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Financial Policymaking after Crises: Public vs. Private Interests

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Financial Policymaking after Crises: Public vs. Private Interests

Abstract

We first present a simple model of post-crisis policymaking driven by both public and private interests. Using a novel dataset covering 94 countries between 1973 and 2015, we then establish that financial crises can lead to government interventions in financial markets. Consistent with a public interest channel, we find post-crisis interventions occur only in democratic countries. However, by using a plausibly exogenous setting -i.e., term limits- muting political accountability, we show that democratic leaders who do not have re-election concerns are substantially more likely to intervene in financial markets after crises, in ways that may promote (obstruct) private (public) interests.

JEL-Codes: G010, G280, P110, P160.

Keywords: financial crises, reform reversals, democracies, term-limits, special-interest groups.

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“Never let a good crisis go to waste.” (Winston Churchill, 1940s)

1. Introduction

Financial crises are an endemic feature of market economies. Banking, currency and sovereign debt crises have occurred in almost all countries throughout history (Reinhart and Rogoff, 2009). The negative effects of these crises on national economies have generally been severe, leading to banking collapses, recessions and marked increases in government debt levels. Invariably this leads governments to intervene in one way or another leading to significant institutional changes in the country’s financial landscape. More importantly, such interventions are likely to be politically motivated for a number of reasons.

First, governments may feel forced to intervene in order to save the banking system as they may otherwise be punished by middle-class voters who are often deeply entrenched within the country’s financial intermediaries with their savings and investments (Chwieroth and Walter, 2019). Second, when the cause of the crisis is commonly perceived to be financial liberalization and the public sentiment turns against the financial industry, governments may be urged to impose new regulations, thereby reversing the process of financial liberalization (Dagher, 2018). Differently from the previous two arguments that emphasize the importance of public preferences, a third reason could be the private incentives of policymakers who may take advantage of the public sentiment in the aftermath of a financial crisis and introduce new policies that will favour the preferences of the financial industry at the expense of the society.¹

¹The idea that regulatory policymaking could be captured by private interest groups goes back to the seminal piece by Stigler (1971) and the following authors (Krueger, 1974; Peltzman, 1976). In a similar fashion, it has long been argued that policymakers tend to behave in ways that would satisfy their own constituents’ demands; that is, in line with the public interest (see, among others, Wittman, 1977; Peltzman, 1984; Alesina, 1988). As a caveat, in defining the public interest in this paper, we take the view that voters generally know what their interests are and realize their gains/losses from previous policies before they can vote in an election. In such a setting, there is no information asymmetry (ie., no possibility of “pandering”) between the policymaker and median voter (Canes-Wrone, Herron, and Shotts, 2001). Hence, we assume that constituents’ current *perception* of ideal policymaking (ie., “public interest”) is what should drive policymakers’ incentives in view of the upcoming elections. Whether and under what circumstances such

In this paper, we aim to shed light on these public and private channels by trying to understand how the level of political accountability within and across countries shapes post-crisis financial policymaking. Based on a novel panel dataset of 94 countries over the period from 1973 to 2015, we begin our analysis by employing a quasi-difference-in-differences methodology and compare the level of financial liberalization between the two periods immediately before and after a financial crisis. This helps us estimate the average impact of a crisis on actual government behaviour across seven distinct financial policy domains; namely, credit controls, interest rate controls, entry barriers, capital account, privatization, banking supervision and security markets. At a first pass, we present strong evidence showing that financial crises in general trigger government interventions and initiate a process of re-regulation in financial markets.²

Interventionary policy stance in the aftermath of financial crises however does not necessarily tell us whether these policy reactions are motivated by public preferences or by policymakers' private interests that may arise due to their personal connections to private businesses and/or career plans after leaving politics. Hence, in order to disentangle between these two channels, we present a simple two-stage model in which re-elections mitigate the 'principal-agent' problem and give incentive to politicians to seek public interests instead of short-term private gains. The main tenet of the model builds on the incumbent politician's trade-off between currently available private rents and the present value of expected future rents in case of re-election. In this setting, we show that the existence (absence) of re-election concerns may be an important mechanism incentivising the politicians to behave more in line with public (private) interests. The model thus produces two key implications that help us interpret our empirical findings.

First, we find that policy interventions and re-regulation after financial crises are only common in democratic settings (as opposed to autocracies), which -in line with our model-

public perception corresponds to optimal policymaking is beyond the scope of this paper.

²This general result holds when different subsamples are chosen for various robustness checks, when different time intervals around crises are used or when an alternative dataset is employed to allow for a wider variety of financial crises.

points to a public interest channel mainly due to increased accountability of the politicians in democratic settings. This finding echoes the earlier argument that policymakers in democracies have to respond to middle-class concerns on financial stability in order to avoid the punishment in the upcoming elections (Chwiero and Walter, 2019) and constitutes evidence that post-crisis policymaking -at least to some extent- is driven by the public interest.

Second, in order to trace the private-interest channel, we benefit from a technical aspect of the election process in democratic countries and use it as a plausibly exogenous setting that increases the possibility that policymakers become less responsive to public concerns and behave more in line with their own private incentives. Our identifying assumption here is that the incumbent policymakers feel politically less accountable and thus put more weight on their private interests when they face a binding term-limit. Empirically, we compare democratic leaders' policy reactions to financial crises when they can be freely re-elected in the upcoming elections and when they cannot due to a binding term limit (i.e., being a *lame-duck* politician).³

By treating the periods with term limits on the incumbent politician as a plausibly exogenous setting that lowers political accountability, we find that a substantial portion of the reversals in the aftermath of financial crises may be driven by private interests in politics. Specifically, we detect that policy interventions occur both when politicians face a binding term limit and when they do not; however the effect is almost *four times larger* in the former case. This result is robust to within-party estimations as well as controlling for various types of political heterogeneity across countries and specifically around crisis episodes. Further employing a test recently proposed by Oster (2019) ensures that our findings are unlikely to be driven by other potentially omitted factors.

³Some democratic countries impose term limits on their political leaders which prevent them from serving after a certain number of election terms. The number of terms in the limit and the duration of servings in each term might change from country to country; however the fact that a politician might be serving her last term due to a term limit gives us a clean counterfactual to see what would happen if policymakers had no (or relatively lower, if one assumes that lame-duck leaders may still be able to change the constitution to remove the term-limit restrictions) re-election chances and thus were less sensitive to public interests in their policies. We also show that restricting our sample to those countries that had at least one term-limited politician during our sample period produces qualitatively similar results.

To test the possibility that these policy changes by the term-limited leaders may be necessary to avert or mitigate the crisis, we first check the timing of both term-limited and unlimited leaders’ policy reactions to crises. While the latter react as soon as the crisis starts and then gradually revert back to the “normal” policy stance, the former react with a 3-year lag and do not seem to revert later. Hence, the additional interventions of the term-limited leaders do not seem to have much to do with the crisis management.

To have a more direct test on the financial stability performance of different types of policymakers, we use a global bank-level dataset. Arguably if term-limited policymakers are busy with engaging in privately-motivated policy reactions, one would not expect them to be very concerned about restoring the much-needed financial stability after a crisis. Indeed we find that domestic banks get closer to default when crises are experienced under term-limited leaders than otherwise. Furthermore, this adverse effect is especially stronger for larger banks, consistent with the argument that such banks might be better at providing a quid-pro-quo and thus less likely to be tamed by a policymaker open to such influences.

When we zoom into specific policy domains, we find that the term-limited interventions mainly operate via the extensive margin of policymaking and, more importantly, emerge in different policy domains than those initiated by unlimited policymakers. In particular, they are reflected in controversial areas such as increasing interest rate controls and raising bank entry barriers that are usually associated with rent extraction ([Friedman, 1970](#); [Goddard, Liu, Molyneux, and Wilson, 2011](#)) and not in areas such as improving banking supervision or restricting capital controls that are usually associated with financial stability and considered as more aligned with public interest ([Mester, 2017](#); [Erten, Korinek, and Ocampo, forthcoming](#)).

Finally, to illustrate one of the plausible mechanisms behind policymakers’ private interests, we focus on three banking-related policy domains in which we can clearly lay out the incumbent banks’ preferences that may be in contradiction with public interests. Exploiting the intensity of the revolving doors between political and financial institutions across

countries, we show that the term-limited politicians further adjust their policies in ways that would be favourable to incumbent banks when they have a higher chance of pursuing a financial career after leaving politics. To be specific, such policymakers are found to be much more likely to use taxpayer money to bailout the banks, raise the entry barriers for the banking industry and lower the supervision on incumbent banks. This suggests that political executives in their last term may advance their own private agendas by resorting to financial interventions that tend to create rent-seeking opportunities for the industry.⁴

While our results suggest that term-limit rules may act as a barrier reducing political accountability and pushing the policymaking process away from a socially-optimal equilibrium, it may be necessary to consider some of the caveats missing so far in our theoretical as well as empirical investigation. First, to the extent that voters may be misinformed about the relative value of the policy options available to them, absence of re-election incentives (i.e., term-limits) may help prevent politicians from resorting to pandering (Canes-Wrone et al., 2001). Second, in countries with weak institutions and high corruption, term-limits may help lower the entrenchment tendency of the incumbent politicians (Dick and Lott Jr, 1993). Thus, our results cannot be interpreted as a warning against term-limits at all costs.

The paper proceeds as follows. The next section surveys the most relevant strands of literature and helps place our contribution. Section 3 lays out a simple model of post-crisis policymaking that will help us later to interpret our empirical results. Section 4 describes the construction of the dataset whereas our methodology and identification strategy are explained in Section 5. Section 6 presents the results and the last section concludes the paper.

⁴It is likely that this goes hand-in-hand with the rising anti-finance sentiment in public which may pave the way for the politician to over-intervene in the sector. See Knell and Stix (2015) for evidence on how financial crises may reduce public trust in the financial system. This argument is also consistent with the fact that we fail to find any policy differences between term-limited and unlimited politicians during normal (non-crisis) times.

2. Survey of the literature

2.1. *Term limits and political accountability*

There is a long stream of papers illustrating how political term-limits may distort socially-optimal policymaking by rendering policymakers less accountable towards their constituencies. In fact, in one of the earliest contributions, [Besley and Case \(1995\)](#) find that gubernatorial term-limits have a negative impact on the tax-raising performance of the US governors after natural disasters (i.e., floods, hurricanes, earthquakes, etc.). Again, in the US setting, [Alt, Bueno de Mesquita, and Rose \(2011\)](#) point out that economic growth is lower when the term-limited governors are in charge than otherwise. Employing a Brazilian municipality-level dataset, [Ferraz and Finan \(2011\)](#) provide evidence on the corruption-enhancing role of the term-limits. Furthermore, these authors show that the term-limit effects are particularly strong in places with lower chances of getting caught/punished. More recently, [Klašnja and Titunik \(2017\)](#) show that the use of term-limits may lead to an incumbency curse when the politicians have weak attachments to their parties and their pursuit of private agendas damages the party reputation in the upcoming elections. We contribute to this literature by showing that post-crisis financial stability in a country suffers under term-limited political leaders, which we attribute to the policies pursued by these politicians who are more (less) likely to be motivated by private (public) interests.⁵

2.2. *Crises and structural reforms*

A large literature exists on the nexus between crises and structural reforms, which was given a boost by the seminal paper of [Drazen and Grilli \(1993\)](#). The insight provided by these authors is that in a society composed of groups with conflicting interests, a lack of consensus over what constitutes welfare improving policies becomes likely. Over time this

⁵The literature has only a few cross-country studies similar to ours. An exception is [Conconi, Sahuguet, and Zanardi \(2014\)](#), who show that international conflicts become much more likely when political leaders in democracies face a binding term-limit.

leads to powerful vested interest groups, whose influence could only be weakened by crises and emergencies. This crisis-begets-reforms hypothesis has been tested empirically in a number of contributions (see [Nelson, 1990](#); [Krueger, 1993](#); [Williamson, 1994](#) and others). On the whole this literature has shown how crises trigger reforms that lead to liberalizations and stronger influences of market forces.⁶

Things are not so simple when analysing financial crises, however. These can damage macroeconomic conditions and sometimes result in severe and long-lasting recessions, leading to calls for reforms in financial sectors to avoid or remedy such crises. Thus, a solid relationship between financial crises and financial policy reform is expected. However, the direction of the reforms is less clear. [Abiad and Mody \(2005\)](#) support the view that financial crises alter the balance of decision-making power and may drive policy changes, though not always in the same direction. While balance-of-payment crises are likely to be pro-liberalization, banking crises turn out to act in the opposite way, encouraging reversals in the liberalization process. Analysing currency crises, [Pepinsky \(2012\)](#) shows that developing countries respond by closing their capital accounts as a form of self-help. [Mian, Sufi, and Trebbi \(2014\)](#) argue that financial liberalizations seem to experience a deadlock and tend to reverse in most post-crisis episodes.

Here we believe that an important recent study warrants special attention. [Gokmen, Nannicini, Onorato, and Papageorgiou \(forthcoming\)](#) find that democracies neither open nor close their economy in the aftermath of financial crises. While this result may at first seem at odds with our findings, it is important to underline the fact that, compared to these authors who focus on a broader set of structural reform measures, we focus solely on financial policymaking and exploit a more comprehensive dataset both in terms of country coverage in our final (i.e., matched crises-reforms) sample and of time span including the Global

⁶Among them, there is a literature concerning reforms triggered by high inflation, fiscal stress and growth crises, see pioneering work by [Bruno and Easterly \(1996\)](#), [Lora \(1998\)](#), [Perotti \(1999\)](#), [Drazen and Easterly \(2001\)](#), [Pitlik and Wirth \(2003\)](#) and [Alesina, Ardagna, and Trebbi \(2006\)](#). Independent of the crisis-begets-reforms hypothesis, some recent papers emphasize the role of democracy in bringing market liberalisation ([Grosjean and Senik, 2011](#); [Giuliano, Mishra, and Spilimbergo, 2013](#)).

Financial Crisis of 2007-2009.⁷ Especially the inclusion of these most recent crisis episodes may tilt the results in favor of policy reversals as this additional period (from 2006 to 2015) is characterised by very high levels of financial liberalization around the world which may provide more space for policy reversals than progress.⁸ Furthermore, both our paper and Gokmen et al. (forthcoming) interpret the differences between democracies and autocracies in the same way as the latter (former) being more likely to give way to private (public) interests. We complement these authors by showing that the policy response to crises in democratic countries is not homogenous and depends on the level of accountability between the political leader and their voters.

2.3. *Politics and finance*

Lastly, our work is related to the recently-flourishing literature on the political economy of finance.⁹ In particular, researchers have studied how legislative processes in general could be influenced by corporate and/or constituent interests, mostly focusing on the US setting (Hall and Wayman, 1990; Stratmann, 1998; 2002; Mian, Sufi, and Trebbi, 2013; Igan and Mishra, 2014). In particular, Mian, Sufi, and Trebbi, 2010 examine the congressional voting on two key pieces of legislation in the immediate aftermath of US mortgage crisis and illustrate how tightly policymakers' behaviour is linked to their constituents' preferences as well as to

⁷Not only the contexts and datasets in these papers differ, but also our estimation strategy takes into account the country-specific nature (i.e., speed) of the liberalization process. Additionally -and importantly-, we benefit from two of the most widely-used sources to determine the political regime types (i.e., Database for Political Institutions and Polity5; see Section 4). In a robustness check, instead of using a dichotomous measure, we also employ multiple subsamples and confirm that the post-crisis policy interventions become larger at higher levels of democracy.

⁸See Figure 1 for a visual inspection of this recent period.

⁹See early (Pagano and Volpin, 2001) and recent (Lambert and Volpin, 2018) reviews. The literature has unfolded itself in various ways including the interactions between median voter preferences and historical financial development (Perotti and Von Thadden, 2006; Benmelech and Moskowitz, 2010; Degryse, Lambert, and Schwenbacher, 2018), between law and finance (Porta, Lopez-de Silanes, Shleifer, and Vishny, 1998; Beck, Demirgüç-Kunt, and Levine, 2003), between labour rights and corporate governance (Pagano and Volpin, 2005a; 2005b; Dessaint, Golubov, and Volpin, 2017), between private interest groups and financial deregulation (Kroszner and Strahan, 1999; Rajan and Zingales, 2003; Chari and Gupta, 2008), between political connections and corporate outcomes (Fisman, 2001; Faccio, 2006; Akey, 2015; Child, Massoud, Schabus, and Zhou, forthcoming), between electoral incentives and credit misallocation (Sapienza, 2004; Dinc, 2005; Englmaier and Stowasser, 2017; Bircan and Saka, forthcoming).

the pressure from special interest groups in the form of campaign contributors. Compared to these studies, ours constitutes the first attempt to simultaneously trace the private and public interest channels of post-crisis policymaking in a cross-country setting.

3. A simple model of post-crisis policymaking

In this section, we present a simple model based on the original work by [Besley \(2006\)](#). This model allows us to interpret our empirical findings concerning post-crisis financial policymaking in a consistent framework. In this model, we assume voters have perfect information on how a particular financial policy affects public interest, but democracy may still suffer from the ‘principal-agent problem’.¹⁰ An elected politician (the ‘agent’) is able to make decisions on behalf of the voters (the ‘principal’) but there is an incentive problem that the politician might be motivated to act in her own private interest rather than in the best interest of the voters. This problem arises when voters do not have perfect information on the type of the politician.

Our model reveals how democratic elections and term limits may shape post-crisis policy choice with respect to the clashing interests between public and private interests. Admittedly, other political and institutional factors such as the role of parliament, parties and etc. may also play a role; these are not the focus of our theoretical discussion but we will control some of these factors in our empirical analysis.

The model assumes that there are two types of politicians. A ‘good’ politician makes financial policy decision based on public demand and a ‘bad’ or ‘rent-seeking’ one pursues her own private interest. Clearly voters have a preference for the good politician, however, the

¹⁰We recognize that there are other term-limit models with different informational assumptions than ours. Some models assume that voters do not know their true interests and hence politicians may manipulate this by pandering to voters ([Canes-Wrone et al., 2001](#)). Such behaviour is not theoretically possible in our setting since we assume that voters realise their gains/losses from previous policies before they can vote for the upcoming election and hence they obtain perfect information regarding which policies are in their best interest. While ‘pandering’ is a practical reality in politics, it is not unreasonable to assume that it constitutes the exception rather than the norm; that is, voters usually have a good idea of which policies are in their best interest.

true type is not observable to voters before politicians are elected. Let π be the probability that a randomly picked politician from the pool is ‘good’ who pursues the public interest and $(1 - \pi)$ is ‘bad’ who pursues her private interest.

In a democracy with a binding term limit, the politician has no concern for being re-elected. The policy choice taken by the politician depends on her type. For the good politician, the binding term limit does not change her internal motivation to act on public demand. For example, when the cause of the crisis is related to financial liberalization and the public sentiment turns against the financial industry, the good politician will support to introduce more “beneficial” regulations (such as capital controls) that may help bring back the financial stability. By contrast, the bad politician cares about her own private interest. This private interest may be linked to serving the banking industry by removing certain “beneficial” regulations (such as bank supervision) but also can take the form of introducing “harmful” regulations (such as bank entry barriers) that may benefit the incumbent players of the industry at the expense of the society.

In a democracy without a binding term limit, the principal-agent problem may be mitigated. To illustrate this, we introduce a two-stage model allowing the politician to have the chance of being re-elected. There are two time periods, $t \in \{1, 2\}$. The politician does not face a binding term limit in her first term, but if elected again, she faces a term limit in her second term. In each period, an elected politician makes a single policy decision, denoted by $e_t \in \{0, 1\}$. The payoff to voters and politicians depend on whether or not the political decision corresponds to the state of the world $s_t \in \{0, 1\}$ which is observed by the incumbent. In the context of our study, the state of the world can be interpreted as a particular policy stance (e.g. whether or not to introduce more regulation in the financial industry) gaining wide public support. For simplicity, each state is assumed to occur with equal probability. Voters receive a payoff Δ if $e_t = s_t$ and zero otherwise.

An elected politician gets a direct payoff E from holding office. This payoff can be considered as pure “ego rents” plus wages and any other material benefits (such as pensions

and free housing) from holding office. The action of a politician at time t is denoted by $e_t(s_t, i)$. In each period, the payoff to a good politician is $E + \Delta$ if $e_t = s_t$; and E if $e_t \neq s_t$. In the former case, the good politician shares the same objective as the voters and hence gets a payoff positively associated with voters' payoff. The bad politician does not share the same objective as the voters but she gets a private benefit r_t when deviating from the public interest, i.e. $e_t \neq s_t$. This private benefit can be considered as a reward of giving special treatment to some interest groups. Assume that this private benefit (r_t) follows a distribution whose cumulative distribution function is $G(\cdot)$, with mean μ and finite rent $[0, R]$. Let $\beta < 1$ be the discount factor in period 2.

We provide the solution and equilibrium of the model in **Appendix A**. This model produces an interesting result on how the prospect of being re-elected in a democracy can change the incentives and action choice of bad politicians. Instead of pursuing her pure short-term private interest, the bad politician also considers the expected future private benefit if she acts on public interests which ensures her to be re-elected. We identify that as long as the present value of the expected future private benefit (i.e. $\beta(\mu + E)$) of being re-elected is higher than her short-term (period 1) private benefit (r_1), the bad politician is willing to act on public interest. The probability of doing so, z , can be described as follows. We find that z increases when the expected future private benefit is higher.

$$z = Pr(r_1 < \beta(E + \mu)) = G(\beta(E + \mu))$$

Our model produces two implications that will guide us in interpreting our empirical findings later. First, democratic elections without a binding term-limit can motivate a bad politician to make policy choices that satisfy voters' interest. There is a pooling equilibrium in which good and bad politicians act on public interest. The probability of a politician pursuing public interest is $\pi + (1 - \pi)z$. In the case of a binding term-limit, as incumbents cannot be re-elected, a bad politician is only interested in seeking private interest instead of public interest. This generates a separating equilibrium in which the probability of a public-oriented policy is only determined by the probability of a good politician, π . It is

clear that *under democracy a binding term limit has its negative impact*: the probability of politicians pursuing public interest decreases from $\pi + (1 - \pi)z$ to π .

Second, the Besley (2006) model can be redeployed to analyze the drawback of an autocratic regime. Under autocracy, the public has little power in deciding which politician to stay in power. The behaviour of the good politician does not change under autocracy: she does what the public demands. However, the bad politician does not need to sacrifice her private benefit for public interest. This generates a similar separating equilibrium as in a democracy with binding terms: only the type of politician matters. Hence, the probability of a politician pursuing public interest under autocracy is π .

This allows us to compare this probability (π) to the one under democracy. Assume that $0 < \gamma < 1$ is the fraction of politicians who have a binding term limit (in a separating equilibrium) and $(1 - \gamma)$ is the fraction of politicians who do not have such a limit (in a pooling equilibrium). Hence the weighted aggregate probability of a publicly-oriented policy is $(\gamma * \pi) + ((1 - \gamma) * (\pi + (1 - \pi)z))$, which is higher than π . Thus, *policymaking under democracy is more likely to serve public interest than under autocracy*.

4. Data

The standard dataset on various areas of financial reform in the cross-country setting has been the one constructed by Abiad, Detragiache and Tressel (2010; henceforth, ADT).¹¹ ADT assesses seven dimensions of financial policy in 91 countries over the years from 1973 to 2005. Specifically, it includes five indices directly related to the domestic banking sector (credit controls, interest rate controls, entry barriers, privatization, and supervision), one index on restrictions in international capital movements and one on asset markets (security market regulation). Each of these variables is constructed through a set of standardized questions for which responses can be coded discretely and then aggregated to represent the

¹¹These authors in turn build on the earlier and smaller set of observations compiled by Abiad and Mody (2005). Some of the recent studies employing this dataset include Mendoza, Quadrini, and Rios-Rull (2009), Prati, Onorato, and Papageorgiou (2013) and Giuliano et al. (2013).

extent of liberalization in each reform area. They take values between 0-1, with higher values implying more liberalization.¹²

One major setback in the empirical research after the Global Financial Crisis has been the fact that these indices have not been updated by the original authors, preventing researchers from analyzing the financial reform dynamics since 2005. Fortunately, [Denk and Gomes \(2017\)](#) have recently attempted to fill in this gap by extending the original ADT until 2015 while also covering a few additional countries (henceforth, DG). These authors follow the same methodological approach for the years from 2005 to 2015 and keep the original coding rules when aggregating responses to individual questions.¹³ Their data also stretch five more years back in time to 2000 where the original ADT series already exist and they confirm that their scores are comparable to the ones obtained in the original dataset.¹⁴

As a result, DG is composed of seven financial reform indices for the years from 1973 to 2015 for 43 countries. 38 of these already existed in the original ADT and five new countries were added by DG.¹⁵ For our analysis, we first take the full panel created by DG and then merge it with the remaining (51) country-time-series from ADT. Hence, we obtain an unbalanced panel consisting of 94 countries over the period from 1973 to 2015. To our knowledge, this is the first study analyzing this combined dataset of policies across seven distinct financial domains.

¹²Except in the area of banking supervision where an increase implies more government intervention, and thus less liberalization. For this reason, we use the banking supervision index in the reversed form (1-x) in our estimations to make sure that our sign interpretations are consistent across different indices. For the details on the specific questions used for each policy index in [Abiad et al. \(2010\)](#), see Table [A1](#).

¹³One exception they make is to change the index on capital account restrictions where, instead of posing the original questions in ADT, they directly input the index built by [Chinn and Ito \(2006\)](#). This is probably the most widely used measure of capital account openness in the literature. As [Denk and Gomes \(2017\)](#) puts it, Chinn-Ito index is highly correlated with the original index in ADT (up to 2005) and other commonly used capital account indices in the literature. Compared to the original methodology of [Abiad et al. \(2010\)](#), DG also drops one question in the credit controls section, which is not a material change given that half of the observations for this question in the original ADT were missing in the first place. Next section ([Method and identification strategy](#)) describes how we control for the possible biases that may arise due to these differences between the two datasets.

¹⁴For the few cases in which there is little divergence, they keep their own scores for consistency. For the details on the specific questions used for each policy index in [Denk and Gomes \(2017\)](#), see Table [A2](#).

¹⁵These new countries -namely, Iceland, Luxembourg, Saudi Arabia, Slovakia and Slovenia- have observations only for the years from 2000 to 2015.

Table 1 presents the summary statistics for each of these domains as well as the overall financial reform variable, which is the simple average of the former.¹⁶ We observe that, within our sample period, there has been at least one country that was not liberalized at all (0) or fully liberalized (1) at some point for each reform area. This is a reassurance that the policy questions composing our measures of liberalization do not specify unachievable targets. However, for the average financial reform, these extreme points have never been reached by any country, implying that there is no country in our sample that receives all 0s or 1s simultaneously at each dimension. On average, liberalization seems to have been highest in banking supervision, followed by entry barriers and interest rate controls. Privatization turns out to be the least liberalized area on average with significant state presence in domestic banking sectors.

For the dating of the financial crises, we resort to the classic dataset from the IMF (Laeven and Valencia, 2013) which has recently been updated by the original authors (2018). This new dataset includes the starting dates for three different types of financial crises, namely banking, currency and sovereign debt crises. Coverage is quite large compared to alternative datasets (such as Reinhart and Rogoff, 2011), containing 165 countries between the years 1970 and 2017.¹⁷

In Table 1, all types of crises are represented by a dummy variable taking the value of 1 in the initial year of the crisis and 0 for the rest. Hence, we are unable to trace the length (duration) of a crisis within the IMF dataset; but -as explained below- we will make use of this dataset to construct an event study setting by comparing the period immediately before and after the initial year of a crisis. After merging financial crises with the reform database previously constructed by joining two separate datasets (ADT & DG), we end up with 105 banking, 121 currency and 38 sovereign debt crises in the full sample.

Lastly, for the political variables, we resort to the Database of Political Institutions

¹⁶Table is constructed only with the observations that remain in the analysis after merging the reform database with financial crises. Less than 2% of the full reform dataset is dropped after the merging process.

¹⁷We also undertake robustness checks later by employing the crisis episodes in a smaller set of countries documented by Reinhart and Rogoff (2011).

(DPI) which was originally created by Beck, Clarke, Groff, Keefer, and Walsh (2001) and later updated by Cruz, Keefer, and Scartascini (2016). The following variables are extracted and merged with the earlier part of our dataset: *TermLimit*, which takes the value of 1 if the country’s executive leader has a binding term limit at a certain time point and 0 if not;¹⁸ *Right* and *Left* are simply dummies for the leader’s ideological position (with *Center* as benchmark); *Presidential* and *Parliamentary* are indicator variables for the country’s system of governance (with *Assembly-elected President* as benchmark); *OfficeYears* count the number of years the leader has been in office; *YearsLeft* are the number of years left in the leader’s current term; *HerfGov* is the Herfindahl index -sum of the squared seat shares of all parties in the government; *GovFrac* is the probability that two deputies picked at random from among the government parties will be of different parties; *GovShare* is the fraction of seats held by the government; and finally *Checks* represents the number of distinct bodies that can act as a veto player in the country’s democratic process. Summary statistics for these variables are all reported in Table 1.

Figure 2 shows the time-trends for term-limits and democracy within our full (unbalanced) sample in Panel A as well as for a more balanced subsample in which we only keep those countries that have more than 30 years of observation (Panel B). There are two broad trends: first, there is a tendency for countries to become more democratic over time; and second, executive term-limits are more prevalent till the late 1980s after which they seem to have declined in importance. For robustness, we also compare our main (DPI) democracy variable to the one constructed via the Polity5 dataset and confirm their similarity despite the latter having a higher threshold to categorise a country as a democracy.¹⁹ The discrete jump visible in all time-trends around 2005 is due to the fact that the policy dataset from

¹⁸To avoid any confusion, remember that the *TermLimit* dummy is time-varying and thus not the same as the country-fixed effect. For instance, in a country with a two-term system the politician can be re-elected when in the first term, but not in the second term.

¹⁹The Polity5 is a graded index composed of 21 levels ranging from autocracy (-10) to democracy (+10) commonly used in the literature (see Goldstone, Bates, Epstein, Gurr, Lustik, Marshall, Ulfelder, and Woodward, 2010). We conventionally label a country-year observation as a democracy if the index value is 5 or higher.

Denk and Gomes (2017) which we employ for the years 2006-2015 covers only a fraction of the original set of countries included in Abiad et al. (2010).

In a similar fashion, we plot the average values over time for each financial policy domain in Figure 1. All of these series show an inclination towards less government intervention over time, except the area of banking supervision where the regulations have become more restrictive. Since early 2000s, financial liberalization seems to have come to a halt; and after the Global Financial Crisis in 2007-08, some of these areas (such as privatization) have faced an interventionary stance from the policymakers.

5. Method and identification strategy

5.1. *Baseline methodology*

We are first interested in the impact of financial crises on the process of financial liberalization, which is not an easy task to accomplish given the possible reverse causality in this kind of a relationship. It has long been suspected that liberalization processes themselves may lead to economic/financial crises, with many anecdotal examples especially from Latin American countries (Green, 1997). Another empirical problem is that countries experiencing crises may have a different reform pace (too fast or too slow) or they may be at a different stage of their liberalization process when they get hit by a financial crisis. If that is the case, one might accidentally capture the country-specific nature of the liberalisation process rather than the effect of the crisis itself.

Despite these empirical concerns, very few papers explicitly tackle the identification issue in a cross-country setting.²⁰ We attempt to solve this problem in three steps. First, we do not only estimate what happens to the reform process after a crisis; but we also explicitly check if the countries had any diverging reform trends before the crises struck so as to make sure

²⁰Two exceptions are Pepinsky (2012), who uses an instrumental-variables approach to analyse the impact of currency crises on capital account liberalisation, and Mian et al. (2014), who use a panel diff-in-diff setting similar to ours comparing the level of reforms before and after crises.

that any pre-crisis trends are controlled for. Hence, we obtain a quasi-diff-in-diff estimate by directly comparing the country’s liberalization levels just before and after a financial crisis.

Second, we non-parametrically control for the pace of the liberalization process specific to each country by including country-specific time trends in our estimations. This is crucial as the cross-sectional comparison of the crisis experiences between countries with different reform speeds may lead to a bias in our estimates, especially if crises are not randomly distributed across varying levels of liberalization.

Third, we benefit from the high dimensionality of our dataset (with multiple reform domains) and include a full set of fixed effects with interactions across dimensions in order to control for potentially omitted variables. In particular, the interacted fixed-effects between reform domains and countries/years will help to absorb any implicit bias that may exist due to the combination of two datasets produced by different researchers (see the [Data](#) section).

Specifically, we estimate the following equation:

$$\begin{aligned} FinancialLiberalisation_{i,t,r} = & \beta_1 \times POSTcrisis_{i,t} + \beta_0 \times PREcrisis_{i,t} \\ & + \sum_i \delta_i \times d_t + \mu_i + \alpha_t + \lambda_r + \varepsilon_{i,t,r} \quad (1) \end{aligned}$$

where i represents country, t year and r specific reform index. δ_i is a dummy for each country and d_t is a linear time trend. In the baseline estimation, we include the basic set of fixed effects at the country (μ_i), year (α_t) and reform (λ_r) levels and saturate the specification in subsequent estimations. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding the same financial crisis.²¹ Therefore, our diff-in-diff estimate (average treatment effect of a crisis) has an event-study nature and is given by the test of the following difference:

²¹Importantly, we abstain from controlling for any country-level economic or financial variables in our estimations as these variables themselves might be strongly influenced by financial crises and could thus be categorised as “bad controls” (see [Angrist and Pischke, 2008](#), p. 64).

$$ATE = \beta_1 - \beta_0$$

5.2. *Identification of policy drivers*

Next, our focus shifts to the public and private drivers of this average treatment effect. In order to have an understanding of what role public interests may play in policy reversals, we re-estimate the Equation 1 separately in democratic and non-democratic country subsamples, expecting that public preferences are more likely to be reflected in the policy outcomes of the former, in line with our discussion in Section 3. This does not necessarily mean that a policymaker would not have private interests in a democratic setting though. Such interests (either through revolving doors or simply cronyism) may indeed be substantial and more visible due to the more transparent nature of democracies. However, our assumption is that such incentives are much more likely to be balanced by public interests due to the competitive nature of elections and the resulting political accountability in democratic countries. Hence, the public interest channel will be given by the following comparison estimated via Equation 1:

$$ATE_{Democracy} - ATE_{Autocracy} = (\beta_{1, Democracy} - \beta_{0, Democracy}) - (\beta_{1, Autocracy} - \beta_{0, Autocracy})$$

To track down the private interests, we use a plausibly exogenous political shock that would mute the public interest channel and thus make private interests more visible, again in line with our discussion in Section 3. Specifically, we are interested in the differential post-crisis behaviour of political leaders when they have a binding term limit in the immediate next election and when they do not. Here, our assumption is that the leaders' post-crisis policymaking is determined in an equilibrium where their sensitivity to public preferences/interests is balanced by their inclination to engage in activities that will serve them privately; but will not necessarily be optimal from the perspective of their voters. Hence, policymakers would have similar private incentives in both periods; however since there is a shock to their political accountability when they cannot run for the next election,

their private interests would begin to dominate. In that case, any difference detected in financial policymaking between the two periods would be considered as a reflection of the politicians' private interests.²² Thus we formally go on to estimate the following model:

$$FL_{i,t,r} = \beta_3 \times POSTcrisis_{i,t} \times TLimit_{i,t} + \beta_2 \times PREcrisis_{i,t} \times TLimit_{i,t} + \eta \times TLimit_{i,t} + \beta_1 \times POSTcrisis_{i,t} + \beta_0 \times PREcrisis_{i,t} + \sum_i \delta_i \times d_t + \mu_i + \alpha_t + \lambda_r + \varepsilon_{i,t,r} \quad (2)$$

where, in addition to the baseline setting in Equation 1, we interact our pre- and post-crisis event dummies with the *TLimit* variable, which is a dummy representing those country-year observations where the political leader cannot run for the next election due to a binding term-limit. We also include the variable itself without the interaction in order to see if the dynamics of financial liberalization are generally different for this type of policymakers. Here the baseline effect of crises on financial policymaking when there is no binding term limit is captured by the following:

$$ATE_{NoLimit} = \beta_1 - \beta_0$$

whereas the behaviour of the political leaders when they cannot run for the next election (and thus less sensitive to public demand) is measured by:

$$ATE_{TermLimit} = (\beta_3 - \beta_2) + (\beta_1 - \beta_0)$$

while, in order to capture the *private interest channel*, we test the differential behaviour between these two cases:

$$ATE_{TermLimit} - ATE_{NoLimit} = \beta_3 - \beta_2$$

²²We are confident that our identification strategy is likely to hold given the overwhelming evidence about the adverse impact of term-limits on political accountability. See, among many others, Besley and Case (1995); Alt et al. (2011); Ferraz and Finan (2011); Conconi, Sahuguet, and Zanardi, 2014; Klačnja and Titiumik (2017).

5.3. Potential threats to identification

Notice that, when using *TLimit* as a treatment variable, we are focusing only on the democratic type of countries that may or may not have term-limits written in their constitution. We think of the country being a democracy as a precondition for its term-limits to be credible and thus to shape the incentives of the executive politician. This assumption of credibility is also embedded within our theoretical framework in Section 3 where we assume that a re-elected politician *believes* that she cannot run again for the next election.²³

Some of the democratic countries however do not impose any explicit term-limits on their executives during our sample period and thus always act as a counterfactual.²⁴ One natural concern could be that the countries that employ explicit term-limits may be politically different from those that do not. If that was the case though, one would also expect a differential behaviour in such countries in the baseline situation which we will measure by estimating the stand-alone effect of *TLimit* captured by the η coefficient in Equation 2. Indeed, apart from the post-crisis interval, we find no evidence that the term-limited politicians behave differently in general or specifically during the time periods just preceding a financial crisis (estimated via η and β_2 in Equation 2).

Nevertheless, there may still be the possibility that an omitted political variable is driving the differential crisis response that the term-limited politicians seem to produce. For example, it is conceivable that presidential democracies might be more effective in reacting to a crisis and thus behave differently in terms of post-crisis policymaking per se. Such democracies are also more likely to impose a term-limit on their presidents. Hence, our estimates of β_3 in Equation 2 might be confounded in the absence of relevant political controls that should also be interacted with the *POSTcrisis* variable. In order to tackle such concerns

²³In line with this intuition, the dataset we employ for term-limits (i.e., DPI) mostly has missing values for such country-year observations that fall within the domain of non-democracies. The inclusion of the few additional non-missing (but non-democratic) observations in our estimations does not qualitatively change our main findings.

²⁴For instance, most parliamentary democracies in which the executive leader is the prime minister do not have term-limits coded in their constitutions.

more directly, we control for all relevant aspects of political heterogeneity (including presidential nature of the democracy) both in the baseline estimation and in interaction with the crisis variables.

To further raise the confidence in our identification strategy, we also focus on a smaller subset of countries comprised of only those with at least some term-limit experience during our sample period and confirm that our main findings still remain intact. Hence, we conclude that it is unlikely that the structural differences between democracies with and without term-limit rules are driving our results.

Another challenge in identification is the possibility that the term-limited politicians are in general more experienced than their counterparts since they are likely to have already survived a re-election in the past, except in countries with a single-term limit (Ferraz and Finan, 2011). In order to adjust our estimations for such potential bias of an omitted variable, we control for the number of years that the executive has been in the office, both in the baseline and also in an interaction with our pre- and post-crisis dummies, which -if anything- strengthens our main findings.

A further criticism could be directed to our setting due to the fact that elections are likely to bring more capable leaders to a country's political scene. Since the leaders facing term-limits -in most cases- must have survived a previous re-election in the past, that may constitute a proof that these leaders are of higher quality compared to their counterparts in the country's pool of politicians. Hence, the difference between the policy reactions of lame-duck politicians and others could be related to the former being potentially more skilful in handling the crisis than the latter. In order to check for this, we control for the vote share of the government party, both in the baseline and also in an interaction with our pre- and post-crisis dummies. Assuming that the public support is a good proxy for leaders' capability/skills, we confirm that our findings remain intact in a robustness check mitigating the above concern for a potentially omitted variable.²⁵

²⁵Since the underlying concern is that more capable leaders might be the ones who end up winning the re-election races (and thus becoming term-limited), leaders' capability and election performance should be

6. Results

6.1. Do governments intervene in financial markets after crises?

6.1.1. Baseline results

Results from the estimation of Equation 1 on the full-sample of observations are reported in Table 2. The first column shows the baseline model with a set of fixed effects at country, year and reform levels. Our concern for the existence of diverging reform trends between crisis and non-crisis countries prior to a financial crisis is confirmed here. However, contrary to the argument that crises themselves may be caused by the liberalization process, the *PREcrisis* variable produces a significantly negative coefficient. Hence, the usual reverse causality concern in the literature (i.e, liberal reforms causing crises), which would predict a positive coefficient for *PREcrisis*, does not show up here and the difference between two coefficients before and after a financial crisis (*PREcrisis* vs. *POSTcrisis*) is estimated as approximately -0.02 at 8% significance level. It seems that governments start de-liberalizing their financial systems much earlier than the initial date of a crisis, the speed of which only accelerates once the crisis hits. It is also possible that crises may show their first signs in advance of the official starting dates reported in [Laeven and Valencia \(2018\)](#), which is a reasonable assumption given that crisis dummies generated in this dataset depend on an arbitrary threshold defined by the intensity of the financial problems in the country. In that case, we are possibly underestimating the true negative effect of a crisis since our pre-treatment periods may have been confounded by the existence of a (potentially smaller-sized) treatment effect.

On the other hand, these pre-trends may still constitute a concern in terms of identification since it is possible that crises only strike countries when they have low levels of

correlated by construction. To the extent that our control variable (government vote share) does not proxy leaders' skills, the identification concern itself becomes irrelevant. In any case, we will later directly test for the crisis management skills of these term-limited leaders showing strong evidence that they perform much worse in terms of restoring post-crisis financial stability.

liberalization or the countries that are too slow (or fast) reformers might experience financial crises with different probabilities. In order to check whether the pace of reforms (or any unobserved country-level factor with a trend) could explain this pattern, we turn to the second column where we add country-specific linear time trends into the baseline specification. It turns out that the previously negative coefficient of the pre-crisis dummy becomes insignificant after this addition, confirming our earlier concern that crises may be hitting the countries with a particular reform speed or level. The diff-in-diff coefficient is even stronger with an estimate lower than -0.03 at 0.1% significance level. Although the magnitude of this average treatment effect is quite modest compared to the average financial liberalisation in the sample (which is 0.58; see Table 1), this constitutes our first evidence showing that policymakers react to financial crises by increasing government intervention in financial markets.²⁶

One more concern for our empirical strategy is the possibility of breaks in the data and how these may bias the estimates in one way or another, especially if the different authors preparing the two datasets had in mind different criteria when judging the countries' liberalization levels in the more subjective parts of the questionnaire. It is hard to imagine a test to check for such differential biases between the two datasets; however what we can do is that, assuming such biases would apply to all countries in the sample, we could add fixed-effects at the interaction of reform types and years. This assures that any systematic bias in any index in any year (conditional on it being applied against or towards all countries for that reform-year pair) is taken into account. The third column in Table 2 reports the results with these fixed-effects and there does not seem to be any material change compared to the previous column, confirming that the combination of indices from two different sources has minimal impact on our estimates.

The fourth and fifth columns in Table 2 add interacted fixed effects at the country and

²⁶Bear in mind that this effect size is the average across all seven dimensions of financial policymaking. We will come back to the discussion of the economic magnitude when we can compare our estimates to the estimated effects of political factors, such as government ideology, in the next sections.

reform levels, meaning that any systematic component of liberalization that may have been missed or not captured constantly over time for a specific country and reform area would be subsumed by these dummies. The results again confirm that such potential mismeasurement issues do not seem to be important in our sample. Overall, we have sufficient evidence to conclude that the average effect of a crisis on financial liberalization is significantly negative.²⁷

6.1.2. *Robustness checks*

For the panel analysis, we have undertaken various robustness checks in the following ways: (1) when defining the financial crises (POSTcrisis & PREcrisis), dummies are turned off for the start-dates and the years immediately before and after the start-dates in order to make sure that we do not pick up any temporary policy response to the crisis (see Appendix Tables B2 and B3); (2) in addition to the previous exclusion, we also exclude the years that fall within both PREcrisis and POSTcrisis periods (see Tables B4 and B5); (3) as an alternative to the list of financial crises in Laeven and Valencia (2018), we re-perform the analysis with the Reinhart and Rogoff (2011) dataset, which has a smaller country coverage (see Table B6 as well as Tables B7a and B7b); (4) we repeat the analysis only with the original financial reform dataset (from Abiad et al., 2010), which ends in 2005 and covers 91 countries (see Tables B8 and B9); (5) we simultaneously include different types of crises in the same estimation in order to mitigate the possibility of one type of crisis driving our results (see Table B10). Our main findings remain intact in all of these alternative tests.

²⁷An important additional investigation can be pursued by separating this average effect for different types of crises. Table B1 re-estimates Equation 1 with separate dummies for banking, sovereign debt and currency crises in the full-sample. Again, our conclusions for different models are very similar to the ones discussed above. Diff-in-diff estimates turn out to be significantly negative for 14 out of 15 estimations, with the exception of the baseline model (column I) for banking crises exhibiting diverging trends between crisis and non-crisis countries prior to the crisis events. In terms of economic magnitude, the largest effect comes from sovereign debt crises (0.064), followed by currency (0.036) and banking crises (0.021).

6.1.3. Timeline of policy interventions

We have so far aggregated the pre- and post-crisis years in Equation 1 in order to create a setting where we could compute the change in policy stance by comparing the periods just before and after a financial crisis and estimating the difference between two corresponding dummy variables. Despite providing us with a good sense for the direction of the effect, this strategy does not tell us much about its timing. Hence, we further resort to the following equation in order to zoom into the 10-year period surrounding a crisis and to trace the timing of the change in financial policies. Consider:

$$FinancialLiberalisation_{i,t,r} = \beta_{\tau} \times Crisis_{i,t+\tau} + \sum_i \delta_i \times d_t + \mu_i + \alpha_t + \lambda_r + \varepsilon_{i,t,r} \quad (3)$$

where, instead of defining two separate crisis dummies, we construct a single variable representing the initial year of the crisis (i.e., $Crisis_{i,t+\tau}$). We employ a rolling definition of this variable for which τ corresponds to the years before and after a crisis. For instance, $Crisis_{i,t-2}$ equals 1 for two years prior to a crisis, and 0 otherwise.

In Figure 3, we re-estimate the Equation 3 for different values of τ ranging from -5 to $+5$ and plot the corresponding coefficient estimates for β_{τ} . In the years preceding a financial crisis, there is very little divergence between countries that are about to be struck by a crisis and those who are not. This visually satisfies the requirement of parallel trends for our diff-in-diff setting to have a causal interpretation. More importantly, policy change occurs exactly in the initial year of a crisis and does not seem to reverse in the next 5 years. These observations confirm our earlier findings in Table 2 and further assures us that the policy change detected via Equation 1 synchronises almost perfectly with the crisis shock.²⁸

²⁸In Figures B1, B2 and B3, we separately estimate the effects by using different types of financial crises. Our results are similar and in line with our findings in Table B1. In Figure B4, we separately estimate the effects on different domains of financial policymaking; again confirming that post-crisis interventions are visible in all domains with the slight exception of bank supervision.

6.2. *Public interests: Democracy vs. autocracy*

6.2.1. *Baseline results*

Next, we turn our attention to investigating which types of political settings drive our results. If they are driven by autocratic systems, it is possible that the state interventions detected in the previous section could be serving the special interest groups who demand policy-related bribes from the autocrat to remedy the potential losses that they may have incurred during the turmoil following the crisis (Gokmen et al., forthcoming). However, if democracies drive our results, we could interpret this more in line with a view where policy reversals may be at least partially in line with the general public interests (i.e., preferences).

Admittedly our identification is rather weak here and builds on the grand assumption that we can compare democracies to autocracies while holding all else fixed in our setting. Notice that this does not necessarily mean that there would not be special-interest groups or lobbying in democracies. Indeed there would be and it is likely that these would be even more visible compared to those in autocracies where negotiation and outcome of such private interests would be less transparent to the public. However, our interpretation implies that, all else being equal, the public would have a stronger position in democracies to demand and obtain the financial policies that they truly prefer.

DPI defines a country as a democracy if its executive index of electoral competitiveness has a value equal to or higher than six (Cruz et al., 2016). Using the same definition, Table 3 reports estimations of Equation 1 on two separate subsamples. As can be observed in the estimated diff-in-diff coefficients, our previous findings are only valid for the subsample of democratic countries which -in line with our discussion in Section 3- implies that a public demand channel might partly be responsible for the state interventionism observed after financial crises. Once autocracies are excluded from the sample, the estimated (negative) effect size increases by more than one third.²⁹

²⁹The difference between the estimates across two subsamples is statistically significant at conventional levels.

Findings in this section align well with those of [Chwieroth and Walter \(2019\)](#) who argue that the middle-class citizens in democracies demand state interventionism in the aftermath of financial crises in order for their wealth to be saved and with those of [Dagher \(2018\)](#) who proposes that policymakers react to regulate the system as a response to the declining public trust. Our interpretation is thus consistent with both of these publicly-driven mechanisms which are more likely to be overlapping than mutually exclusive.

6.2.2. Robustness checks

In **Appendix C**, we provide the descriptions of the following robustness checks: making sure that each country falls only within a single subsample by categorizing democracies/autocracies based on the country averages across our sample period (Table [C1](#)); using an alternative (but more conservative) democracy index (Tables [C2](#) and [C3](#)); checking multiple subsamples with varying degrees of democratic experience (Table [C4](#)). Our main finding that democracies exhibit a larger tendency to intervene in financial markets after crises remains unchallenged.

6.3. Private interests: term-limits as a natural experiment

6.3.1. Baseline results

As previously discussed in length, we exploit the term-limit restrictions that exist in a country’s constitution in order to generate a plausibly exogenous setting in which the policymakers’ political accountability is substantially reduced (i.e., they act as lame-ducks). Given the extensive literature supporting our identifying assumption, we go on to estimate Equation [2](#) only in the subsample of the democratic countries identified in the previous section.

Table [4](#) shows that policy reversals are substantially larger after financial crises when the executive leader of the country has a binding term limit on their re-election chances. The upper diff-in-diff row here specifies the estimated difference between β_3 and β_2 and

the lower one is for the estimated difference between β_1 and β_0 in Equation 2. That is, the post-crisis behaviour of the democratic but lame-duck policymakers accumulates to the sum of these two diff-in-diff estimates whereas the behaviour of the democratic leaders who are not bounded by a term-limit is approximated only by the latter. Table 4 illustrates that the de-liberalizations undertaken by the term-limited policymakers are approximately *four times larger* compared to those undertaken by their unlimited counterparts.³⁰ In line with our theoretical model of policymaking, we infer that such differential behaviour by the term-limited politicians is likely to be due to their private interests.³¹

Mitigating the possibility that countries with term-limits are structurally different from others in their financial policies, the estimated coefficient on *TermLimit* is not statistically significant. Again, the coefficient on the interaction of *TermLimit* with the pre-crisis period is also small and insignificant. Hence, when it comes to financial policymaking, there is no evidence that the term-limited policymakers in general behave differently compared to unlimited ones. However, their differential behaviour occurs exactly *after* the financial crises and not before, confirming that the effect is specific to post-crisis episodes and cannot be explained by the general cross-sectional differences between those countries that impose term-limits on their leaders and those who do not.³²

6.3.2. Robustness checks

In **Appendix D**, we provide the descriptions of various robustness checks that confirm our findings in Table 4. To be specific, we show that controlling for various aspects of political heterogeneity in our estimations both in the baseline (Tables D2 and D3) and in interaction

³⁰These findings are qualitatively unchanged when we reconstruct the democratic subsample by using the country averages in electoral competitiveness to create a balanced sample (see Table D1).

³¹In later sections we will provide supporting evidence for this view by looking at how revolving doors may shape such interventions and what the consequences would be for bank-level financial stability (i.e., a key proxy of public interest).

³²The existence of a relationship between term-limits and financial policies conditional on crises is consistent with the argument that the privately-motivated policymakers may find it optimal to hide their acts behind the anti-finance sentiment in public that may be particularly prevalent after a financial crisis (see Knell and Stix, 2015). This is similar in spirit to the finding of Ferraz and Finan (2011) who show that term-limits increase corruption especially when politicians are less likely to be caught.

with crisis variables (Tables D4 and D5); formally testing the importance of potentially omitted variables in our estimations (Table D6); including fixed effects for the executive leader’s political party to control for the ideological intensity (Tables D7 and D8); including interacted fixed effects between parties and decades to control for the time-varying aspects of ideology (Tables D9 and D10) and restricting our sample only to those countries that have some term-limit experience during our sample period (Table D11) do not qualitatively change any of our previous conclusions.

6.3.3. *Timeline of policy interventions: term-limited vs unlimited policymakers*

Similar to the analysis in Section 6.1.3, we adjust our specification in the following way in order to zoom into the 10-year period surrounding a crisis and to trace the timing of the change in financial policies. Consider:

$$FL_{i,t,r} = \beta_\tau \times Crisis_{i,t+\tau} \times TLimit_{i,t} + \eta \times TLimit_{i,t} + \gamma_\tau \times Crisis_{i,t+\tau} + \sum_i \delta_i \times d_t + \mu_i + \alpha_t + \lambda_r + \varepsilon_{i,t,r} \quad (4)$$

where, instead of defining two separate crisis dummies, we construct a single variable representing the initial year of the crisis (i.e., $Crisis_{i,t+\tau}$). We employ a rolling definition of this variable for which τ corresponds to the years before and after a crisis.

In Figure 4, we re-estimate the Equation 4 for different values of τ ranging from -5 to $+5$ and plot the corresponding coefficient estimates for β_τ as well as γ_τ . The estimates of the former plotted in Panel A represent the private interest channel (i.e., differential behaviour of the term-limited leaders in democracies) and the estimates of the latter plotted in Panel B capture the public interest channel (i.e., baseline behaviour of the unlimited leaders in democracies). On the one hand, the only channel that seems to instantly react to the crisis is the one that is publicly driven, which is consistent with the intuition that the public would require policymakers to generate an immediate response to the crisis in order to avert the

financial doom. Furthermore, it gradually disappears over time, which is again consistent with the idea that these interventions are meant to be only temporary and do not represent permanent changes in a country’s financial policy stance. On the other hand, the private interest channel becomes active much later (three years after a crisis) and -in line with our previous findings- its magnitude is considerably larger. This also explains the somewhat permanent effect we previously detected in Figure 3. The combination of these two channels makes the aggregate trends look like there is no reversal in interventions whereas we find that the public interest channel actually reverses (and private one not) when we separately analyse them.

6.4. *Public vs. private interests: financial stability, policy domains and revolving doors*

6.4.1. *Bank-level evidence on post-crisis instability under term-limited leaders*

Despite our intuition that the term-limited interventions are more likely to be motivated by private interests, one might still think that the more active interventionary stance by the term-limited leaders might be due to the possibility that they happen to be free of the popular (but potentially conflicting and incapacitating) demands of their voters; and hence can intervene more intensely to stop or mitigate an ongoing financial crisis.³³ Indeed there is some evidence in the literature that points to a phenomenon of policy paralysis after financial crises (Mian et al., 2014). Being less sensitive to the parliamentary and public pressures that may exist in a democratic system, the term-limited leaders may perhaps bring the much-needed financial stability back to their countries by intervening more proactively.

In order to answer whether or not the term-limited leaders are better at dealing with financial crises and the resulting instability, we employ a global bank-level dataset from

³³This is similar to the previously-mentioned argument of “pandering” between political leaders and voters, which may force the former to choose wrong-but-highly-demanded policies in order to get re-elected by the latter (see Canes-Wrone et al., 2001).

Bankscope that covers the years from 1999 to 2014 in up to 123 countries.³⁴ We construct measures of distance to default by computing the z-scores for each bank; that is, we focus on how much equity as well as income buffer each bank has compared to the volatility of its past income flows.³⁵ The advantage of using information at bank-level is the ability to separate our results by bank-specific characteristics such as the relative size of a bank in a country.

Table 5 reports the results where we estimate a specification à la Equation 2 over a balanced panel of banks with log of the z-score as the dependent variable (i.e., Full Model).³⁶ Due to short time-series in this dataset, we also include a less demanding specification where we drop the *PREcrisis* variable both in the baseline and interaction (i.e., Partial Model). First, and perhaps not surprisingly, we find that domestic banks run closer to default when a country experiences a financial crisis. However, and more importantly, having a term-limited policymaker during a financial crisis is associated with significantly *higher* financial instability, not lower. This observation is obtained both in partial and full models, in estimations that control for country-specific time-trends (see Table E2), or in the unbalanced panel where we include all bank-year observations available in Bankscope (see Table E3). Even in the most conservative estimates of Table 5, the negative effect of a financial crisis on banks' z-scores more than doubles when a term-limited politician is in charge. Furthermore, the harmful association between term-limits and financial instability seems to be much stronger for larger banks.

Clearly, it does not seem to be the case that the term-limited leaders perform better

³⁴See Silva (2019) for the cleaning and construction of the baseline bank-year panel from the raw Bankscope dataset.

³⁵See, for example, Laeven and Levine (2009). More formally, we compute the following for each bank and year: $\frac{(Income_t + Equity_t)}{Assets_t}$ divided by the standard deviation of return on assets over the past four years, that is $\sigma_{ROA, t-1, t-4}$. As we have a relatively short time span in our sample, we use the previous four years as the window over which to compute the volatility. We present alternative windows of three and five years to show that our results are not sensitive despite the fact longer durations reduce our sample size. Table E1 summarises the key statistics for our dependent variables.

³⁶Our results without log transformation are qualitatively similar; but the log version of the z-score is our baseline as it takes into account the non-linearity between crises and financial stability. Due to multicollinearity, we are unable to estimate the *PREcrisis* interaction in one estimation in the Full Model. Hence, the Partial Model provides more comparable estimates across different versions of our dependent variable.

in managing financial crises and bringing back the stability to their countries. If anything, they seem to perform worse. To the extent that financial stability could be considered as a public good, these findings provide evidence that term-limits obstruct its provision, rather than promoting it.

6.4.2. Do term-limited and unlimited leaders intervene in the same policy domains?

In Section 6.3, we have focused on the broader impact of term-limits across the whole spectrum of financial policymaking. However, a more granular analysis can be performed by zooming into the specific domains of financial liberalization. It may be crucial to see if the differential behaviour of the lame-duck politicians comes from the same areas as their unlimited counterparts or alternatively they may prefer to intervene in the financial markets in different ways which may be informative about their intentions.

By using each financial domain as a separate dependent variable, Table 6 estimates a specification similar to Equation 2. It is visible that the term-limited and unlimited policymakers focus on very different areas to intervene. For instance, unlimited democratic leaders focus on interventions that are potentially aligned with public demand (such as introducing capital controls and tightening banking supervision) in which the term-limited policymakers do not seem to take any additional steps.³⁷ On the contrary, when political accountability is reduced via binding term-limits, policymakers seem to focus on controversial interventions that are more likely to be associated with financial repression and consequently serve special-interest groups, such as introducing credit or interest rate controls and raising the bank entry barriers. Interestingly, there is no reversal in these areas when democratic leaders do not face term-limits. The stark contrast in policy stance between the two types of policymakers is strongest in the domain of bank entry barriers, which is consistent with a view of rent extraction for incumbent banks by discouraging new entry into the financial

³⁷The significant negative effects on capital account (see [Pepinsky, 2012](#)) and bank privatizations (see [Chwiero and Walter, 2019](#)) are also in line with the “public-interest” interpretations in the previous literature.

industry.³⁸

6.4.3. *Do revolving doors influence term-limited leaders' policymaking?*

The previous analysis of separate policy domains can be sharpened to test our hypothesis on private interests more directly. For this purpose, we resort to a dataset compiled by [Braun and Raddatz \(2010\)](#) in which authors rank a large cross-section of countries based on the frequency of the directors in their banks who used to be high-ranking politicians in the past. It is a somewhat noisy measure not only because it provides a single snapshot as of year 2006 but also it is potentially biased against countries whose bank coverage may not be so widespread in the Bankscope dataset. Nevertheless, we still think that it could proxy the structural career linkages between politics and financial industry across countries. Importantly, it directly speaks to the “incentives” of the policymakers in our setting as their likelihood of acting in favour of the financial industry will eventually depend on how much they can “privately” gain from such quid pro quo transactions. Figure [E1](#) maps the intensity of this revolving door phenomenon across the globe.

As first argued by [Peltzman \(1985\)](#) and more recently by [Mian et al. \(2010; 2013\)](#), an important pre-condition to identify the private interests in policymaking is to clarify the winners and losers from a certain policy action. There are three policy domains in our dataset that directly speak to the incentives of the banking industry: namely, bank entry barriers, bank privatization and bank supervision. In all states of the world, higher entry barriers and less bank supervision would be favoured by the incumbent banks. While nationalisation is not something that the banking industry would enjoy under normal circumstances, this policy domain translates into government bailouts (in the form of equity injections) for troubled banks after financial crises. This implies that interventions in this domain would be much more likely to be appreciated in our context.³⁹ Therefore, in countries where policymakers'

³⁸Table [E4](#) shows that the results in Table [6](#) are similar when we also use interacted political controls in these policy-specific estimations.

³⁹As argued earlier, bailouts could be demanded directly by the constituents as well, making them in line with the public interest channel to some extent. Indeed the evidence documented in Table [6](#) shows that such

private incentives are more salient due to their career concerns in the financial industry, we would expect the term limits to have a larger negative impact (i.e., de-liberalizing) on bank entry barriers and privatization as well as a larger positive impact (i.e., liberalizing) on bank supervision.⁴⁰

In Table 7, we restrict our analysis to these three policy domains in which the incentives of the incumbent banks in the financial industry are sufficiently clear. Our full sample is then cross-sectionally divided into two equal portions conditional on the average intensity of the revolving door phenomenon in a country;⁴¹ and we then re-estimate Equation 2 by adjusting for the loss of the reform area dimension in the dataset. In line with our expectations, the diff-in-diff estimates for term-limits are larger in countries with high revolving doors despite not always being statistically significant, potentially due to low statistical power in these small-sample tests. It seems that when policymakers are more motivated to align themselves with the banking industry, they tend to raise entry barriers higher, which likely prevents future competition for incumbent banks, and they also tend to buy equity in private banks more aggressively by using taxpayer money. The effect on bank supervision is also in the expected direction albeit statistically insignificant.

The final columns of both panels in Table 7 pull together all three policy domains and re-estimates them in the same specification as in Table 4.⁴² “Bank-friendly” policy changes in the aftermath of financial crises are found to be *almost three times larger* when a term-

interventions are likely even when leaders are not term-limited. However, there is also plenty of evidence that bailouts are used strategically by politicians in order to generate private rents (see Brown and Dinc, 2005; Faccio, Masulis, and McConnell, 2006; Duchin and Sosyura, 2012). An additional motivation for the executive politician in injecting equity (instead of lending money) to a failing bank could be to aim for lower seniority in case of failure and thus to protect the financial creditors which are again likely to be the financial institutions in the same country (see Veronesi and Zingales, 2010, for an example in US context).

⁴⁰Remember that a negative impact in a policy domain means more state intervention and less liberalization.

⁴¹Figure E2 maps the countries that fall into each category. We aim to minimize the variation in this variable in order to lessen the potential reverse causality between financial crises prior to 2006 and the resulting subsequent political connections between banks and politics that may impact the recent cross-sectional snapshot provided by Braun and Raddatz (2010). Having said that, our results are similar when we employ more variation from this variable and focus on alternative subsamples.

⁴²For this estimation, we multiply the supervision domain by a minus to make it in line with our expected direction of private interests.

limited political leader is in charge of a country with a high number of revolving doors.⁴³ Results are consistent with the hypothesis that policymakers favour the policy preferences of the incumbent banks when they cannot run for the next election but have higher chances of being employed in the financial industry.

7. Conclusion

In this paper, we aim to shed light on the public and private channels of post-crisis financial policymaking by trying to understand how it is shaped by the level of political accountability within and across countries. Based on a novel panel dataset of 94 countries over the period from 1973 to 2015, we first present strong evidence showing that financial crises in general trigger government interventions and initiate a process of re-regulation in financial markets.

Further investigating the political dynamics behind the scenes, we find that such interventions are only common in democratic settings, which implies a public interest channel either due to a change in general sentiments about financial regulation and/or because a vast majority of (middle-class) citizens would be financially better off in case of an intervention that may mitigate the financial crisis.

In order to understand how much private interests may matter for policy reversals, we benefit from a technical aspect of the election process in democratic countries and use it as a plausibly exogenous setting in which policymakers would face a lower level of political accountability. Empirically, we compare democratic leaders' policy reactions to financial crises when they can be freely re-elected in the next term and when they cannot because of a binding term limit. As a result, we find that a large part of the interventionary stance in the aftermath of financial crises are driven by these term-limited politicians. Specifically, we detect that the policy reversals occur both when politicians face a binding term limit and when they do not; however the effect is almost *four times larger* in the former case.

⁴³The difference between the estimates reported in the final columns across two subsamples is statistically significant at conventional levels.

We then go on to test whether these additional policy changes by the term-limited leaders are aimed for crisis management and help the policymakers to restore the much-needed financial stability. On the contrary, we find that these interventions take place much later than the initial year of the crisis and thus cannot be associated with the immediate policy response to avert the crisis. Moreover, a bank-level analysis shows that the term-limited leaders have a worse crisis performance than their unlimited counterparts. To the extent that financial stability could be considered as a public good, these findings provide evidence that term-limits obstruct its provision, rather than promoting it.

A more granular look into which policy domains drive these additional interventions reveal that the term-limited leaders intervene in more controversial parts of the financial markets and not in those usually motivated by public interest. Finally, we present evidence that the term-limited policymakers are more likely to intervene in ways that will be beneficial for incumbent banks in countries where they are more likely to be employed by the financial industry after leaving politics, signalling an intention to advance their own private agendas by distributing rents to special-interest groups.

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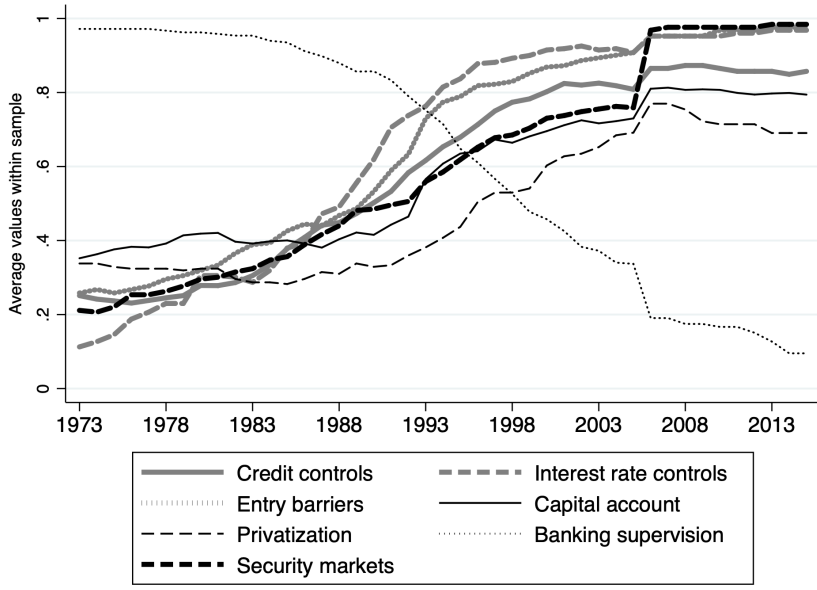
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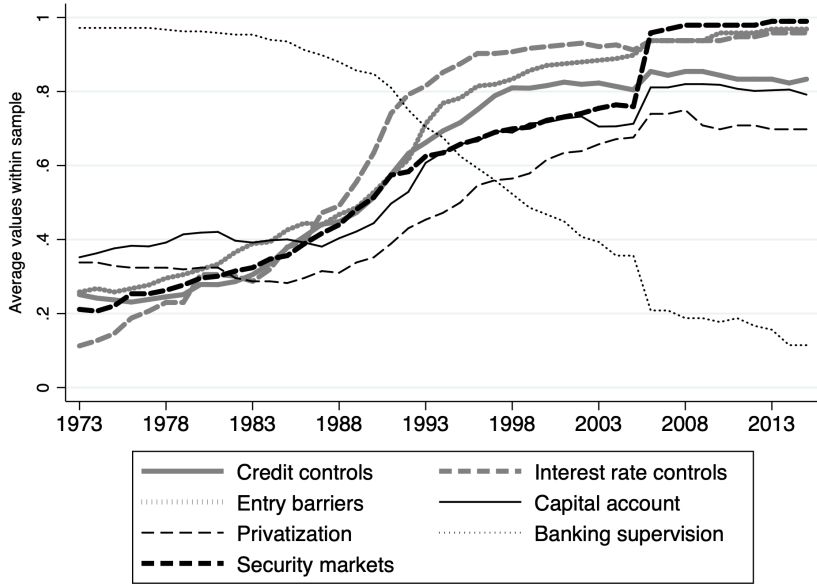
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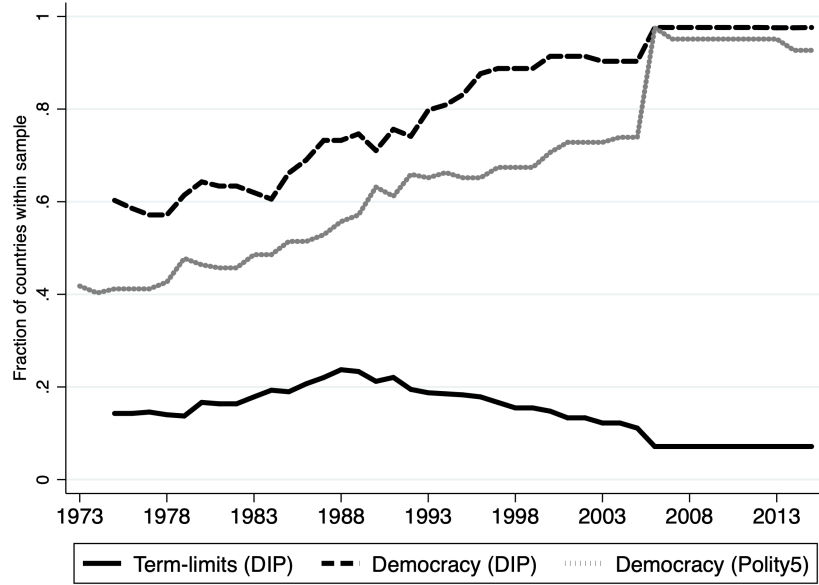


(a) Full (unbalanced) sample

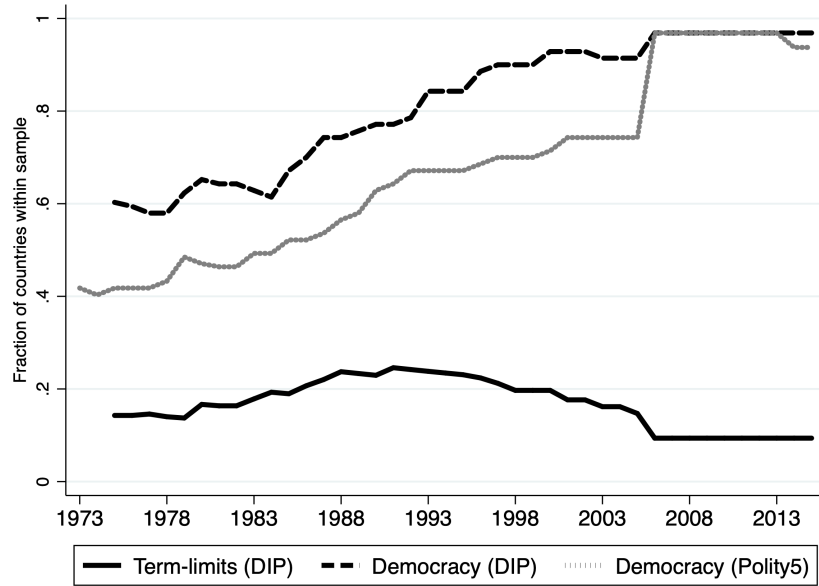


(b) Balanced sample

Fig. 1. The evolution of financial policy domains within our sample period. The figure illustrates the average value for each financial policy domain across all countries within our sample in each year. Panel A is for the full sample employed in our analysis and Panel B illustrates a more balanced subsample in which we only include those countries that have more than 30 years of observations. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#).



(a) Full (unbalanced) sample



(b) Balanced sample

Fig. 2. The evolution of term-limits and democracy within our sample period. The figure illustrates the fraction of countries in our sample that can be categorised as democratic as well as the fraction of those whose leaders can be labelled as term-limited in each year. Panel A is for the full sample employed in our analysis and Panel B illustrates a more balanced subsample in which we only include those countries that have more than 30 years of observations. DPI represents the Database for Political Institutions derived from [Cruz et al. \(2016\)](#) and Polity5 is the most recent release of the political regime types from the Center for Systemic Peace.

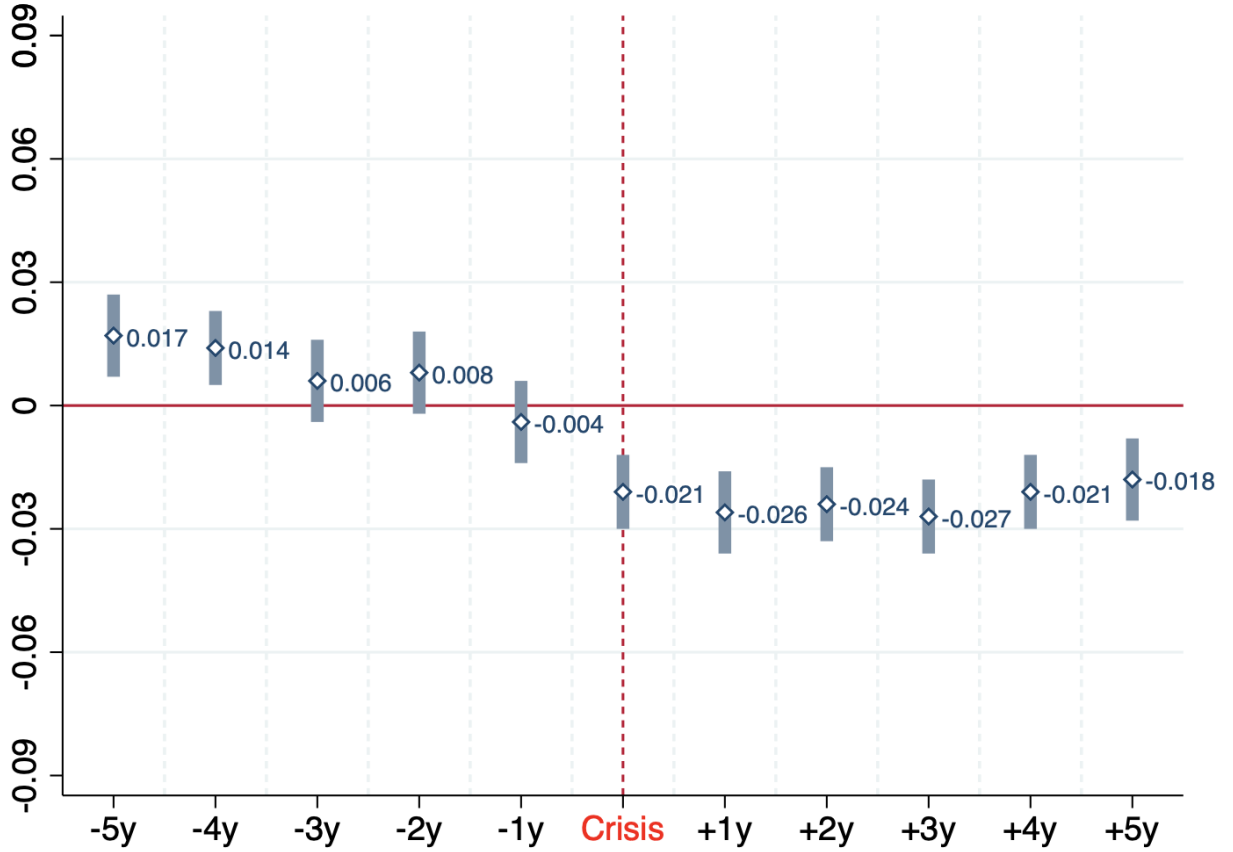
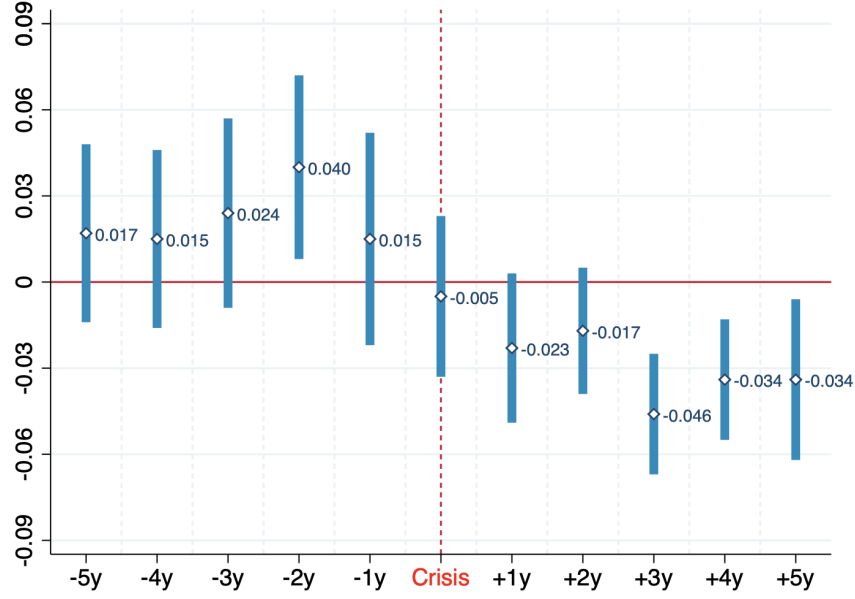
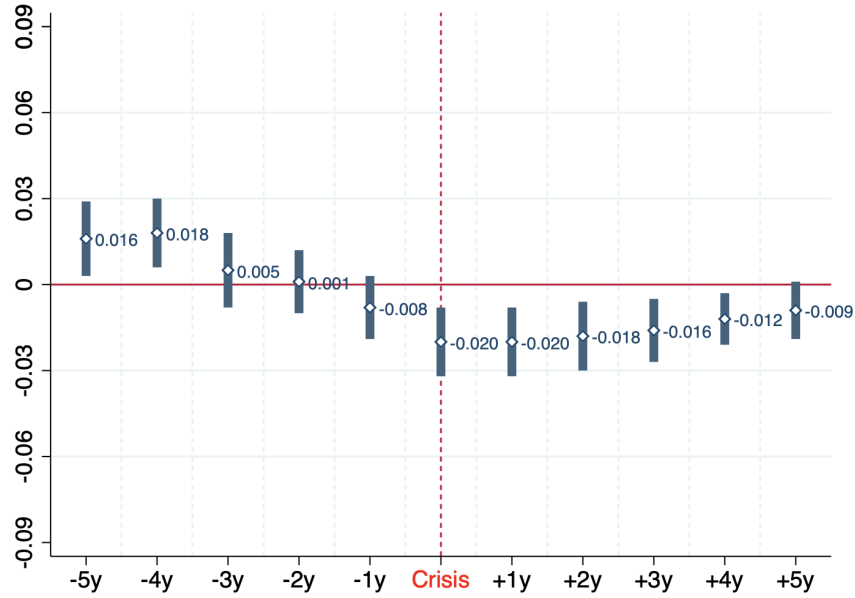


Fig. 3. **Timeline for the effect of a crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and confidence intervals are at 90% significance level.



(a) Private interest channel



(b) Public interest channel

Fig. 4. **Timeline for the effect of a crisis year (interacted with term-limits) on average financial liberalization.** The figure plots the estimates for β_τ in Panel A and γ_τ in Panel B, both from the rolling specification in Equation 4. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and confidence intervals are at 90% significance level.

Variables		Mean	Median	Std. Deviation	Min	Max	Observations
<i>Indices of financial liberalization</i>							
<i>Financial liberalization (Average)</i>		0.59	0.63	0.23	0.14	0.96	3,046
<i>Credit controls</i>		0.58	0.67	0.38	0.00	1.00	3,046
<i>Interest rate controls</i>		0.65	1.00	0.43	0.00	1.00	3,082
<i>Entry barriers</i>		0.65	0.67	0.39	0.00	1.00	3,082
<i>International capital controls</i>		0.56	0.67	0.37	0.00	1.00	3,082
<i>Privatization</i>		0.47	0.33	0.40	0.00	1.00	3,082
<i>Banking supervision</i>		0.66	0.67	0.37	0.00	1.00	3,082
<i>Security markets</i>		0.57	0.67	0.39	0.00	1.00	3,082
<i>Measures of financial crises</i>							
<i>Financial crises (any crisis)</i>		0.08	0.00	0.27	0.00	1.00	3,082
<i>Banking crises</i>		0.04	0.00	0.19	0.00	1.00	3,082
<i>Sovereign debt crises</i>		0.02	0.00	0.13	0.00	1.00	3,082
<i>Currency crises</i>		0.04	0.00	0.19	0.00	1.00	3,082
<i>Variables for political dynamics</i>							
<i>TermLimit</i>		0.17	0.00	0.38	0.00	1.00	2,108
<i>Right</i>		0.37	0.00	0.48	0.00	1.00	2,108
<i>Left</i>		0.33	0.00	0.47	0.00	1.00	2,108
<i>Presidential</i>		0.38	0.00	0.49	0.00	1.00	2,108
<i>Parliamentary</i>		0.56	1.00	0.50	0.00	1.00	2,108
<i>OfficeYears</i>		4.68	3.00	4.66	1.00	35.00	2,108
<i>YearsLeft</i>		1.92	2.00	1.40	0.00	6.00	2,108
<i>HerfGov</i>		0.76	0.89	0.27	0.11	1.00	2,108
<i>GovFrac</i>		0.24	0.12	0.27	0.00	0.89	2,108
<i>GovShare</i>		0.59	0.55	0.16	0.11	1.00	2,108
<i>Checks</i>		3.77	4.00	1.68	1.00	18.00	2,108

Table 1: **Summary statistics for main variables.** The table outlines the summary statistics for variables related to financial reforms and crises. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Dummies for the initial year of various types of financial crises are obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#).

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis</i>	-0.046*** [0.009]	-0.035*** [0.008]	-0.035*** [0.008]	-0.035*** [0.008]	-0.035*** [0.008]
<i>PREcrisis</i>	-0.028** [0.011]	-0.004 [0.009]	-0.004 [0.009]	-0.004 [0.009]	-0.004 [0.009]
Diff-in-diff	-0.017*	-0.031***	-0.031***	-0.031***	-0.031***
P-value	0.075	0.001	0.001	0.001	0.001
N	21,538	21,538	21,538	21,538	21,538
Adj-R-sq	0.177	0.200	0.474	0.534	0.746
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table 2: **Full sample: Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Sample: Models:	Financial Liberalization									
	Democratic countries					Autocratic countries				
	I	II	III	IV	V	I	II	III	IV	V
POSTcrisis	-0.050*** [0.011]	-0.023** [0.009]	-0.023** [0.009]	-0.023** [0.009]	-0.023** [0.009]	-0.046*** [0.017]	-0.031* [0.018]	-0.031* [0.018]	-0.031* [0.019]	-0.031 [0.019]
PREcrisis	-0.016 [0.014]	0.019* [0.010]	0.019* [0.010]	0.019* [0.010]	0.019* [0.010]	-0.038*** [0.012]	-0.024 [0.017]	-0.024 [0.017]	-0.024 [0.018]	-0.024 [0.018]
Diff-in-diff	-0.034***	-0.042***	-0.042***	-0.042***	-0.042***	-0.008	-0.007	-0.007	-0.007	-0.007
P-value	0.002	0.000	0.000	0.000	0.000	0.607	0.562	0.574	0.578	0.590
N	16,088	16,088	16,088	16,088	16,088	4,143	4,143	4,143	4,143	4,143
Adj-R-sq	0.163	0.190	0.440	0.536	0.747	0.460	0.467	0.573	0.756	0.843
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes			Yes	Yes	Yes		
Reform FE	Yes	Yes				Yes	Yes			
Year FE	Yes	Yes				Yes	Yes			
CountryTime Trend				Yes				Yes	Yes	Yes
Country x Reform FE		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes

Table 3: **Democracy vs. autocracy: Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results over two subsamples with the specification in Equation 1. A country is categorised as *democratic* if its executive index of electoral competitiveness has a value equal to or higher than six; otherwise it is categorised as *autocratic*. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis x TermLimit</i>	-0.041*	-0.072***	-0.072***	-0.072***	-0.072***
	[0.021]	[0.022]	[0.022]	[0.022]	[0.022]
<i>PREcrisis x TermLimit</i>	0.035	0.013	0.013	0.013	0.013
	[0.027]	[0.028]	[0.028]	[0.029]	[0.029]
<i>POSTcrisis</i>	-0.039***	-0.007	-0.007	-0.007	-0.007
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]
<i>PREcrisis</i>	-0.020	0.020*	0.020*	0.020*	0.020*
	[0.014]	[0.010]	[0.010]	[0.010]	[0.010]
<i>TermLimit</i>	-0.058	-0.014	-0.015	-0.014	-0.015
	[0.046]	[0.036]	[0.036]	[0.037]	[0.037]
Diff-in-diff for Term Limit	-0.077***	-0.084***	-0.084***	-0.084***	-0.084***
P-value	0.001	0.000	0.000	0.000	0.000
Diff-in-diff	-0.019	-0.027**	-0.027**	-0.027**	-0.027**
P-value	0.109	0.023	0.024	0.025	0.026
N	15,696	15,696	15,696	15,696	15,696
Adj-R-sq	0.166	0.193	0.438	0.539	0.749
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table 4: **Term limits in democracies: Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results with the specification in Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Bank sample:	Partial model						Full model					
	All (Balanced Panel)			Small			All (Balanced Panel)			Small		
	Log Z-score (3-year)	Log Z-score (4-year)	Log Z-score (5-year)	Log Z-score (4-year)	Log Z-score (4-year)	Log Z-score (4-year)	Log Z-score (3-year)	Log Z-score (4-year)	Log Z-score (5-year)	Log Z-score (4-year)	Log Z-score (4-year)	Log Z-score (4-year)
Dependent variable:												
<i>POSTcrisis</i> x <i>TermLimit</i>	-0.645*** [0.214]	-0.429** [0.191]	-0.310*** [0.111]	-0.346** [0.156]	-0.674*** [0.235]		-0.679*** [0.240]	-0.552*** [0.206]	-0.463*** [0.101]	-0.411** [0.156]	-0.682** [0.313]	
<i>PREcrisis</i> x <i>TermLimit</i>							0.225 [0.322]	0.194 [0.398]		0.102 [0.453]	0.297 [0.199]	
<i>POSTcrisis</i>	-0.395*** [0.098]	-0.354*** [0.080]	-0.293*** [0.059]	-0.220*** [0.063]	-0.561*** [0.157]		-0.329*** [0.100]	-0.209** [0.094]	-0.052 [0.079]	-0.125* [0.064]	-0.319* [0.175]	
<i>PREcrisis</i>							0.167* [0.084]	0.280** [0.113]	0.408*** [0.127]	0.191** [0.090]	0.415** [0.195]	
<i>TermLimit</i>	-0.434** [0.182]	-0.609*** [0.177]	-1.040*** [0.076]	-0.476** [0.207]	-0.407** [0.163]		-0.509*** [0.145]	-0.647*** [0.114]	-0.966*** [0.069]	-0.524*** [0.149]	-0.639*** [0.117]	
Diff-in-diff for Term Limit							-0.904*** 0.003	-0.746* 0.060	-0.463*** 0.000	-0.514 0.204	-0.979*** 0.000	
P-value												
Diff-in-diff							-0.496*** 0.000	-0.490*** 0.000	-0.460*** 0.000	-0.317*** 0.001	-0.734*** 0.000	
P-value												
N	13,047	12,130	11,198	5,989	6,624		13,047	12,130	11,198	5,989	6,141	
Adj-R-sq	0.387	0.464	0.528	0.529	0.334		0.388	0.468	0.537	0.531	0.415	
Clustering	Country	Country	Country	Country	Country		Country	Country	Country	Country	Country	
Bank FE	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	

Table 5: **Term limits in democracies: Difference-in-differences estimates for bank-level stability.** The table summarizes the estimation results with a specification à la Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six and only those banks that have observations in each year from 1999 to 2014. Dependent variable is *Log Z-score* varying over banks and years measuring the total equity plus income buffer of a bank with respect to the volatility of its past (3-year, 4-year or 5-year) cash flows. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Bank-level data is obtained from Bankscope. Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Credit controls	Interest rate controls	Entry Barriers	Capital account	Privatisation	Banking supervision	Security markets
<i>POSTcrisis x TermLimit</i>	-0.078 [0.049]	-0.082 [0.056]	-0.146*** [0.050]	-0.035 [0.059]	-0.103* [0.059]	-0.012 [0.028]	-0.012 [0.036]
<i>PREcrisis x TermLimit</i>	0.035 [0.041]	0.034 [0.064]	-0.001 [0.061]	-0.012 [0.053]	0.025 [0.063]	-0.016 [0.029]	0.048 [0.034]
<i>All baseline political controls</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Diff-in-diff for Term Limit	-0.113**	-0.116**	-0.145**	-0.023	-0.129*	0.004	-0.059*
P-value	0.025	0.015	0.021	0.663	0.091	0.910	0.069
Diff-in-diff	-0.018	-0.020	0.007	-0.051**	-0.076***	-0.021	-0.022
P-value	0.460	0.412	0.746	0.036	0.009	0.196	0.211
N	2,077	2,108	2,108	2,108	2,108	2,108	2,108
Adj-R-sq	0.720	0.702	0.789	0.648	0.654	0.814	0.768
Clustering	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country/Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: **Term limits in democracies: Difference-in-differences estimates for policy domains.** The table summarizes the estimation results with a specification à la Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six. Dependent variable is a financial policy domain varying over countries and years. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Online Appendix

FINANCIAL POLICYMAKING AFTER CRISES:

PUBLIC VS. PRIVATE INTERESTS

Orkun Saka

Yuemei Ji

Paul De Grauwe

31 May 2021

Appendix A

The timeline is described in Figure A1. A politician is elected at the beginning of each period, after which nature reveals to the incumbent the state of the world. If she is newly elected, nature also reveals her type (still unobservable to voters). In the case of a bad incumbent, she also receives a random draw r_1 from the distribution $G(\cdot)$ of private rent. After the policy is set, voters observe their payoffs and then decide whether to re-elect the incumbent or select a challenger who would be drawn at random from the pool of potential politicians. After the re-election is held, the bad politician (if re-elected) receives a fresh (independent) draw r_2 from the distribution $G(\cdot)$. Period 2 action then follows, payoffs are realized and the game ends.

To solve the problem, the perfect Bayesian Nash equilibrium of this game requires that: (1) in every period each type of politician behaves optimally given the re-election condition that the voters put in place; (2) voters use Bayes rule to update their beliefs about the type of politician and hence make their voting decision.

For interested readers, a game tree is provided in the Figure A2 to facilitate our analysis. In period 2, the choice for the politician (provided that she is re-elected) is straightforward. Each type of politician chooses an action by optimizing her short-term (one-period) payoffs. Since there is a binding term limit, the bad politician in this period only cares about her own private interest, and thus $e_2(s_2, bad) = 1 - s_2$. For the good politician, the binding term limit does not play a role as she always cares about the voters' interest and shares the same objective and utility Δ . Hence it is optimal for her to choose $e_2(s_2, good) = s_2$. This result confirms a separating equilibrium if there is a binding term limit for politicians.

In period 1, the behaviour of the good politician does not change: she always does what voters want independent of whether or not she is re-elected for doing so, $e_1(s_1, good) = s_1$. However, the behaviour of the bad politician is more complex. The latter needs to consider the trade-off between her current private benefit (r_1) and the discounted value of the expected future benefit ($\beta(\mu + E)$) if she is re-elected in period two. When this current private benefit

is lower than the present value of the expected future benefit of being re-elected, the bad politician will choose the policy action in line with the public interest, $e_1(s_1, bad) = s_1$. The probability of this choice can be expressed as:

$$z = Pr(r_1 < \beta(\mu + E)) = G(\beta(\mu + E))$$

The question is whether this probability (z) would ensure that the bad politician will be re-elected by the voters. To verify this, we use Bayes rule to describe voters' belief that the politician is good conditional on having received a payoff of Δ . This probability can be expressed as:

$$\pi^* = \frac{\pi}{\pi + (1 - \pi)z} \geq \pi$$

Obviously, this good behaviour ($e_1(s_1, bad) = s_1$) improves a bad politician's reputation (measured by probability $\pi^* > \pi$). It implies that there is always an equilibrium in which any politician who produces Δ for voters (i.e. as long as $e_1 = s_1$ in period one) is re-elected when voters only use the incumbent's performance during period one as their basis for voting. A politician who fails to produce Δ for voters is not re-elected since such a politician is considered to be bad for sure. This result confirms the pooling equilibrium when politicians do not face a binding term limit and hereby proves that, conditional upon our set of assumptions, *existence (absence) of re-election incentives drives policymakers to behave more in line with public (private) interests.*

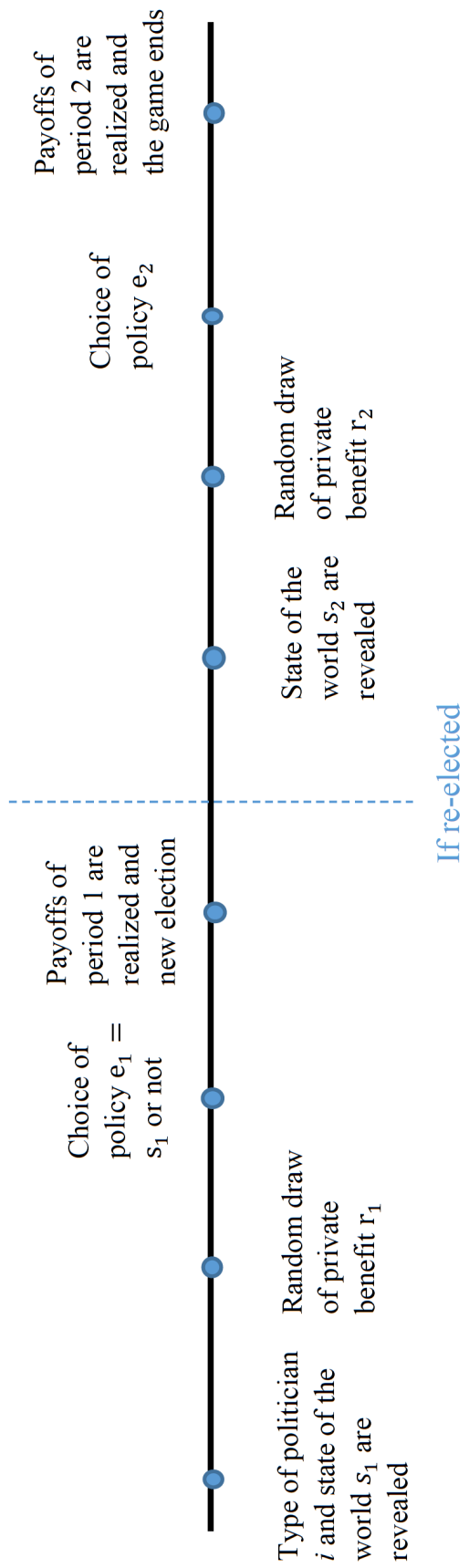


Fig. A1. **Timeline of moves and actions in a simple model of post-crisis policymaking.** The figure illustrates the time-wise order of the steps and decisions that take place in the game theoretical model discussed in Section 3.

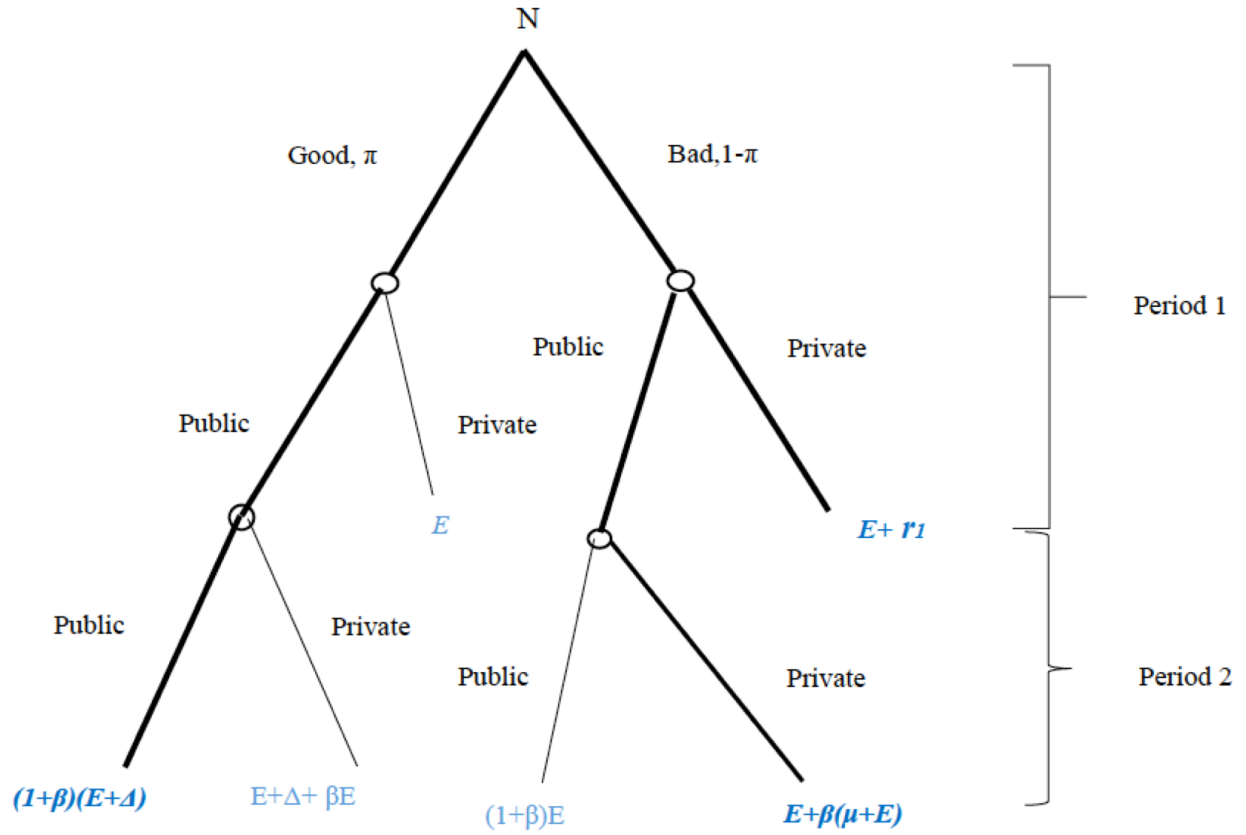


Fig. A2. **Game tree with payoffs in a simple model of post-crisis policymaking.** The figure illustrates the time-wise order of the steps, decisions and payoffs that take place in the game theoretical model discussed in Section 3.

Name of the sub-index	Questions within the sub-index	Numerical value	Answer categories
(1) Credit controls	(1.a) Are reserve requirements restrictive?	0 1 2	(1.a.1) More than 20% (1.a.2) Between 10% and 20% (1.a.3) Less than 10%
	(1.b) Are there minimum amounts of credit that must be channeled to certain sectors?	0 1	(1.b.1) credit allocations determined by CB or mandatory allocations to certain sectors exist (1.b.2) no mandatory credit allocations to any sector
	(1.c) Are there any credits supplied to certain sectors at subsidized rates?	0 1 2	(1.c.1) banks have to supply credit at subsidized rates to certain sectors (1.c.2) no mandatory credit allocation or subsidized rates to any sector (1.d.1) ceilings on credit expansions exist (1.d.2) no ceilings on credit expansion
(2) Interest rate controls	(1.d) Are there any aggregate credit ceilings?	0 1	(1.d.1) ceilings on credit expansions exist (1.d.2) no ceilings on credit expansion
	(2.a) Are deposit rates free?	0 1 2	(2.a.1) Set by gov't or subject to a binding constraint (2.a.2) Fluctuating within a set band (2.a.3) Freely floating
	(2.b) Are lending rates free?	0 1 2	(2.b.1) Set by gov't or subject to a binding constraint (2.b.2) Fluctuating within a set band (2.b.3) Freely floating
(3) Entry Barriers	(3.a) To what extent does the gov't allow foreign banks to enter into a domestic market?	0 1 2	(3.a.1) No entry allowed or tight restrictions in place (3.a.2) Allowed but no more than 50% equity to be held (3.a.3) Majority ownership allowed; or equal treatment for domestic and foreign banks; or no limit on foreign branching
	(3.b) Does the gov't allow the entry of new domestic banks?	0 1	(3.b.1) Not allowed or strictly regulated (3.b.2) Allowed
	(3.c) Are there restrictions on branching?	0 1	(3.c.1) Restrictions in place (3.c.2) No or light restrictions
(4) Capital account	(3.d) Does the gov't allow banks to engage in a wide range of activities?	0 1	(3.d.1) Only banking activities allowed (3.d.2) Universal banking allowed
	(4.a) Is the exchange rate system unified?	0 1	(4.a.1) Special ex. rate regime for either capital or current account exists (4.a.2) Unified ex. rate system
	(4.b) Does a country set restrictions on capital inflow?	0 1	(4.b.1) Restrictions exist on capital inflows (4.b.2) Bank are allowed to borrow from abroad freely and no tight restrictions on other capital inflows
(5) Privatization	(4.c) Does a country set restrictions on capital outflow?	0 1	(4.c.1) Restrictions exist on capital outflows (4.c.2) Capital outflows are free or with minimal approval restrictions
	(5.a) What is the share of bank assets that are state-owned?	0 0.33 0.67	(5.a.1) State-owned bank assets more than 50% of total bank assets (5.a.2) State-owned bank assets between 50% and 25% of total bank assets (5.a.3) State-owned bank assets between 25% and 10% of total bank assets (5.a.4) State-owned bank assets less than 10% of total bank assets
	(6.a) Has a country adopted a capital adequacy ratio based on the Basel standard?	0 1	(6.a.1) Basel CAR not implemented (always the case before 1993) (6.a.2) Basel CAR adopted or banks already abide by
(6) Banking supervision	(6.b) Is the bank supervisor independent from executive's influence?	0 1 2	(6.b.1) No adequate framework for resolution and no legal independence from the executive (6.b.2) Either adequate framework for resolution or legal independence from the executive (6.b.3) Both adequate framework for resolution and legal independence from the executive
	(6.c) Does supervisor conduct effective on-site and off-site examinations?	0 1 2	(6.c.1) No legal framework and no examinations in practice (6.c.2) Legal framework and examinations exist but inefficient or ineffective (6.c.3) Effective and sophisticated examinations take place
	(6.d) Does supervisor cover all financial institutions without exception?	0 1	(6.d.2) Some are excluded (6.d.1) All included
(7) Security markets	(7.a) Has a country taken measures to develop securities markets?	0 1 2 3	(7.a.1) SM does not exist (7.a.2) SM starting to form with T-bill auctions and SM commission (7.a.3) Further measures taken to develop SM (7.a.4) Further measures to develop derivatives market or complete deregulation of stock exchanges
	(7.b) Is a country's equity market open to foreign investors?	0 1 2	(7.b.1) No foreign ownership allowed (7.b.2) Foreign ownership allowed but less than 50% max (7.b.3) Majority foreign ownership allowed

Table A1: Details of the financial policy indices constructed by [Abiad et al. \(2010\)](#). The table summarizes the construction of the seven financial policy indices. Each index is composed of several questions that in return have various numbers of categorical answers. Each answer corresponds to a numerical value where higher values represent more liberalization, except in the (6) banking supervision index which generally carries higher values for increasing levels of government intervention. For the details on how questions are aggregated to compose the financial policy indices, see the original paper.

Name of the sub-index	Questions within the sub-index	Numerical value	Answer categories
(1) Credit controls	(1.a) Are reserve requirements restrictive?	0 1 2	(1.a.1) More than 20% (1.a.2) Between 10% and 20% (1.a.3) Less than 10%
	(1.b) Are there minimum amounts of credit that must be channeled to certain sectors?	0 1	(1.b.1) credit allocations determined by CB or mandatory allocations to certain sectors exist (1.b.2) no mandatory credit allocations to any sector
	(1.c) Are there mandatory requirements on credit allocation at subsidised rates?	0 1	(1.c.1) banks have to supply credit at subsidized rates to certain sectors (1.c.2) no mandatory credit allocation or subsidized rates to any sector
(2) Interest rate controls	(2.a) To what extent does the government control deposit rates?	0 1 2	(2.a.1) Set by gov't (2.a.2) Subject to a ceiling or floor (2.a.3) Freely floating
	(2.b) To what extent does the government control lending rates?	0 1 2	(2.b.1) Set by gov't (2.b.2) Subject to a ceiling or floor (2.b.3) Freely floating
	(3.a) To what extent are foreign banks allowed to enter the domestic market?	0 1 2	(3.a.1) No entry allowed (3.a.2) Allowed but no more than 50% equity to be held (3.a.3) Majority ownership allowed; or equal treatment for domestic and foreign banks
(3) Entry Barriers	(3.b) Are new domestic banks allowed to enter the market?	0 1	(3.b.1) Not allowed or strictly regulated (3.b.2) Allowed
	(3.c) Are there restrictions on branching?	0 1	(3.c.1) Tight restrictions in place (3.c.2) No or few restrictions
	(3.d) Are banks allowed to become universal banks?	0 1	(3.d.1) Only banking activities allowed (3.d.2) Universal banking allowed
(4) Capital account	(4.a) Chinn and Ito (2006) index	0 - 1	Based on IMF's Annual Report on Exchange Arrangements and Exchange Restrictions
(5) Privatization	(5.a) What is the share of bank assets that are state-owned?	0 0.33 0.67	(5.a.1) State-owned bank assets more than 50% of total bank assets (5.a.2) State-owned bank assets between 50% and 25% of total bank assets (5.a.3) State-owned bank assets between 25% and 10% of total bank assets (5.a.4) State-owned bank assets less than 10% of total bank assets
	(6.a) Has a country adopted a capital adequacy ratio based on the latest Basel standard?	0 1	(6.a.1) Latest Basel CAR not adopted (6.a.2) Latest Basel CAR adopted
	(6.b) Is the bank supervisor independent from executive's influence?	0 1 2	(6.b.1) No adequate framework for resolution or no legal independence from the executive or frequent turnover of the supervisor (6.b.2) Either adequate framework for resolution or legal independence from the executive (6.b.3) Both adequate framework for resolution and legal independence from the executive
(6.c) Does the supervisor conduct on-site and off-site examinations?	(6.c.1) No legal framework and no examinations in practice	0	
	(6.c.2) Legal framework and examinations exist but inefficient or ineffective	1	
	(6.c.3) Effective and sophisticated examinations take place	2	
(6.d) Does the supervisory agency cover all financial institutions?	(6.d.1) Some are excluded	0	
	(6.d.2) All included	1	
	(7.a) To what extent are securities markets developed?	0 1 2 3	(7.a.1) SM does not exist (7.a.2) SM starting to form with T-bill auctions and SM commission (7.a.3) Further measures taken to develop SM (7.a.4) Further measures to develop derivatives market or complete deregulation of stock exchanges
(7) Security markets	(7.b) Is a country's equity market open to foreign investors?	0 1 2	(7.b.1) No foreign ownership allowed (7.b.2) Foreign ownership allowed but less than 50% max (7.b.3) Majority foreign ownership allowed

Table A2: Details of the financial policy indices constructed by Denk and Gomes (2017). The table summarizes the construction of the seven financial policy indices. Each index is composed of several questions that in return have various numbers of categorical answers. Each answer corresponds to a numerical value where higher values represent more liberalization, except in the (6) banking supervision index which generally carries higher values for increasing levels of government intervention. For the details on how questions are aggregated to compose the financial policy indices, see the original paper.

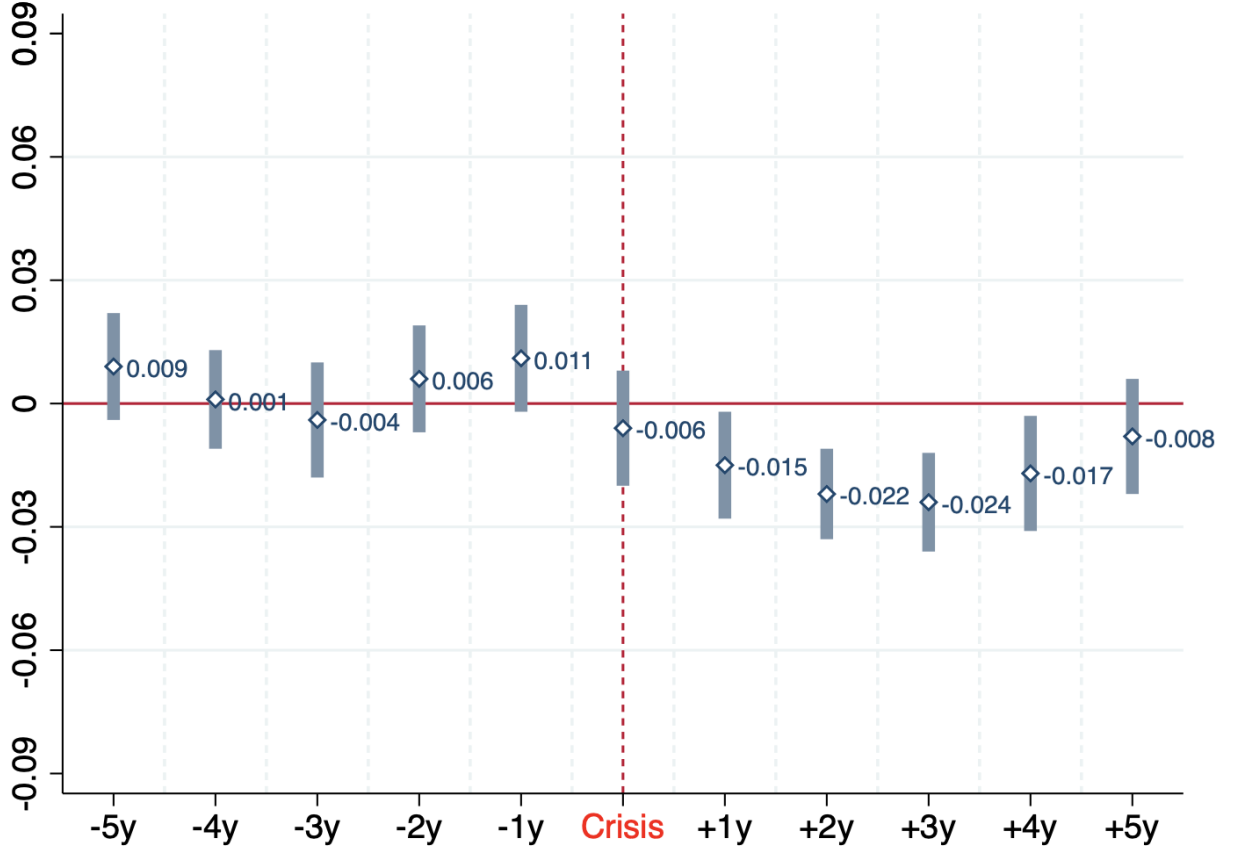


Fig. B1. **Timeline for the effect of a banking crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different types of financial crises. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and confidence intervals are at 90% significance level.

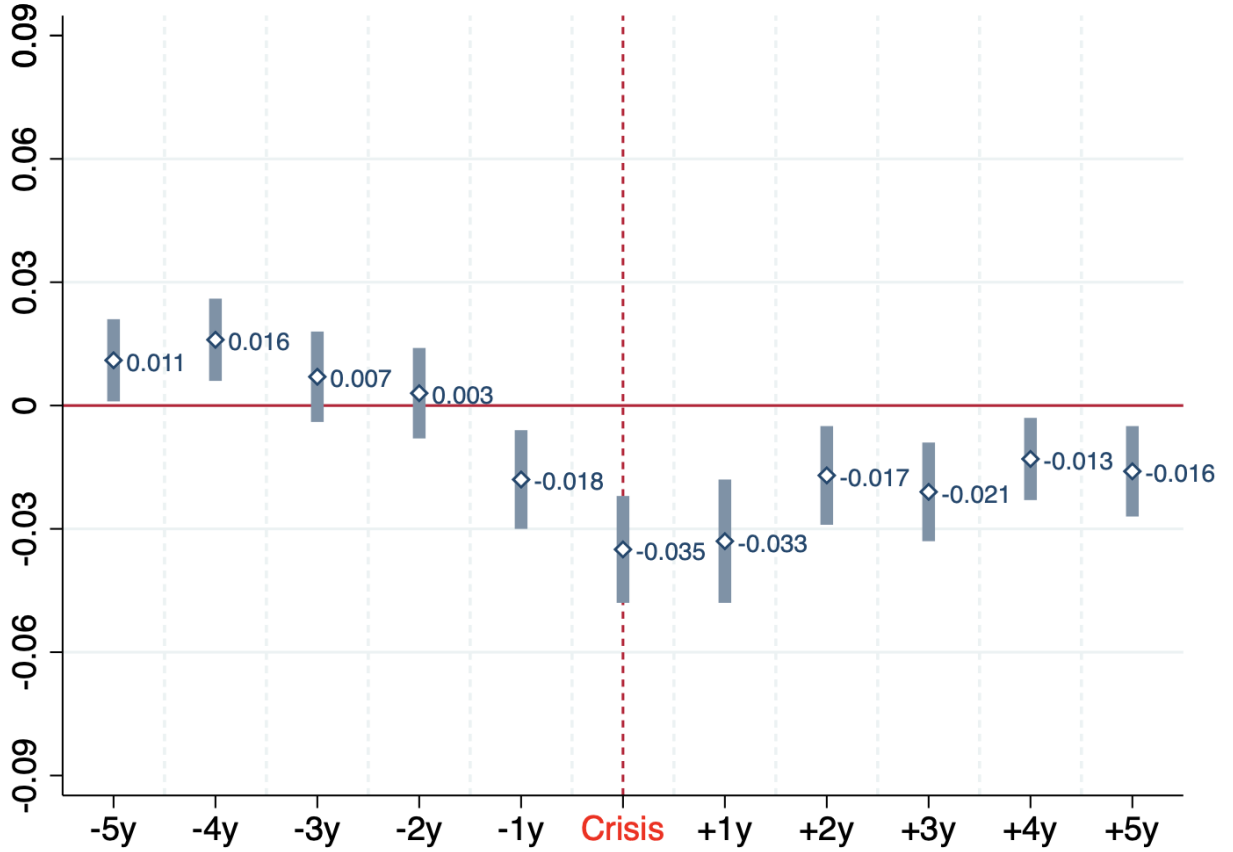


Fig. B2. **Timeline for the effect of a currency crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different types of financial crises. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and confidence intervals are at 90% significance level.

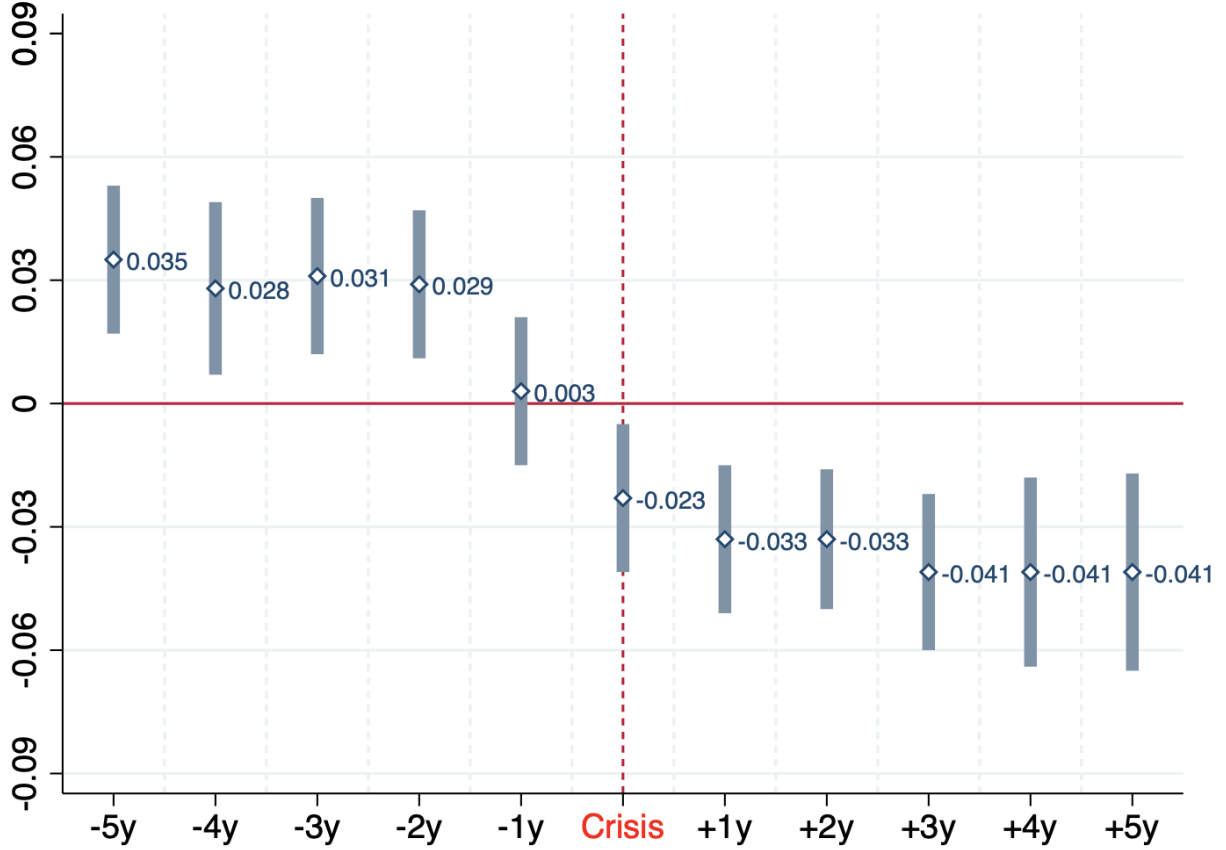


Fig. B3. **Timeline for the effect of a sovereign debt crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different types of financial crises. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and confidence intervals are at 90% significance level.

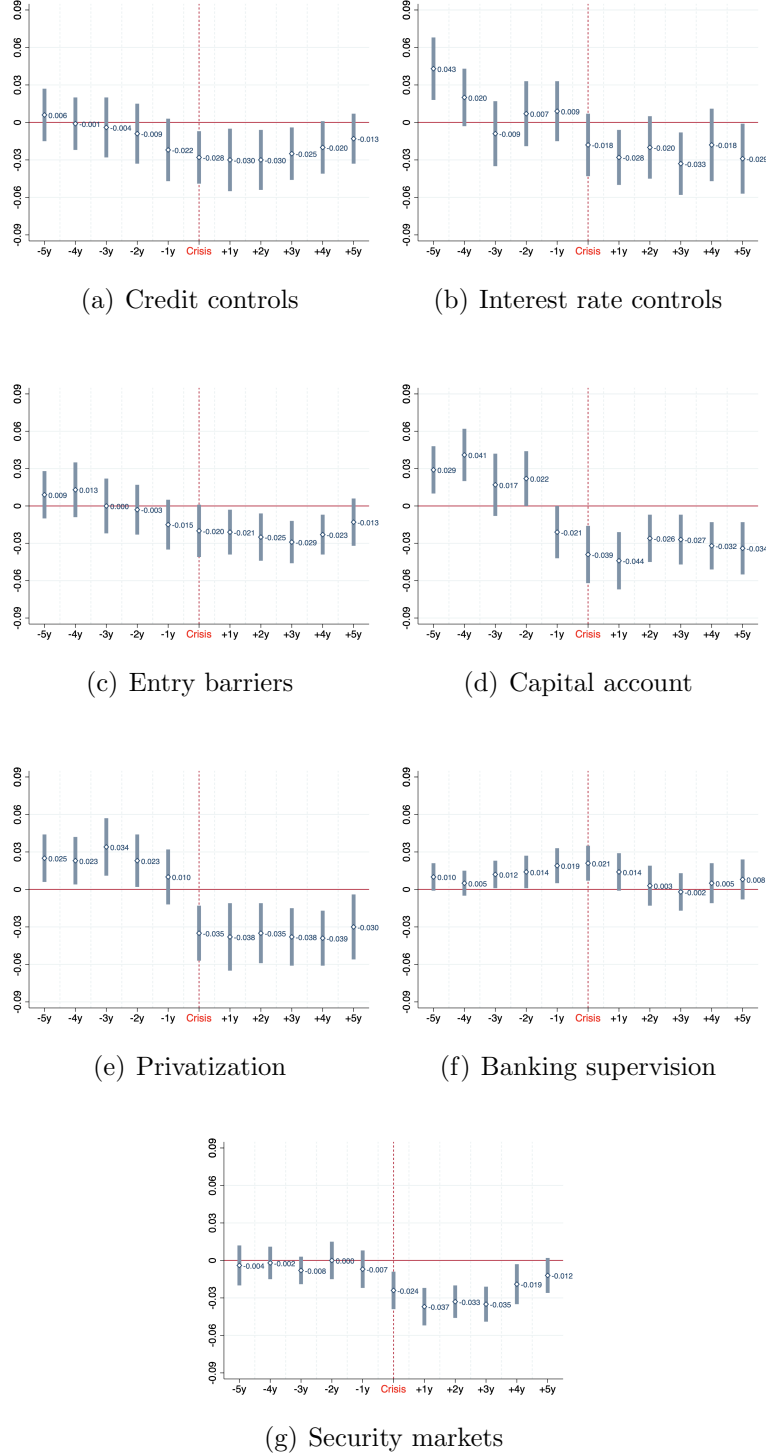


Fig. B4. **Timeline for the effect of a crisis year on financial policy domains.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different domains of financial policymaking. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and confidence intervals are at 90% significance level.

Dependent variable: Models:	Financial Liberalization					Financial Liberalization				
	I	II	III	IV	V	I	II	III	IV	V
<i>POSTcrisis_banking</i>	-0.036*** [0.010]	-0.024*** [0.008]	-0.024*** [0.009]	-0.024*** [0.009]	-0.024*** [0.009]					
<i>PREcrisis_banking</i>	-0.026** [0.011]	-0.001 [0.009]	-0.001 [0.009]	-0.001 [0.009]	-0.001 [0.009]					
<i>POSTcrisis_debt</i>						-0.066*** [0.019]	-0.040** [0.015]	-0.040** [0.016]	-0.040** [0.016]	-0.040** [0.016]
<i>PREcrisis_debt</i>						-0.007 [0.021]	0.022 [0.016]	0.022 [0.016]	0.022 [0.016]	0.022 [0.016]
<i>POSTcrisis_currency</i>										
<i>PREcrisis_currency</i>										
						-0.056*** [0.011]	-0.044*** [0.011]	-0.044*** [0.011]	-0.044*** [0.011]	-0.044*** [0.011]
						-0.033** [0.012]	-0.009 [0.011]	-0.009 [0.011]	-0.009 [0.011]	-0.009 [0.011]
						-0.023** [0.012]	-0.035*** [0.011]	-0.035*** [0.011]	-0.035*** [0.011]	-0.035*** [0.011]
Diff-in-diff	-0.010	-0.023**	-0.023**	-0.023**	-0.023**	-0.059***	-0.062***	-0.062***	-0.062***	-0.062***
P-value	0.314	0.015	0.016	0.017	0.017	0.000	0.000	0.000	0.000	0.001
N	21,538	21,538	21,538	21,538	21,538	21,538	21,538	21,538	21,538	21,538
Adj-R-sq	0.175	0.199	0.473	0.533	0.745	0.175	0.199	0.474	0.533	0.746
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CountryTime Trend				Yes	Yes			Yes	Yes	Yes
Country x Reform FE		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Reform x Year FE			Yes	Yes	Yes			Yes	Yes	Yes

Table B1: Full sample: Difference-in-differences estimates for financial liberalization (separately with banking, sovereign debt and currency crises). The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis_x* is a binary dummy variable turning on in the first 5 years after a financial (x=banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis_x* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis_x* and *PREcrisis_x* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis</i>	-0.019*** [0.006]	-0.016*** [0.006]	-0.016*** [0.006]	-0.016** [0.006]	-0.016** [0.006]
<i>PREcrisis</i>	-0.010 [0.008]	0.008 [0.006]	0.008 [0.006]	0.008 [0.006]	0.008 [0.006]
Diff-in-diff	-0.009	-0.024**	-0.024**	-0.024**	-0.024**
P-value	0.415	0.015	0.015	0.017	0.017
N	21,538	21,538	21,538	21,538	21,538
Adj-R-sq	0.174	0.199	0.473	0.533	0.745
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table B2: **Full sample: Difference-in-differences estimates for financial liberalization (excluding the crisis start-year and ± 1 years).** The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample excluding the crisis start-year and ± 1 years around it. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis excluding the crisis start-year and ± 1 years around it. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis</i>	-0.025*** [0.007]	-0.017** [0.007]	-0.017** [0.007]	-0.017** [0.007]	-0.017** [0.007]
<i>PREcrisis</i>	-0.016* [0.009]	0.007 [0.007]	0.007 [0.007]	0.007 [0.007]	0.007 [0.007]
Diff-in-diff	-0.008	-0.024**	-0.024**	-0.024**	-0.024**
P-value	0.437	0.016	0.016	0.017	0.018
N	21,538	21,538	21,538	21,538	21,538
Adj-R-sq	0.174	0.199	0.473	0.533	0.745
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table B4: **Full sample: Difference-in-differences estimates for financial liberalization (excluding the crisis start-year, ± 1 years and common years before and after a crisis).** The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample excluding the crisis start-year and ± 1 years around it. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis excluding the crisis start-year and ± 1 years around it. Years that correspond to both pre- and post-crisis episodes are turned off (=0) in both dummy variables. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis</i>	-0.024*** [0.005]	-0.021*** [0.004]	-0.021*** [0.004]	-0.021*** [0.004]	-0.021*** [0.004]
<i>PREcrisis</i>	0.004 [0.007]	0.005 [0.005]	0.005 [0.005]	0.005 [0.005]	0.004 [0.005]
Diff-in-diff	-0.027***	-0.026***	-0.025***	-0.025***	-0.025***
P-value	0.003	0.000	0.000	0.000	0.000
N	15,000	15,000	15,000	15,000	15,000
Adj-R-sq	0.193	0.219	0.465	0.519	0.744
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table B6: **Full sample: Difference-in-differences estimates for financial liberalization (with crisis episodes from Reinhart and Rogoff (2011)).** The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on during any financial (banking, domestic debt, external debt, currency, stock market or inflation) crisis in the sample. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from Abiad et al. (2010) and Denk and Gomes (2017). Data on financial crises is obtained from Reinhart and Rogoff (2011). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Models:	Financial Liberalization					Financial Liberalization					Financial Liberalization				
	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V
<i>POSTcrisis_banking</i>	-0.010 [0.012]	-0.004 [0.011]	-0.005 [0.011]	-0.005 [0.011]	-0.005 [0.011]										
<i>PREcrisis_banking</i>	0.015 [0.012]	0.025** [0.010]	0.025** [0.010]	0.025** [0.010]	0.025** [0.010]										
<i>POSTcrisis_domdebt</i>						-0.083*** [0.028]	-0.085*** [0.023]	-0.085*** [0.023]	-0.085*** [0.023]	-0.085*** [0.023]					
<i>PREcrisis_domdebt</i>						-0.025 [0.025]	-0.005 [0.019]	-0.005 [0.019]	-0.005 [0.019]	-0.005 [0.019]					
<i>POSTcrisis_extdebt</i>											-0.056*** [0.017]	-0.042** [0.018]	-0.042** [0.018]	-0.042** [0.018]	-0.042** [0.018]
<i>PREcrisis_extdebt</i>											-0.022 [0.019]	0.006 [0.017]	0.006 [0.018]	0.006 [0.018]	0.006 [0.018]
Diff-in-diff	-0.025**	-0.029***	-0.029***	-0.029***	-0.030***	-0.058***	-0.080***	-0.080***	-0.080***	-0.080***	-0.034*	-0.048***	-0.048***	-0.048***	-0.048***
P-value	0.015	0.001	0.001	0.001	0.002	0.002	0.000	0.000	0.000	0.000	0.061	0.008	0.009	0.009	0.010
N	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000
Adj-R-sq	0.190	0.217	0.463	0.518	0.742	0.190	0.217	0.463	0.518	0.743	0.191	0.217	0.463	0.518	0.743
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
<i>Country FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Reform FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>CountryTime Trend</i>															
<i>Country x Reform FE</i>															
<i>Reform x Year FE</i>			Yes	Yes	Yes			Yes	Yes	Yes			Yes	Yes	Yes

Table B7a: Full sample: Difference-in-differences estimates for financial liberalization (with crisis episodes from Reinhart and Rogoff (2011) and separately with banking, domestic debt and external debt crises). The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis_x* is a binary dummy variable turning on during a financial (x=banking, domestic debt and external debt) crisis in the sample. *PREcrisis_x* is a binary dummy for the 5 years immediately preceding a financial crisis. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis_x* and *PREcrisis_x* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from Abiad et al. (2010) and Denk and Gomes (2017). Data on financial crises is obtained from Reinhart and Rogoff (2011). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Models:	Financial Liberalization					Financial Liberalization					Financial Liberalization				
	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V
POSTcrisis_currency	-0.063*** [0.013]	-0.046*** [0.011]	-0.046*** [0.011]	-0.046*** [0.011]	-0.046*** [0.011]										
PREcrisis_currency	-0.040*** [0.011]	-0.020*** [0.009]	-0.020*** [0.009]	-0.020*** [0.009]	-0.020*** [0.009]										
POSTcrisis_stock						-0.025 [0.016]	-0.001 [0.012]	-0.001 [0.013]	-0.001 [0.013]	-0.001 [0.013]					
PREcrisis_stock						-0.013 [0.017]	0.010 [0.013]	0.010 [0.013]	0.010 [0.013]	0.010 [0.013]					
POSTcrisis_inflation											-0.086*** [0.019]	-0.070*** [0.014]	-0.070*** [0.015]	-0.070*** [0.015]	-0.070*** [0.015]
PREcrisis_inflation											-0.066*** [0.016]	-0.043*** [0.012]	-0.043*** [0.012]	-0.043*** [0.012]	-0.043*** [0.012]
Diff-in-diff	-0.023**	-0.026***	-0.026***	-0.026***	-0.026***	-0.012*	-0.011*	-0.011*	-0.011*	-0.011*	-0.020	-0.026*	-0.027*	-0.027*	-0.027*
P-value	0.017	0.004	0.004	0.004	0.005	0.088	0.053	0.057	0.055	0.059	0.205	0.065	0.066	0.068	0.070
N	14,986	14,986	14,986	14,986	14,986	11,661	11,661	11,661	11,661	11,661	15,000	15,000	15,000	15,000	15,000
Adj-R-sq	0.192	0.218	0.464	0.518	0.743	0.164	0.194	0.447	0.484	0.730	0.194	0.218	0.464	0.519	0.744
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CountryTime Trend															
Country x Reform FE															
Reform x Year FE															

Table B7b: Full sample: Difference-in-differences estimates for financial liberalization (with crisis episodes from Reinhart and Rogoff (2011) and separately with currency, stock market and inflation crises). The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis_x* is a binary dummy variable turning on during a financial (x=currency, stock market and inflation) crisis in the sample. *PREcrisis_x* is a binary dummy for the 5 years immediately preceding a financial crisis. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis_x* and *PREcrisis_x* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from Abiad et al. (2010) and Denk and Gomes (2017). Data on financial crises is obtained from Reinhart and Rogoff (2011). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis</i>	-0.048*** [0.010]	-0.043*** [0.009]	-0.044*** [0.009]	-0.044*** [0.009]	-0.044*** [0.009]
<i>PREcrisis</i>	-0.031*** [0.011]	-0.006 [0.009]	-0.006 [0.009]	-0.006 [0.009]	-0.006 [0.009]
Diff-in-diff	-0.016	-0.037***	-0.037***	-0.037***	-0.037***
P-value	0.147	0.000	0.000	0.001	0.001
N	18,430	18,430	18,430	18,430	18,430
Adj-R-sq	0.221	0.243	0.439	0.543	0.739
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table B8: **Full sample: Difference-in-differences estimates for financial liberalization (only with the reform dataset from [Abiad et al. \(2010\)](#))**. The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained from [Abiad et al. \(2010\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Models:	Financial Liberalization					Financial Liberalization				
	I	II	III	IV	V	I	II	III	IV	V
<i>POSTcrisis_banking</i>	-0.035*** [0.011]	-0.031*** [0.010]	-0.031*** [0.010]	-0.031*** [0.010]	-0.031*** [0.010]					
<i>PREcrisis_banking</i>	-0.026** [0.011]	-0.004 [0.010]	-0.004 [0.010]	-0.004 [0.010]	-0.004 [0.010]					
<i>POSTcrisis_debt</i>						-0.074*** [0.017]	-0.044** [0.017]	-0.044** [0.017]	-0.044** [0.017]	-0.044** [0.017]
<i>PREcrisis_debt</i>						-0.015 [0.020]	0.023 [0.016]	0.023 [0.016]	0.023 [0.017]	0.023 [0.017]
<i>POSTcrisis_currency</i>										
<i>PREcrisis_currency</i>						-0.054*** [0.011]	-0.049*** [0.011]	-0.049*** [0.011]	-0.049*** [0.011]	-0.049*** [0.011]
						-0.034*** [0.013]	-0.013 [0.011]	-0.013 [0.011]	-0.013 [0.011]	-0.013 [0.011]
						-0.021*	-0.036***	-0.036***	-0.036***	-0.036***
Diff-in-diff	-0.009	-0.027***	-0.027***	-0.027**	-0.027**	-0.059***	-0.066***	-0.066***	-0.066***	-0.066***
P-value	0.469	0.010	0.010	0.011	0.011	0.001	0.000	0.000	0.000	0.001
N	18,430	18,430	18,430	18,430	18,430	18,430	18,430	18,430	18,430	18,430
Adj-R-sq	0.219	0.242	0.438	0.542	0.738	0.220	0.243	0.438	0.542	0.738
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CountryTime Trend				Yes	Yes			Yes	Yes	Yes
Country x Reform FE		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Reform x Year FE			Yes	Yes	Yes		Yes	Yes	Yes	Yes

Table B9: **Full sample: Difference-in-differences estimates for financial liberalization (only with the reform dataset from Abiad et al. (2010) and separately with banking, sovereign debt and currency crises).** The table summarizes the estimation results with the specification in Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis_x* is a binary dummy variable turning on in the first 5 years after a financial (x=banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis_x* is a binary dummy for the 5 years immediately preceding a financial crisis. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis_x* and *PREcrisis_x* and p-values are reported underneath. Reform database is from Abiad et al. (2010). Data on financial crises is obtained from Laeven and Valencia (2018). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis_banking</i>	-0.021** [0.010]	-0.014* [0.008]	-0.014* [0.008]	-0.014 [0.008]	-0.014 [0.009]
<i>PREcrisis_banking</i>	-0.018* [0.010]	-0.001 [0.008]	-0.001 [0.008]	-0.001 [0.008]	-0.001 [0.008]
<i>POSTcrisis_debt</i>	-0.042** [0.018]	-0.025 [0.017]	-0.025 [0.017]	-0.025 [0.017]	-0.025 [0.017]
<i>PREcrisis_debt</i>	0.002 [0.021]	0.021 [0.017]	0.021 [0.017]	0.021 [0.017]	0.021 [0.017]
<i>POSTcrisis_currency</i>	-0.044*** [0.012]	-0.035*** [0.011]	-0.035*** [0.011]	-0.036*** [0.012]	-0.035*** [0.012]
<i>PREcrisis_currency</i>	-0.025** [0.012]	-0.009 [0.011]	-0.009 [0.012]	-0.009 [0.012]	-0.009 [0.012]
Diff-in-diff for Banking Crises	-0.003	-0.013	-0.013	-0.013	-0.013
P-value	0.800	0.139	0.141	0.146	0.147
Diff-in-diff for Sovereign Debt Crises	-0.044***	-0.045***	-0.045***	-0.045***	-0.045***
P-value	0.008	0.005	0.005	0.005	0.005
Diff-in-diff for Currency Crises	-0.020**	-0.026***	-0.026***	-0.026***	-0.026***
P-value	0.052	0.007	0.008	0.008	0.008
N	21,538	21,538	21,538	21,538	21,538
Adj-R-sq	0.178	0.200	0.475	0.534	0.747
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table B10: **Full sample: Difference-in-differences estimates for financial liberalization (simultaneous estimation with banking, sovereign debt and currency crises).** The table summarizes the estimation results with a specification à la Equation 1. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis_x* is a binary dummy variable turning on in the first 5 years after a financial (x=banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis_x* is a binary dummy for the 5 years immediately preceding a financial crisis. Robust standard errors are clustered at the country level and standard errors are reported in brackets. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis_x* and *PREcrisis_x* and p-values are reported underneath. Reform database is from Abiad et al. (2010) and Denk and Gomes (2017). Data on financial crises is obtained from Laeven and Valencia (2018). *p<0.1, **p<0.05, ***p<0.01.

Appendix C

Since the electoral competitiveness index in Cruz et al. (2016) is time-varying, it is possible that subsample construction via imposing a threshold on this index disrupts the country composition and leads to an unbalanced subsample where the observations for a given country might fall into different regime categories. That is why we alternatively take the average values of this index over time for each country and use this ranking of countries to divide the full sample into two similarly-proportioned subsamples. This means each country with its full time-series observations gets only into one of these democratic or autocratic subsamples. The updated results reported in Table C1 are very similar to those in Table 3.

As previously seen in Figure 2, the DPI dataset may not have a sufficiently high threshold for a country to be categorised as a democracy. Hence, we resort to another established dataset, namely Polity5, which provides some of the most commonly used regime-type indices with the widest coverage across countries and years (see Goldstone et al., 2010). Table C2 re-produces our results with Polity5 indices where we define a country to be a democracy if its index value is 5 or above in a particular year. As expected, there are now more of the autocratic and less of the democratic observations in our sample; but our main finding that democracies exhibit a larger tendency to intervene in financial markets after crises remains unchallenged.

In the same spirit as in Table C1, we report the balanced-sample results generated with Polity5 indices in Table C3 in which we restrict all observations of a given country to fall into a single subsample. Finally, in Table C4, we re-estimate the same specification over three different levels of democracy generated via Polity5 to illustrate that our estimates tend to get larger as a country gets more and more democratic. These tests provide assurance that democratic accountability is positively associated with government interventions in the aftermath of financial crises.

Dependent variable: Sample: Models:	Financial Liberalization									
	Democratic countries					Autocratic countries				
	I	II	III	IV	V	I	II	III	IV	V
POSTcrisis	-0.045*** [0.011]	-0.027*** [0.009]	-0.027*** [0.009]	-0.027*** [0.009]	-0.027*** [0.009]	-0.050** [0.020]	-0.044* [0.022]	-0.044* [0.023]	-0.044* [0.023]	-0.044* [0.024]
PREcrisis	-0.022 [0.013]	0.010 [0.010]	0.010 [0.010]	0.010 [0.010]	0.010 [0.010]	-0.040** [0.014]	-0.028 [0.016]	-0.028 [0.017]	-0.028 [0.017]	-0.028 [0.017]
Diff-in-diff	-0.023**	-0.038***	-0.038***	-0.037***	-0.038***	-0.010	-0.016	-0.016	-0.016	-0.016
P-value	0.041	0.001	0.001	0.001	0.001	0.516	0.272	0.287	0.279	0.293
N	16,974	16,974	16,974	16,974	16,974	4,333	4,333	4,333	4,333	4,333
Adj-R-sq	0.168	0.195	0.483	0.513	0.742	0.435	0.442	0.598	0.618	0.789
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes			Yes	Yes	Yes		
Reform FE	Yes	Yes				Yes	Yes			
Year FE	Yes	Yes				Yes	Yes			
CountryTime Trend				Yes					Yes	
Country x Reform FE		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes

Table C1: Democracy vs. autocracy (balanced sample): Difference-in-differences estimates for financial liberalization. The table summarizes the estimation results over two subsamples with the specification in Equation 1. A country is categorised as *democratic* if its (time-series) average of executive index of electoral competitiveness is ranked in the upper 80% across all countries in our sample; otherwise it is categorised as *autocratic*. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Sample: Models:	Financial Liberalization									
	Democratic countries					Autocratic countries				
	I	II	III	IV	V	I	II	III	IV	V
POSTcrisis	-0.045*** [0.011]	-0.022** [0.009]	-0.022** [0.009]	-0.022** [0.009]	-0.022** [0.009]	-0.053*** [0.015]	-0.037** [0.015]	-0.037** [0.016]	-0.037** [0.016]	-0.037** [0.016]
PREcrisis	-0.015 [0.015]	0.018* [0.010]	0.018* [0.010]	0.018* [0.010]	0.018* [0.010]	-0.043*** [0.016]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]
Diff-in-diff	-0.030***	-0.039***	-0.039***	-0.039***	-0.039***	-0.011	-0.024*	-0.024*	-0.024*	-0.024*
P-value	0.009	0.001	0.001	0.001	0.001	0.489	0.063	0.067	0.069	0.074
N	13,362	13,362	13,362	13,362	13,362	7,644	7,644	7,644	7,644	7,644
Adj-R-sq	0.180	0.207	0.475	0.521	0.753	0.357	0.372	0.517	0.653	0.776
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes			Yes	Yes	Yes		
Reform FE	Yes	Yes				Yes	Yes			
Year FE	Yes	Yes				Yes	Yes			
CountryTime Trend				Yes					Yes	
Country x Reform FE		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes

Table C2: Democracy vs. autocracy (constructed via Polity5): Difference-in-differences estimates for financial liberalization. The table summarizes the estimation results over two subsamples with the specification in Equation 1. A country is categorised as *democratic* if its Polity5 index value is 5 or above in a particular year; otherwise it is categorised as *autocratic*. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from the Center for Systemic Peace. Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Sample: Models:	Financial Liberalization									
	Democratic countries					Autocratic countries				
	I	II	III	IV	V	I	II	III	IV	V
POSTcrisis	-0.045*** [0.011]	-0.029*** [0.009]	-0.020*** [0.009]	-0.029*** [0.009]	-0.029*** [0.009]	-0.044** [0.021]	-0.038* [0.021]	-0.038 [0.022]	-0.038* [0.022]	-0.038 [0.022]
PREcrisis	-0.017 [0.013]	0.011 [0.009]	0.011 [0.009]	0.011 [0.009]	0.011 [0.009]	-0.059*** [0.020]	-0.033* [0.017]	-0.033* [0.018]	-0.033* [0.018]	-0.033* [0.018]
Diff-in-diff	-0.028***	-0.041***	-0.041***	-0.041***	-0.041***	0.015	-0.004	-0.004	-0.004	-0.004
P-value	0.010	0.000	0.000	0.000	0.000	0.432	0.775	0.782	0.778	0.785
N	16,953	16,953	16,953	16,953	16,953	4,242	4,242	4,242	4,242	4,242
Adj-R-sq	0.179	0.203	0.497	0.520	0.748	0.328	0.342	0.482	0.592	0.750
Clustering	Country	Country	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes			Yes	Yes	Yes		
Reform FE	Yes	Yes				Yes	Yes		Yes	
Year FE	Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend				Yes					Yes	
Country x Reform FE		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes

Table C3: **Democracy vs. autocracy (balanced sample constructed via Polity5): Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results over two subsamples with the specification in Equation 1. A country is categorised as *democratic* if its (time-series) average of Polity5 index value is ranked in the upper 80% across all countries in our sample; otherwise it is categorised as *autocratic*. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from the Center for Systemic Peace. Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Sample: Models:	Financial Liberalization					
	Democracy		Semi-Democracy		Autocracy	
	I	V	I	V	I	V
<i>POSTcrisis</i>	-0.048*** [0.012]	-0.018 [0.011]	-0.034* [0.019]	-0.020 [0.017]	-0.043** [0.017]	-0.028* [0.016]
<i>PREcrisis</i>	0.001 [0.021]	0.022 [0.017]	-0.009 [0.017]	0.010 [0.013]	-0.040** [0.017]	-0.020 [0.017]
Diff-in-diff	-0.049***	-0.041**	-0.026**	-0.030**	-0.003	-0.008
P-value	0.009	0.024	0.024	0.011	0.874	0.577
N	8,831	8,831	6,533	6,533	5,642	5,642
Adj-R-sq	0.214	0.780	0.229	0.764	0.400	0.799
Clustering	Country	Country	Country	Country	Country	Country
Country FE	Yes		Yes		Yes	
Reform FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
CountryTime Trend		Yes		Yes		Yes
Country x Reform FE		Yes		Yes		Yes
Reform x Year FE		Yes		Yes		Yes

Table C4: Democracy, semi-democracy and autocracy (multiple subsamples constructed via Polity5): Difference-in-differences estimates for financial liberalization. The table summarizes the estimation results over three subsamples with the specification in Equation 1. A country is categorised as a *democracy* if its (time-series) average of Polity5 index value is ranked in the upper 33% across all countries in our sample; categorised as a *semi-democracy* if its (time-series) average of Polity5 index value is ranked in the middle 33% across all countries in our sample; otherwise it is categorised as an *autocracy*. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from Abiad et al. (2010) and Denk and Gomes (2017). Data on financial crises is obtained from Laeven and Valencia (2018). Political variables are obtained from the Center for Systemic Peace. Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Appendix D

Does political heterogeneity matter in general?

It is possible that *TermLimit* variable proxies an unobserved characteristic of the policymakers or the political setting of the country. In order to reduce the omitted political variable concerns, we input various dimensions of the politics in these countries as controls in Equation 2. This rich set of additional controls range from the political ideology of the executive to the number of years they spent in the office and the strength or fractionalization of their government.⁴⁴

Table D2 presents the results updating the estimated specification step-by-step from partial to the full set of additional controls.⁴⁵ The only variable that consistently comes out as significant is the right-wing ideology of the executive leader, which unsurprisingly predicts a positive influence on financial liberalization. While it is clear that none of these additional controls lead to a noticeable change in our main findings, the significant coefficient on the right-wing ideology gives us the chance to benchmark our main coefficient of interest. The “additional” effect of a term-limit on post-crisis policymaking is *more than three times larger* than the baseline effect of a political leader having right-wing (compared to a more centric) ideology. Given the theoretical importance of ideology (and party affiliation) in executive policymaking, this corresponds to a truly substantial effect and implies that simply comparing our estimates to sample averages may not be ideal in this setting.⁴⁶

⁴⁴See the Data section for the exact definitions. Importantly, we abstain from controlling for any country-level economic or financial variables in our estimations as these variables themselves might be strongly influenced by financial crises and could thus be categorised as “bad controls” (see Angrist and Pischke, 2008, p. 64).

⁴⁵Table D3 re-estimates the Table D2 with a fully saturated model.

⁴⁶See some of the recent surveys, such as Potrafke (2018) and the references therein, on the role of government ideology in economic policymaking.

Does political heterogeneity matter in the aftermath of crises?

Despite including them as stand-alone controls in our estimations, one could still argue that these political variables may matter exactly at the time of the crisis. Hence, our diff-in-diff setting may be violated by the potential effect of an omitted variable conditional on the occurrence of a financial crisis. Such a concern necessitates the inclusion of these controls in interactions with both post-crisis and pre-crisis dummies.

Table D4 updates the results where each political variable is interacted in the same way as *TermLimits* in addition to being included in the baseline specification.⁴⁷ If anything, the effect of a term-limit increases substantially when these controls are added to the estimation. The largest jump in the coefficient size comes from the switch between first and second columns where we include the presidential nature of the democracy in interaction with the post-crisis dummy. There is some evidence that presidential systems react differently to financial crises; but the direction of the effect is the opposite of what one might consider as a threat to our identification strategy. Presidents in general seem to react more positively to crises and hence this seems to raise the negative impact of term-limits once we take into account this positive relationship.

Can the results be driven by unobservables (i.e., omitted variables)?

Despite the fact that we control for a variety of political factors (both in the baseline and in interaction), there is a chance that unobservable factors may drive our findings, particularly the estimated coefficient on the interaction between *POSTcrisis* and *TermLimit*. Thus, we follow the method proposed by Oster (2019) to shed light on the importance of unobservables in Table D6, where the first column is based on the model with no controls as in Table 4 and the second one is based on the model with full political controls as in Table D4.

The last column in Table D4 then presents the estimation bounds where we define R_{max}

⁴⁷Table D5 re-estimates the Table D4 with a fully saturated model.

upper bound as 1.3 times the R-squared in specifications that control for observables following Oster (2019). The bottom row presents *Oster’s Delta*, which indicates the degree of selection on unobservables relative to observables that would be needed to fully explain our results by omitted variable bias. The high delta value of 5.7 is reassuring and, given the wide range of controls we include in our models, it seems implausible that unobserved factors are up to 6 times more important than the observables included in our specification with full controls.⁴⁸

Can extreme ideological shifts after crises play a role?

Recent literature has emphasised the importance of the rise in extreme politics in the aftermath of financial crises (Funke, Schularick, and Trebesch, 2016; Doerr, Gissler, Peydró, and Voth, 2020; Gyöngyösi and Verner, 2020). If the public discontent with crises leads ideologically more extreme parties to come to power, this may explain the interventionary policy stance we report in this paper. Although we control for the right and left-wing ideology of the executive leader in Tables D2, D3, D4 and D5, these variables fail to take into account the *intensity* of the ideology.

In order to mitigate this concern, we first extract all the party names reported in DPI that corresponds to each country-year observation in our sample.⁴⁹ We then add separate dummies in our main specifications for those country-year observations when a particular party was in executive power. In other words, we estimate a within-party specification in order to make sure that the effect of an extreme party coming to power in the aftermath of a crisis is automatically absorbed by these party dummies conditional on the assumption that party ideology is fixed over time. Tables D7 and D8 re-estimate Tables 4 and D4 by including these party fixed-effects and confirm that our findings remain qualitatively the same and thus are unlikely to be explained by the rise in extreme politics after crises.⁵⁰

⁴⁸The rule of thumb to be able to argue that unobservables cannot fully explain the treatment effect is for Oster’s delta to be over the value of one.

⁴⁹Our sample contains more than 250 different political parties.

⁵⁰In Tables D9 and D10, we relax the assumption of the ideology of each party being fixed over time and estimate a model with fixed effects at the levels of interaction between parties and decades in our sample.

Are countries with term-limits structurally different?

In order to make sure that we are not picking up any unobserved heterogeneity between countries that have term-limits in their constitutions and those who don't (such as most parliamentary democracies), we drop the countries whose leaders have never experienced term-limits during our sample period and re-estimate the Equation 2 for this subsample.⁵¹ Table D11 reports the results. We naturally end up with a much smaller set of countries when we focus solely on those with a term-limit experience.⁵² Despite the fact that small sample size magnifies standard errors, our coefficient estimates are still similar to those in Table 4 and our main finding that term-limits have negative effects on post-crisis financial liberalization remains robust at conventional levels of statistical significance.

We again find similar results for our main coefficients of interest.

⁵¹Notice that this is a conservative approach as we are likely to drop also those countries that actually had term-limits written in their constitutions but they never became binding since the country's incumbent political leader never got re-elected.

⁵²These countries are Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, Guatemala, Hong Kong, Jordan, South Korea, Morocco, Mexico, Peru, Philippines, Paraguay, El Salvador, Uruguay, United States and Venezuela.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis x TermLimit</i>	-0.044** [0.019]	-0.063*** [0.020]	-0.062*** [0.020]	-0.063*** [0.021]	-0.063*** [0.021]
<i>PREcrisis x TermLimit</i>	0.034 [0.027]	0.019 [0.027]	0.019 [0.028]	0.019 [0.028]	0.019 [0.028]
<i>POSTcrisis</i>	-0.037*** [0.011]	-0.009 [0.009]	-0.009 [0.009]	-0.009 [0.009]	-0.009 [0.009]
<i>PREcrisis</i>	-0.023 [0.014]	0.016 [0.010]	0.016 [0.010]	0.016 [0.010]	0.016 [0.010]
<i>TermLimit</i>	-0.049 [0.038]	-0.031 [0.034]	-0.032 [0.034]	-0.031 [0.034]	-0.032 [0.034]
Diff-in-diff for Term Limit	-0.078***	-0.082***	-0.082***	-0.082***	-0.082***
P-value	0.001	0.000	0.000	0.000	0.000
Diff-in-diff	-0.015	-0.025**	-0.025**	-0.025**	-0.025**
P-value	0.234	0.033	0.035	0.036	0.037
N	15,359	15,359	15,359	15,359	15,359
Adj-R-sq	0.166	0.193	0.460	0.521	0.741
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table D1: **Term limits in democracies (balanced sample): Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results with the specification in Equation 2. Sample includes only democratic countries whose (time-series) average of executive index of electoral competitiveness is ranked in the upper 80% across all countries in our sample. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

<i>Dependent variable:</i>	<i>Financial Liberalization</i>				
<i>Models:</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
<i>POSTcrisis x TermLimit</i>	-0.066*** [0.024]	-0.066*** [0.024]	-0.066*** [0.024]	-0.067*** [0.024]	-0.067*** [0.024]
<i>PREcrisis x TermLimit</i>	0.017 [0.029]	0.016 [0.029]	0.016 [0.029]	0.017 [0.029]	0.016 [0.028]
<i>POSTcrisis</i>	-0.010 [0.009]	-0.010 [0.009]	-0.010 [0.009]	-0.009 [0.009]	-0.009 [0.009]
<i>PREcrisis</i>	0.018* [0.010]	0.020* [0.010]	0.020* [0.010]	0.019* [0.010]	0.019* [0.010]
<i>TermLimit</i>	-0.010 [0.036]	-0.005 [0.037]	-0.004 [0.036]	-0.005 [0.036]	-0.003 [0.036]
<i>Right</i>	0.027** [0.012]	0.025** [0.011]	0.025** [0.011]	0.026** [0.012]	0.026** [0.012]
<i>Left</i>	0.014 [0.011]	0.012 [0.011]	0.012 [0.011]	0.010 [0.011]	0.010 [0.011]
<i>Presidential</i>		-0.029 [0.028]	-0.030 [0.028]	-0.032 [0.029]	-0.031 [0.029]
<i>Parliamentary</i>		0.028 [0.030]	0.028 [0.030]	0.027 [0.031]	0.028 [0.030]
<i>OfficeYears</i>			0.000 [0.001]	0.000 [0.001]	0.000 [0.001]
<i>YearsLeft</i>			0.001 [0.001]	0.001 [0.001]	0.001 [0.001]
<i>HerfGov</i>				0.638 [1.118]	0.461 [1.284]
<i>GovFrac</i>				0.596 [1.107]	0.429 [1.268]
<i>GovShare</i>					-0.010 [0.036]
<i>Checks</i>					-0.002 [0.003]
Diff-in-diff for Term Limit	-0.082***	-0.081***	-0.082***	-0.084***	-0.083***
P-value	0.000	0.000	0.000	0.000	0.000
Diff-in-diff	-0.028**	-0.030**	-0.029**	-0.028**	-0.029**
P-value	0.024	0.016	0.018	0.022	0.022
N	14,725	14,725	14,725	14,725	14,725
Adj-R-sq	0.191	0.191	0.191	0.191	0.191
Clustering	Country	Country	Country	Country	Country
<i>Country FE</i>	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes
<i>Reform FE</i>	Yes	Yes	Yes	Yes	Yes
<i>CountryTime Trend</i>	Yes	Yes	Yes	Yes	Yes

Table D2: **Term limits in democracies (with political controls): Difference-in-differences estimates for financial liberalization.** See the notes in Table 4. *p<0.1, **p<0.05, ***p<0.01.

<i>Dependent variable:</i>	<i>Financial Liberalization</i>				
<i>Models:</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
<i>POSTcrisis x TermLimit</i>	-0.066*** [0.025]	-0.066*** [0.025]	-0.066*** [0.025]	-0.067*** [0.025]	-0.067*** [0.025]
<i>PREcrisis x TermLimit</i>	0.017 [0.030]	0.015 [0.029]	0.016 [0.030]	0.017 [0.029]	0.016 [0.029]
<i>POSTcrisis</i>	-0.010 [0.009]	-0.010 [0.009]	-0.010 [0.009]	-0.009 [0.009]	-0.010 [0.009]
<i>PREcrisis</i>	0.018* [0.011]	0.020* [0.010]	0.020* [0.010]	0.019* [0.010]	0.019* [0.010]
<i>TermLimit</i>	-0.010 [0.036]	-0.006 [0.038]	-0.005 [0.037]	-0.005 [0.037]	-0.004 [0.037]
<i>Right</i>	0.026** [0.012]	0.025** [0.012]	0.025** [0.012]	0.026** [0.012]	0.026** [0.012]
<i>Left</i>	0.014 [0.012]	0.011 [0.012]	0.011 [0.012]	0.010 [0.011]	0.010 [0.011]
<i>Presidential</i>		-0.029 [0.028]	-0.030 [0.029]	-0.032 [0.030]	-0.031 [0.030]
<i>Parliamentary</i>		0.028 [0.030]	0.028 [0.031]	0.027 [0.031]	0.028 [0.031]
<i>OfficeYears</i>			0.000 [0.001]	0.000 [0.001]	0.000 [0.001]
<i>YearsLeft</i>			0.001 [0.001]	0.001 [0.001]	0.001 [0.001]
<i>HerfGov</i>				0.605 [1.146]	0.467 [1.317]
<i>GovFrac</i>				0.565 [1.135]	0.435 [1.301]
<i>GovShare</i>					-0.008 [0.037]
<i>Checks</i>					-0.002 [0.003]
Diff-in-diff for Term Limit	-0.082***	-0.081***	-0.082***	-0.084***	-0.083***
P-value	0.000	0.001	0.001	0.001	0.001
Diff-in-diff	-0.028**	-0.030**	-0.029**	-0.028**	-0.029**
P-value	0.028	0.019	0.020	0.025	0.025
N	14,725	14,725	14,725	14,725	14,725
Adj-R-sq	0.749	0.749	0.749	0.749	0.749
Clustering	Country	Country	Country	Country	Country
<i>CountryTime Trend</i>	Yes	Yes	Yes	Yes	Yes
<i>Country x Reform FE</i>	Yes	Yes	Yes	Yes	Yes
<i>Reform x Year FE</i>	Yes	Yes	Yes	Yes	Yes

Table D3: Term limits in democracies (with political controls and fully saturated specification): Difference-in-differences³⁴ estimates for financial liberalization. See the notes in Table 4. *p<0.1, **p<0.05, ***p<0.01.

<i>Dependent variable:</i>	<i>Financial Liberalization</i>				
<i>Models:</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
<i>POSTcrisis x TermLimit</i>	-0.067*** [0.025]	-0.092*** [0.026]	-0.103*** [0.027]	-0.106*** [0.027]	-0.105*** [0.026]
<i>PREcrisis x TermLimit</i>	0.019 [0.029]	0.014 [0.029]	0.009 [0.030]	0.009 [0.030]	0.010 [0.028]
<i>POSTcrisis x Right</i>	0.008 [0.017]	0.012 [0.016]	0.010 [0.016]	0.010 [0.016]	0.010 [0.016]
<i>POSTcrisis x Left</i>	0.000 [0.020]	0.003 [0.019]	0.000 [0.019]	0.000 [0.019]	0.003 [0.018]
<i>POSTcrisis x Presidential</i>		0.055 [0.041]	0.060 [0.038]	0.063* [0.037]	0.065* [0.037]
<i>POSTcrisis x Parliamentary</i>		0.022 [0.040]	0.017 [0.038]	0.020 [0.037]	0.020 [0.037]
<i>POSTcrisis x OfficeYears</i>			-0.002 [0.001]	-0.002 [0.001]	-0.002* [0.001]
<i>POSTcrisis x YearsLeft</i>			-0.003 [0.002]	-0.002 [0.002]	-0.003 [0.002]
<i>POSTcrisis x HerfGov</i>				1.428 [1.932]	2.250 [2.038]
<i>POSTcrisis x GovFrac</i>				1.415 [1.909]	2.223 [2.013]
<i>POSTcrisis x GovShare</i>					0.057 [0.042]
<i>POSTcrisis x Checks</i>					0.002 [0.004]
<i>PREcrisis interactions</i>	Yes	Yes	Yes	Yes	Yes
<i>Baseline controls</i>	Yes	Yes	Yes	Yes	Yes
Diff-in-diff for Term Limit	-0.086***	-0.105***	-0.112***	-0.115***	-0.115***
P-value	0.000	0.000	0.000	0.000	0.000
N	14,725	14,725	14,725	14,725	14,725
Adj-R-sq	0.191	0.191	0.191	0.191	0.191
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes	Yes	Yes
CountryTime Trend	Yes	Yes	Yes	Yes	Yes

Table D4: Term limits in democracies (with interacted political controls): Difference-in-differences estimates for financial liberalization. See the notes in Table 4. *p<0.1, **p<0.05, ***p<0.01.

<i>Dependent variable:</i>	<i>Financial Liberalization</i>				
<i>Models:</i>	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
<i>POSTcrisis x TermLimit</i>	-0.066** [0.026]	-0.091*** [0.026]	-0.101*** [0.027]	-0.105*** [0.026]	-0.106*** [0.026]
<i>PREcrisis x TermLimit</i>	0.017 [0.029]	0.014 [0.029]	0.014 [0.030]	0.014 [0.029]	0.014 [0.029]
<i>POSTcrisis x Right</i>	0.004 [0.017]	0.010 [0.016]	0.008 [0.016]	0.008 [0.016]	0.010 [0.016]
<i>POSTcrisis x Left</i>	-0.004 [0.019]	0.001 [0.019]	-0.002 [0.019]	-0.002 [0.018]	-0.001 [0.017]
<i>POSTcrisis x Presidential</i>		0.046 [0.037]	0.049 [0.034]	0.054 [0.034]	0.057* [0.032]
<i>POSTcrisis x Parliamentary</i>		0.012 [0.037]	0.007 [0.035]	0.011 [0.035]	0.012 [0.034]
<i>POSTcrisis x OfficeYears</i>			-0.002 [0.001]	-0.002 [0.001]	-0.002 [0.001]
<i>POSTcrisis x YearsLeft</i>			-0.002 [0.002]	-0.002 [0.002]	-0.003 [0.002]
<i>POSTcrisis x HerfGov</i>				0.943 [1.855]	1.589 [1.983]
<i>POSTcrisis x GovFrac</i>				0.932 [1.833]	1.567 [1.958]
<i>POSTcrisis x GovShare</i>					0.049 [0.044]
<i>POSTcrisis x Checks</i>					0.001 [0.004]
<i>All baseline controls</i>	Yes	Yes	Yes	Yes	Yes
Diff-in-diff for Term Limit	-0.083***	-0.106***	-0.115***	-0.119***	-0.120***
P-value	0.000	0.000	0.000	0.000	0.000
N	14,725	14,725	14,725	14,725	14,725
Adj-R-sq	0.749	0.749	0.749	0.749	0.749
Clustering	Country	Country	Country	Country	Country
<i>CountryTime Trend</i>	Yes	Yes	Yes	Yes	Yes
<i>Country x Reform FE</i>	Yes	Yes	Yes	Yes	Yes
<i>Reform x Year FE</i>	Yes	Yes	Yes	Yes	Yes

Table D5: **Term limits in democracies (with interacted political controls and fully saturated specification): Difference-in-differences estimates for financial liberalization.** See the notes in Table 4. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable: Models:	Financial Liberalization		
	No controls	Full controls	Oster test
<i>POSTcrisis x TermLimit</i>	-0.067*** [0.025]	-0.105*** [0.026]	
<i>PREcrisis x TermLimit</i>	Yes	Yes	
<i>Political interactions</i>	No	Yes	
<i>Baseline political controls</i>	No	Yes	
Diff-in-diff for Term Limit	-0.081***	-0.115***	
P-value	0.000	0.000	
N	14,725	14,725	
R-sq	0.199	0.200	
Clustering	Country	Country	
Country FE	Yes	Yes	
Year FE	Yes	Yes	
Reform FE	Yes	Yes	
CountryTime Trend	Yes	Yes	
Bounds on the treatment effect ($\delta=1$, $Rmax=1.3*R$)			(-0.067, -0.105)
Treatment effect excludes 0			Yes
Delta ($Rmax=1.3*R$)			5.70

Table D6: **Term limits in democracies (with and without interacted political controls): Omitted variables test for financial liberalization.** Samples include only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six. Bounds on the *POSTcrisis x TermLimit* effect are calculated using Stata code `psacalc`, which calculates estimates of treatment effects and relative degree of selection in linear models as proposed in [Oster \(2019\)](#). Delta, δ , calculates an estimate of the proportional degree of selection given a maximum value of the R-squared. $Rmax$ specifies the maximum R-squared which would result if all unobservables were included in the regression. We define $Rmax$ upper bound as 1.3 times the R-squared from the main specification that controls for all observables. *Oster's Delta* indicates the degree of selection on unobservables relative to observables that would be needed to fully explain our results by omitted variable bias. Robust standard errors are clustered at the country level and reported in brackets. Reform database is from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

<i>Dependent variable:</i> <i>Models:</i>	<i>Financial Liberalization</i>				
	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
<i>POSTcrisis x TermLimit</i>	-0.023 [0.016]	-0.050*** [0.018]	-0.049*** [0.018]	-0.050*** [0.019]	-0.049*** [0.019]
<i>PREcrisis x TermLimit</i>	0.053* [0.027]	0.032 [0.033]	0.032 [0.034]	0.032 [0.034]	0.032 [0.034]
<i>POSTcrisis</i>	-0.042*** [0.012]	-0.008 [0.009]	-0.008 [0.009]	-0.008 [0.009]	-0.008 [0.009]
<i>PREcrisis</i>	-0.022 [0.015]	0.018* [0.010]	0.017* [0.010]	0.018* [0.010]	0.017* [0.010]
<i>TermLimit</i>	-0.036 [0.030]	-0.008 [0.030]	-0.008 [0.030]	-0.008 [0.030]	-0.008 [0.030]
Diff-in-diff for Term Limit	-0.076***	-0.081***	-0.081***	-0.081***	-0.081***
P-value	0.006	0.006	0.006	0.007	0.007
Diff-in-diff	-0.02*	-0.026**	-0.026**	-0.026**	-0.026**
P-value	0.090	0.029	0.030	0.032	0.033
N	15,598	15,598	15,598	15,598	15,598
Adj-R-sq	0.173	0.190	0.436	0.543	0.753
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
Party FE	Yes	Yes	Yes	Yes	Yes
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table D7: **Term limits in democracies (with political party dummies): Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results with the specification in Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis x TermLimit</i>	-0.045** [0.019]	-0.064*** [0.021]	-0.077*** [0.022]	-0.078*** [0.022]	-0.072*** [0.021]
<i>PREcrisis x TermLimit</i>	0.039 [0.028]	0.030 [0.029]	0.024 [0.031]	0.027 [0.031]	0.028 [0.028]
<i>POSTcrisis x Right</i>	-0.006 [0.020]	-0.003 [0.019]	-0.005 [0.018]	-0.009 [0.018]	-0.008 [0.017]
<i>POSTcrisis x Left</i>	-0.004 [0.021]	0.002 [0.020]	-0.002 [0.019]	-0.009 [0.017]	-0.007 [0.018]
<i>POSTcrisis x Presidential</i>		0.036 [0.043]	0.044 [0.041]	0.044 [0.040]	0.036 [0.041]
<i>POSTcrisis x Parliamentary</i>		0.005 [0.040]	0.003 [0.038]	0.014 [0.036]	0.005 [0.038]
<i>POSTcrisis x OfficeYears</i>			-0.002 [0.001]	-0.002 [0.001]	-0.002 [0.001]
<i>POSTcrisis x YearsLeft</i>			-0.002 [0.003]	-0.003 [0.003]	-0.003 [0.002]
<i>POSTcrisis x HerfGov</i>				2.091 [1.508]	3.333* [1.684]
<i>POSTcrisis x GovFrac</i>				2.028 [1.479]	3.230* [1.652]
<i>POSTcrisis x GovShare</i>					0.079 [0.052]
<i>POSTcrisis x Checks</i>					0.010** [0.004]
<i>PREcrisis interactions</i>	Yes	Yes	Yes	Yes	Yes
<i>Baseline controls</i>	Yes	Yes	Yes	Yes	Yes
Diff-in-diff for Term Limit	-0.084***	-0.094***	-0.101***	-0.104***	-0.100***
P-value	0.002	0.000	0.000	0.000	0.000
N	14,683	14,683	14,683	14,683	14,683
Adj-R-sq	0.190	0.190	0.190	0.190	0.189
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes
CountryTime Trend	Yes	Yes	Yes	Yes	Yes

Table D8: Term limits in democracies (with political party dummies and interacted political controls): Difference-in-differences estimates for financial liberalization. See the notes in Table 4. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis x TermLimit</i>	-0.025 [0.017]	-0.047*** [0.017]	-0.047*** [0.017]	-0.047*** [0.017]	-0.047*** [0.017]
<i>PREcrisis x TermLimit</i>	0.061** [0.029]	0.043 [0.031]	0.043 [0.032]	0.044 [0.032]	0.044 [0.032]
<i>POSTcrisis</i>	-0.018** [0.008]	-0.007 [0.008]	-0.007 [0.008]	-0.007 [0.008]	-0.007 [0.008]
<i>PREcrisis</i>	0.002 [0.009]	0.011 [0.011]	0.011 [0.011]	0.011 [0.011]	0.011 [0.011]
<i>TermLimit</i>	-0.012 [0.020]	-0.006 [0.022]	-0.006 [0.022]	-0.007 [0.023]	-0.007 [0.023]
Diff-in-diff for Term Limit	-0.087**	-0.090**	-0.090**	-0.090**	-0.090**
P-value	0.020	0.017	0.018	0.019	0.020
Diff-in-diff	-0.020**	-0.019*	-0.019*	-0.019*	-0.019*
P-value	0.026	0.060	0.062	0.066	0.068
N	15,598	15,598	15,598	15,598	15,598
Adj-R-sq	0.192	0.190	0.439	0.548	0.761
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes		
Reform FE	Yes	Yes			
Year FE	Yes	Yes		Yes	
Party x Decade FE	Yes	Yes	Yes	Yes	Yes
CountryTime Trend		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes
Reform x Year FE			Yes		Yes

Table D9: **Term limits in democracies (with political party-decade dummies): Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results with the specification in Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#). Data on financial crises is obtained from [Laeven and Valencia \(2018\)](#). Political variables are obtained from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	Financial Liberalization				
Models:	I	II	III	IV	V
<i>POSTcrisis x TermLimit</i>	-0.049*** [0.018]	-0.057*** [0.019]	-0.061*** [0.019]	-0.061*** [0.018]	-0.061*** [0.018]
<i>PREcrisis x TermLimit</i>	0.054* [0.029]	0.053* [0.030]	0.053* [0.031]	0.053* [0.030]	0.053* [0.027]
<i>POSTcrisis x Right</i>	-0.006 [0.018]	0.000 [0.019]	0.001 [0.019]	0.000 [0.019]	0.001 [0.019]
<i>POSTcrisis x Left</i>	-0.001 [0.017]	0.007 [0.019]	0.005 [0.019]	0.006 [0.019]	0.010 [0.019]
<i>POSTcrisis x Presidential</i>		-0.016 [0.028]	-0.013 [0.027]	-0.014 [0.026]	-0.017 [0.026]
<i>POSTcrisis x Parliamentary</i>		-0.039 [0.029]	-0.039 [0.027]	-0.034 [0.026]	-0.042* [0.025]
<i>POSTcrisis x OfficeYears</i>			-0.002 [0.001]	-0.002 [0.001]	-0.002 [0.001]
<i>POSTcrisis x YearsLeft</i>			0.003 [0.002]	0.003 [0.002]	0.002 [0.002]
<i>POSTcrisis x HerfGov</i>				2.359* [1.265]	3.780** [1.507]
<i>POSTcrisis x GovFrac</i>				2.308* [1.241]	3.694** [1.479]
<i>POSTcrisis x GovShare</i>					0.091** [0.041]
<i>POSTcrisis x Checks</i>					0.008** [0.004]
<i>PREcrisis interactions</i>	Yes	Yes	Yes	Yes	Yes
<i>Baseline controls</i>	Yes	Yes	Yes	Yes	Yes
Diff-in-diff for Term Limit	-0.103***	-0.110***	-0.114***	-0.114***	-0.113***
P-value	0.004	0.002	0.002	0.002	0.001
N	14,683	14,683	14,683	14,683	14,683
Adj-R-sq	0.189	0.189	0.189	0.188	0.188
Clustering	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes	Yes	Yes
Party x Decade FE	Yes	Yes	Yes	Yes	Yes
CountryTime Trend	Yes	Yes	Yes	Yes	Yes

Table D10: Term limits in democracies (with political party-decade dummies and interacted political controls): Difference-in-differences estimates for financial liberalization. See the notes in Table 4. *p<0.1, **p<0.05, ***p<0.01.

<i>Dependent variable: Models:</i>	<i>Financial Liberalization</i>		
	<i>I</i>	<i>II</i>	<i>III</i>
<i>POSTcrisis x TermLimit</i>	-0.037 [0.023]	-0.055* [0.030]	-0.017 [0.028]
<i>PREcrisis x TermLimit</i>	0.019 [0.029]	0.021 [0.033]	0.042 [0.029]
<i>POSTcrisis</i>	-0.014 [0.026]	0.005 [0.025]	-6.821*** [2.182]
<i>PREcrisis</i>	0.020 [0.022]	0.027 [0.026]	-6.804** [2.737]
<i>TermLimit</i>	-0.047 [0.030]	-0.020 [0.040]	-0.042 [0.037]
Diff-in-diff for Term Limit	-0.055*	-0.076*	-0.058
P-value	0.096	0.077	0.143
N	3,332	3,220	3,220
Adj-R-sq	0.301	0.301	0.301
Clustering	Country	Country	Country
<i>Country FE</i>	Yes	Yes	Yes
<i>Reform FE</i>	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes
<i>CountryTime Trend</i>	Yes	Yes	Yes
<i>Baseline political controls</i>	No	Yes	Yes
<i>Interacted political controls</i>	No	No	Yes

Table D11: **Term limits in democracies (countries with term-limit experience): Difference-in-differences estimates for financial liberalization.** The table summarizes the estimation results with the specification in Equation 2. Sample includes only democratic countries who had at least one term-limited political leader during our sample period. Dependent variable is *Financial Liberalization* varying over countries, years and reform areas. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Reforms are from [Abiad et al. \(2010\)](#) and [Denk and Gomes \(2017\)](#); financial crises from [Laeven and Valencia \(2018\)](#) and political variables from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01

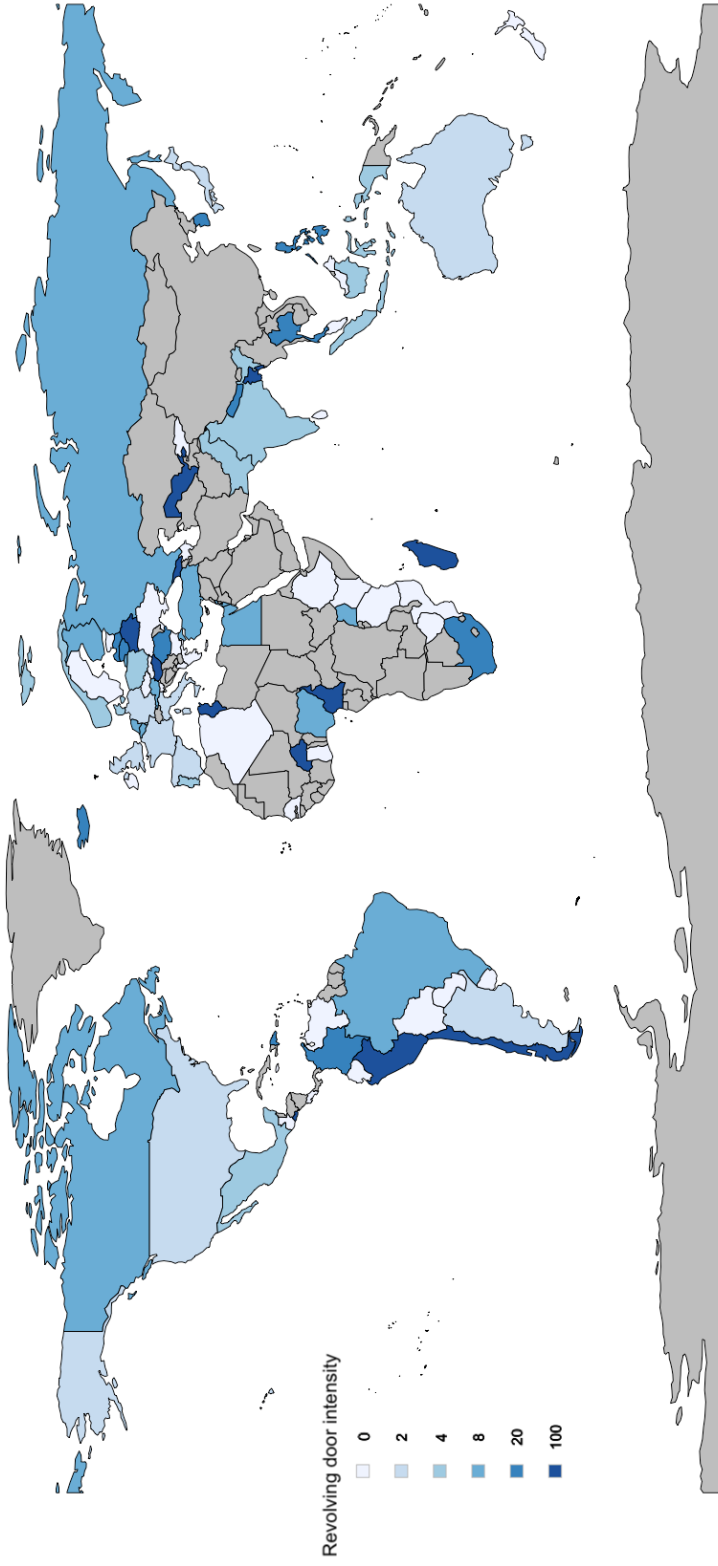


Fig. E1. **Revolving doors between financial and political institutions across the globe: Continuous version.** The figure maps each country depending on the fraction of its politically-connected banks which is the number of banks with at least one former politician on the board of directors divided by the number of banks for which there are data on board members in Bankscope as of year 2006. The measures are obtained from [Braun and Raddatz \(2010\)](#).

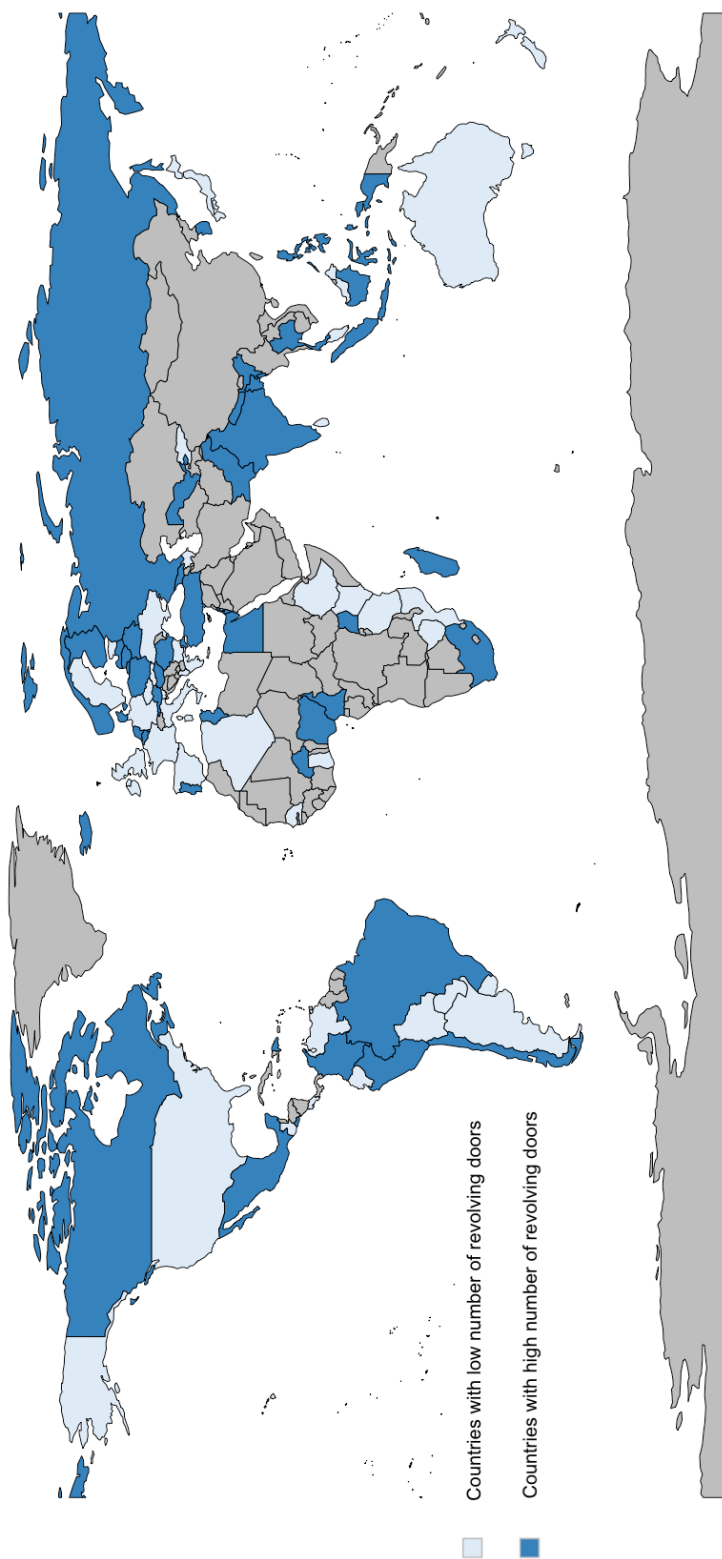


Fig. E2. **Revolving doors between financial and political institutions across the globe: Dichotomous version.** The figure maps each country into one of the two categories (with high or low number of revolving doors) depending on the fraction of its politically-connected banks which is the number of banks with at least one former politician on the board of directors divided by the number of banks for which there are data on board members in Bankscope as of year 2006. The measures are obtained from [Braun and Raddatz \(2010\)](#).

Variables	Mean	Median	Std. Deviation	Min	Max	Observations
<i>Balanced bank sample</i>						
<i>Log Z-score (3-year)</i>	3.70	3.70	1.29	-5.78	6.92	13,047
<i>Log Z-score (4-year)</i>	3.47	3.51	1.18	-4.89	6.33	12,130
<i>Log Z-score (5-year)</i>	3.32	3.39	1.10	-5.15	5.98	11,198
<i>Unbalanced bank sample</i>						
<i>Log Z-score (3-year)</i>	3.52	3.54	1.31	-5.78	6.92	25,804
<i>Log Z-score (4-year)</i>	3.31	3.37	1.19	-4.89	6.33	22,585
<i>Log Z-score (5-year)</i>	3.19	3.26	1.12	-5.15	5.98	19,675

Table E1: **Summary statistics for bank-level variables.** The table outlines the summary statistics for variables related to bank-level financial stability. *Log Z-score* varies over banks and years measuring the logarithm of total equity plus income buffer of a bank with respect to the volatility of its past (3-year, 4-year or 5-year) cash flows. More formally, we compute the following for each bank and year: $\frac{Assets_t}{(Income_{t-1,t-4} + Equity_t)}$ divided by the standard deviation of return on assets over the past four (or three or five) years, that is $\sigma_{ROA,t-1,t-4}$. Balanced sample includes only those banks that have observations in each year from 1999 to 2014 and unbalanced sample includes all available bank-year observations. Bank-level data is obtained from Bankscope.

Bank sample:	Partial model			Full model		
	All (Balanced Panel)			All (Balanced Panel)		
	Log Z-score (3-year)	Log Z-score (4-year)	Log Z-score (5-year)	Log Z-score (3-year)	Log Z-score (4-year)	Log Z-score (5-year)
Dependent variable:						
<i>POSTcrisis</i> x <i>TermLimit</i>	-0.541** [0.218]	-0.164 [0.000]	-0.226** [0.105]	-0.423* [0.227]	-0.120 [0.168]	-0.233** [0.115]
<i>PREcrisis</i> x <i>TermLimit</i>				0.661* [0.390]	0.476** [0.205]	
<i>POSTcrisis</i>	-0.219 [0.142]	-0.220 [0.000]	-0.200*** [0.063]	-0.210 [0.148]	-0.223** [0.097]	-0.192** [0.077]
<i>PREcrisis</i>				0.018 [0.075]	-0.010 [0.070]	0.020 [0.081]
<i>TermLimit</i>	-0.138 [0.171]	-0.137 [0.000]	-0.403*** [0.043]	-0.348** [0.134]	-0.236*** [0.075]	-0.403*** [0.043]
Diff-in-diff for <i>TermLimit</i>				-1.084***	-0.596***	-0.233**
P-value				0.003	0.005	0.045
Diff-in-diff				-0.228	-0.213**	-0.212***
P-value				0.108	0.043	0.005
N	13,047	12,130	11,198	13,047	12,130	11,198
Adj-R-sq	0.423	0.510	0.580	0.423	0.510	0.580
Clustering	Country	Country	Country	Country	Country	Country
<i>CountryTimeTrend</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Bank FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes

Table E2: **Term limits in democracies (with country-specific time trends): Difference-in-differences estimates for bank-level stability.** The table summarizes the estimation results with a specification à la Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six and only those banks that have observations in each year from 1999 to 2014. Dependent variable is *Log Z-score* varying over banks and years measuring the total equity plus income buffer of a bank with respect to the volatility of its past (3-year, 4-year or 5-year) cash flows. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Bank-level data is obtained from Bankscope; financial crises from [Laeven and Valencia \(2018\)](#); political variables from [Cruz et al. \(2016\)](#). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

Bank sample:	Partial model			Full model		
	All (Unbalanced Panel)			All (Unbalanced Panel)		
	Log Z-score (3-year)	Log Z-score (4-year)	Log Z-score (5-year)	Log Z-score (3-year)	Log Z-score (4-year)	Log Z-score (5-year)
Dependent variable:						
<i>POSTcrisis x TermLimit</i>	-0.635*** [0.217]	-0.569*** [0.161]	-0.509*** [0.118]	-0.663*** [0.227]	-0.665*** [0.162]	-0.638*** [0.110]
<i>PREcrisis x TermLimit</i>				0.204 [0.226]	0.127 [0.427]	
<i>POSTcrisis</i>	-0.340*** [0.073]	-0.311*** [0.064]	-0.269*** [0.051]	-0.286*** [0.075]	-0.205*** [0.069]	-0.064 [0.060]
<i>PREcrisis</i>				0.137** [0.060]	0.213** [0.082]	0.361*** [0.103]
<i>TermLimit</i>	-0.113 [0.265]	-0.362 [0.246]	-0.935*** [0.063]	-0.211 [0.180]	-0.400** [0.171]	-0.878*** [0.059]
Diff-in-diff for Term Limit				-0.867***	-0.792*	-0.638***
P-value				0.001	0.070	0.000
Diff-in-diff				-0.423***	-0.419***	-0.425***
P-value				0.000	0.000	0.000
N	25,501	22,322	19,476	25,501	22,322	19,476
Adj-R-sq	0.426	0.507	0.566	0.427	0.509	0.571
Clustering	Country	Country	Country	Country	Country	Country
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table E3: **Term limits in democracies (unbalanced bank panel): Difference-in-differences estimates for bank-level stability.** The table summarizes the estimation results with a specification à la Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six and all available bank-year observations from 1999 to 2014. Dependent variable is *Log Z-score* varying over banks and years measuring the total equity plus income buffer of a bank with respect to the volatility of its past (3-year, 4-year or 5-year) cash flows. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (both in the baseline and in interaction with *TLimit*) and p-values are reported underneath. Bank-level data is obtained from Bankscope; financial crises from Laeven and Valencia (2018); political variables from Cruz et al. (2016). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.

<i>Dependent variable:</i>		<i>Credit controls</i>	<i>Interest rate controls</i>	<i>Entry Barriers</i>	<i>Capital account</i>	<i>Privatisation</i>	<i>Banking supervision</i>	<i>Security markets</i>
<i>POSTcrisis x TermLimit</i>		-0.097*	-0.148**	-0.170***	-0.089	-0.133**	-0.043	-0.054
		[0.058]	[0.070]	[0.053]	[0.069]	[0.057]	[0.042]	[0.041]
		0.040	0.021	-0.003	-0.069	0.037	-0.017	0.057
<i>PREcrisis x TermLimit</i>		[0.047]	[0.065]	[0.061]	[0.056]	[0.060]	[0.039]	[0.037]
		Yes	Yes	Yes	Yes	Yes	Yes	Yes
	<i>All political controls</i>							
Diff-in-diff for Term Limit		-0.137**	-0.168***	-0.173***	-0.020	-0.170**	-0.025	-0.111***
P-value		0.031	0.004	0.012	0.712	0.035	0.569	0.003
N		2,077	2,108	2,108	2,108	2,108	2,108	2,108
Adj-R-sq		0.722	0.708	0.795	0.661	0.663	0.815	0.774
Clustering		Country	Country	Country	Country	Country	Country	Country
<i>CountryTime Trend</i>		Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Country FE</i>		Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>		Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table E4: **Term limits in democracies (with interacted political controls): Difference-in-differences estimates for policy domains.** The table summarizes the estimation results with a specification à la Equation 2. Sample includes only democratic countries whose executive index of electoral competitiveness has a value equal to or higher than six. Dependent variable is a financial policy domain varying over countries and years. *POSTcrisis* is a binary dummy variable turning on in the first 5 years after any financial (banking, sovereign debt or currency) crisis in the sample including the starting year itself. *PREcrisis* is a binary dummy for the 5 years immediately preceding a financial crisis. *TLimit* is a dummy variable taking the value of one when the incumbent executive leader in a country is bounded by a term-limit and zero otherwise. Diff-in-diff estimates test the difference between the coefficients estimated for *POSTcrisis* and *PREcrisis* (in interaction with *TLimit*) and p-values are reported underneath. Reform database is obtained by merging two subsets of observations from Abiad et al. (2010) and Denk and Gomes (2017). Data on financial crises is obtained from Laeven and Valencia (2018). Political variables are obtained from Cruz et al. (2016). Robust standard errors are clustered at the country level and standard errors are reported in brackets. *p<0.1, **p<0.05, ***p<0.01.