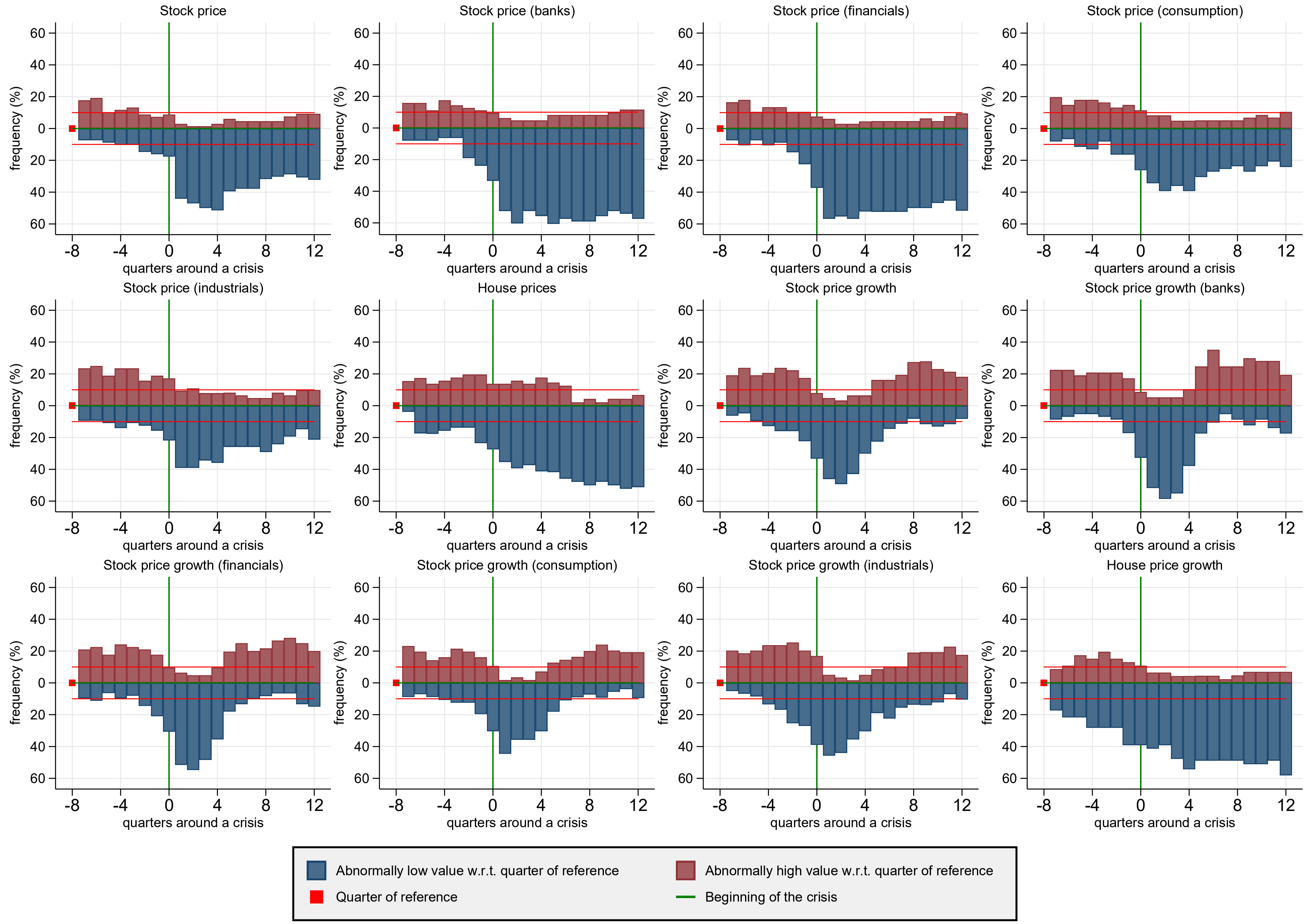
Note: Breaks in the horizontal lines indicates the starting/end dates of bank distress episodes. When the end date of a crisis is not reported, we assume that the crisis lasts eight quarters.

Figure D: Weights of anomalies in cross–border borrowing (V1)



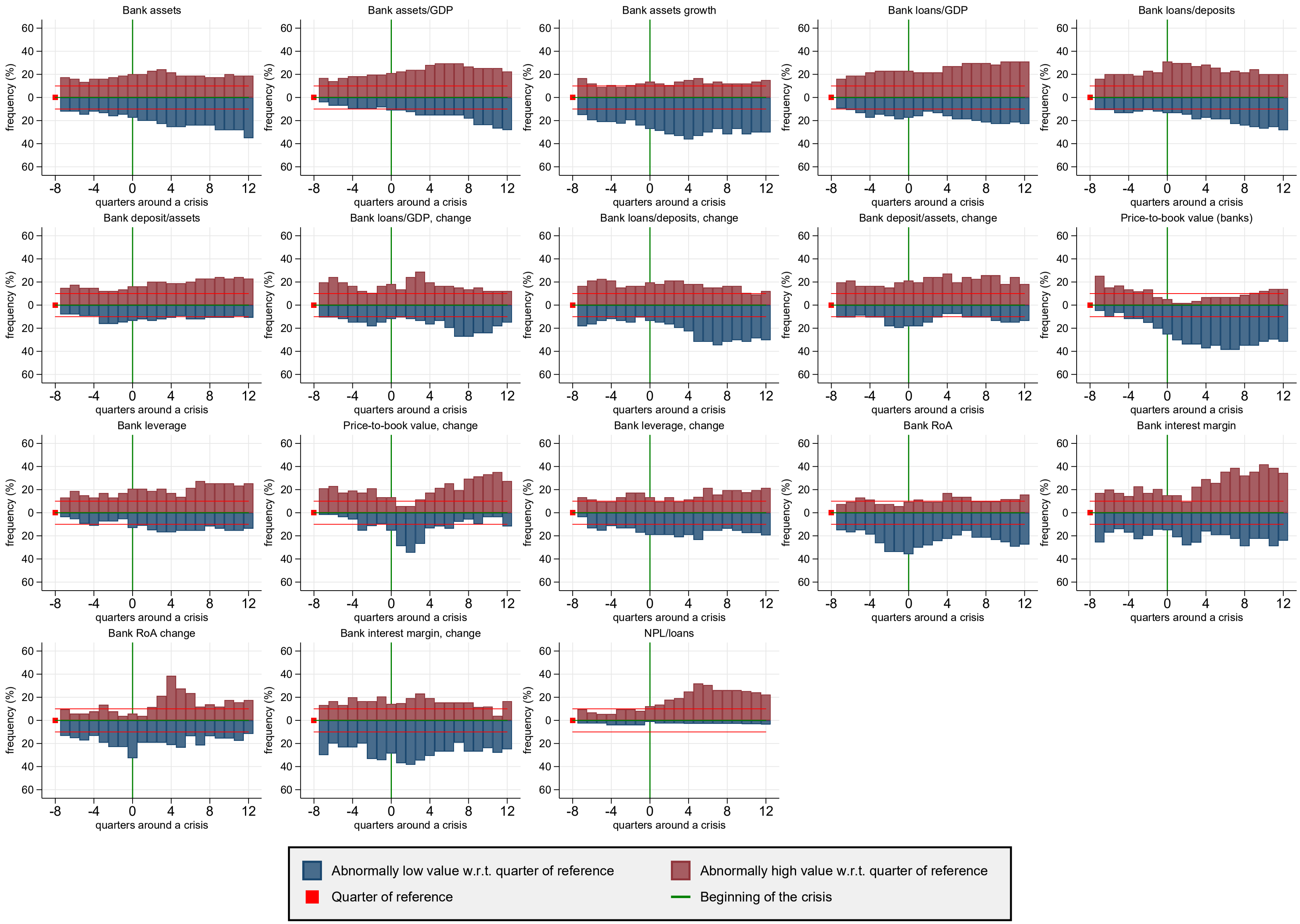
Note: A bar measures how frequently the variable has had a abnormally high (red bar) or low (blue bar) value in a given quarter ($t_0 - 8 + q$, for $q = 1, ..., 20$) around the beginning of a bank distress episodes (t_0), in the whole history of bank distress episodes (110 episodes in 60 countries since 1980q1), as reported in BVX and Basten, Bengtsson, Detken, Koban, Klaus, Lang, Lo-Duca, and Peltonen (2017). For example, cross–border foreign currency denominated liabilities ("X-border FC liab.") are abnormally high in the second quarter before the start of a distress episode (in $t_0 - 2$) in almost 50% of the episodes. A variable v is abnormally high (low) in quarter $t_0 + q$ when its change between $t_0 - 8$ and $t_0 + q$, $v_{t_0+q} - v_{t_0-8}$ is in the upper (lower) 10% of the distribution of this change in normal times (with $q = -8, ..., -1, 0, +1, ..., +12$). The red square corresponds to the quarter of reference, $t_0 - 8$. The red horizontal lines correspond to the 10% threshold, which by construction corresponds to the frequency of an anomaly in normal times. Frequencies in excess of this threshold point to a statistically significant anomaly. The absence of bars indicates that there was not enough observations to calculate the weights.

Figure E: Weights of anomalies in asset valuations (V2)



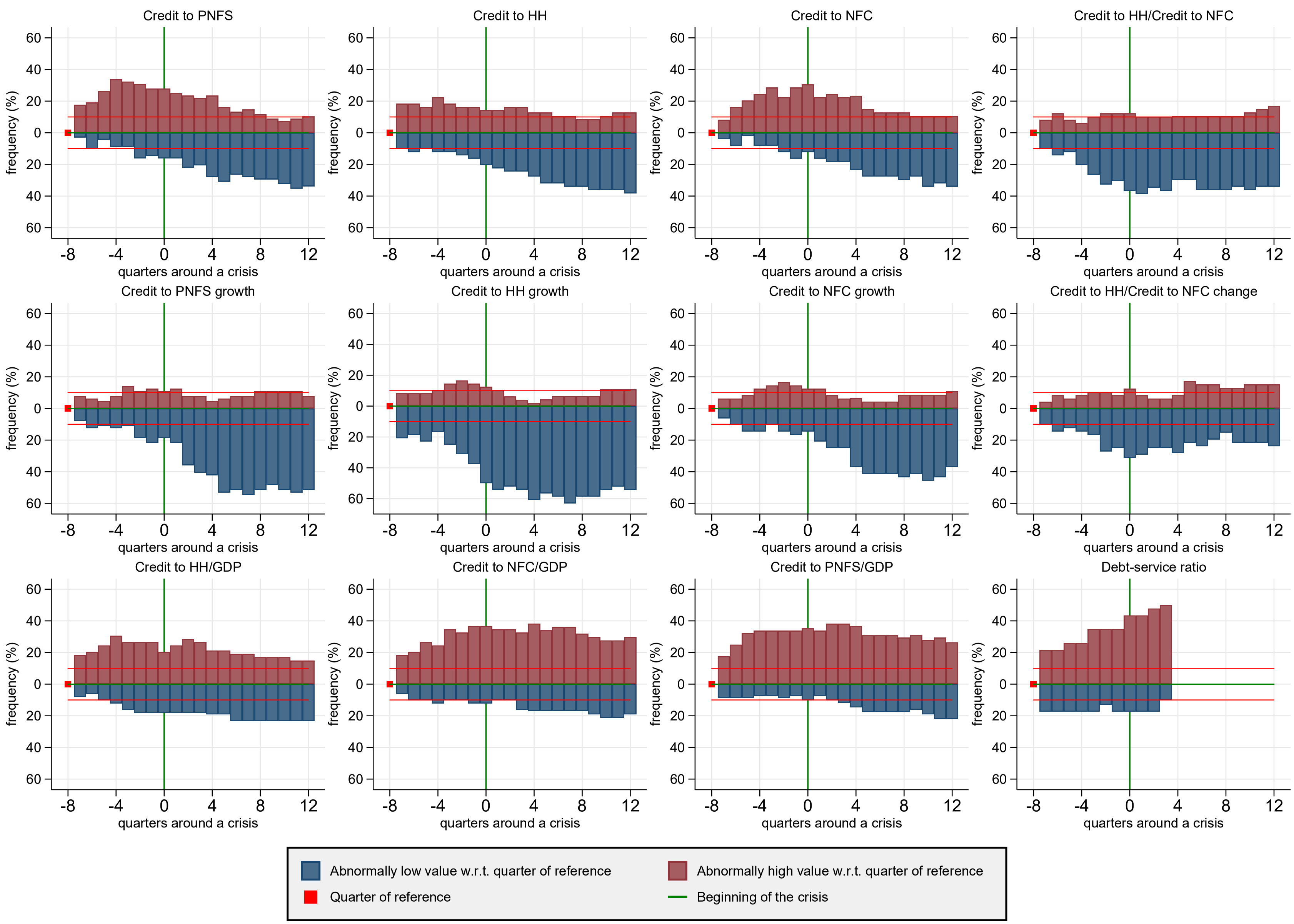
Note: Same as in Figure D.

Figure F: Weights of anomalies in bank health (V3)



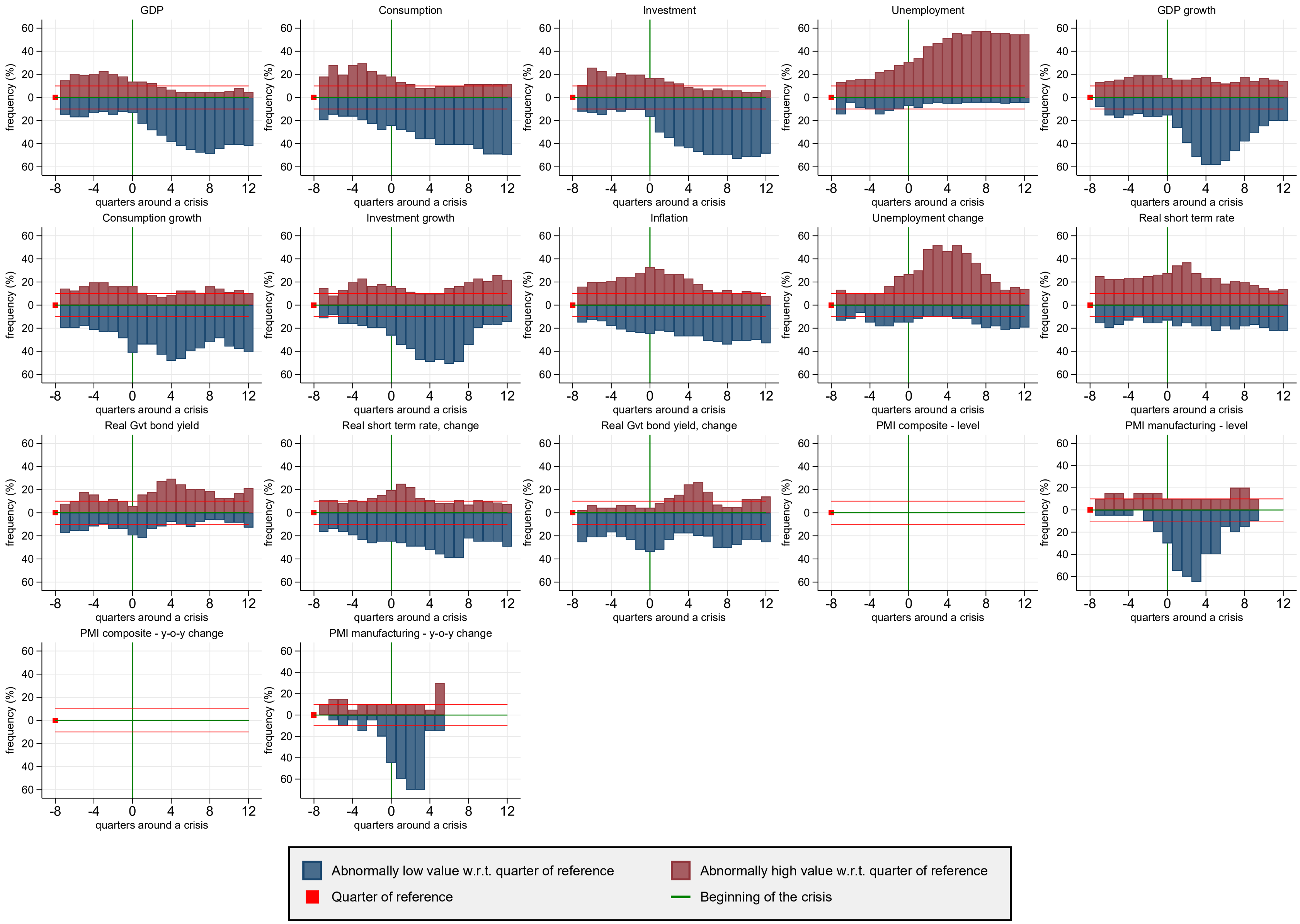
Note: Same as in Figure D.

Figure G: Weights of anomalies in private non-financial sector leverage (V4)



Note: Same as in Figure D.

Figure H: Weights of anomalies in real economic performance (V5)



Note: Same as in Figure D.

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