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JEL Classification: G01, G28, P11, P16.

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Abstract

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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 and such interventions are likely to be politically motivated for several reasons.

First, governments often feel forced to save the 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). A third, and a much more dismal, reason could be the private incentives of policymakers who may feel tempted to take advantage of the interventionary sentiment prevailing in the society 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]

In this paper, based on a newly-merged panel of 94 countries over the period from 1973 to 2015, we employ 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 capture the causal impact of a financial crisis on actual government policies across seven different financial domains; namely, credit controls, interest rate controls, entry barriers, capital account, privatization, banking supervision and security markets. As a result, we present strong evidence showing that financial crises in general trigger government interventions and initiate a process of re-regulation in financial markets. This 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

[1]The 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). In defining the public interest in this paper, we do not differentiate between what is the ideal policy for constituents in the long-term and what they currently demand. Our assumption is that constituents' current perception of ideal policymaking is what should drive policymakers' incentives in view of the upcoming elections.

a wider variety of financial crises.

Interventionary policy stance in the aftermath of financial crises however does not necessarily tell us whether these policy reactions are motivated by public demand (interests) or by policymakers' private interests that may arise due to their personal connections to private businesses and/or career plans after leaving politics. In a democracy, politicians are likely to balance their private incentives with those stemming from the best interest of their voters. Hence, in order to disentangle between these two channels, we present a simple two-stage model where a politician does not face a binding term limit in the short-term and is likely to be re-elected. 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 prove that the lack of re-election concerns due to term limits may be a very important mechanism incentivising the politicians to behave more in line with their private interests. The model produces two key implications that we later expose to robust empirical testing.

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-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 (Chwieroth and Walter, 2019) and constitutes evidence that post-crisis policymaking -at least to some extent- is driven by the public interest.[2]

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 term-limit. 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 due to a binding term limit (i.e., being a *lame-duck* politician).[3]

[2]In the meantime, this observation goes somewhat against the earlier findings in the literature (such as Gokmen, Nannicini, Onorato, and Papageorgiou, 2017) which show structural reform reversals to be more likely for autocratic countries in the aftermath of financial crises. Compared to these authors, we exploit a much more comprehensive dataset (both in terms of financial policies and crises) and an empirical specification that takes into account the country-specific nature of the liberalization process. Additionally -and importantly-, we use two of the most widely-used datasets to determine the political regime types in our sample, namely Database for Political Institutions and Polity5 (see Section 3). In a robustness check, we use varying levels of democracy to show that the post-crisis policy interventions become larger as a country gets more and more democratic.

[3]Some democratic countries impose term limits on their political leaders which prevent them from serving

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 is 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.

We additionally find that these privately-motivated policies do not immediately follow the financial crises which goes against the idea that they may be necessary for crisis response. Such interventions mainly operate via the extensive margin of policymaking and, more importantly, emerge in different policy domains than those motivated by public interests. 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).

To illustrate one of the mechanisms behind policymakers' private interests, we later focus on three banking-related policy domains in which we can clearly lay out the preferences for incumbent banks. 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. This suggests that political executives in their last term advance their own private agendas by resorting to financial repression that tends to create rent-seeking opportunities.[4]

Our findings are indeed closely in line with a long stream of papers illustrating how po-

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) 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.

[4] It 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.

litical term-limits may distort socially-optimal policymaking. 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.) and authors explain this by referring to the reduced accountability of the lame-duck politicians toward their constituencies. 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 municipality-level dataset collected from the audit reports in Brazil, Ferraz and Finan (2011) provide evidence on the corruption-enhancing role of the term-limits. Authors show that lame-duck politicians are much more likely to engage in corruption and the effects are particularly strong in places with lower chances of getting caught/punished. Furthermore, even the probability of an interstate conflict has been related to political accountability via the comparison of term-limits in a cross-country setting (Conconi, Sahuguet, and Zanardi, 2014). More recently, Klašnja and Titiunik (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 term-limited policymakers in the aftermath of financial crises are more likely to serve their private interests by potentially exchanging favours with financial industry.

The systematic evidence we present in this paper also builds on the somewhat ambiguous –and not always consistent- results provided by the literature on crises and structural reforms.[5] Lora (1998) is one of the first to construct actual (de-facto) policy indices and to find that certain reform efforts respond to certain types of crises. Specifically, liberalizations in trade and labour markets seem to be triggered by drops in growth and income whereas liberal financial reforms are pushed by inflationary problems. Following a similar de-facto policy measurement approach, Abiad and Mody (2005) construct a more granular index of financial reforms for a global set of countries and support the view that financial crises 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,

[5]Earlier literature treating crises as a pre-condition for reform mostly depends on country-specific case studies. For seminal examples, see Nelson (1990), Krueger (1993) and Williamson (1994). Bruno and Easterly (1996), in possibly the first systematic attempt to tackle the question of whether crises feed reforms, show that countries experiencing high-inflation periods are more likely to undertake efforts for subsequent macroeconomic stabilisation. Perotti (1999) illustrates that fiscal adjustments are more likely to be successful during times of fiscal stress than in normal times. Drazen and Easterly (2001) point out that the positive relationship between high inflation (or black market premium) today and that in the future turns negative in extreme cases which is consistent with the idea that only sufficiently high economic turbulence leads to subsequent corrections in macroeconomic policies. Alesina, Ardagna, and Trebbi (2006) analyse the interaction between crises and political environment and provide evidence that inflation and budget crises lead to better macroeconomic performance later, especially when the government has strong popular support.

encouraging reversals. 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, potentially due to rise in political fragmentation and extreme ideological views. We complement these studies by establishing the first systematic evidence on the negative impact of crises on financial liberalization and by further tracing the reasons back to political accountability of policymakers.

Lastly, our work is related to the recently-flourishing literature on political economy of finance.[6] 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 policymakers' behaviour is tightly linked to the pressure from their constituents as well as from special interest groups in the form of campaign contributors. To the best of our knowledge, compared to these studies, ours constitutes the first attempt to isolate the private and public interest channels of post-crisis policymaking in a cross-country setting.

The paper proceeds as follows. The next section lays out a simple model of post-crisis policymaking based on the existence of both public and private interests. Section 3 describes the construction of the dataset whereas our methodology and identification strategy are explained in Section 4. Section 5 presents the results and the last section concludes the paper.

2. A simple model of post-crisis policymaking

Democracy is not perfect. One of the fundamental problems democracy suffers from is what we call the 'principal-agent problem'. 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

[6] See 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 Schwienbacher, 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; Dinç, 2005; Englmaier and Stowasser, 2017; Bircan and Saka, 2019).

best interest of the voters. This problem usually arises when the two sides have conflicting interests (i.e. private versus public) and there is asymmetric information (i.e. the politician has more information than the voters do).

In this section, we use the original game theoretical model by Besley (2006) to analyse how imposing term limits on the executive politician under democracy can worsen the principal-agent problem and increase the possibility that politicians act based on their private interests. To do this, we will introduce a two-stage model allowing the politician to have the chance of being re-elected. We first present the basic model and its equilibrium solution. This will allow us to discuss two key implications concerning how democratic elections and term limits shape policy choices with respect to the clashing interests between public and private interest groups. Admittedly, other political and institutional factors such as the role of parliament, parties and etc. may also play a role; however, these are not the focus of our theoretical discussion.[7]

There are two time periods denoted by $t \in \{1, 2\}$. In each period, a politician is elected to make a single political decision, denoted by $e_t \in \{0, 1\}$. The payoffs to voters and politicians depend on whether or not the political decision corresponds to the state of the world $s_t \in \{0, 1\}$, only observed by the incumbent politician. In the context of our study, the state of the world can be interpreted as a particular (and different) policy stance producing socially optimal outcomes in a crisis (1) or a non-crisis (0) situation. For simplicity, each state is assumed to occur with equal probability. Voters receive a payoff $\triangle$ if $e_t = s_t$ and zero otherwise. This implies that voters cannot directly observe whether or not the politician adopts a socially optimal policy stance at any moment but can derive this information from the effect of optimal policy stance on voters' utility.

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. Voters and politicians discount the future with a common discount factor $\beta < 1$.

There are two types of politicians. "Good" politicians always make policy decisions based on public interest and "bad" or "rent-seeking" politicians may pursue their own private interest if the benefits of doing so are larger than those of staying in the office. The type is denoted by $i \in \{good, bad\}$. Clearly, voters have a preference for a good politician; however, the type is not observable to voters. Let π be the probability that a randomly picked politician from the pool is a good one and $(1 - \pi)$ the probability that she is bad.

[7] See the seminal works by Barro (1973) and Ferejohn (1986) for a theoretical framework on how electoral processes and other institutional arrangements can act in a way to control the private interests of the political actors.

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 $E+ \triangle$ if $e_t = s_t$ and only E if $e_t \neq s_t$. In other words, the good politician shares the same objective as the voters and hence gets an additional payoff when she serves the public. On the other hand, a 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, $e_t \neq s_t$. This private benefit can be considered as the rent/reward of giving special treatment to some interest groups. Assume that this benefit r_t follows a distribution whose cumulative function is $G(.)$, with mean μ and finite rent $[0, R]$. We assume that the possible maximum rent $R > \beta(\mu + E)$, which guarantees that the bad politician in her first term may have an incentive to choose the policy that deviates from the voters' interests.

The timeline is described in Figure 1. 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 $G(.)$ 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 $G(.)$. 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. A game tree is provided in Figure 2 to facilitate our analysis.

In period two, the choice for the politician (provided that she is re-elected) is straightforward. It is essentially the same as a model with only one stage. 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 $e_2(s_2, \mathit{bad}) = 1 - s_2$. 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 $\triangle$. Hence it is optimal for her to choose $e_2(s_2, \mathit{good}) = s_2$. This result confirms a separating equilibrium if there is a binding term limit for politicians.

In period one, the type of politician is not the only factor that matters. The behaviour of a good politician does not change: she always does what voters want provided that she is re-elected for doing so, $e_1(s_1, \mathit{good}) = s_1$. However, the behaviour of a bad politician is more complex. The latter needs to consider the trade-off between her current private benefit

(r_1) and the expected future benefit $(\beta(\mu + E))$ if she is re-elected in period two. When this current private benefit is lower than 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:

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

However, the question here 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 $\triangle$. This probability can be expressed as follows:

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

Obviously, this good behaviour $(e_1(s_1, bad) = s_1)$ always improves a bad politician's reputation (measured by the probability of $\pi^* > \pi$). It implies that there is always an equilibrium in which any politician who produces $\triangle$ 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 $\triangle$ for voters is not re-elected since such a politician is considered to be bad for sure. This result confirms that a pooling equilibrium exists if there is no binding term limit for politicians.

2.1. Policymaking under democracy: the effect of a binding term limit

The previous discussion shows that democratic elections (without a binding term-limit) can motivate politicians, even the bad ones, to make policy choices that satisfy voters' demand so as to be re-elected. We calculate that in this pooling equilibrium (in period one) the probability of the politician making a public-oriented policy decision is $\pi + (1 - \pi)z$.

However, democracies may not be perfect especially in the case of a binding term-limit. As the incumbents cannot be re-elected any more, the bad politician is only interested in seeking private benefit instead of public interest. In another word, this is a separating equilibrium similar to the situation we described in period two of the model. In such an equilibrium, the probability of a public-oriented policy is purely determined by the probability of good politicians, π. This comparison allows us to conclude that under democracy a binding term-limit has its drawback: it reduces the probability of politicians seeking public interest from $\pi + (1 - \pi)z$ to π. By the same token, term-limits increase the probability of the policy outcome being in line with the politicians' private interests.

2.2. Policymaking under autocracy vs. democracy

The Besley (2006) model can also be redeployed to analyze the drawback of an autocratic regime. Under autocracy, the public has very little power in deciding which politician to stay in power. The behaviour of a good politician does not change under autocracy: she does what the public wants. However, the bad politician does not need to sacrifice her private benefit (r_t) for public interest since she would not have a re-election concern. This is a separating equilibrium: policymaking under autocracy solely depends on the type of politicians. Hence, the probability of a public-oriented policy (i.e., $e_t = s_t$) under autocracy is π.

We compare this probability to the one under democracy. Assume that $0 < \gamma < 1$ is the fraction of the politicians who have a binding term limit (in a separating equilibrium) and $(1 - \gamma)$ is the fraction of the politicians who do not have such a limit (in a pooling equilibrium). According to the earlier discussion in Section 2.1, the weighted aggregate probability of a public-oriented policy is $(\gamma * \pi) + ((1 - \gamma) * (\pi + (1 - \pi)z))$, which is higher than π. Thus, we can conclude that the policymaking under democracy is more likely to serve public interests than the one under autocracy.

Hence, we conclude this section with the following two propositions:

Proposition 1: Policy choices in democracies are more likely to reflect public interests than are the ones in autocracies.

Proposition 2: Policy choices when politicians face a term limit are more likely to reflect private interests than are the ones when politicians do not face a term-limit.

In the next sections, we will empirically test these two propositions directly derived from our model.

3. 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).[8] 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

[8]These 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, Mishra, and Spilimbergo (2013).

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 extent of liberalization in each reform area. They take values between 0-1, with higher values implying more liberalization.[9]

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 (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. One 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)[10] 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.[11] 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. For the few cases in which there is little divergence, they keep their own scores for consistency.[12]

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; hence the new ones only have observations for the years from 2000 to 2015.[13] 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

[9]Except 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.

[10]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.

[11]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.

[12]For the details on the specific questions used for each policy index in Denk and Gomes (2017), see Table A2.

[13]These new countries are Iceland, Luxembourg, Saudi Arabia, Slovakia and Slovenia.

first study analyzing this most comprehensive and recent dataset of financial reforms.

Table 1 presents the summary statistics for the seven sub-indices as well as the overall financial reform variable, which is the simple average of these sub-indices.[14] 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), covering 165 countries between the years 1970 and 2017.

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 classical Database of Political Institutions (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; *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

[14] Table 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.

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 3 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.[15] The discrete jump visible in all time-trends around 2005 is due to the fact that the policy dataset from Denk and Gomes (2017) that we employ for the years 2006-2015 only covers 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 4. 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.

4. Method and identification strategy

4.1. Baseline methodology

We are first interested in the causal 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

[15]The 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.

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.[16] 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 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 absorb any implicit bias that may exist in our data due to the combination of two reform datasets created 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. Therefore, our diff-in-diff estimate (average treatment effect of a crisis) has an event-study nature and is given

[16] Two 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.

by the test of the following difference:

$$ATE = \beta_1 - \beta_0$$

4.2. 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 demand may play in policy reversals, we re-estimate the Equation 1 separately in democratic and non-democratic country subsamples, expecting that public demand is more likely to be reflected in the policy outcomes of the former, in line with our *Proposition 1* in Section 2. 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, in line with our *Proposition 2* in Section 2. 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 demand/interest 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.[17]

[17]We 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; Klašnja and Titiunik (2017).

Thus we formally go on to estimate the following model:

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

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 in such periods. 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 the test the differential behaviour between these two cases:

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

4.3. 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 2 where we assume that a re-elected politician *believes* that she cannot run again for the next election. In 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.[18]

[18]The inclusion of the few additional non-missing (but non-democratic) observations in our estimations does not qualitatively change our main findings.

Some of the democratic countries however do not impose any explicit term-limits on their executives during our sample period[19] and thus always act as a counterfactual when we compare them to those country-year observations where there is an explicit term-limit in a country that binds the executive leader during a specific time period. 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 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, the specific effects we capture via β_1 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 more directly, we control for all relevant aspects of political heterogeneity within our sample. In particular, we control for the parliamentary or presidential nature of the democracy both in the baseline estimation and in interaction with our crisis variables.

To further raise the confidence in our identification strategy, we also focus on a small subset of countries only comprised of 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.

A more important empirical concern could be that the political leaders in their last term may take a different stance in financial policymaking which may lead to a financial crisis. If this is the case, however, one should observe a policy change before the crisis strikes. In Equation 2, this corresponds to the coefficient on the interaction term between *TLimit* and *PREcrisis*. Accordingly, the coefficient β_2 represents whether or not there are any differential liberalization trends between term-limited and unlimited politicians before crises hit their countries. For identification purposes, the implication is that one can not get away with only focusing on the differences in coefficients (as in the definitions of the *ATEs* above) but also

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

needs to interpret the individual coefficients, especially the ones on the pre-crisis dummy and its interaction with term-limits. In our setting, we find no evidence of differential policy trends in term-limited politicians before crises strike.

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.[20]

5. Results

5.1. *Do governments intervene in financial markets after crises?*

5.1.1. *Baseline results*

Results from the estimation of Equation 1 on the global full-sample 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

[20]Since 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 correlated by construction. To the extent that our control variable (government vote share) does not proxy leaders' skills, the identification concern itself becomes irrelevant.

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 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.[21]

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

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

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 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.

An important additional investigation can be pursued by separating this average effect for different types of crises. Table 3 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).

5.1.2. Robustness checks

For the panel analysis, we have undertaken various robustness checks in the following way: (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 B1 and B2); (2) in addition to the previous exclusion, we also exclude the years that fall within both PREcrisis and POSTcrisis periods (see Tables B3 and B4); (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 B5 as well as Tables B6a and B6b); (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 B7 and B8); (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 B9). It is clear that our main findings remain intact in all of these alternative tests.

5.1.3. *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 financial crises 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 5, 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 confirms the requirement of parallel trends for our diff-in-diff setting. 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.[22]

5.2. *Public interests: Democracy vs. autocracy*

5.2.1. *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 loses that they may have incurred during the financial crisis (Gokmen et al., 2017). However, if democracies drive our

[22]In 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 3. 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.

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.

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, 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 4 reports estimations of Equation 1 on these two separate subsamples. As can be clearly seen in the estimated diff-in-diff coefficients, our previous findings are only valid for the subsample of democratic countries which -in line with our *Proposition 1* in Section 2- implies that a public demand channel might partly be responsible for the state interventionism observed after financial crises.[23]

5.2.2. Robustness checks

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 4.

As previously seen in Figure 3, 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

[23]The difference between the estimates across two subsamples is statistically significant at conventional levels.

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.

Findings in this section align well with those of Chwieroth and Walter (2019) and Dagher (2018) 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/or the general public distrust increases after crises and leads policymakers to regulate the system. Our interpretation is thus consistent with both of these publicly-driven mechanisms which are more likely to be overlapping than mutually exclusive.

5.3. *Private interests: term-limits as a natural experiment*

5.3.1. *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 5 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 5 illustrates that the de-liberalizations undertaken by the term-limited policymakers are almost *four times larger* compared to those undertaken by their unlimited counterparts. This is consistent with our *Proposition 2* which predicts that the term-limited politicians must behave differently due to their private interests.

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.

This finding is consistent with our theoretical conjecture and implies that the policymakers who weight their private interests more heavily go on to manipulate the anti-finance sentiment in the society that may escalate particularly after a financial crisis. 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. Thus, politicians in our setting are able to harness the negative public attitudes towards the financial sector and further intervene in the economy with the real (but unobserved) purpose of distributing rents, potentially for themselves as well as for their allies.[24]

5.3.2. 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.[25]

Table 6 presents the results updating the estimated specification step-by-step from partial to the full set of additional controls. 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

[24]These 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).

[25]See the Data section for the exact definitions.

centric) ideology.[26] 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.[27]

5.3.3. 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 7 updates the results where each political variable is interacted in the same way as *TermLimits* in addition to being included in the baseline specification. 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.[28]

5.3.4. 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 D4, where the first column is based on the model with no controls as in Table 5 and the second one is based on the model with full political controls as in Table 7.

The last column in Table D4 then presents the estimation bounds where we define Rmax 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

[26] Table D2 re-estimates the Table 6 with a fully saturated model and confirms the findings in the latter.

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

[28] Table D3 re-estimates the Table 7 with a fully saturated model and confirms the findings in the latter.

by omitted variable bias. The high delta value 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.[29]

5.3.5. 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 Table 7, 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.[30] 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 D5 and D6 re-estimate Tables 5 and 7 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.[31]

5.3.6. 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.[32] Table D9 reports the results. We naturally end up with a much smaller set of countries

[29] The 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.

[30] Our sample contains more than 250 different political parties.

[31] In Tables D7 and D8, we relax the assumption of fixed ideology for each party and estimate a model with fixed effects at the levels of interaction between parties and decades in our sample, again finding similar results for our main coefficients of interest.

[32] Notice 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.

when we focus solely on those with a term-limit experience.[33] Despite the fact that small sample size magnifies standard errors, our coefficient estimates are still similar to those in Table 5 and our main finding that term-limits have negative effects on post-crisis financial liberalization remains robust at conventional levels of statistical significance.

5.3.7. Do public and private interests have the same timeline?

Similar to the analysis in Section 5.1.3, 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 6, 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 former (in Panel A) represents the private interest channel and the latter (in Panel B) captures the one for public interests. 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 5. 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.

[33] These 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.

5.3.8. *Do private interests operate via intensive or extensive margin of policymaking?*

A more granular analysis can be performed by focusing on the sub-areas of financial liberalization instead of the overall effect. 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.

Table 8 estimates a specification similar to Equation 2 but adjusted for the loss of the reform dimension in the dataset. Thus, the estimation takes place separately for each domain of financial policymaking. It is quite clear 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 much additional action.[34] On the contrary, when political accountability is reduced via binding term-limits, policymakers seem to focus on controversial interventions that are more likely to serve special-interest groups, such as introducing interest rate controls or 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 industry.[35]

5.3.9. *Do revolving doors influence privately-motivated 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

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

[35]Table D10 shows that the results in Table 8 are similar when we also use interacted political controls in these policy-specific estimations.

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 D1 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 relate 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. Even though bank nationalisation (opposite of bank privatization) is not something that the banking industry would enjoy in general, this policy domain translates into government bailouts for incumbent banks in the specific aftermath of a financial crisis and thus is much more likely to be appreciated.[36] Therefore, in countries where policymakers' private interests are more salient, we would expect the term limits to have a larger negative impact (i.e., raising) on bank entry barriers and (i.e., lowering) privatization as well as a larger positive impact (i.e., lowering) on bank supervision.[37]

In Table 9, 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 divided into two equal portions conditional on the intensity of the revolving door phenomenon in a country;[38] 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 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

[36]As argued earlier, bailouts could be demanded directly by the constituents, making them in line with the public interest channel to some extent. 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 less 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).

[37]Remember that negative impact in any policy domain means more state intervention.

[38]Figure D2 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.

albeit statistically insignificant. The final column pulls together all three policy domains and re-estimates them in the same specification as in Table 5.[39] 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.[40]

6. Conclusion

The literature on the determinants of liberalization generally suggests that turbulent periods should play a key role in changing the policy equilibrium and thus spurring reforms. Despite various theoretical mechanisms that may support this prediction, the empirical evidence in the literature so far seems to have been mixed at best . Using a recent comprehensive dataset on financial reforms across 94 countries for the period between 1973 and 2015, we test the validity of this prediction for the financial sector specifically in the aftermath of financial crises and further investigate the potential drivers of post-crisis policymaking.

First, by using a quasi-difference-in-differences methodology in a panel setting, we compare the level of financial liberalisation between the two periods immediately before and after a financial crisis, which helps us capture the causal impact of the financial crisis itself. Our findings suggest that financial crises lead to a reversal in financial liberalization and encourage a relatively more interventionary stance on the part of the policymakers in its aftermath. These reversals are the strongest in the case of sovereign debt defaults followed by currency crises whereas banking crises seem to generate a more modest impact.

Further investigating the political dynamics behind the scenes, we find that such interventions are only common in democratic settings, which at face value points to a public demand 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.

In order to understand how much private interests 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. Some 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

[39]For this estimation, we multiply the supervision domain by a minus to make it aligned with our expected direction of influence.

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

fact that a politician might be serving their last term due to a term limit gives us a clean counterfactual to see what would happen if policymakers had a lower re-election chance and thus were less sensitive to public demand in their policies. 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 be elected due to a binding term limit.

Hence, by using such technical limits as a natural shock to politicians' sensitivity to public demand, we find that a large part of the interventionary stance in the aftermath of financial crises is driven by private interests in politics. 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. These results get even stronger when one controls for a rich set of country-level dynamics both in the baseline and also in interaction with our crisis-event dummies. Furthermore, 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. We also illustrate 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. Finally, we present evidence that 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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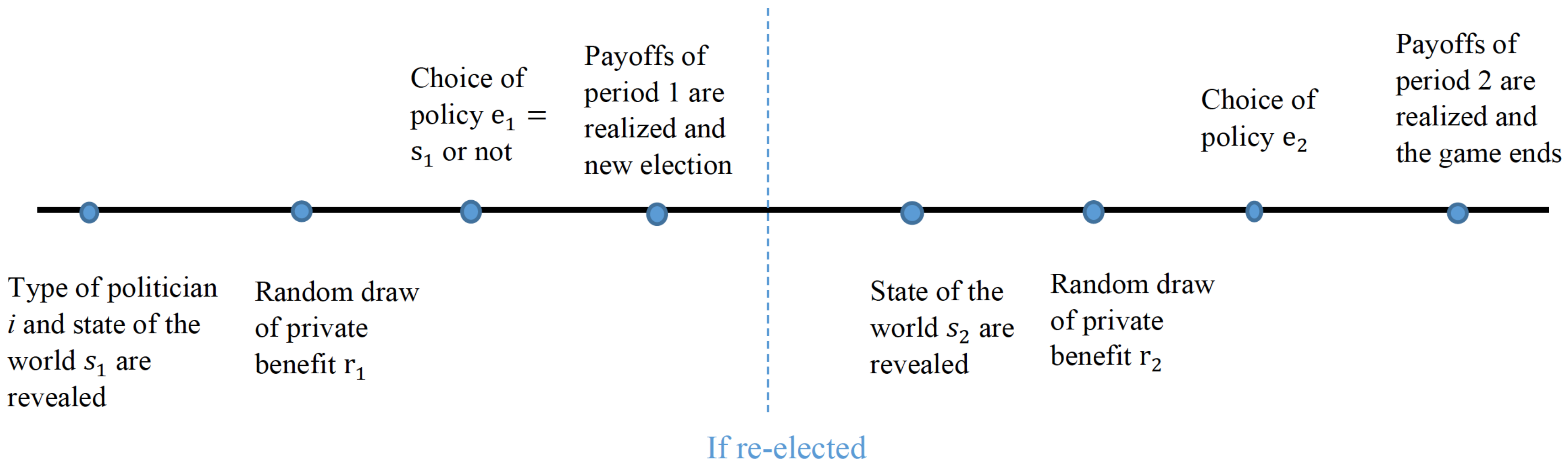


Fig. 1. **Timeline of moves and actions in a simple model of post-crisis policymaking.** The figure illustrates the time-wise order of the steps and decisions that take place in the game theoretical model discussed in Section 2.

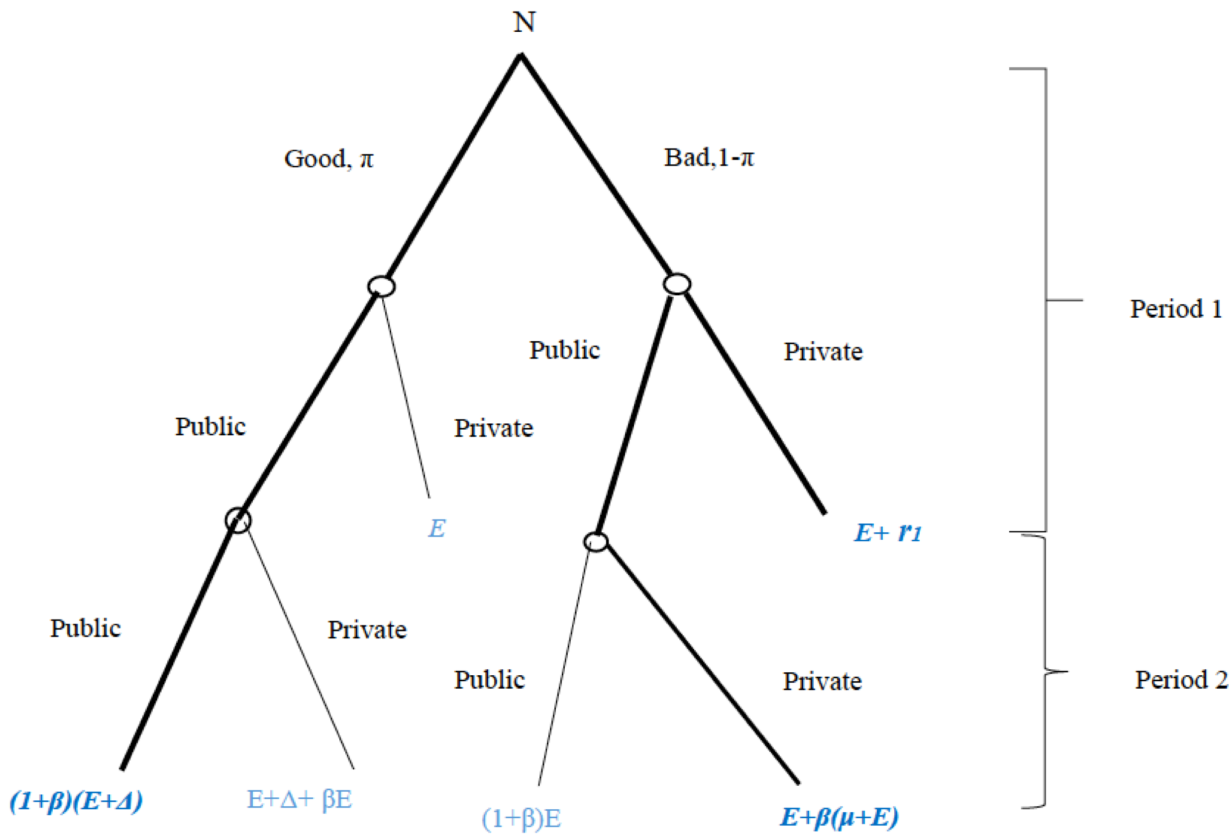
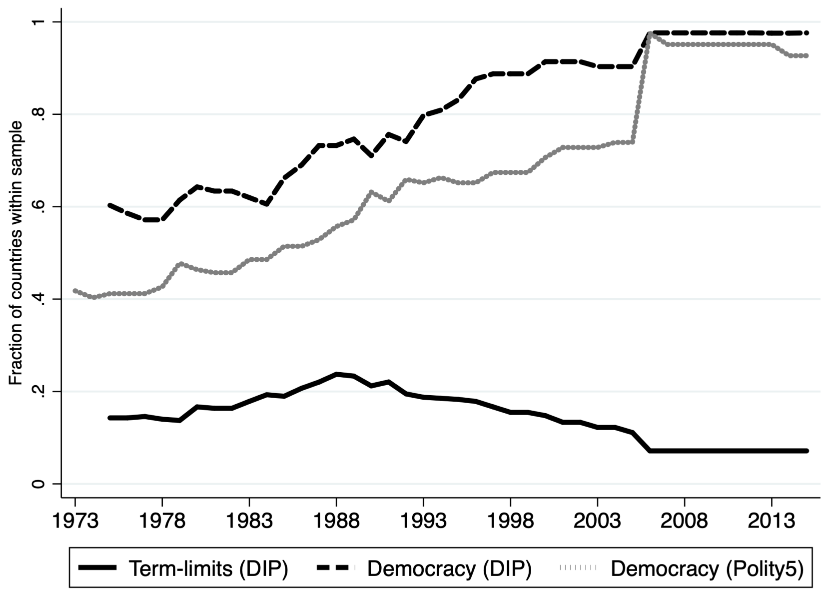
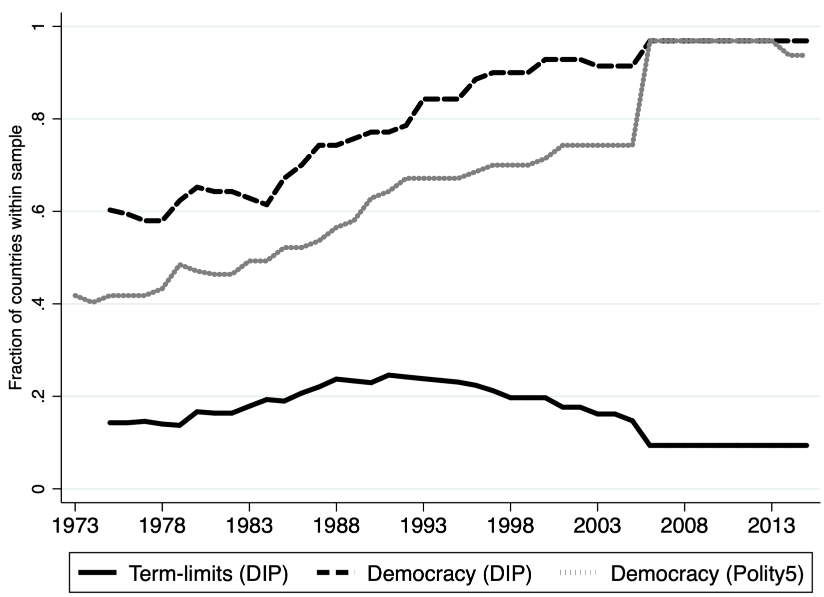


Fig. 2. **Game tree with payoffs in a simple model of post-crisis policymaking.** The figure illustrates the time-wise order of the steps, decisions and payoffs that take place in the game theoretical model discussed in Section 2.

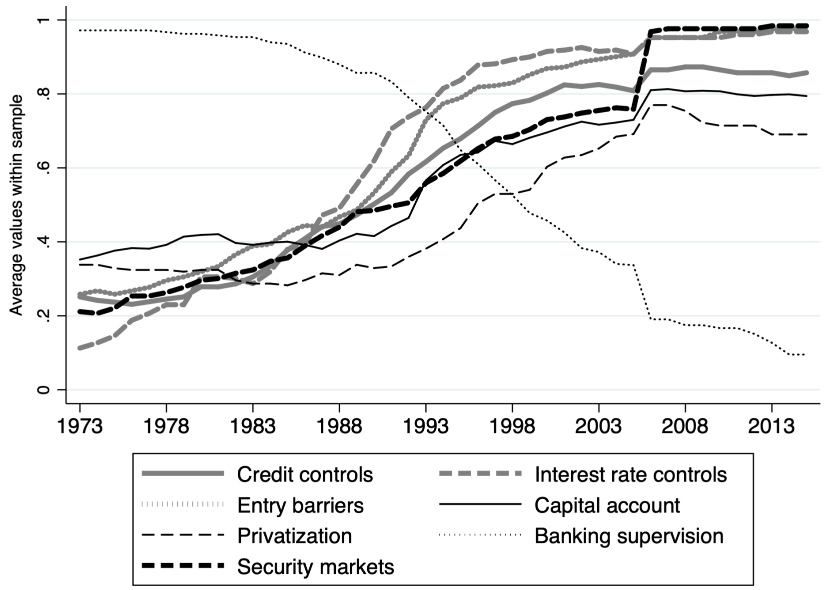


(a) Full (unbalanced) sample

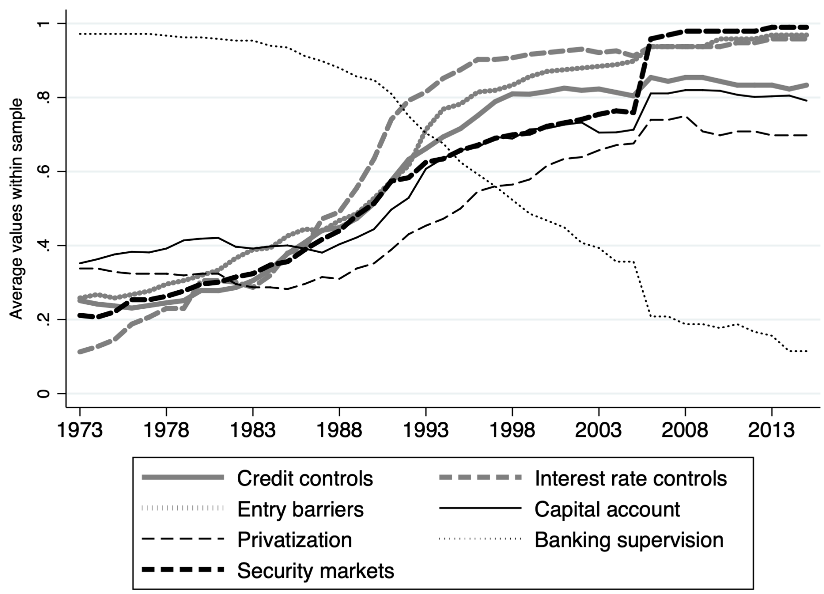


(b) Balanced sample

Fig. 3. **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.



(a) Full (unbalanced) sample



(b) Balanced sample

Fig. 4. **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).

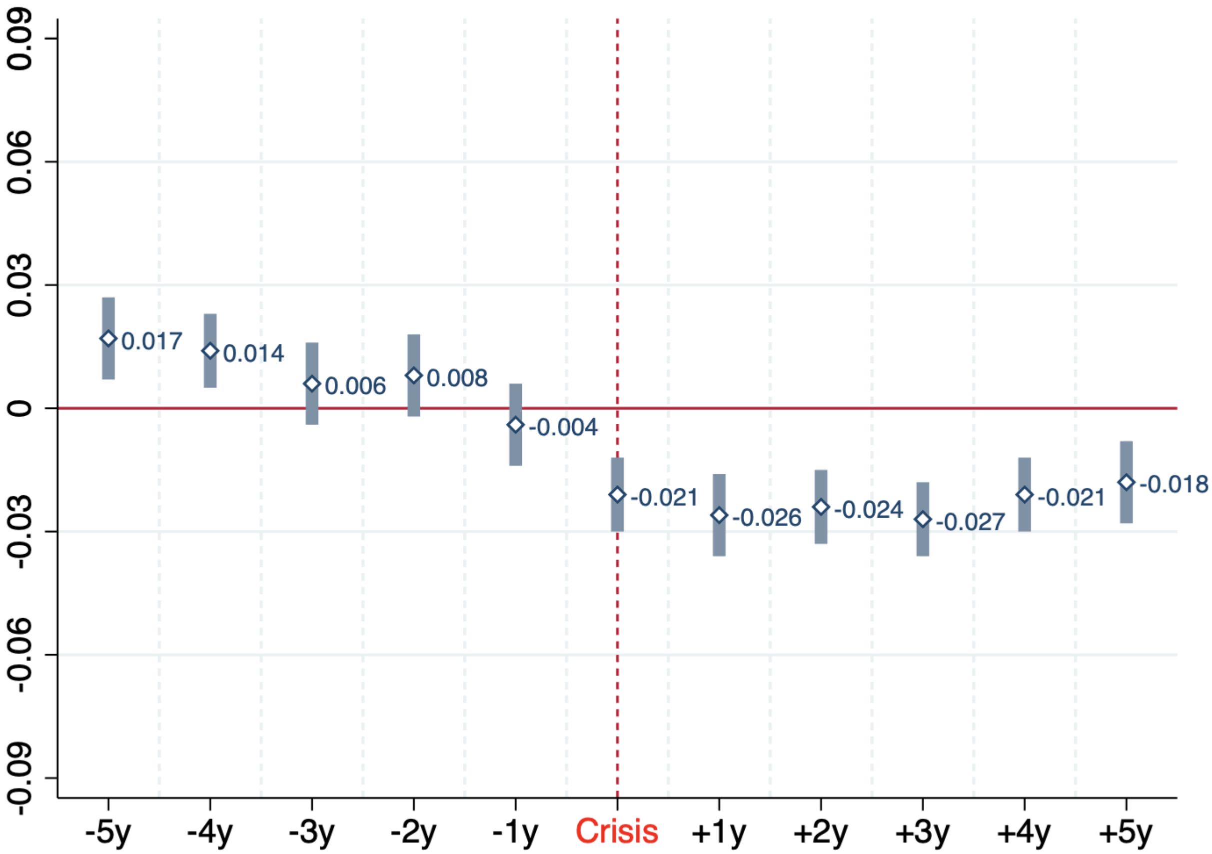
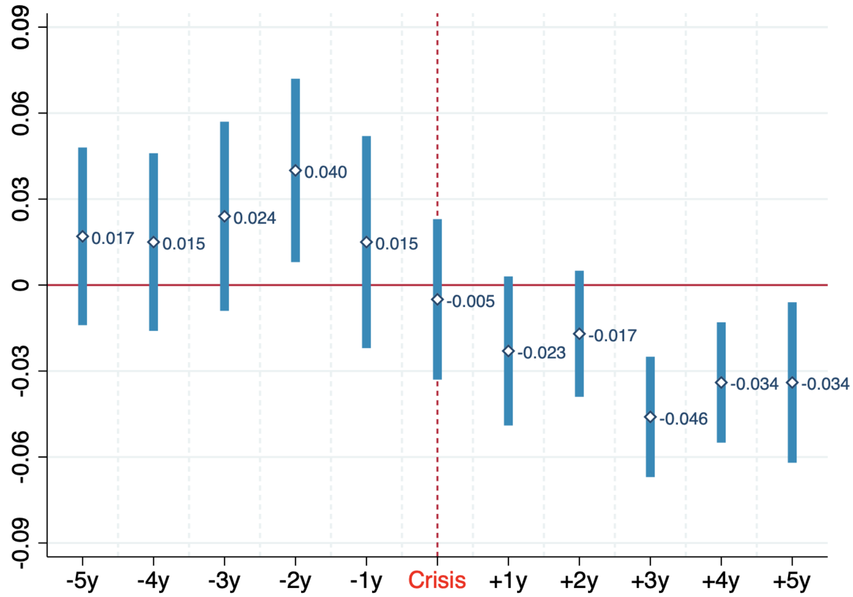
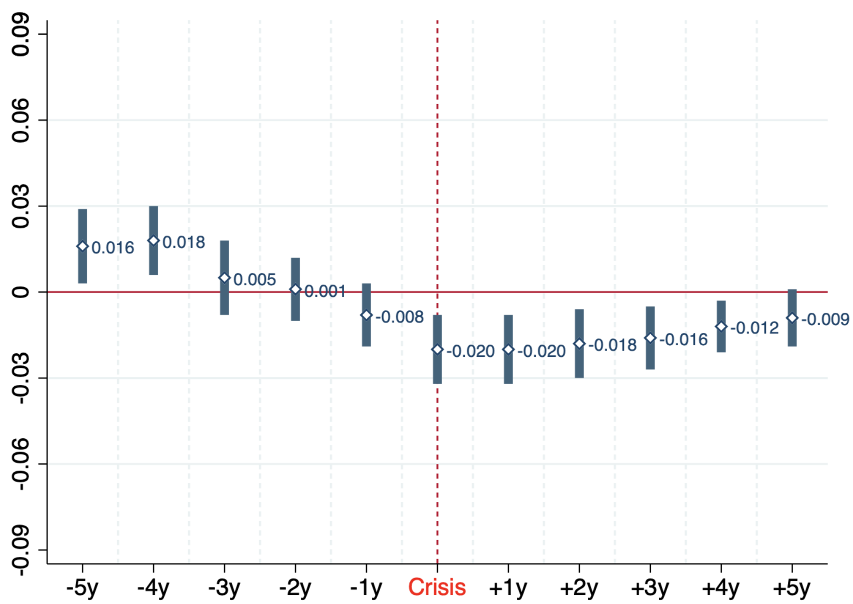


Fig. 5. **Timeline for the effect of a crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3. Reform database is obtained by merging two subsets of observations from Abiad et al. (2010) and Denk and Gomes (2017). Data on 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. 6. **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
Indices of financial liberalization						
Financial liberalization (Average)	0.59	0.63	0.23	0.14	0.96	3,046
Credit controls	0.58	0.67	0.38	0.00	1.00	3,046
Interest rate controls	0.65	1.00	0.43	0.00	1.00	3,082
Entry barriers	0.65	0.67	0.39	0.00	1.00	3,082
International capital controls	0.56	0.67	0.37	0.00	1.00	3,082
Privatization	0.47	0.33	0.40	0.00	1.00	3,082
Banking supervision	0.66	0.67	0.37	0.00	1.00	3,082
Security markets	0.57	0.67	0.39	0.00	1.00	3,082
Measures of financial crises						
Financial crises (any crisis)	0.08	0.00	0.27	0.00	1.00	3,082
Banking crises	0.04	0.00	0.19	0.00	1.00	3,082
Sovereign debt crises	0.02	0.00	0.13	0.00	1.00	3,082
Currency crises	0.04	0.00	0.19	0.00	1.00	3,082
Variables for political dynamics						
TermLimit	0.17	0.00	0.38	0.00	1.00	2,108
Right	0.37	0.00	0.48	0.00	1.00	2,108
Left	0.33	0.00	0.47	0.00	1.00	2,108
Presidential	0.38	0.00	0.49	0.00	1.00	2,108
Parliamentary	0.56	1.00	0.50	0.00	1.00	2,108
OfficeYears	4.68	3.00	4.66	1.00	35.00	2,108
YearsLeft	1.92	2.00	1.40	0.00	6.00	2,108
HerfGov	0.76	0.89	0.27	0.11	1.00	2,108
GovFrac	0.24	0.12	0.27	0.00	0.89	2,108
GovShare	0.59	0.55	0.16	0.11	1.00	2,108
Checks	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*
POSTcrisis	-0.046***	-0.035***	-0.035***	-0.035***	-0.035***
	[0.009]	[0.008]	[0.008]	[0.008]	[0.008]
PREcrisis	-0.028**	-0.004	-0.004	-0.004	-0.004
	[0.011]	[0.009]	[0.009]	[0.009]	[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: **Difference-in-differences estimates for the effect of a financial crisis on average 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:	*Financial Liberalization*					*Financial Liberalization*					*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis_banking	-0.036***	-0.024***	-0.024***	-0.024***	-0.024***										
	[0.010]	[0.008]	[0.008]	[0.009]	[0.009]										
PREcrisis_banking	-0.026**	-0.001	-0.001	-0.001	-0.001										
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]										
POSTcrisis_debt						-0.066***	-0.040**	-0.040**	-0.040**	-0.040**					
						[0.019]	[0.015]	[0.016]	[0.016]	[0.016]					
PREcrisis_debt						-0.007	0.022	0.022	0.022	0.022					
						[0.021]	[0.016]	[0.016]	[0.016]	[0.016]					
POSTcrisis_currency											-0.056***	-0.044***	-0.044***	-0.044***	-0.044***
											[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
PREcrisis_currency											-0.033**	-0.009	-0.009	-0.009	-0.009
											[0.012]	[0.011]	[0.011]	[0.011]	[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*****	**-0.023****	**-0.035*****	**-0.035*****	**-0.035*****	**-0.035*****
P-value	0.314	0.015	0.016	0.017	0.017	0.000	0.000	0.000	0.000	0.000	0.030	0.001	0.001	0.001	0.001
N	21,538	21,538	21,538	21,538	21,538	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	0.177	0.200	0.474	0.534	0.746
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		
Reform FE	Yes	Yes				Yes	Yes				Yes	Yes			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes			Yes		Yes

Table 3: **Difference-in-differences estimates for the effect of banking, sovereign debt and currency crises on average 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. $POSTcrisis_x$ 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*									
Sample:	*Democratic countries*					*Autocratic countries*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.050***	-0.023**	-0.023**	-0.023**	-0.023**	-0.046***	-0.031*	-0.031*	-0.031*	-0.031
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]	[0.017]	[0.018]	[0.018]	[0.019]	[0.019]
PREcrisis	-0.016	0.019*	0.019*	0.019*	0.019*	-0.038***	-0.024	-0.024	-0.024	-0.024
	[0.014]	[0.010]	[0.010]	[0.010]	[0.010]	[0.012]	[0.017]	[0.017]	[0.018]	[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	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes

Table 4: **Democracy vs. autocracy: Difference-in-differences estimates for the effect of a financial crisis on average financial liberalization.** The table summarizes the estimation results over two subsamples 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). 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*
POSTcrisis x TermLimit	-0.041*	-0.072***	-0.072***	-0.072***	-0.072***
	[0.021]	[0.022]	[0.022]	[0.022]	[0.022]
PREcrisis x TermLimit	0.035	0.013	0.013	0.013	0.013
	[0.027]	[0.028]	[0.028]	[0.029]	[0.029]
POSTcrisis	-0.039***	-0.007	-0.007	-0.007	-0.007
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]
PREcrisis	-0.020	0.020*	0.020*	0.020*	0.020*
	[0.014]	[0.010]	[0.010]	[0.010]	[0.010]
TermLimit	-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 5: **Term limits in democracies: Difference-in-differences estimates.** The table summarizes the estimation results with the specification in Equation 2. 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*
POSTcrisis x TermLimit	-0.066***	-0.066***	-0.066***	-0.067***	-0.067***
	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]
PREcrisis x TermLimit	0.017	0.016	0.016	0.017	0.016
	[0.029]	[0.029]	[0.029]	[0.029]	[0.028]
POSTcrisis	-0.010	-0.010	-0.010	-0.009	-0.009
	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
PREcrisis	0.018*	0.020*	0.020*	0.019*	0.019*
	[0.010]	[0.010]	[0.010]	[0.010]	[0.010]
TermLimit	-0.010	-0.005	-0.004	-0.005	-0.003
	[0.036]	[0.037]	[0.036]	[0.036]	[0.036]
Right	0.027**	0.025**	0.025**	0.026**	0.026**
	[0.012]	[0.011]	[0.011]	[0.012]	[0.012]
Left	0.014	0.012	0.012	0.010	0.010
	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
Presidential		-0.029	-0.030	-0.032	-0.031
		[0.028]	[0.028]	[0.029]	[0.029]
Parliamentary		0.028	0.028	0.027	0.028
		[0.030]	[0.030]	[0.031]	[0.030]
OfficeYears			0.000	0.000	0.000
			[0.001]	[0.001]	[0.001]
YearsLeft			0.001	0.001	0.001
			[0.001]	[0.001]	[0.001]
HerfGov				0.638	0.461
				[1.118]	[1.284]
GovFrac				0.596	0.429
				[1.107]	[1.268]
GovShare					-0.010
					[0.036]
Checks					-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
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 6: **Term limits in democracies with political controls: Difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.067***	-0.092***	-0.103***	-0.106***	-0.105***
	[0.025]	[0.026]	[0.027]	[0.027]	[0.026]
PREcrisis x TermLimit	0.019	0.014	0.009	0.009	0.010
	[0.029]	[0.029]	[0.030]	[0.030]	[0.028]
POSTcrisis x Right	0.008	0.012	0.010	0.010	0.010
	[0.017]	[0.016]	[0.016]	[0.016]	[0.016]
POSTcrisis x Left	0.000	0.003	0.000	0.000	0.003
	[0.020]	[0.019]	[0.019]	[0.019]	[0.018]
POSTcrisis x Presidential		0.055	0.060	0.063*	0.065*
		[0.041]	[0.038]	[0.037]	[0.037]
POSTcrisis x Parliamentary		0.022	0.017	0.020	0.020
		[0.040]	[0.038]	[0.037]	[0.037]
POSTcrisis x OfficeYears			-0.002	-0.002	-0.002*
			[0.001]	[0.001]	[0.001]
POSTcrisis x YearsLeft			-0.003	-0.002	-0.003
			[0.002]	[0.002]	[0.002]
POSTcrisis x HerfGov				1.428	2.250
				[1.932]	[2.038]
POSTcrisis x GovFrac				1.415	2.223
				[1.909]	[2.013]
POSTcrisis x GovShare					0.057
					[0.042]
POSTcrisis x Checks					0.002
					[0.004]
PREcrisis interactions	Yes	Yes	Yes	Yes	Yes
Baseline controls	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 7: **Term limits in democracies with interacted political controls: Difference-in-differences estimates.** See the notes in Table 5. *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*
POSTcrisis x TermLimit	-0.078	-0.082	-0.146***	-0.035	-0.103*	-0.012	-0.012
	[0.049]	[0.056]	[0.050]	[0.059]	[0.059]	[0.028]	[0.036]
PREcrisis x TermLimit	0.035	0.034	-0.001	-0.012	0.025	-0.016	0.048
	[0.041]	[0.064]	[0.061]	[0.053]	[0.063]	[0.029]	[0.034]
All controls (Table 6)	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
CountryTime Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: **Policy domains: Term limits in democracies with political controls.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Revolving doors:	*High*				*Low*			
Dependent variable:	*Entry Barriers*	*Privatisation*	*Banking supervision*	*All areas combined*	*Entry Barriers*	*Privatisation*	*Banking supervision*	*All areas combined*
POSTcrisis x TermLimit	-0.175**	-0.140*	0.039	-0.118**	-0.145**	-0.076	-0.039	-0.061**
	[0.080]	[0.077]	[0.043]	[0.044]	[0.062]	[0.046]	[0.032]	[0.026]
PREcrisis x TermLimit	0.018	0.066	-0.008	0.031	-0.030	0.001	-0.025	-0.001
	[0.092]	[0.101]	[0.046]	[0.040]	[0.077]	[0.044]	[0.030]	[0.035]
All controls (Table 6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Diff-in-diff for Term Limit	**-0.194***	**-0.206***	**0.047**	**-0.149*****	**-0.115****	**-0.077***	**-0.014**	**-0.059****
P-value	0.098	0.053	0.402	0.000	0.012	0.089	0.691	0.038
N	1,052	1,052	1,052	3,156	1,044	1,044	1,044	3,132
Adj-R-sq	0.798	0.668	0.823	0.874	0.783	0.669	0.815	0.871
Clustering	Country	Country	Country	Country	Country	Country	Country	Country
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reform FE	-	-	-	Yes	-	-	-	Yes
CountryTime Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: **Banking-related policy domains: Revolving doors and term limits in democracies.** See the notes in Table 5. *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

26 October 2020

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

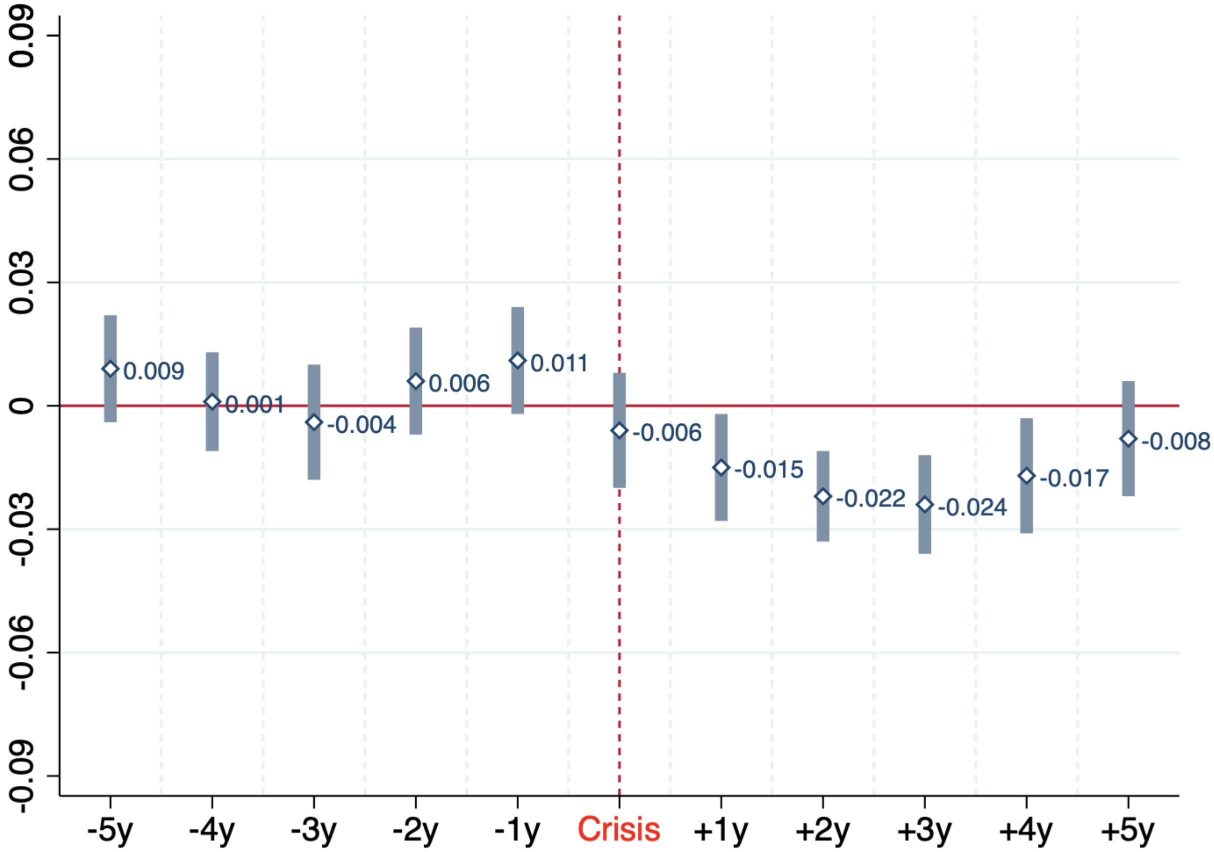


Fig. B1. **Timeline for the effect of a banking crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different types of financial crises. Reform database is obtained by merging two subsets of observations 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.

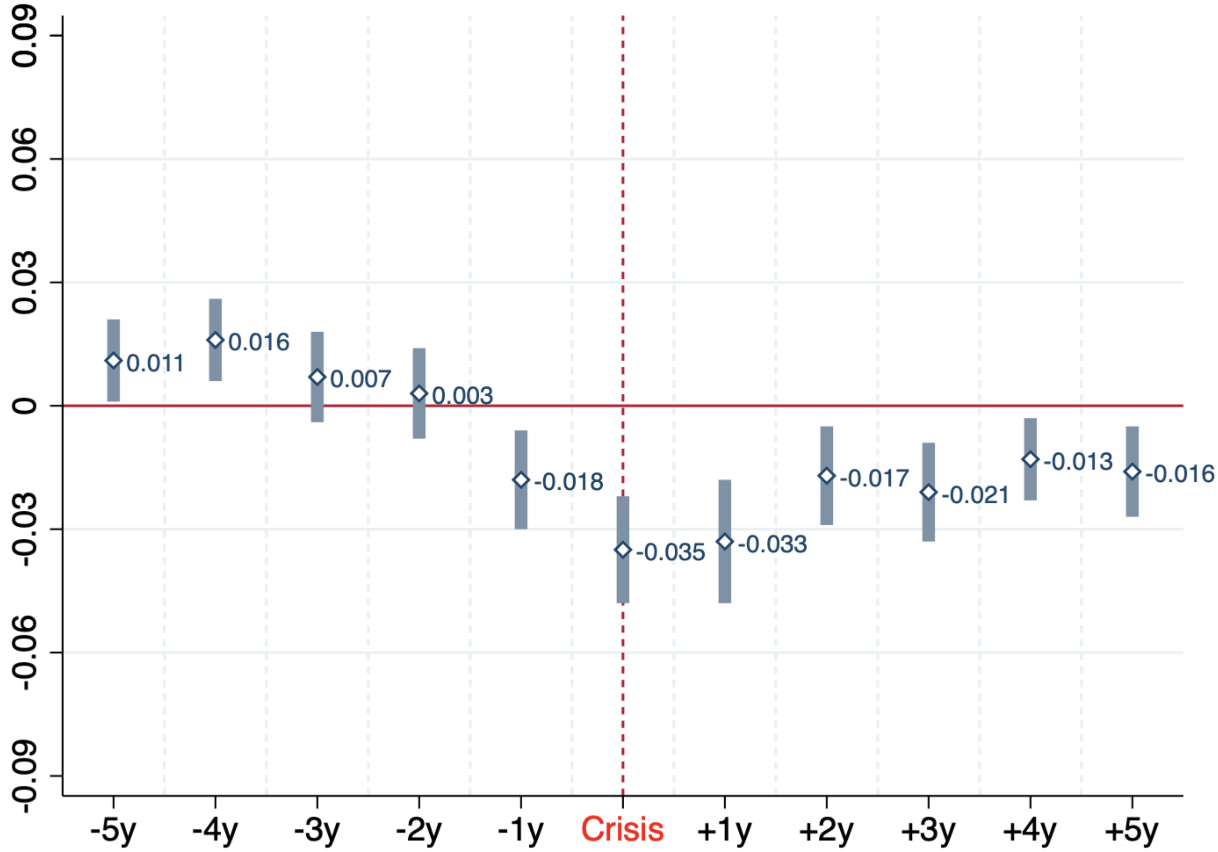


Fig. B2. **Timeline for the effect of a currency crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different types of financial crises. Reform database is obtained by merging two subsets of observations 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.

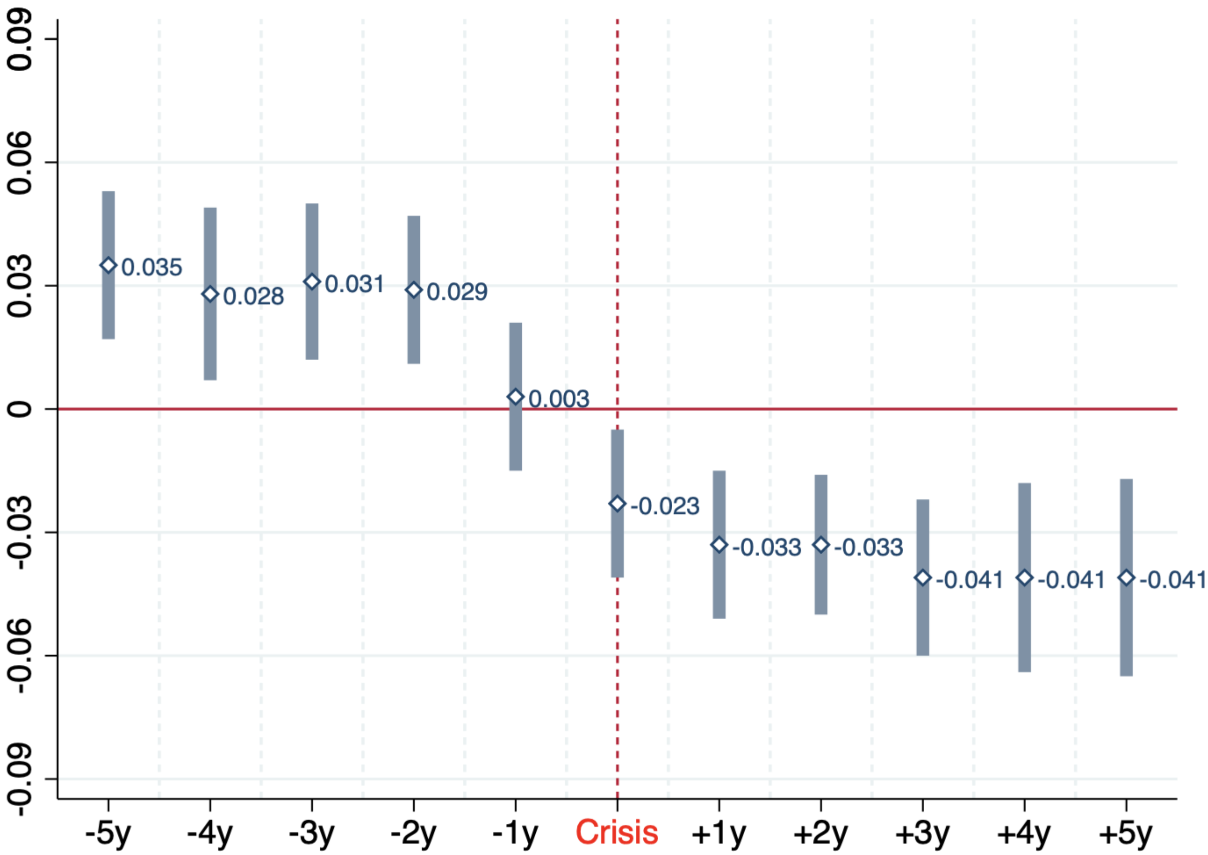
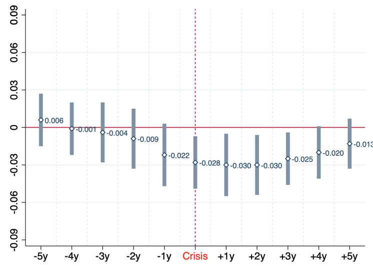
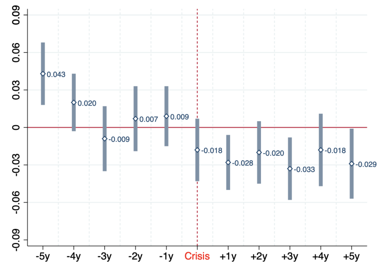


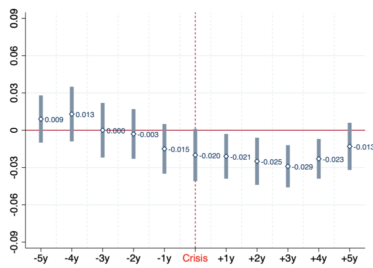
Fig. B3. **Timeline for the effect of a sovereign debt crisis year on average financial liberalization.** The figure plots the estimates for β_τ from the rolling specification in Equation 3 separately estimated for different types of financial crises. Reform database is obtained by merging two subsets 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.



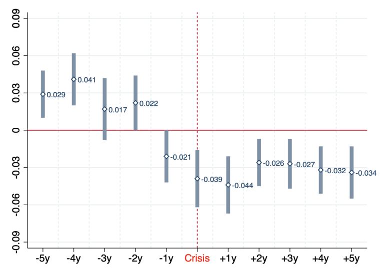
(a) Credit controls



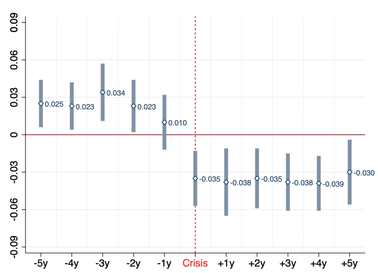
(b) Interest rate controls



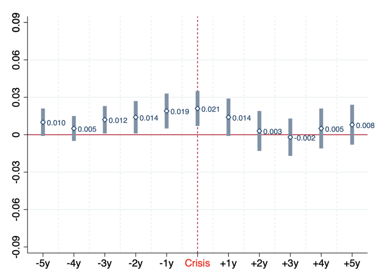
(c) Entry barriers



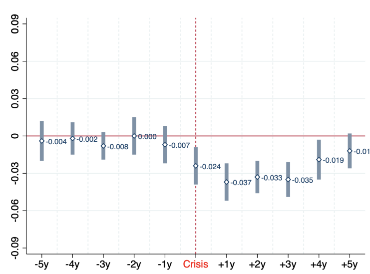
(d) Capital account



(e) Privatization



(f) Banking supervision



(g) Security markets

Fig. B4. **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 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:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.019***	-0.016***	-0.016***	-0.016**	-0.016**
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
PREcrisis	-0.010	0.008	0.008	0.008	0.008
	[0.008]	[0.006]	[0.006]	[0.006]	[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 B1: **Global sample: Difference-in-differences estimates for the effect of a financial crisis on average 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*					*Financial Liberalization*					*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis_banking	-0.031***	-0.026***	-0.026***	-0.026***	-0.026***										
	[0.009]	[0.007]	[0.007]	[0.007]	[0.008]										
PREcrisis_banking	-0.024**	-0.002	-0.002	-0.002	-0.002										
	[0.010]	[0.008]	[0.008]	[0.008]	[0.008]										
POSTcrisis_debt						-0.060***	-0.041***	-0.041***	-0.041***	-0.041***					
						[0.015]	[0.013]	[0.013]	[0.013]	[0.014]					
PREcrisis_debt						0.006	0.029**	0.029**	0.029**	0.029**					
						[0.018]	[0.013]	[0.013]	[0.013]	[0.013]					
POSTcrisis_currency											-0.024***	-0.018**	-0.018**	-0.018**	-0.018**
											[0.008]	[0.007]	[0.007]	[0.008]	[0.008]
PREcrisis_currency											-0.007	0.012	0.012	0.011	0.012
											[0.009]	[0.008]	[0.008]	[0.008]	[0.008]
Diff-in-diff	**-0.007**	**-0.023****	**-0.023****	**-0.023****	**-0.023****	**-0.066*****	**-0.070*****	**-0.070*****	**-0.070*****	**-0.070*****	**-0.016**	**-0.029****	**-0.029****	**-0.029****	**-0.029****
P-value	0.548	0.036	0.037	0.039	0.040	0.000	0.000	0.000	0.000	0.000	0.174	0.014	0.014	0.015	0.015
N	21,538	21,538	21,538	21,538	21,538	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	0.473	0.533	0.745	0.174	0.199	0.473	0.533	0.745
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		
Reform FE	Yes	Yes				Yes	Yes				Yes	Yes			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes			Yes		Yes

Table B2: **Global sample: Difference-in-differences estimates for the effect of banking, sovereign debt and currency crises on average 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. $POSTcrisis_x$ 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 excluding the crisis start-year and ±1 years around it. $PREcrisis_x$ 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 $POSTcrisis_x$ 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). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.025***	-0.017**	-0.017**	-0.017**	-0.017**
	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]
PREcrisis	-0.016*	0.007	0.007	0.007	0.007
	[0.009]	[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 B3: **Global sample: Difference-in-differences estimates for the effect of a financial crisis on average 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- episodes are also dropped. 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*					*Financial Liberalization*					*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis_banking	-0.037***	-0.030***	-0.030***	-0.030***	-0.030***										
	[0.009]	[0.007]	[0.007]	[0.008]	[0.008]										
PREcrisis_banking	-0.030***	-0.007	-0.007	-0.007	-0.007										
	[0.011]	[0.008]	[0.008]	[0.008]	[0.008]										
POSTcrisis_debt						-0.060***	-0.041***	-0.041***	-0.041***	-0.041***					
						[0.015]	[0.013]	[0.013]	[0.013]	[0.014]					
PREcrisis_debt						0.006	0.029**	0.029**	0.029**	0.029**					
						[0.018]	[0.013]	[0.013]	[0.013]	[0.013]					
POSTcrisis_currency											-0.027***	-0.020**	-0.020**	-0.020**	-0.020**
											[0.009]	[0.008]	[0.008]	[0.008]	[0.009]
PREcrisis_currency											-0.011	0.009	0.009	0.009	0.009
											[0.010]	[0.008]	[0.008]	[0.008]	[0.008]
Diff-in-diff	**-0.007**	**-0.023****	**-0.023****	**-0.023****	**-0.023****	**-0.066*****	**-0.070*****	**-0.070*****	**-0.070*****	**-0.070*****	**-0.016**	**-0.029****	**-0.029****	**-0.029****	**-0.029****
P-value	0.549	0.037	0.038	0.040	0.041	0.000	0.000	0.000	0.000	0.000	0.177	0.014	0.014	0.015	0.015
N	21,538	21,538	21,538	21,538	21,538	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	0.473	0.533	0.745	0.174	0.199	0.473	0.533	0.745
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		
Reform FE	Yes	Yes				Yes	Yes				Yes	Yes			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes			Yes		Yes

Table B4: **Global sample: Difference-in-differences estimates for the effect of banking, sovereign debt and currency crises on average 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. $POSTcrisis_x$ 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 excluding the crisis start-year and ± 1 years around it. $PREcrisis_x$ 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- episodes are also dropped. 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 Laeven and Valencia (2018). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.024***	-0.021***	-0.021***	-0.021***	-0.021***
	[0.005]	[0.004]	[0.004]	[0.004]	[0.004]
PREcrisis	0.004	0.005	0.005	0.005	0.004
	[0.007]	[0.005]	[0.005]	[0.005]	[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 B5: **Global sample: Difference-in-differences estimates for the effect of a financial crisis on average financial liberalization (Crises dataset 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:	Financial Liberalization					Financial Liberalization					Financial Liberalization				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis_banking	-0.010	-0.004	-0.005	-0.005	-0.005										
	[0.012]	[0.011]	[0.011]	[0.011]	[0.011]										
PREcrisis_banking	0.015	0.025**	0.025**	0.025**	0.025**										
	[0.012]	[0.010]	[0.010]	[0.010]	[0.010]										
POSTcrisis_domdebt						-0.083***	-0.085***	-0.085***	-0.085***	-0.085***					
						[0.028]	[0.023]	[0.023]	[0.023]	[0.023]					
PREcrisis_domdebt						-0.025	-0.005	-0.005	-0.005	-0.005					
						[0.025]	[0.019]	[0.019]	[0.019]	[0.020]					
POSTcrisis_extdebt											-0.056***	-0.042**	-0.042**	-0.042**	-0.042**
											[0.017]	[0.018]	[0.018]	[0.018]	[0.018]
PREcrisis_extdebt											-0.022	0.006	0.006	0.006	0.006
											[0.019]	[0.017]	[0.018]	[0.018]	[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
Country FE	Yes	Yes	Yes			Yes	Yes	Yes			Yes	Yes	Yes		
Reform FE	Yes	Yes				Yes	Yes				Yes	Yes			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes			Yes		Yes

Table B6a: **Global sample: Difference-in-differences estimates for the effect of banking, domestic debt and external debt crises on average financial liberalization (Crises dataset 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. $POSTcrisis_x$ 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:	*Financial Liberalization*					*Financial Liberalization*					*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis_currency	-0.063***	-0.046***	-0.046***	-0.046***	-0.046***										
	[0.013]	[0.011]	[0.011]	[0.011]	[0.011]										
PREcrisis_currency	-0.040***	-0.020**	-0.020**	-0.020**	-0.020**										
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]										
POSTcrisis_stock						-0.025	-0.001	-0.001	-0.001	-0.001					
						[0.016]	[0.012]	[0.013]	[0.013]	[0.013]					
PREcrisis_stock						-0.013	0.010	0.010	0.010	0.010					
						[0.017]	[0.013]	[0.013]	[0.013]	[0.013]					
POSTcrisis_inflation											-0.086***	-0.070***	-0.070***	-0.070***	-0.070***
											[0.019]	[0.014]	[0.015]	[0.015]	[0.015]
PREcrisis_inflation											-0.066***	-0.043***	-0.043***	-0.043***	-0.043***
											[0.016]	[0.012]	[0.012]	[0.012]	[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		
Reform FE	Yes	Yes				Yes	Yes				Yes	Yes			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes			Yes		Yes

Table B6b: **Global sample: Difference-in-differences estimates for the effect of currency, stock market and inflation crises on average financial liberalization (Crises dataset 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. $POSTcrisis_x$ 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*
POSTcrisis	-0.048***	-0.043***	-0.044***	-0.044***	-0.044***
	[0.010]	[0.009]	[0.009]	[0.009]	[0.009]
PREcrisis	-0.031***	-0.006	-0.006	-0.006	-0.006
	[0.011]	[0.009]	[0.009]	[0.009]	[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 B7: **Global sample: Difference-in-differences estimates for the effect of a financial crisis on average financial liberalization (only with the original 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:	*Financial Liberalization*					*Financial Liberalization*					*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis_banking	-0.035***	-0.031***	-0.031***	-0.031***	-0.031***										
	[0.011]	[0.010]	[0.010]	[0.010]	[0.010]										
PREcrisis_banking	-0.026**	-0.004	-0.004	-0.004	-0.004										
	[0.011]	[0.010]	[0.010]	[0.010]	[0.010]										
POSTcrisis_debt						-0.074***	-0.044**	-0.044**	-0.044**	-0.044**					
						[0.017]	[0.017]	[0.017]	[0.017]	[0.017]					
PREcrisis_debt						-0.015	0.023	0.023	0.023	0.023					
						[0.020]	[0.016]	[0.016]	[0.017]	[0.017]					
POSTcrisis_currency											-0.054***	-0.049***	-0.049***	-0.049***	-0.049***
											[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
PREcrisis_currency											-0.034***	-0.013	-0.013	-0.013	-0.013
											[0.013]	[0.011]	[0.011]	[0.011]	[0.011]
Diff-in-diff	**-0.009**	**-0.027*** **	**-0.027*** **	**-0.027** **	**-0.027** **	**-0.059*** **	**-0.066*** **	**-0.066*** **	**-0.066*** **	**-0.066*** **	**-0.021* **	**-0.036*** **	**-0.036*** **	**-0.036*** **	**-0.036*** **
P-value	0.469	0.010	0.010	0.011	0.011	0.001	0.000	0.000	0.000	0.000	0.062	0.001	0.001	0.001	0.001
N	18,430	18,430	18,430	18,430	18,430	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	0.221	0.243	0.439	0.543	0.739
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		
Reform FE	Yes	Yes				Yes	Yes				Yes	Yes			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes			Yes		Yes

Table B8: **Global sample: Difference-in-differences estimates for the effect of banking, sovereign debt and currency crises on average financial liberalization (only with the original 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. $POSTcrisis_x$ 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*
POSTcrisis_banking	-0.021**	-0.014*	-0.014*	-0.014	-0.014
	[0.010]	[0.008]	[0.008]	[0.008]	[0.009]
PREcrisis_banking	-0.018*	-0.001	-0.001	-0.001	-0.001
	[0.010]	[0.008]	[0.008]	[0.008]	[0.008]
POSTcrisis_debt	-0.042**	-0.025	-0.025	-0.025	-0.025
	[0.018]	[0.017]	[0.017]	[0.017]	[0.017]
PREcrisis_debt	0.002	0.021	0.021	0.021	0.021
	[0.021]	[0.017]	[0.017]	[0.017]	[0.017]
POSTcrisis_currency	-0.044***	-0.035***	-0.035***	-0.036***	-0.035***
	[0.012]	[0.011]	[0.011]	[0.012]	[0.012]
PREcrisis_currency	-0.025**	-0.009	-0.009	-0.009	-0.009
	[0.012]	[0.011]	[0.012]	[0.012]	[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 B9: **Global sample: Difference-in-differences estimates for the effect of banking, sovereign debt and currency crises on average financial liberalization (simultaneous estimation).** 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) 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									
Sample:	*Democratic countries*					*Autocratic countries*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.045***	-0.027***	-0.027***	-0.027***	-0.027***	-0.050**	-0.044*	-0.044*	-0.044*	-0.044*
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]	[0.020]	[0.022]	[0.023]	[0.023]	[0.024]
PREcrisis	-0.022	0.010	0.010	0.010	0.010	-0.040**	-0.028	-0.028	-0.028	-0.028
	[0.013]	[0.010]	[0.010]	[0.010]	[0.010]	[0.014]	[0.016]	[0.017]	[0.017]	[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	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				Yes	Yes				Yes	Yes
Reform x Year FE			Yes		Yes			Yes		Yes

Table C1: **Democracy vs. autocracy (Balanced sample): Difference-in-differences estimates for the effect of a financial crisis on average financial liberalization.** The table summarizes the estimation results over two subsamples 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). 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*									
Sample:	*Democratic countries*					*Autocratic countries*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.045***	-0.022**	-0.022**	-0.022**	-0.022**	-0.053***	-0.037**	-0.037**	-0.037**	-0.037**
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]	[0.015]	[0.015]	[0.016]	[0.016]	[0.016]
PREcrisis	-0.015	0.018*	0.018*	0.018*	0.018*	-0.043***	-0.013	-0.013	-0.013	-0.013
	[0.015]	[0.010]	[0.010]	[0.010]	[0.010]	[0.016]	[0.017]	[0.017]	[0.017]	[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	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				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 the effect of a financial crisis on average financial liberalization.** The table summarizes the estimation results over two subsamples 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). 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:	*Financial Liberalization*									
Sample:	*Democratic countries*					*Autocratic countries*				
Models:	*I*	*II*	*III*	*IV*	*V*	*I*	*II*	*III*	*IV*	*V*
POSTcrisis	-0.045***	-0.029***	-0.029***	-0.029***	-0.029***	-0.044**	-0.038*	-0.038	-0.038*	-0.038
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]	[0.021]	[0.021]	[0.022]	[0.022]	[0.022]
PREcrisis	-0.017	0.011	0.011	0.011	0.011	-0.059***	-0.033*	-0.033*	-0.033*	-0.033*
	[0.013]	[0.009]	[0.009]	[0.009]	[0.009]	[0.020]	[0.017]	[0.018]	[0.018]	[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			
Year FE	Yes	Yes		Yes		Yes	Yes		Yes	
CountryTime Trend		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Country x Reform FE				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 the effect of a financial crisis on average financial liberalization.** The table summarizes the estimation results over two subsamples 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). 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:	*Financial Liberalization*					
Sample:	*Democracy*		*Semi-Democracy*		*Autocracy*	
Models:	*I*	*V*	*I*	*V*	*I*	*V*
POSTcrisis	-0.048***	-0.018	-0.034*	-0.020	-0.043**	-0.028*
	[0.012]	[0.011]	[0.019]	[0.017]	[0.017]	[0.016]
PREcrisis	0.001	0.022	-0.009	0.010	-0.040**	-0.020
	[0.021]	[0.017]	[0.017]	[0.013]	[0.017]	[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 (constructed via Polity5): Difference-in-differences estimates for the effect of a financial crisis on average financial liberalization.** The table summarizes the estimation results over two subsamples 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). 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.

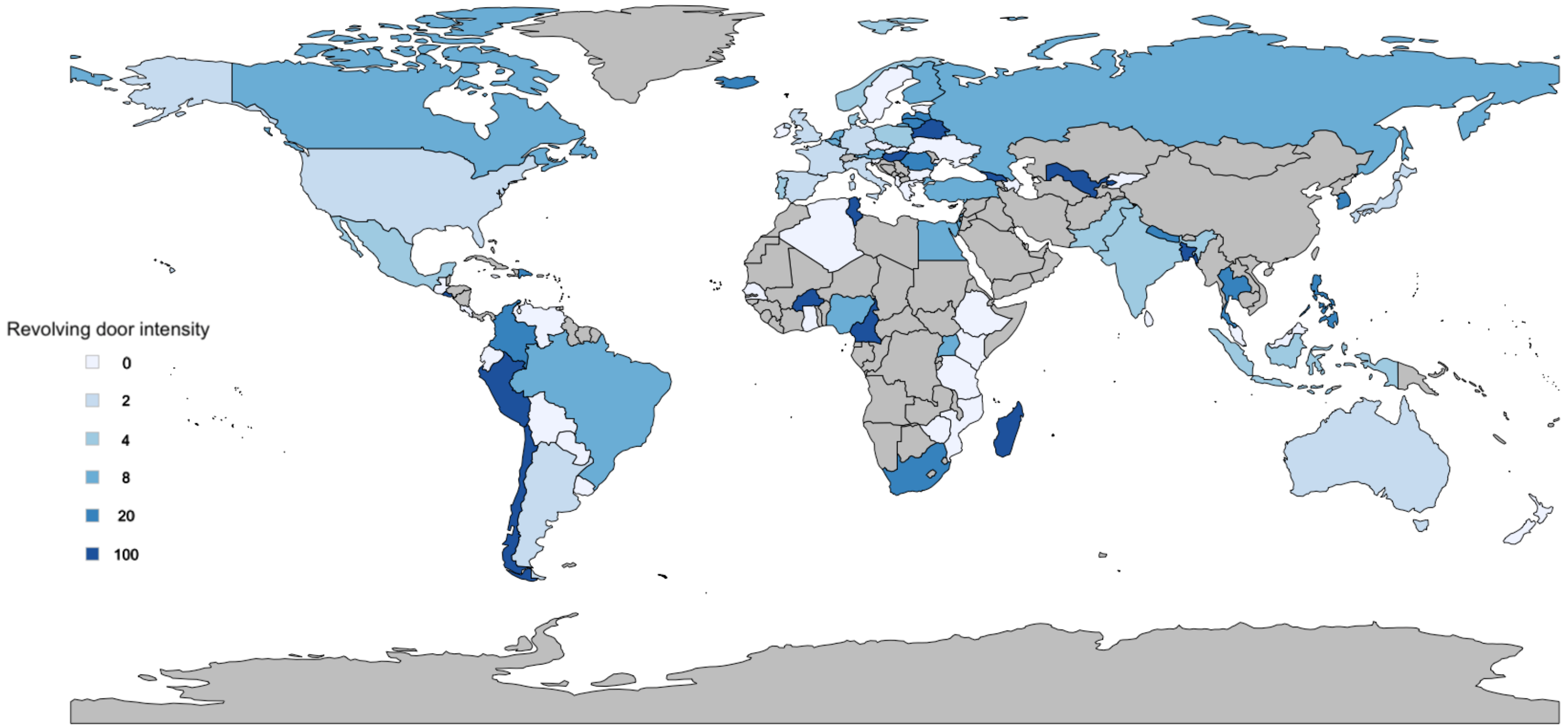


Fig. D1. **Revolving doors between financial and political institutions across the globe: Continuous version.** The figure maps each country depending on the fraction of its politically-connected banks which is the number of banks with at least one former politician on the board of directors divided by the number of banks for which there are data on board members as of year 2006. The measures are obtained from Braun and Raddatz (2010).

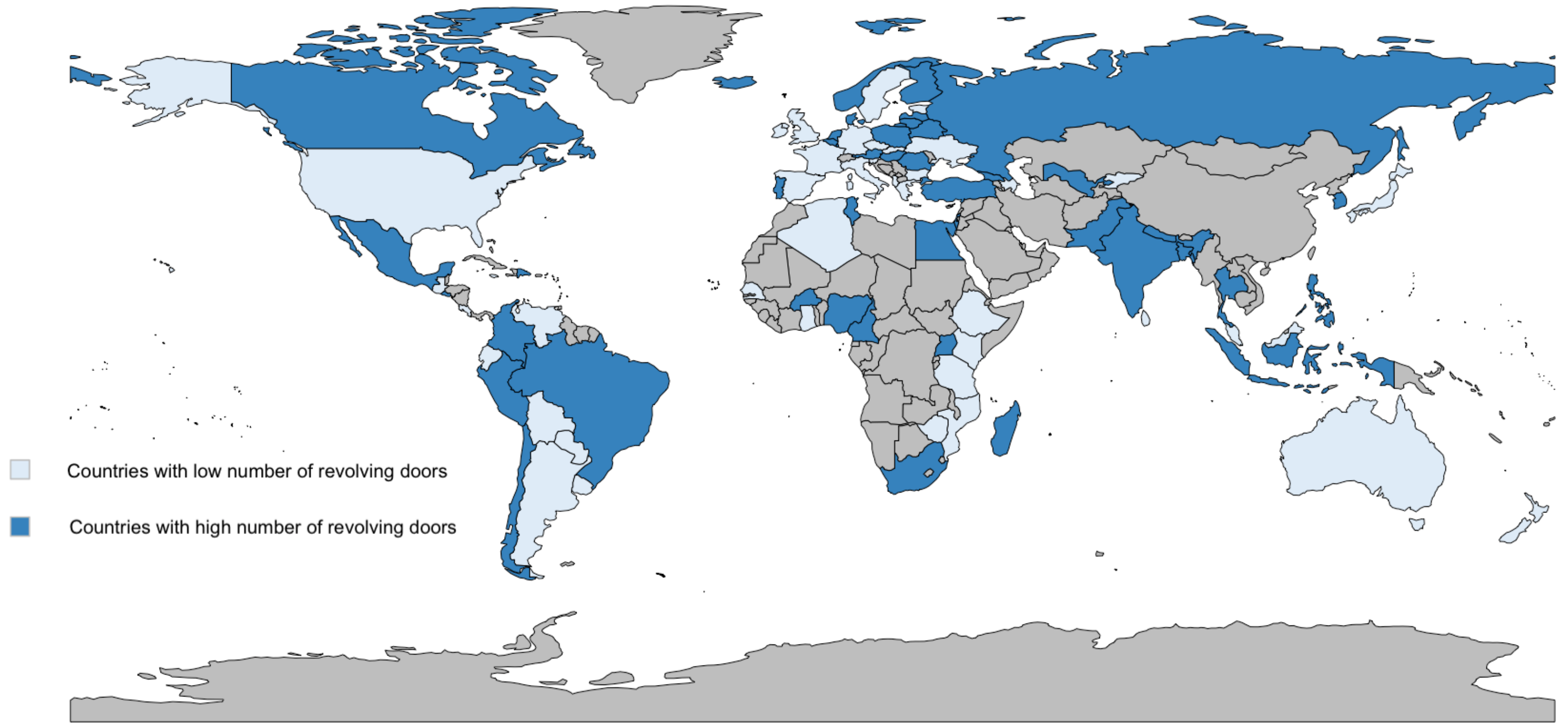


Fig. D2. **Revolving doors between financial and political institutions across the globe: Dichotomous version.** The figure maps each country into one of the two categories (with high or low number of revolving doors) depending on the fraction of its politically-connected banks which is the number of 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 as of year 2006. The measures are obtained from Braun and Raddatz (2010).

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.044**	-0.063***	-0.062***	-0.063***	-0.063***
	[0.019]	[0.020]	[0.020]	[0.021]	[0.021]
PREcrisis x TermLimit	0.034	0.019	0.019	0.019	0.019
	[0.027]	[0.027]	[0.028]	[0.028]	[0.028]
POSTcrisis	-0.037***	-0.009	-0.009	-0.009	-0.009
	[0.011]	[0.009]	[0.009]	[0.009]	[0.009]
PREcrisis	-0.023	0.016	0.016	0.016	0.016
	[0.014]	[0.010]	[0.010]	[0.010]	[0.010]
TermLimit	-0.049	-0.031	-0.032	-0.031	-0.032
	[0.038]	[0.034]	[0.034]	[0.034]	[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.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.066***	-0.066***	-0.066***	-0.067***	-0.067***
	[0.025]	[0.025]	[0.025]	[0.025]	[0.025]
PREcrisis x TermLimit	0.017	0.015	0.016	0.017	0.016
	[0.030]	[0.029]	[0.030]	[0.029]	[0.029]
POSTcrisis	-0.010	-0.010	-0.010	-0.009	-0.010
	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
PREcrisis	0.018*	0.020*	0.020*	0.019*	0.019*
	[0.011]	[0.010]	[0.010]	[0.010]	[0.010]
TermLimit	-0.010	-0.006	-0.005	-0.005	-0.004
	[0.036]	[0.038]	[0.037]	[0.037]	[0.037]
Right	0.026**	0.025**	0.025**	0.026**	0.026**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]
Left	0.014	0.011	0.011	0.010	0.010
	[0.012]	[0.012]	[0.012]	[0.011]	[0.011]
Presidential		-0.029	-0.030	-0.032	-0.031
		[0.028]	[0.029]	[0.030]	[0.030]
Parliamentary		0.028	0.028	0.027	0.028
		[0.030]	[0.031]	[0.031]	[0.031]
OfficeYears			0.000	0.000	0.000
			[0.001]	[0.001]	[0.001]
YearsLeft			0.001	0.001	0.001
			[0.001]	[0.001]	[0.001]
HerfGov				0.605	0.467
				[1.146]	[1.317]
GovFrac				0.565	0.435
				[1.135]	[1.301]
GovShare					-0.008
					[0.037]
Checks					-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
CountryTime Trend	Yes	Yes	Yes	Yes	Yes
Country x Reform FE	Yes	Yes	Yes	Yes	Yes
Reform x Year FE	Yes	Yes	Yes	Yes	Yes

Table D2: **Term limits in democracies with political controls and fully saturated specification: Difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.066**	-0.091***	-0.101***	-0.105***	-0.106***
	[0.026]	[0.026]	[0.027]	[0.026]	[0.026]
PREcrisis x TermLimit	0.017	0.014	0.014	0.014	0.014
	[0.029]	[0.029]	[0.030]	[0.029]	[0.029]
POSTcrisis x Right	0.004	0.010	0.008	0.008	0.010
	[0.017]	[0.016]	[0.016]	[0.016]	[0.016]
POSTcrisis x Left	-0.004	0.001	-0.002	-0.002	-0.001
	[0.019]	[0.019]	[0.019]	[0.018]	[0.017]
POSTcrisis x Presidential		0.046	0.049	0.054	0.057*
		[0.037]	[0.034]	[0.034]	[0.032]
POSTcrisis x Parliamentary		0.012	0.007	0.011	0.012
		[0.037]	[0.035]	[0.035]	[0.034]
POSTcrisis x OfficeYears			-0.002	-0.002	-0.002
			[0.001]	[0.001]	[0.001]
POSTcrisis x YearsLeft			-0.002	-0.002	-0.003
			[0.002]	[0.002]	[0.002]
POSTcrisis x HerfGov				0.943	1.589
				[1.855]	[1.983]
POSTcrisis x GovFrac				0.932	1.567
				[1.833]	[1.958]
POSTcrisis x GovShare					0.049
					[0.044]
POSTcrisis x Checks					0.001
					[0.004]
All baseline controls	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
CountryTime Trend	Yes	Yes	Yes	Yes	Yes
Country x Reform FE	Yes	Yes	Yes	Yes	Yes
Reform x Year FE	Yes	Yes	Yes	Yes	Yes

Table D3: **Term limits in democracies with interacted political controls and fully saturated specification: Difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*		
Models:	*No controls*	*Full controls*	*Oster test*
POSTcrisis x TermLimit	-0.067***	-0.105***	
	[0.025]	[0.026]	
PREcrisis x TermLimit	Yes	Yes	
Political interactions	No	Yes	
Baseline political controls	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 (δ=1, Rmax=1.3*R)			**(-0.067, -0.105)**
Treatment effect excludes 0			**Yes**
Delta (Rmax=1.3*R)			**5.70**

Table D4: **Robustness to omitted variables bias.** 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). *TermLimit* as well as other political variables come from the Database of Political Institutions (DPI). *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.023	-0.050***	-0.049***	-0.050***	-0.049***
	[0.016]	[0.018]	[0.018]	[0.019]	[0.019]
PREcrisis x TermLimit	0.053*	0.032	0.032	0.032	0.032
	[0.027]	[0.033]	[0.034]	[0.034]	[0.034]
POSTcrisis	-0.042***	-0.008	-0.008	-0.008	-0.008
	[0.012]	[0.009]	[0.009]	[0.009]	[0.009]
PREcrisis	-0.022	0.018*	0.017*	0.018*	0.017*
	[0.015]	[0.010]	[0.010]	[0.010]	[0.010]
TermLimit	-0.036	-0.008	-0.008	-0.008	-0.008
	[0.030]	[0.030]	[0.030]	[0.030]	[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 D5: **Term limits in democracies: Within-party difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.045**	-0.064***	-0.077***	-0.078***	-0.072***
	[0.019]	[0.021]	[0.022]	[0.022]	[0.021]
PREcrisis x TermLimit	0.039	0.030	0.024	0.027	0.028
	[0.028]	[0.029]	[0.031]	[0.031]	[0.028]
POSTcrisis x Right	-0.006	-0.003	-0.005	-0.009	-0.008
	[0.020]	[0.019]	[0.018]	[0.018]	[0.017]
POSTcrisis x Left	-0.004	0.002	-0.002	-0.009	-0.007
	[0.021]	[0.020]	[0.019]	[0.017]	[0.018]
POSTcrisis x Presidential		0.036	0.044	0.044	0.036
		[0.043]	[0.041]	[0.040]	[0.041]
POSTcrisis x Parliamentary		0.005	0.003	0.014	0.005
		[0.040]	[0.038]	[0.036]	[0.038]
POSTcrisis x OfficeYears			-0.002	-0.002	-0.002
			[0.001]	[0.001]	[0.001]
POSTcrisis x YearsLeft			-0.002	-0.003	-0.003
			[0.003]	[0.003]	[0.002]
POSTcrisis x HerfGov				2.091	3.333*
				[1.508]	[1.684]
POSTcrisis x GovFrac				2.028	3.230*
				[1.479]	[1.652]
POSTcrisis x GovShare					0.079
					[0.052]
POSTcrisis x Checks					0.010**
					[0.004]
PREcrisis interactions	Yes	Yes	Yes	Yes	Yes
Baseline controls	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 D6: **Term limits in democracies with interacted political controls: Within-party difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.025	-0.047***	-0.047***	-0.047***	-0.047***
	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]
PREcrisis x TermLimit	0.061**	0.043	0.043	0.044	0.044
	[0.029]	[0.031]	[0.032]	[0.032]	[0.032]
POSTcrisis	-0.018**	-0.007	-0.007	-0.007	-0.007
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
PREcrisis	0.002	0.011	0.011	0.011	0.011
	[0.009]	[0.011]	[0.011]	[0.011]	[0.011]
TermLimit	-0.012	-0.006	-0.006	-0.007	-0.007
	[0.020]	[0.022]	[0.022]	[0.023]	[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 D7: **Term limits in democracies: Within-party-decade difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*				
Models:	*I*	*II*	*III*	*IV*	*V*
POSTcrisis x TermLimit	-0.049***	-0.057***	-0.061***	-0.061***	-0.061***
	[0.018]	[0.019]	[0.019]	[0.018]	[0.018]
PREcrisis x TermLimit	0.054*	0.053*	0.053*	0.053*	0.053*
	[0.029]	[0.030]	[0.031]	[0.030]	[0.027]
POSTcrisis x Right	-0.006	0.000	0.001	0.000	0.001
	[0.018]	[0.019]	[0.019]	[0.019]	[0.019]
POSTcrisis x Left	-0.001	0.007	0.005	0.006	0.010
	[0.017]	[0.019]	[0.019]	[0.019]	[0.019]
POSTcrisis x Presidential		-0.016	-0.013	-0.014	-0.017
		[0.028]	[0.027]	[0.026]	[0.026]
POSTcrisis x Parliamentary		-0.039	-0.039	-0.034	-0.042*
		[0.029]	[0.027]	[0.026]	[0.025]
POSTcrisis x OfficeYears			-0.002	-0.002	-0.002
			[0.001]	[0.001]	[0.001]
POSTcrisis x YearsLeft			0.003	0.003	0.002
			[0.002]	[0.002]	[0.002]
POSTcrisis x HerfGov				2.359*	3.780**
				[1.265]	[1.507]
POSTcrisis x GovFrac				2.308*	3.694**
				[1.241]	[1.479]
POSTcrisis x GovShare					0.091**
					[0.041]
POSTcrisis x Checks					0.008**
					[0.004]
PREcrisis interactions	Yes	Yes	Yes	Yes	Yes
Baseline controls	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 D8: **Term limits in democracies with interacted political controls: Within-party-decade difference-in-differences estimates.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

Dependent variable:	*Financial Liberalization*		
Models:	*I*	*II*	*III*
POSTcrisis x TermLimit	-0.037	-0.055*	-0.017
	[0.023]	[0.030]	[0.028]
PREcrisis x TermLimit	0.019	0.021	0.042
	[0.029]	[0.033]	[0.029]
POSTcrisis	-0.014	0.005	-6.821***
	[0.026]	[0.025]	[2.182]
PREcrisis	0.020	0.027	-6.804**
	[0.022]	[0.026]	[2.737]
TermLimit	-0.047	-0.020	-0.042
	[0.030]	[0.040]	[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
Country FE	Yes	Yes	Yes
Reform FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
CountryTime Trend	Yes	Yes	Yes
Baseline political controls	No	Yes	Yes
Interacted political controls	No	No	Yes

Table D9: **Term limits in democracies: Countries with term-limit experience.** See the notes in Table 5. *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*
POSTcrisis x TermLimit	-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]
PREcrisis x TermLimit	0.040	0.021	-0.003	-0.069	0.037	-0.017	0.057
	[0.047]	[0.065]	[0.061]	[0.056]	[0.060]	[0.039]	[0.037]
All controls (Table 7)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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
CountryTime Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table D10: **Policy domains: Term limits in democracies with interacted political controls.** See the notes in Table 5. *p<0.1, **p<0.05, ***p<0.01.

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